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The Freedom to Choose: Theory and Quasi-Experimental Evidence on Cash Transfer Restrictions

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Should cash transfer programmes restrict consumer choice? For example, should food assistance delivered in cash be restricted to food and exclude temptation goods? Theoretically, if transfers are extra-marginal, restrictions induce (1) a substitution effect away from restricted goods and (2) a negative wealth effect if transfer recipients resell unrestricted goods at a loss to access restricted goods. The welfare impact on transfer recipients is negative. We test and corroborate these predictions by exploiting a natural experiment in a refugee settlement in Kenya, where some refugees receive monthly cash transfers restricted to food while others get unrestricted cash transfers.

Keywords: Cash transfers, Vouchers, Restrictions, Debt

JEL Classifications: O12, O15, I38, G51

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Billions of dollars of cash-based assistance are distributed yearly in the form of vouchers or digital cash transfers with restrictions on the types of goods that can be purchased (Gentilini et al., 2020; Girling and Urquhart, 2021).[1] For example, in 2019, about half of the cash-based assistance distributed by the World Food Programme (WFP) – nearly USD 1 billion – was distributed with restrictions (in comparison, the NGO GiveDirectly distributed about USD 34 million worth of unrestricted cash transfers in 2019). In the US, the Supplemental Nutrition Assistance Program (SNAP) is distributing electronic cash transfers restricted to food (excluding alcohol and tobacco), reaching 40 million Americans in 2018 with total benefits amounting to USD 61 billion. Restricted cash transfers are also used to encourage more ecological consumption (e.g. EcoCheque programme in Belgium).[2]

The impact of these restrictions is not well understood, especially for poor households who are likely to benefit most from assistance. How do restrictions actually affect the consumer choices of transfer recipients? Do recipients comply with restrictions or, instead, do they find ways to circumvent them? Are restrictions welfare-improving or welfare-decreasing? Theoretically, restrictions only affect consumer choices if transfers are extra-marginal (i.e. binding) and resale of unrestricted goods is costly (Southworth, 1945). If both conditions are satisfied, restrictions induce a substitution effect away from restricted goods and a negative wealth effect, leaving consumers worse off. If transfers are infra-marginal or resale is costless, restrictions should have no impact.

Empirical evidence on the impact of cash transfer restrictions is surprisingly scarce. A key gap in the literature is that most empirical studies focus on infra-marginal transfers, implying that impacts are limited. Evidence on the US Food Stamp program shows that transfers are infra-marginal and households respond similarly to cash income and food stamps (Hoynes and Schanzenbach, 2009; Hastings and Shapiro, 2018). In developing countries, Hidrobo et al. (2014, 2016) compares infra-marginal cash, in-kind, and voucher transfers and show that the different modalities had similar impacts on the value of food, non-food, and total consumption as well as on intimate partner violence. Only Aker et al. (2016) assessed the relative impact of cash transfers and vouchers in a context where transfers were extra-marginal; yet, her study found no difference in food consumption, asset ownership, and other measures of well-being, in part due to the fact that voucher households were able to resell part of what they purchased.[3] To the best

[1] The term "restriction" is borrowed from Nichols and Zeckhauser (1982)'s study of transfer targeting through restrictions. Restrictions should not be mixed up with conditionalities: while conditionalities determine whether households can receive the transfers or not, restrictions determine how transfers can be used.

[2] For providers, different motives can justify the imposition of restrictions, including targeting (Nichols and Zeckhauser, 1982; Blackorby and Donaldson, 1988), political feasibility (De Janvry et al., 1991), and paternalism (Cunha, 2014).

[3] Another limitation of existing studies is that the voucher programs differ from cash transfers in many

of our knowledge, the theory of Southworth (1945) has never been tested in a context where restrictions actually matter.

Our paper fills this gap by providing robust evidence on the net effect of cash transfer restrictions, in a context where cash transfers are extra-marginal and the resale of unrestricted goods is costly. Studying extra-marginal transfers is more important, both from an academic and from a policy point of view. From an academic standpoint, studying extra-marginal transfers is more relevant for theory testing. From a policy standpoint, cash transfers are extra-marginal if beneficiaries are extremely poor, if transfers are large, or both, implying that minor differences in cash transfer modalities can have important real-life consequences for transfer recipients. Our research is particularly relevant for humanitarian contexts, where most cash-based interventions are expected to be extra-marginal.[4]

Our empirical analysis exploits a natural experiment in the Kalobeyei settlement in Kenya. Since the creation of the Kalobeyei settlement in 2016, WFP has been distributing food assistance to refugees using cash transfers. Each refugee receives USD 14 monthly, which should in theory be sufficient to afford 2,100kcal per day. Until mid-2019, all refugee households were receiving mobile-money transfers restricted to food items, excluding alcohol, tobacco, and non-food items. In June 2019, the restriction was lifted for 1,050 households living in one geographically bounded part of the settlement. Meanwhile, the settlement's remaining 7,000 households continued to benefit from the restricted programme. We show that assignment to the policy change was quasi-random and exploit this natural experiment to assess the impact of cash transfer restrictions.

Our empirical results are consistent with theory. First, we document the existence of a resale shadow market for food items in which a large proportion of households receiving restricted transfers resell or exchange basic food items in order to access cash or non-food items. Buyers in the resale market are either the minority of refugees who have an income or the host population. Median prices in the resale market are 18 to 38% lower than retail market prices, implying that households selling goods in the resale shadow market incur large losses. Households benefiting from unrestricted cash transfers are significantly less likely to resell food items in the resale shadow market.

more ways than a restriction on what products beneficiaries can purchase. In Aker (2017), voucher recipients could only purchase at pre-organised fairs whereas cash recipients could spend transfer money anytime and anywhere. In Hidrobo et al. (2014), households who received vouchers could only redeem their voucher twice a month in supermarkets and had to redeem vouchers within 30 days, whereas cash recipients did not have any of those restrictions. As a result, the net effect of restrictions cannot be clearly isolated from these studies.

[4]The importance of cash-based humanitarian assistance has more than doubled over the past five years, reaching 6.3 billion USD in 2020. About a third is provided as vouchers (Girling and Urquhart, 2021).

Second, households receiving cash transfers restricted to food do not have significantly better nutrition outcomes. This result suggests that one of the key objectives of the restriction - to improve food security - is not achieved. We nevertheless find that the restriction works, in the sense that households receiving restricted transfers have lower non-food expenditure and lower expenditure on temptation goods (alcohol, tobacco, and restaurants). We also find suggestive evidence that households receiving restricted transfers have lower levels of asset ownership and asset purchases.

Finally, households receiving unrestricted cash transfers report significantly higher levels of subjective well-being compared to those facing restrictions. Overall, our results suggest that the switch from restricted to unrestricted cash transfers was welfare enhancing for refugees.

We uncover heterogeneity in effects by household indebtedness – an issue faced by 89 percent of households. Ethnographic data reveal that vulnerable households often purchase food from shopkeepers on credit, especially when they face economic shocks due to medical emergencies, loss of assets to theft or disaster, price fluctuations, and delays of the cash transfers themselves. Because of debt, households become dependent on their creditor, who holds their SIM or ATM cards as a guarantee. We find that the effects of lifting the cash transfer restrictions are significantly larger for households that are not dependent on their food retailer, because they can more easily access cash. These results suggest that addressing indebtedness is essential to harness the full potential of unrestricted cash transfers.

Our study speaks to three strands of literature. First, it contributes to the literature on cash-based assistance, which overwhelmingly finds that cash transfers have positive and long-lasting effects on a wide variety of outcomes, from household expenditure and health to employment and empowerment (see e.g. Bastagli et al. 2016 for a review). Part of this literature examines the relative effects of different transfer modalities[5] (see e.g. Gentilini 2016 for a review) and of conditionalities (see e.g. Gertler 2004; Baird et al. 2011; Attanasio et al. 2015; Duflo et al. 2015).[6] Our paper contributes to this literature by studying theoretically and empirically the impact of restrictions on how cash transfers

[5]Cash-based assistance includes various payment modalities, from hard cash and digital cash transfers, to electronic or paper vouchers. Conceptually, these modalities differ in three main ways: (1) what goods can be purchased with the transfers, (2) where the transfers can be used, and (3) when the transfers can be used. Contrarily to hard cash, vouchers are usually restricted to certain types of purchases, they can only be used at certain times and places, and they may have an expiry date. Depending on their design, digital cash transfers can be very similar to hard cash transfers – with practically no restriction on how cash can be spent – or closer to vouchers, with restrictions on how, where, and when digital money can be spent. In this paper, the digital cash transfer that we are examining is more akin to a hard cash transfer, except that some households face a restriction on what goods can be purchased.

[6]We emphasize that restrictions should not be mixed up with conditionalities. Restrictions and conditionalities differ in their purpose and mechanism of action: while conditionalities determine whether households can receive the transfers or not, restrictions determine how transfers can be used.

can be spent in a context where cash transfers are extra-marginal. Our paper also contributes to the recent literature on the externalities of cash transfer programmes (Angelucci and De Giorgi, 2009; D'Aoust et al., 2018; Cunha et al., 2019; Delius and Sterck, 2019; Egger et al., 2019) by showing that cash transfers that are restricted to certain goods may generate spillovers through the resale market.

Our study also contributes to the literature on vulnerability and coping strategies by examining the relationship between shocks, cash transfers, and debt. Poor households are highly vulnerable to negative shocks (Morduch, 1994), and borrowing from informal lenders, merchants, and family members is one of the main coping strategies against shocks documented in poor settings (Corbett, 1988; Udry, 1994; Dercon, 2002; Fafchamps and Lund, 2003; Santos and Barrett, 2011). Cash transfers could affect indebtedness in two opposite ways. While some households have been observed to spend the extra income from cash transfers to pay off debts or reduce demand for loans (Angelucci et al., 2012; Aker, 2017; Hoddinott et al., 2018), others could also be perceived to be more creditworthy and subsequently take out more loans (Merttens et al., 2013; Merttens and Jones, 2014; Angelucci, 2015; Gazeaud et al., 2021; Torkelson, 2020). Our research contributes to this literature by considering debt as one factor that can undermine the effectiveness of cash transfers.

Lastly, our study speaks to the literature on humanitarian aid and refugee economies. A growing body of literature examines the economic impact of hosting displaced persons on local economies (Maystadt et al., 2019; Verme and Schuettler, 2021).[7] There have been comparatively fewer studies studying the impact of humanitarian and development assistance on refugees themselves. A series of recent studies examined the impact of different modalities of assistance on households (Hidrobo et al., 2014; Altindag and O'Connell, 2020; MacPherson and Sterck, 2021) and businesses (Delius and Sterck, 2019). We contribute to this literature by studying the relative impacts of two modalities of assistance to refugees and by highlighting the interdependence between aid, debt, and markets in a humanitarian context.

The remainder of the paper is structured as follows. Section 1 presents the theoretical model. Section 2 describes the context, the cash transfer programme, and the data. Section 3 introduces the quasi-experimental design and the identification strategy. Section 4 discusses the results. Section 5 shows that results are robust to various tests. Section 6 examines the issue of household debt and its interaction with the programmes. Section 7 concludes.

[7] For example, the effect of the inflow of refugees on the housing market (Depetris-Chauvin and Santos, 2018; Rozo and Sviatschi, 2020), firm productivity (Altındağ et al., 2020), labour market (Tumen, 2016; Clemens and Hunt, 2019; Fallah et al., 2019; Akgündüz and Torun, 2020; Brell et al., 2020), or agricultural production (Alix-Garcia et al., 2018).

1 Theoretical model

We study the decision-making process of households benefiting from an cash transfer programme. We explore in particular how their decisions are affected by restrictions on how cash transfers can be spent. We focus on extra-marginal transfers and extend the theoretical framework proposed by Southworth (1945) to allow for the resale of unrestricted goods in a shadow market. One interesting contribution of our model is to explain why two different sets of price may co-exist in a local economy that benefits from restricted cash transfers: high prices in the retail market and low prices in the shadow market for the resale of unrestricted goods.

We proceed in three steps. We first introduce and solve a basic model of cash transfer programme that is unrestricted (Section 1.1). We then build on this basic model to study the case of a cash transfer programme that is restricted to one type of good (Section 1.2). Finally, we compare the outcomes of the two model versions and generate testable predictions that will be examined in the empirical analysis (Section 1.3). We also briefly discuss how predictions are affected by changes in the model hypotheses.

1.1 A model of unrestricted cash transfers

We consider a local economy with n households and two types of good, which are denoted A and B. The n households are split into two groups. On the one hand, n^a *assisted* households fully rely on a cash assistance programme to survive. They receive a cash transfer of value t, which can be spent on both types of goods. On the other hand, n^e households are *employed*. They earn a wage w but do not benefit from the cash transfer programme. We use the superscript a and e to refer to assisted and employed households respectively. The income of households is denoted y, with $y = t$ for assisted households and $y = w$ for employed households.

Households maximise a utility function which, for simplicity, is assumed to be a Cobb-Douglas (the implications of this hypothesis are discussed in Section 1.3):

$$u = log(c_A^i) + \gamma log(c_B^i) \text{ with } i = a, e \tag{1}$$

where c_j^i is the quantity of good j consumed by a household of type i and γ is a preference parameter for the good B. The goods A and B are sold by local shops at prices p_j with $j = A, B$. Retail prices in the local economy are assumed to be exogenous.[8] We denote

[8]We assume that retail prices are determined at the global level and the local economy is too small to

b_j^i the quantity of good i purchased in the retail market, with $i = a, e$ and $j = A, B$.

In this simple framework, households simply buy the optimal quantity they want to consume in the retail market ($b_j^i = c_j^i$). The budget constraint of households is: $y \geq c_A^i p_A + c_B^i p_B$.

The maximisation of the utility function yields the following equilibrium quantities:

$$c_A^i = \frac{y}{p_A(1+\gamma)}, \tag{2}$$

$$c_B^i = \frac{\gamma y}{p_B(1+\gamma)}. \tag{3}$$

1.2 A model of restricted cash transfers

We adapt this basic model to study the case of a cash transfer programme that is restricted to the good A. Because of the restriction, assisted households cannot use the cash transfer money to directly purchase the good B. As their only income source is the cash transfer programme, they cannot consume any unit of B without any possibility to circumvent the restriction, implying that their utility tends to minus infinity.

We therefore allow for the existence of a resale market, and study under which conditions the resale market is functional (i.e. there is a positive demand and a positive supply). In this resale market, households can resell units of the good A previously purchased with money from the cash transfer programme. The cash from the resale can then be used to purchase the good B in the retail market. The price in the resale market, denoted q_A, is determined endogenously such that the supply equals the demand. We denote d_A^i the quantity demanded by a household of type i in the resale market and s_A^i the quantity supplied, with $i = a, e$.

We first solve the model assuming the equilibrium price in the resale market is below the retail price ($q_A^* < p_A$) and then explore the other case ($q_A^* \geq p_A$).

Solution of the model if $q_A^* < p_A$

In order to solve the model, we first derive the demand function in the resale market by solving the optimisation programme for assisted households. We then derive the supply function in the resale market by solving the optimisation programme for employed households. Finally, we equal the total demand and total supply functions to obtain

affect these prices.

the equilibrium price in the resale market. This value is then used to calculate optimal quantities consumed at equilibrium.

Assisted households Given the restriction and the absence of other income, assisted households have no other choice than to use all their cash transfer money to buy a quantity $b_A^a = t/p_A$ of good A. They can then resell a quantity s_A^a at price q_A to obtain cash. The cash – an amount $s_A^a q_A$ – is used to buy and consume a quantity b_B^a of good B at price p_B. We therefore have $c_a^a = b_A^a - s_A^a = t/p_A - s_A^a$ and $c_B^a = b_B^a = s_A^a q_A / p_B$. The budget constraint of assisted households is given by:

$$\begin{aligned} t &\geq b_A^a p_A \\ &\geq c_A^a p_A + c_B^a p_B \frac{p_A}{q_A}. \end{aligned} \tag{4}$$

This latter equation shows that one consequence of restrictions is to increase the implicit price of the good B, which is multiplied by the ratio p_A/q_A (remember this section builds on the assumption $q_A < p_A$). We solve the maximisation programme of assisted households and obtain optimal quantities:

$$c_A^a = \frac{t}{p_A(1+\gamma)}, \tag{5}$$

$$c_B^a = \frac{q_A}{p_A} \frac{\gamma t}{p_B(1+\gamma)}. \tag{6}$$

We plug this latter equation into the equation $c_B^a = s_A^a q_A / p_B$ to obtain the individual supply of an assisted household in the resale market:

$$s_A^a = c_B^a p_B / q_A = \frac{\gamma t}{p_A(1+\gamma)}. \tag{7}$$

We note that the supply function is perfectly inelastic (i.e. it does not depend on q_A). The total quantity supplied in the resale market of good A is $n^a s_A^a$.

Employed households The wage of employed households can be used to purchase some good A in the retail or the resale market and some good B in the retail market. Because this section builds on the assumption $q_A < p_A$, utility maximisation implies that the good A is bought in the resale market ($d_A^e > 0$ and $b_A^e = 0$). The quantities

consumed by employed households are given by: $c_A^e = d_A^e$ and $c_B^e = b_B^e = (w - d_A^e q_A)/p_B$. We plug these quantities into the utility function and find that the optimal demand in the resale market is:

$$d_A^e = \frac{w}{q_A(1+\gamma)}. \tag{8}$$

The total quantity demanded in the resale market is then given by: $n^e d_A^e$.

Equilibrium We calculate the equilibrium price in the resale market by equalling the total demand from employed households with the total supply by assisted households:

$$q_a^* = \frac{n^e w p_A}{n^a t \gamma}. \tag{9}$$

The equilibrium price in the resale market is an increasing function of the proportion of employed households in the population and of their wages, as these factors increase the demand. The equilibrium price is increasing in the price of the good A in the retail market, as this factor reduces supply. Finally, the equilibrium price is decreasing in the cash transfer value and the preference parameter for the good B, as these factors increase the supply.

We obtain the following equilibrium quantities for employed households:

$$c_A^e = \frac{n^a t \gamma}{n^e p_A(1+\gamma)}, \tag{10}$$

$$c_B^e = \frac{w\gamma}{p_B(1+\gamma)}. \tag{11}$$

The equilibrium quantities for assisted households are:

$$c_A^a = \frac{t}{p_A(1+\gamma)}, \tag{12}$$

$$c_B^a = \frac{n^e w}{n^a p_B(1+\gamma)}. \tag{13}$$

In this section, we have assumed that $q_A^* < p_A$. This condition is satisfied if the employment rate and wages are low and the cash transfer value is high such that:

$$n^e w < n^a t \gamma. \tag{14}$$

Solution of the model if $q_A^* \geq p_A$

The price in the resale market cannot be above p_A, otherwise the demand from employed households collapses. Therefore, q_A is capped at p_A. Consequently, if condition (14) is not satisfied, then the equilibrium price in the resale market is equal to p_A and not q_A^*. In this case, consumed quantities for both household types are equivalent to those with unrestricted transfers (equations (2) and (3)). This type of equilibrium will occur if the employment rate and wages are high and if the cash transfer value is low.

1.3 Testable predictions and robustness to alternative hypotheses

We derive two testable predictions on the relative impacts of unrestricted versus restricted cash transfers and assess the robustness of these predictions to changes in the model hypotheses. Proofs are presented in Appendix A. We focus on an economy in which condition (14) is satisfied. If condition (14) is not satisfied, then the unrestricted and restricted cash transfer programmes lead to the same outcomes.

Proposition 1 (Resale market)**.** *Restricted cash transfer programmes lead to the creation of a resale market, whose size increases with the value of the transfer t, the number of assisted households n^a, and the preference parameter for the restricted good γ. The price in the resale market is lower than the retail price if $q_A^* < p_A$ and equal to p_A if $q_A^* \leq p_A$.*

In our model, there is always a positive demand and a positive supply in the resale market, implying that the resale market is always functional. The demand in the resale market is indeed positive as long as $q_A \geq p_A$. And the supply is always positive because we assumed that assisted households have no other source of income. As a result, they are *extra-marginal*, in the sense that they always want to sell some good A to be able to purchase the good B. As explained below, the assumption that assisted households have no other income source is a good approximation in the context of our empirical analysis, where only 5.9% of adults have an income. Yet, this assumption can easily be relaxed, by assuming that assisted households earn a wage w^a in addition to the cash transfer, while employed households earn an income w^e. In this case, assisted households are extra-marginal if $t > w_u/\gamma$ (i.e. if the value of the transfer is large compared to their wage) and a resale market emerges. If $t < w_u/\gamma$ instead, assisted

households are infra-marginal and the equilibrium of the economy is similar to case of unrestricted cash transfers.

Proposition 2 (Assisted households)**.** *For assisted households, the consumption level of the unrestricted good A is unaffected by the restriction. If $q_A^* < p_A$, their consumption level of the restricted good B is reduced, implying that they are worse off as a consequence of the restriction.*

For assisted households, the restriction generates a substitution effect away from good B as well as a negative wealth effect due to the loss incurred in the resale market. For good A, the substitution and wealth effects operate in an opposite direction. They actually cancel out in our model because we opted for a simple Cobb-Douglas utility function. With a more general utility function, the sum of wealth and substitution effects depends on the elasticity of substitution. For good B, the substitution and wealth effects are both negative. Overall, the restriction reduces the consumption of assisted individuals and the welfare impact is therefore negative.

These results have important policy implications. When using restricted cash transfers, governments, international organisations, and NGOs aim to encourage the consumption of good A. This objective can only be achieved if the goods A and B are strong substitutes, such that the substitution effect is more important than the wealth effect. If goods A and B are strong complements, the consumption of both A and B will be reduced if restrictions are imposed. Organisations willing to use restrictions on cash transfers to increase the consumption of certain goods should first assess whether these goods are substitutes or complements.

In Appendix A, we discuss three other propositions, which characterise the relative impacts of unrestricted versus restricted cash transfers on employed households and businesses. We cannot test these supplementary propositions with our data.

2 Context and data

2.1 The Kalobeyei settlement

At the time of our fieldwork in November 2019, the Kalobeyei settlement was accommodating about 36,000 refugees, mainly originating from South Sudan (74 percent), Ethiopia (13 percent), and Burundi (7 percent). The settlement was opened in May 2016 with a dual objective. First, the nearby camp of Kakuma[9] was considered to be full and space was needed to accommodate the continued influx of South Sudanese and

[9]The minimum distance between Kakuma refugee camp and Kalobeyei Settlement is approximately 3.5 kilometres.

Burundian refugees fleeing violence. Second, the Government of Kenya and UNHCR, recognising the limits of the humanitarian approach promoted in Kakuma, were willing "*to pilot a new approach by developing a settlement promoting the self-reliance of refugees and the host population by enhancing livelihood opportunities and promoting inclusive service delivery*" (UNHCR, 2018).

Administratively, the settlement is divided into three villages (see Figure 1). Each village is further divided into approximately 40 neighbourhoods, and each neighbourhood is divided into a maximum of nine compounds. Compounds accommodate 10 households on average.

Extreme poverty is widespread in Kalobeyei (Fix et al., 2019). In our sample, a staggering 73% of households are severely food insecure, and dietary diversity is low:[10] only 3.5% eat fruit and 17% eat meat on a regular basis. Nearly half of our sample are single-adult households, of which 87% are female-headed. With an average of five children in a household, households are generally burdened with a high dependency ratio.

Employment and business opportunities in Kalobeyei are scarce. Only 5.9% of adults in our sample worked for an income at the time of our survey and the median income for those working was only KES 5,000 (USD 49) per month.[11] Only 8.3% of households had received remittances in the three months preceding the survey. Consequently, most refugee households living in Kalobeyei depend entirely on food assistance to survive.

2.2 Food assistance in Kalobeyei

Since the creation of the Kalobeyei settlement, food assistance has been delivered by WFP through monthly cash transfers to households. The use of cash transfers instead of food rations is part of the effort to develop local markets and promote self-reliance in the settlement. Every refugee is entitled KES 1,400 (USD 14) per month. With this amount, refugees should in theory be able to consume 2,100 kcal per day at current market prices (MacPherson and Sterck, 2021).[12] In total, WFP injects approximately USD 500,000 monthly into the local economy, which generates positive impacts on refugee beneficiaries (MacPherson and Sterck, 2021) and on businesses (Delius and Sterck, 2019).

Until June 2019, food assistance was delivered to all refugee households through

[10] Variables are described in Appendix B.

[11] Kenyan Shillings (KES) to US Dollar (USD) exchange rate in November 2019 was 0.00989 on average.

[12] Before January 2019, cash transfers were supplemented by an in-kind transfer of enriched corn-soy blend (CSB) to avoid malnutrition. The CSB distribution was however discontinued at the time of our survey.

a cash transfer programme called Bamba Chakula (which means "get your food" in Kiswahili). The programme entails monthly money transfers on SIM cards. The initial intention of WFP was for Bamba Chakula to be as close as possible to an unrestricted cash transfer programme. However, legal restrictions were imposed by Kenyan authorities due to concerns that cash transfers to refugees could be diverted to finance terrorist activities. For this reason, purchases can only be made at licensed shops. Bamba Chakula transfers are also restricted to food items, excluding alcohol and tobacco. Licensed shops were selected from a pool of existing refugee and Kenyan food retailers through a series of competitive selection processes. In July 2018, 45 Bamba Chakula traders were operating in Kalobeyei.

In June 2019, the Bamba Chakula programme was replaced by a new pilot programme of unrestricted cash transfers for 1,050 households living in the southern part of Kalobeyei Village 3 (see Figure 1). Cash transfers are unrestricted in the sense that they can be spent on any type of goods or services including tobacco and alcohol. This programme is implemented in collaboration with Equity Bank, one of the largest Kenyan commercial banks. Equity Bank issued MasterCards and bank accounts to all beneficiary households. Cash transfers are delivered to these bank accounts monthly and households can use the MasterCard to make purchase or withdraw cash. At the time of our survey, 48 Equity Bank agents were operating in Kalobeyei.[13]

Except for the restriction on food items, the two cash transfer programmes are very similar. The value and timing of the transfers are the same. For both programmes, a fingerprint or a correct pin should be provided to use the money. Both programmes are using digital transfers and rely on a similar number of agents. Most Bamba Chakula traders in Kalobeyei are actually also Equity agents and can accept both modes of payments.[14] While refugees can in theory use their MasterCard to withdraw cash, this practice is rare. In our sample, 89% of households on unrestricted cash transfers never withdrew cash.

At the time of our research, about 7,000 households in Kalobeyei were receiving food assistance through Bamba Chakula and 1,050 were benefiting from unrestricted cash transfers on Equity bank accounts. Our research aims to assess the impact of restrictions by comparing these two groups of households. More details on the context and the two cash transfer programmes are available in the policy report associated with this paper (Sterck et al., 2020).

[13] An ATM machine was also available in Kakuma town. However, movements between Kalobeyei and Kakuma town are rare because of distance and transportation cost.

[14] During a qualitative interview, the *supply chains and market specialist* at WFP Kakuma explained that *"the shift to unrestricted does not seem to have hurt refugee Bamba Chakula shops in Village 3 because most Bamba Chakula traders in Kalobeyei are also Equity agents"*.

2.3 Data

We conducted a representative survey of South Sudanese households in Village 3 in November 2019, five months after the first unrestricted cash transfer was made. We interviewed 1,529 South Sudanese adults living in 896 households. Out of these households, 525 households (with 910 adults) were receiving unrestricted cash transfers, and 371 households (with 619 adults) were receiving Bamba Chakula transfers.[15] We provided a week-long intensive training to a group of refugee enumerators.

Our sample was randomly selected using two-stage cluster sampling. Among 189 compounds in Village 3, we stratified by treatment, compound size, and language, and randomly selected 134 compounds. All households found in the selected compound were interviewed and all household members of ages 18 or above were interviewed.

The questionnaire contained standard questions about food and non-food expenditure, food consumption, asset ownership, income, remittances, savings and loans, subjective well-being, women's participation in household decision making, and preferences between the two modalities. In addition, households were asked about all their shop transactions in the past 30 days. This set of transaction-level data provides information about the products households bought using cash transfers, the products they resold or exchanged for other items, and the amount of cash or goods paid for or received during these transactions.

We also undertook 50 semi-structured interviews and several focus group discussions with refugees, shopkeepers, Equity Bank agents, as well as with various people working for international organisations and NGOs. More details on the quantitative and qualitative surveys are provided in the policy report associated with this paper (Sterck et al., 2020).

3 Empirical strategy

We describe the natural experiment in Section 3.1, the outcome variables in Section 3.2, and the identification strategy in Section 3.3.[16]

[15] Four households and 13 individuals did not give consent. These are numbers for households who reported receiving restricted and unrestricted transfers. We discuss non-compliance in Section 3.3.

[16] The research design and the empirical strategy were pre-specified in a pre-analysis plan that was registered before data collection. The pre-analysis plan is available here. Differences between what we committed to do in the pre-analysis plan and the actual analysis are described in Appendix E.

3.1 A natural experiment

Whether households receive restricted or unrestricted cash transfers depends on where they were offered to settle upon arrival in the settlement (see Figure 1). Kalobeyei is divided in three villages and each village is further divided into neighbourhoods. At the time of our research, households residing in the Villages 1 and 2 and in the northern side of village 3 (neighbourhoods 28 to 43) were receiving cash transfers restricted to food. Most of those residing in the southern side of Village 3 (neighbourhoods 1 to 27) were receiving unrestricted cash transfers (see Section 3.3 for a discussion of imperfect compliance).

To assess the impact of restrictions on cash transfers, we exploit the fact that shelters were allocated quasi-randomly to refugee households conditional on their arrival date in the settlement. Most households living in Kalobeyei were registered between 14 May 2016 and 21 June 2017. After 22 June 2017, the settlement was considered full and new arrivals were accommodated in the neighbouring refugee camp of Kakuma. Figure 2 illustrates how shelters were allocated based on refugees' registration date. It shows that refugees who are residing in Village 1 and Village 2 registered earlier than those in Village 3. Interestingly, it also shows that households residing in the southern and northern parts of Village 3 arrived at similar dates, suggesting that the allocation of refugees within Village 3 might be quasi-random.

We use our survey data to assess whether the allocation of refugees between the two parts of Village 3 could be considered as a natural experiment. We examine differences between the two groups in terms of a series of variables that have been determined before refugees' arrival in the camps (*pre-determined* variables). First, we regress a dummy variable identifying the two groups on the vector of pre-determined variables and run an omnibus F-test of the joint hypothesis that the coefficients of the pre-determined variables are equal to zero. The p-value of the F-test is 0.54, showing that we cannot reject the null hypothesis that households on restricted and unrestricted transfers had similar characteristics when they arrived in Kalobeyei. Second, we examine the magnitudes of differences between the two groups. In Table A.7 in Appendix, we show the size of normalised differences in a characteristics of households and individuals before their arrival into Kalobeyei. The normalised differences of all variables are less than 0.25 – an indicator that the balance is achieved (Imbens and Rubin, 2015). Finally, we run simple t-tests for each pre-determined variable, which are all insignificant at conventional levels. The results of these tests confirm that the two groups are well balanced in terms of pre-existing characteristics. Differences between the two groups measured after the switch to unrestricted cash transfers are unlikely to be driven by differences that were existing before refugees' arrival in the settlement.

Showing that the two parts of Village 3 are similar in terms of pre-determined characteristics is encouraging, but not sufficient to validate the research design. We also need to show that the two parts of the Village 3 benefited from similar programmes and faced similar socio-economic conditions between refugees' arrival in the settlement and the switch to unrestricted cash transfers. In Table A.8 in Appendix, we assess the balance of 10 *programmatic* variables indicating whether households benefited from the other programmes implemented by NGOs and international organisations in Kalobeyei Village 3 (e.g. free meals at school, therapeutic food against malnutrition, and support for agriculture and poultry farming). In Table A.9 in Appendix, we assess the balance of 18 *socio-economic* variables, which capture the socio-economic characteristics of the households at the time of the survey and could be affected by local conditions in the settlement (e.g. job dummy, remittances, and relationship with neighbourhood leader). The results of these balance tests are encouraging. The omnibus F-tests of joint significance are insignificant at conventional thresholds (p-values = 0.54 and 0.12 for programmatic and socio-economic variables respectively). For all variables, the sizes of normalised differences between the two groups are small (lower than 0.25). For three variables, individual t-tests are however statistically significant at conventional levels: a dummy identifying households who received poultry, a measure of English command, and a measure of incidence of non-health related household shocks (e.g. robbery and theft).[17] We are not overly worried by these results, because normalised differences are relatively small. Yet, we will control for pre-determined, programmatic, and socio-economic variables in the analysis to reduce possible biases due to these differences and improve precision.

The results of these tests indicate that the restricted and unrestricted groups are well balanced, suggesting that the switch from restricted to unrestricted cash transfers in Village 3 of Kalobeyei may constitute a valid natural experiment. Below, we exploit this natural experiment to assess the impact of cash transfer restrictions.

3.2 Outcome variables

We assess the impact of cash transfer restrictions on 11 outcome variables. In line with the theoretical model, we group these variables into four categories: (1) the resale and exchange of food items, (2) assets and non-food expenditures, (3) food consumption and expenditures, and (4) subjective well-being. We briefly describe these outcomes below. Further details are provided in Appendix B.

[17] We study these differences in Section 5.1. We show that these differences are driven by a few households living in remote neighbourhoods. Dropping these outlying observations does not affect the results.

(1a) **Resale dummy:** This dummy variable is equal to one if the household resold food for cash in the past 30 days and equal to zero otherwise.

(1b) **Exchange dummy:** This dummy variable is equal to one if the household exchanged food for other items in the past 30 days and equal to zero otherwise.

(2a) **Asset value:** This variable aggregates the value of assets owned by the household expressed in KES.[18]

(2b) **Asset purchase value:** This variable aggregates the value of asset purchases during the three months preceding the survey expressed in KES.

(2c) **Non-food expenditure:** This variable aggregates non-food expenditures expressed in KES per month.[19]

(2d) **Temptation goods:** This variable aggregates spending on soda, tobacco, eating out, alcohol, and video halls expressed in KES per month.

(3a) **Calories per adult equivalent (log):** For 37 types of food, we convert the quantity consumed by the household during the seven days preceding the survey into calories.[20] We aggregate the calories and express the total in "per adult equivalent" terms following the method proposed by Deaton and Zaidi (2002). We consider the log-transformed variable.

(3b) **Food consumption per adult equivalent (log):** For 37 types of food, the quantity consumed by the household (in kg/day) is multiplied by the median price per kilo in our data. We then aggregate these amounts and divide the total by the number of adult equivalent in the household. We consider the log-transformed variable.

(3c) **Dietary diversity:** This variable is calculated by counting the number of twelve different food types which have been consumed at any time within the seven days preceding the survey, resulting in a score from 0 to 12.

(3d) **Food insecurity dummy:** This dummy variable is equal to one if the household is categorised as "severely food insecure" according to the Household Food Insecurity Access Scale (HFIAS), and equal to zero otherwise. The HFIAS aggregates

[18]To construct the variables for asset ownership and asset purchases, we aggregate values of 12 types of assets, expressed in KES. The assets are radio, television, computer and tablets, refrigerator, solar panel, generator, table, chair, sofa, mattress, cupboard, clock, mobile phone, mp3, watch, bicycle, motorcycle, and car.

[19]We aggregate non-food expenditures of 26 items expressed in KES per month. See Table A.1 in Appendix for the full list.

[20]Data on calories were obtained from the U.S. Department of Agriculture.

respondents' perceptions of food vulnerability and the frequency with which shortages occurred.

(4) **Subjective well-being:** We consider the answers to the question "All things considered, how satisfied are you with your life as a whole these days?". Answers range from 1 "very unsatisfied" to 5 "Very satisfied".

Summary statistics are reported in Table A.2 in Appendix.

3.3 Identification strategy

We estimate the intent-to-treat (ITT) effects of the switch from restricted to unrestricted transfers using ordinary least squares (OLS). We estimate the following equation:

$$y_i = \beta T_i^{Register} + \gamma' X_i + \mu_i + \tau_i + \lambda_i + \epsilon_i \tag{15}$$

where y_i is an outcome variable, $T_i^{Register}$ is a dummy variable which takes a value of one if household/individual i is recorded by UN agencies as living in a neighbourhood where unrestricted cash transfers are distributed, and zero otherwise, and X_i is a vector of control variables. The vector of controls X_i includes four categories of variables: 1) *pre-determined*, 2) *programmatic*, 3) *socio-economic*, and 4) *geographic* variables. These variables are defined in Tables A.4 and A.5 in Appendix, and summary statistics are reported in Table A.6 in Appendix. Although we have shown in Section 3.1 that pre-determined, programmatic, and socio-economic variables are well balanced across the two groups, we control for these variables to minimise biases due to small differences between the two groups and improve precision. Because assignment to treatment was determined geographically, we control for a series of distance variables (distance to markets, road, and Kakuma camp) which are unlikely to be balanced across the treatment and control groups and could affect outcomes of interest. Regressions also include date of interview fixed effects (μ_i), enumerator fixed effects (τ_i) and week of arrival fixed effects (λ_i).[21]

The main coefficient of interest is β^{ITT}. It captures the ITT effect of the change from cash transfers restricted to food purchases to unrestricted cash transfers. We also estimate the average treatment effect on the treated (ATET) by using two-stage least squares (2SLS).

[21] Enumerators were randomly assigned to the different clusters. We ensured that each enumerator was randomly assigned to both treated and control clusters. The timing of the survey in the different clusters was also randomised. We include fixed effects capturing the week of respondents' arrival in Kalobeyei since the initial allocation of shelters depended on households' date of arrival.

To account for the sampling design, we report robust standard errors clustered at the compound level. We account for survey weights, stratification, and finite sample correction. The fact that we use multiple indicators to test individual hypotheses (e.g. four indicators to assess the impact on consumption) raises the question of whether inferences are robust to corrections for multiple hypothesis testing. We estimate sharpened q-values that control the false discovery rate (FDR) following the two-step procedure described by Anderson (2008). We compute sharpened q-values across the different outcomes.

Prior to data collection, we expected perfect compliance with the switch from restricted to unrestricted cash transfers. We therefore intended to estimate the average treatment effects (ATE) of the change from restricted to unrestricted cash transfers on household outcomes. Our data however show that 11.9% of the households who were registered by WFP as living in the treated area of Village 3 did not receive unrestricted cash transfers, and instead continued to receive restricted cash transfers (See Table A.3 in Appendix for details). This occurred when the adult members of households were away at the time of the modality switch or when there were technical problems with the new ATM cards.[22] Reassuringly, we find no defiers, i.e. households receiving unrestricted cash transfers amongst households registered as living in the control area. Due to the presence of imperfect compliance, we estimate ITT effects rather than ATE.[23]

4 Results

We first study the characteristics of the resale shadow market in Kalobeyei and test whether the Bamba Chakula restrictions impact participation in the resale market (Section 4.1). We then assess the impact of restrictions on household outcomes (Section 4.2).

[22] The registration list used by WFP to determine which households would get unrestricted cash transfers was the most up-to-date list at the time of data collection. Our data show that few households have moved between the two parts of Village 3 without informing UN agencies. Four households were found to be living in the treated area while being registered as living in the control area, and six households were found to be living in the control area while being registered as living in the treated area. These movements do not affect the validity of the ITT estimates.

[23] The ATE is likely to be biased by selection bias because compliers are likely to be different from non-compliers in terms of observables and unobservables. For transparency purposes, we report ATE estimates in Table A.19 in Appendix.

4.1 Resale shadow market

A sizeable resale market exists in Kalobeyei.[24] During the month preceding the survey, as many as 69% of households on restricted cash transfers resold food and 78% exchanged food against other goods.

Reselling food in the resale market entails a substantial loss (Table 1). Maize and wheat flour are by far the most common good traded by refugees in the resale market: more than 85% of transactions in the resale market involve one of these two goods. The median price of a kilo of maize in the retail market is KES 49 (USD 0.5).[25] In the resale market, the median price of a kilo of maize is KES 40 (USD 0.48). The median loss of value incurred by households reselling maize is 18%. For wheat flour, the median loss of value is even higher – 38% – which explains why refugees are less likely to resell wheat flour compared to maize.[26]

Households also use barter trade and exchange food items against other products that are not available in retail shops. Maize and wheat flour are commonly exchanged against charcoal and greens.[27] The median loss of value incurred following the exchange of maize against charcoal or greens is 18%. For wheat, the median loss of value is higher: 34% when exchanged against charcoal, and 38% when exchanged against greens. We note that losses from exchanging and reselling food are of similar magnitude.

Despite substantial losses incurred, a large proportion of households receiving restricted cash transfers resell and exchange food items. We test whether the switch from restricted to unrestricted transfers reduces the proportion of households participating in the resale market. Results are shown in rows (1) and (2) of Table 2. In line with theory, we find that households assigned to unrestricted cash transfers are 10 percentage points less likely to have resold food items in the month preceding the survey compared to households receiving unrestricted transfers. The effect is significant at the 1% threshold. The ITT effect of the restriction on food exchange is also negative but not significant at conventional level. We study the intensive margin of participation in the resale market in Table A.12 in Appendix. ITT effects on the value of goods resold or exchanged in the resale market are negative and statistically significant.

The results of this section are broadly consistent with Proposition 1 of our theoretical model: restricted cash transfers seem to favour the emergence of a resale market in which

[24] The existence of a resale market in the neighbouring Kakuma refugee camp was documented by Oka (2014) using qualitative methods.

[25] In the retail market, refugees typically buy bags of 45kg of maize for 2,200 KES (with Bamba Chakula).

[26] In the retail market, refugees typically buy bags of 12kg of maize for 1,000 KES (with Bamba Chakula). We obtain similar results with regression analysis and control variables (Table A.11 in Appendix).

[27] Firewood is also a common item but the transaction value proved to be difficult to measure accurately due to the varying sizes in which it is sold.

unrestricted goods are resold or exchanged. Results also suggest that condition (14) – which determines when prices are lower in the resale market – is satisfied. Consequently, the restriction imposed on cash transfers should impact the consumption choices of beneficiaries, in line with the Proposition 2 of the theoretical model. We test this proposition in the next section.

4.2 Effects on refugee households

We report estimates of the ITT effects of the switch from restricted to unrestricted transfers on indicators of food and non-food consumption and expenditure and on subjective well-being in Table 2, rows (3) to (11). We make three observations, which are all consistent with Proposition 2 of the theoretical model.

First, the impact of lifting the restriction on non-food expenditure is positive, suggesting that the restriction effectively constraints households' choices. Households assigned to unrestricted transfers spend KES 241 per month (USD 2.3) more than those assigned to restricted transfers. This is 25 % of the control group mean of KES 971 (USD 9.6). Part of this effect seems to be driven by expenditure on temptation goods. We find that households living in the area with unrestricted transfers spend KES 109 (USD 1.1) more per month on temptation goods expenditure than those in the area with restricted transfers. This is large, about 1.7 times the control mean. This effect is driven by a significant increase in expenditure on tobacco, meals outside home, and alcohol, both at extensive and intensive margins (Table A.13 in Appendix).[28] We also find positive effects on asset ownership and asset purchases, but these effect are borderline significant (p-values = 0.14 and 0.15 respectively). This lack of significance can be explained by extremely low levels of asset ownership; only 14% of households purchased some assets in the three months preceding the survey. Taken together, these results are consistent with our prediction that the lifting the restriction should increase spending on restricted goods, i.e. non-food items, temptation goods, and assets in the case of Bamba Chakula and good B in the model.

Second, the restriction does not seem to significantly affect food consumption. We find no significant effect on food insecurity, calories intake per adult equivalent (log), food expenditure per adult equivalent (log), and dietary diversity. The signs of the effects do not point in a particular direction. We therefore conclude that the restriction does not affect much food consumption (good A in the model). If anything, the effect is small. In line with theory, the substitution and wealth effects induced by the restriction appear to balance each other. This result suggests that our choice of a Cobb-Douglas

[28]The video hall in Village 3 was situated in the area where households were receiving restricted cash transfers. This might explain the negative and significant ITT effect on spending on video halls.

utility function in the model is a good approximation.

Finally, we find strong evidence that lifting the restriction significantly increases subjective well-being. The effect is moderate – 0.18 standard deviations of the control group – and statistically significant at the 1% threshold. This result is also consistent with theory.

Estimates of ATET are shown in Table A.14 in Appendix. Given the high compliance rate (88%, see Table A.3 in Appendix), the first-stage of IV regressions is very strong (Effective F-statistics > 400) and the coefficient of treatment assignment in the first-stage regressions is close to 1. The conclusion of the ITT and ATET analyses are therefore qualitatively similar.

5 Robustness checks

We examine whether our main results could be driven by outlying observations (Section 5.1), by difference in registration dates or distance to WFP compound (Section 5.2), by the selection or omission of certain variables and fixed effects (Section 5.3), or by spillovers (Section 5.4). Tables and Figures are shown in Appendix.

5.1 Outliers

In Section 3.1, we used balance checks to show that households receiving restricted versus unrestricted cash transfers are broadly similar in terms of pre-determined characteristics, in terms of access to humanitarian and development programmes, and in terms of the socio-economic conditions they face. While omnibus F-tests of joint significance are insignificant at conventional thresholds and normalized differences are small, three t-tests are statistically significant at conventional levels: a dummy identifying households who received poultry, a measure of English command, and a measure of incidence of non-health related household shocks (e.g. robbery and theft). In Appendix, we show that these differences are driven by a few outlying neighbourhoods: neighbourhood 39 for exposure to poultry distribution (Figure A.1), neighbourhood 43 for command of English language (Figure A.2), and neighbourhoods 39 and 41 for exposure to robbery and thefts (Figures A.3 and A.4). These neighbourhoods are located in the northern part of Village 3, close to Kakuma camp. Reassuringly, results remain qualitatively similar when we exclude these outlying neighbourhoods from the sample (see Tables A.23-A.25).

Qualitative evidence and descriptive statistics show that most households in Village

3 have similar habits and constraints when in comes to market participation. Most households survive thanks to humanitarian aid, never have cash, only shop in Kalobeyei, and always go to the same food retailer, to whom they are usually indebted. However, a few households reported different behaviour, either because they withdrew cash, shopped in Kakuma (where prices are typically lower), or visited different shops. We show that these outlying households are not driving our results in Tables A.26-A.29. If anything, results are stronger without these outliers.

5.2 Registration dates and distance to WFP compound

Our natural experiment assumes that households were quasi-randomly allocated between the northern and southern parts of Village 3, conditionally on their registration date in Kalobeyei. Regressions include week of arrival fixed effects to control for unobserved differences correlated with the timing of arrival in the settlement. In fact, we find no significant correlation between registration date and treatment assignment in our data (p-value = 0.66) and in UNHCR registration data (p-value = 0.80). Figure 2 also shows that registration dates are broadly similar in the treatment and control areas. Most registrations in Village 3 took place from early March 2017 until mid-June 2017, when the Kalobeyei settlement was considered to be full and new arrivals were registered in Kakuma. In Table A.30, we restrict the sample to households who registered during this time period. Results are actually more salient with this specification: the effects on the Exchange dummy, on asset ownership, and on asset purchases become statistically significant at conventional levels.

The treatment was assigned based on geography, which is why the vector of control variables includes several measures of distance that were pre-specified in our pre-analysis plan (distances to markets, to the road, and to Kakuma camp). One measure that we did not consider in the pre-analysis plan is the distance to the WFP compound and Equity Bank (both institutions are located next to each other). Controlling for this distance may matter, because the area of the Kalobeyei settlement that was chosen by WFP to pilot unrestricted cash transfers is actually the area of the settlement that is the closest to the WFP compound and Equity Bank. In Table A.31, we show that our results are robust to controlling for distance to the WFP compound.

5.3 Omitted variables versus overfitting

Our preferred specification includes a long list of control variables and fixed effects to minimise the risk of omitted variable bias (equation (15)). We have to balance two

risks when selecting the list of control variables and fixed effects: the risk of omitted variable bias if the list is incomplete and the risk of overfitting if the list is too long.

A series of methods have been recently developed to assess the sensitivity of results to omitted variable bias (Altonji et al., 2005; Oster, 2019). These methods estimate how strong the selection on unobservables should be to change the research conclusions. We implement the method of Oster (2019) in Table A.35. The statistic δ indicates how much larger the selection on unobservables would have to be compared to the selection on observables for the true ITT effect to be zero.[29] For statistically significant variables, δ that are positive and low (i.e. $0 < \delta \ll 1$) signal a possible risk.[30] For the resale dummy, asset ownership, asset purchases, non-food expenditures, and temptation good spending, the δ are outside of the interval $[0, 1]$, which is reassuring. For *Subjective well-being*, δ is 0.62, which means that selection on unobservables would need to be 0.62 times the selection on observables for the true ITT effect to be zero.

The fact that equation 15 includes a long list of control variables and fixed effects implies that overfitting could be an issue. We use two approaches to shorten the list of control variables and fixed effects and thereby test whether overfitting is an issue.

First, we re-estimate the regression equation (1) without controlling for pre-determined variables – the natural experiment we exploit assumes that these are balanced – and (2) without including enumerator and date of interview fixed effects – the enumerators and timing of surveys in the different clusters were randomised, implying that these fixed effects are unnecessary. This reduces the number of control variables included in the model from 177 to 81. Results are qualitatively similar (Table A.32). We note that the effect on the Exchange dummy becomes almost significant at conventional levels (p-value = 0.10) and the effect on asset ownership and asset purchases become highly significant. The effect on subjective well-being becomes insignificant at conventional levels (p-value = 0.27).[31]

Second, we implement the double Least Absolute Shrinkage and Selection Operator (LASSO) regression method to select relevant control variables and fixed effects. We then re-estimate the model specified in equation 15 using OLS with this set of controls (Belloni et al., 2014; Urminsky et al., 2016). We implement two versions of the procedure.

[29] Following Oster (2019), we assume that the maximum R^2 is 1.25 times the reported R^2 in the regression with the full set of observables.

[30] If δ is above 1, the selection on unobservables would have to be larger than the selection on observables to have a true ITT effect equal to zero. A negative δ suggests that controlling for unobservables would lead to larger ITT effects.

[31] We are not overly worried by the instability of the effect on subjective well-being, as this instability does not seem to be driven by overfitting. The effect is highly significant if we add back enumerator fixed effects, which seem extremely important for this highly-subjective outcome (enumerator fixed effects alone explain 38% of the outcome, which is much higher than for other outcomes). The effect is also highly significant if we further drop week of arrival fixed effects or pre-determined controls.

In Table A.33, only pre-determined variables, enumerator fixed effects, and date of interview fixed effects can be penalised by the procedure.[32] In Table A.34, all controls and fixed effects can be penalised. The results are generally consistent with those of Table 2, suggesting that overfitting is unlikely to be an important issue.

5.4 Spillover

We defined households to be "individuals who live, eat meals, and share resources together". This definition aims to minimise unknown spillovers as a result of sharing resources between treated and control households. Nevertheless, there are control neighbourhoods which are only across the road to the treated neighbourhoods (see Figure 2).

In Table A.36 , we report direct and indirect effects at household and individual levels after restricting the sample to households whose distance away from the road is more than the 10th percentile of the distribution of distances. We report corresponding results when we further restricted our sample to households whose distance away from the road is more than the lower quartile of the distribution of distances in Table A.37. We find that the treatment effects remain statistically significant, of the same sign, and of similar magnitude.

6 Debt and cash transfers

Our empirical results are broadly consistent with theory. Yet, three empirical findings require further investigation. First, while the switch to unrestricted cash transfers reduced the likelihood of reselling food items in the resale market, the size of the effect is rather small. Surprisingly, 85% of households on unrestricted transfers still resell or exchange food items despite incurring a loss. We also find no significant effect on barter trade in the resale market. In theory, the removal of the restriction should have substantially reduced these costly practices.[33] Second, cash withdrawal is surprisingly rare. Only 11% of households on unrestricted transfers withdrew cash using their ATM card in the month preceding our survey. This means most households obtain items directly using their ATM card. Finally, only a minority of households reported

[32]The risk of excluding these controls and fixed effects seems low, because our natural experiment assumes that the treatment and control groups were similar before arrival in the settlement, and because we randomised the allocation of enumerators and the timing of interviews. It is less obvious that other controls and fixed effects can be excluded without generating omitted variable bias.

[33]With unrestricted cash transfers, a sizeable resale market could still exist if reselling food is a coping strategy in case of shock.

preferring unrestricted cash transfers: 39% of those receiving unrestricted cash transfers prefer unrestricted cash transfers and 59% percent think that the two modalities are similar.

In this section, we show that these three puzzling facts have the same underlying explanation: the high prevalence of indebtedness in Kalobeyei. First, we use ethnographic evidence to characterise the causes and consequences of indebtedness in Kalobeyei (Section 6.1). We then explore heterogeneous treatment effects by household indebtedness (Section 6.2).

6.1 The debt trap

The prevalence of household debt is high for both treated and control households. 89 percent of households in our sample are indebted towards their food retailers,[34] and the average debt level per head is more than one month's worth of transfer (Table 3).

Ethnographic data shows that indebtedness initially arose as a form of social support from credit-granting food retailers willing to assist food insecure clients. Economic opportunities are scarce, aid is often insufficient, and refugees have low levels of savings and asset holding. Refugee households in Kalobeyei are also extremely vulnerable to shocks. And shocks are unfortunately frequent. In the 12 months preceding the survey, 48% of households in our survey had experienced at least one incident of theft, and 28% had at least one adult admitted to the hospital. Delays in humanitarian assistance put extra strain on households. While refugees in Kalobeyei expect to receive their cash transfers on the 10th of each month, disbursements are systematically delayed because of technical and administrative issues. Disbursements after the 15th of each month happen about 30 percent of the time (Sterck et al., 2020). Disruptions in other humanitarian programmes are also frequent. Because delays are systemic shocks that affect all households at the same time, refugees cannot rely on kin or neighbours to provide temporary relief until transfers are received.

For many households, borrowing food from food retailers is the only safety net in times of hardship. When refugees suffer from a shock, shopkeepers take this into consideration when extending credit. One South Sudanese refugee explained that if a family's rations are stolen from their home, they report the incident to the village chairman as well as the local security staff, who "*will escort the beneficiary to the shop where they usually collect their food. They will talk to the shopkeeper for the beneficiary to be assisted.*" There are moral pressures urging shopkeepers to extend credit to needy customers:

[34] Altindag and O'Connell (2020) finds a similar prevalence of indebtedness among Syrian refugees in Lebanon.

> The people are hungry and the shopkeeper can't let them go. Someone will come and say my children are hungry, and he will be given whatever he needs. (Ethiopian-Somali woman)

> If there is no food in the house, you can go to the shop and talk to the shop owner. He can give you food on credit. He cannot refuse you. (South Sudanese woman)

> This indebtedness happens when the customer's money is not there, and the food has finished. Because of the good relationship I have with someone as my routine customer, I am forced to give them food on credit, and that sometimes leads to a loss in my business. (Bamba Chakula shopkeeper)

The material technology required for receipt of cash assistance – SIM cards for restricted cash transfers and ATM cards for unrestricted cash transfers – provides a physical object that can be retained by food retailers as a guarantee. While households have few assets to offer as conventional collateral when taking credit, shopkeepers can hold customers' SIM cards or ATM cards to ensure debt repayment. From our survey data, 97 percent of indebted households reported that their SIM card or their ATM card was held by a shop owner (compared to 65 percent for debt-free households). When they hand over their cards, most customers also hand over their private PIN numbers to the shop owners (97 percent for indebted households and 73 percent for households who are not indebted). Shop owners can then make withdrawals on their customers' behalf, without negotiating prices.

The three surprising findings described at the beginning of this section can be explained by the high prevalence of indebtedness. First, food retailers provide loans in the form of staple food, to ensure households can survive the hard times. In exchange for this service, retailers keep the SIM or ATM cards of their clients. As a result, households have no other choice than to resell or exchange food items in the resale market to access non-food items. We create a dummy equal to 1 for households that are "dependent" on their retailer, either because they are in-debt or because their SIM or ATM card is kept by their retailer (the two conditions usually overlap) and equal to 0 otherwise. We find that "dependent" households are 38 percentage points more likely to resell food and 50 percentage points more likely to exchange food against other items (see Table A.16 in Appendix). This provides a plausible explanation to why the effect sizes of the switch from restricted to unrestricted transfers on the likelihood of reselling or exchanging food items are smaller than expected. The following quote from a South Sudanese refugee illustrates well this modus operandi:

> After I received my Bamba Chakula line, I gave it to the shop owner. When the money was sent, the shop owner would withdraw the money, and I would collect the food. I have to exchange some of this food for firewood or charcoal, and I sell part of the food for cash, which I can use for grinding or buying vegetables. Because of selling the food, I continued taking food on credit. On the day Bamba Chakula was changed to Equity, the shop owners immediately received my money. My ATM card remains with the shop owner because I had credit with him, and I continue taking food on credit from the same shop. I cannot change the shop because the shop owner has my ATM card, and he always withdraws the money directly.

Second, indebtedness also explains why many households on unrestricted cash transfers never withdrew cash using their ATM card. Most indebted households simply do not have access to their ATM card. As one South Sudanese man explained:

> Since I was introduced to the Equity Bank programme, I have not withdrawn any money using my ATM card...The shop owner withdraws the money himself. If the shops are not operating that day, it means the shopkeepers are travelling to withdraw money from the bank in Kakuma. They go to town carrying with them all of our ATM cards to collect the money. I cannot receive money from an ATM. The money remains with the shop owner. Items in the shop are very expensive, so I always borrow my food [on credit]. I have just borrowed for the month of March [three months beyond December interview]. I always borrow in advance.

Third, indebtedness partly explains why many households report that the restricted and unrestricted modalities of cash transfer are similar. Indeed, indebted households on unrestricted transfers receive only food from their credit-granting retailer. They do not have direct access to cash, even though providing beneficiaries with access to cash was one of the main objectives of the switch to unrestricted transfers. We find that indebted households are significantly less likely to prefer unrestricted cash transfers (p-value = 0.00) and significantly more likely to report that both modalities are similar (p-value = 0.00).

We find suggestive evidence that indebtedness reduces the value of purchases and consumption. Because ATM or SIM cards are kept by food retailers as a guarantee, indebted households cannot shop around and compare prices. Instead, they remain committed to the shop that has provided credit. This means even unrestricted households cannot take advantage of the increased autonomy that unrestricted cash transfers could offer. They cannot benefit from the fact that prices are lower with cash (Table

A.17 in Appendix).[35] Dependency on food retailers is positively associated with severe food insecurity and negatively associated with asset holding, non-food expenditures, dietary diversity, and subjective well-being (see Table A.16 in Appendix). These results should be interpreted with caution given the important risk of reverse causality. While many refugees reported that the system of credit between customers and shopkeepers is essential to cope with risk, some complained that indebtedness reinforce dependency and poverty traps:

> The shop owner sometimes gives me rotten food. It is sometimes like we are in a prison... The food is sometimes rotten, especially fish and beans. Since you don't have any other option, you must take the food, because of your debt with the shop owner. If I could get cash, then I could go to other shops... But now, even though I tell the shop owner to give me good food, he will refuse. (South Sudanese man)

6.2 Heterogeneous effects by household indebtedness

We use our data to test whether debt-dependency moderates the effect of cash transfer restrictions. Households that are "dependent" on their retailer – because they are in-debt or do not hold their SIM or ATM card – receive only food from their credit-granting retailer. For "dependent" households, the effect of the switch from restricted to unrestricted cash transfers is expected to be null as – like households receiving restricted cash transfers – they have to use the resale market to access non-food items.

We modify our original empirical model described in equation (15) by including dummy variable identifying "dependent" households ($Dependent_i$) and an interaction term between the "dependent" household dummy variable and the ITT variable ($Dependent_i \times T_i^{Register}$):

$$y_i = \beta_1 T_i^{Register} + \beta_2 Dependent_i + \beta_3 Dependent_i \times T_i^{Register} + \gamma' X_i + \mu_i + \tau_i + \lambda_i + \epsilon_i. \quad (16)$$

The interaction term captures the difference in ITT effect of the change in modality between indebted households and households who are not indebted.

Standardised ITT effects are shown in Figure 3 (see Table A.18 in Appendix for regression results). Extreme caution should be exercised when interpreting these results

[35] MacPherson and Sterck (2021) and Delius and Sterck (2019) find similar results with data collected in 2017 and 2018 respectively.

since variation in indebtedness status is not exogenous or quasi-exogenous. The risk of reverse causality is therefore important, because many of the outcomes studied are themselves causes of indebtedness. Furthermore, households that are not "dependent" are limited in number and they differ from other households in many respects. This analysis should therefore be seen as exploratory and results should not be interpreted as causal.

In line with our intuitions, the effects of the switch from restricted to unrestricted cash transfers appear to be much stronger for households that are not dependent on their retailers, because they are not indebted and hold their ATM card. These households are significantly less likely to exchange goods in the shadow market. They also seem to drive positive effects of the switch to unrestricted cash transfers on assets, assets purchases, non-food expenditures, and temptation-good spending. The estimated effects appear to be much lower for households that are "dependent" on their food retailers.

7 Conclusion

Governments, international organisations, and NGOs are increasingly using cash transfers to assist vulnerable populations. In order to achieve specific objectives, for example reducing food insecurity, limiting spending on temptation goods, or encouraging certain investments or behaviours, these organisations often impose restrictions on how transfers can be spent. In this paper, we studied theoretically and empirically the impact of such restrictions on purchases, consumption, welfare, and markets.

We developed a theoretical model to compare the effects of unrestricted and restricted cash transfer programmes. Our model focuses on extra-marginal transfers and allows beneficiaries to resell unrestricted goods in a shadow market to access restricted goods. Our model shows that prices in the shadow market diverge from those in the retail market if the size of the cash transfer programme is large compared to the size of the economy. If this condition is satisfied, restrictions induce two effects: (1) a substitution effect away from restricted goods and (2) a negative wealth effect because transfer recipients resell unrestricted goods at a loss in the shadow market in order to access restricted goods. The welfare impact on transfer recipients is negative.

To test these predictions, we exploited a natural experiment in the Kalobeyei settlement in Kenya. Until June 2019, all refugees living in the settlement were receiving cash transfers restricted to food items (exclusive of alcohol and tobacco). In June 2019, WFP lifted this restriction for 1,050 households living in one geographically bounded part of the settlement. Our paper studies the impact of this modality change. We draw

three main conclusions from the analysis. First, we observe a massive shadow market in which households resell food at a loss to access other commodities. Unrestricted cash transfers reduce this costly practice. Second, we find no evidence that households receiving cash transfers restricted to food had better nutrition outcomes. The key objective of the restriction - to improve food security - was therefore not achieved. Restricted transfers led to lower non-food expenditure, lower expenditure on temptation goods, lower subjective well-being, and possibly lower levels of asset ownership and asset purchases. Finally, we observe a massive indebtedness problem in the settlement and find suggestive evidence that indebtedness attenuates the positive effects of the modality change.

Our analysis has important policy implications. Overall, results suggest that the switch from restricted to unrestricted cash transfers was welfare enhancing for transfer beneficiaries. Governments, international organisations, and NGOs should therefore carefully weigh the benefits and costs of restrictions before adopting a restricted modality. Organisations often opt for restricted cash transfers in order to encourage the consumption of certain goods. This objective can only be achieved if the restricted and unrestricted goods are strong substitutes, such that the substitution effect induced by the restriction exceeds the negative wealth effect. If the restricted and unrestricted goods are strong complements, the consumption of both types of good is lower with restricted cash transfers. Organisations willing to use restrictions on cash transfers to increase the consumption of certain goods should first assess whether these goods are substitutes or complements. More generally, practitioners and researchers designing social protection programmes should develop a deep understanding of markets and social systems in which transfers are made to ensure the programme's full potential can be realised. Our results show that particular attention should be paid to creditor-debtor relationships, as debt dependency can severely constrain debtors and undermine the impacts of cash transfers.

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Tables and figures

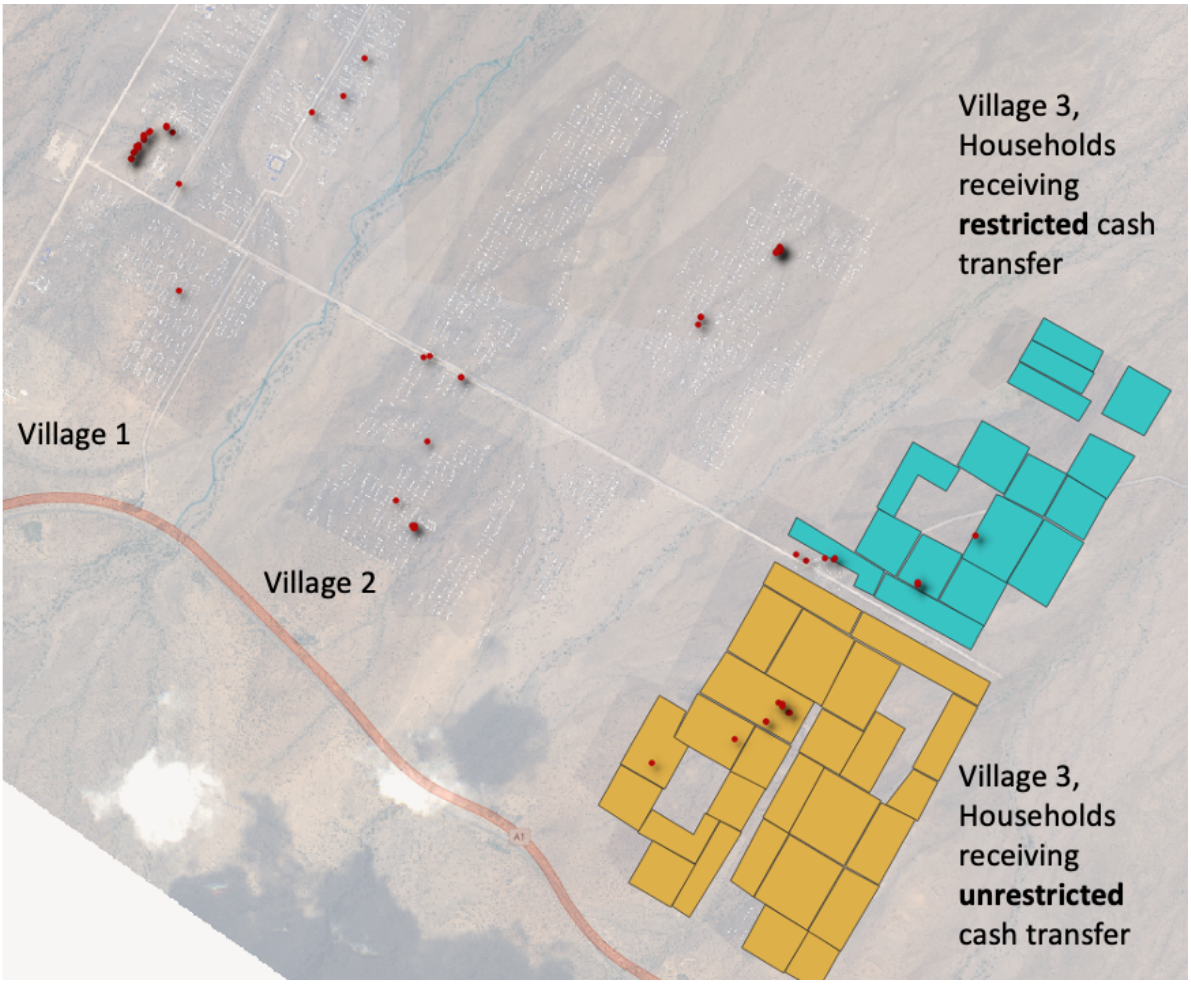

Figure 1: Kalobeyei Settlement - Treatment and Control Areas

Notes: Satellite image of Kalobeyei Settlement taken in 2017. The image shows three villages in Kalobeyei. Each rectangle represents a neighbourhood in Village 3. The households residing in the orange neighbourhoods (southern part of the village) have been receiving unrestricted cash transfers since June 2019. At the time of our research, the households residing in the turquoise neighbourhoods (northern part of the village) remained to be receiving restricted cash transfers. The red dots represents food shops and Equity Bank agents. The road splitting dividing the "northern" and "southern" parts of Village 3 is named as "Jomo Kenyatta Road".

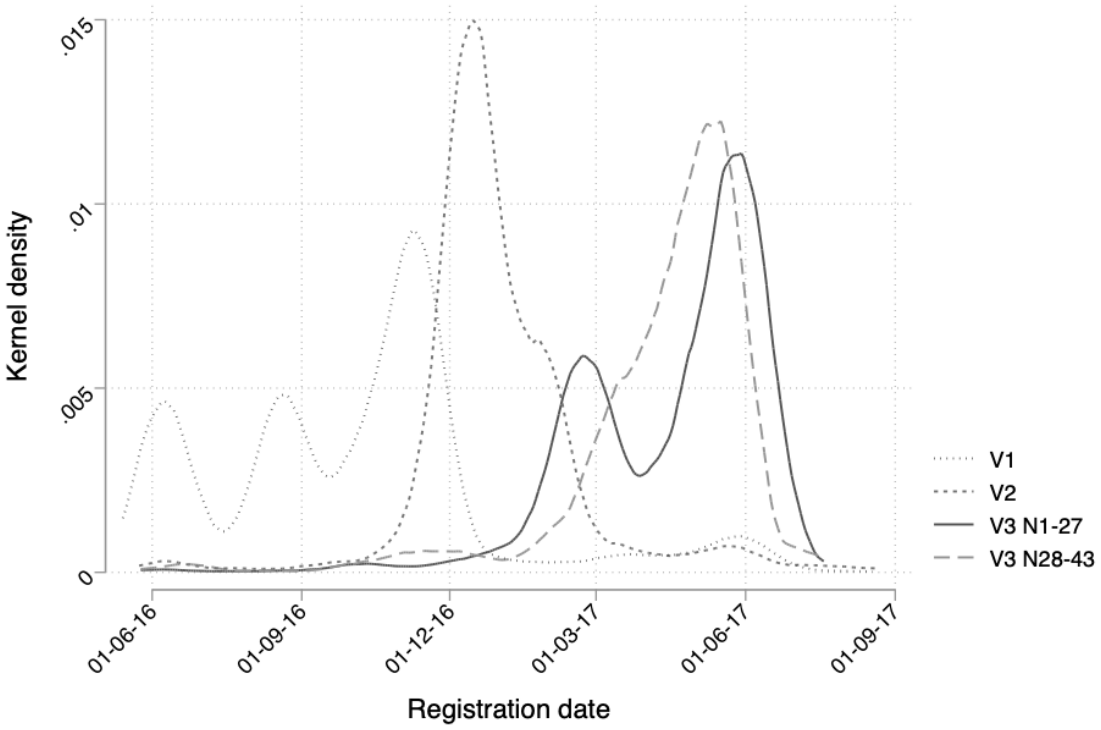

Figure 2: Registration Dates of South Sudanese Refugees in Kalobeyei Settlement

Notes: Dates of registration of South Sudanese refugees as they arrive into Kalobeyei Settlement (UNHCR registration data). The line labelled as "V1" corresponds to registration dates for those who were assigned to live in Village 1. The line labelled as "V2" corresponds to registration dates for those who were assigned to live in Village 2. The line labelled as "V3 N1-27" corresponds to registration dates for those who were assigned to live in Neighbourhoods 1-27 in Village 3. At the time of data collection, these households were receiving unrestricted cash transfers, while all other households in Kalobeyei are receiving restricted cash transfers. The line labelled as "V3 N1-27" corresponds to registration dates for those who were assigned to live in Neighbourhoods 28-43 in Village 3.

Table 1: Prices of Transactions in the Resale Market

	Food items to be resold or exchanged	All products	Maize		Wheat flour	
1	Median price bought using transfers (/kg)		49		80	
	Panel A: Resell food items for cash					
2	Resell prevalence (% of restricted HH)	69	52		32	
2a	Median cash received (KES/kg)		40		50	
2b	Loss (% of (1))		18		38	
	Panel B: Exchange food items for...	**All products**	**Charcoal**	**Greens**	**Charcoal**	**Greens**
3	Exchange prevalence (% of restricted HH)	78	38	6	2.4	3.5
3a	Quantity received against 1kg of maize/wheat		0.2 kg	4 bunches	0.25 kg	5 bunches
3b	Equivalent value of (3b) at market price (KES)		40	40	53	50
3c	Loss (% of (1))		18	18	34	38

Notes: Prevalence of resell and exchange of food items bought using restricted cash transfers and the losses incurred as a result of these activities. This information is captured using the transaction data of self-reported households' usage of their monthly transfer in shop visits in the past 30 days. Maize and wheat flour are the most common good traded by refugees in the resale market. About half of households resold some maize in the month preceding the survey. Row 1 reports the median price of maize and wheat flour, expressed in KES per kilo. Row 2 reports the percentage of households receiving restricted cash transfers who resell maize or wheat flour against cash. Row 2a reports the median value of cash households receive by selling one kilo of maize or wheat flour, expressed in KES. Row 2b reports the loss by computing 2b divided by 1a, expressed in percent. Row 3 reports the percentage of households receiving restricted cash transfers who exchange maize or wheat flour against other goods, such as charcoal and greens. Row 3a reports the median quantity of the good received by giving away one kilo of maize of wheat flour. Row 3b reports the equivalent market value of the quantity reported in Row 3a. Row 3c reports the loss by computing 3b divided by 1, expressed in percent.

Table 2: ITT Effect of the Switch from Restricted to Unrestricted Transfers

	ITT	N	R-squared	Mean of control
Resell food items for cash dummy	-0.107***	893	0.361	0.721
	(0.0301)			
	[0.003]			
Exchange food items for other items dummy	-0.0274	893	0.301	0.799
	(0.0308)			
	[0.358]			
Asset value (KES)	579.4	896	0.454	5085.4
	(392.1)			
	[0.205]			
Asset purchase value (KES)	175.9	896	0.271	392.3
	(121.2)			
	[0.205]			
Non-food expenditure (KES)	241.2***	896	0.537	970.5
	(76.22)			
	[0.006]			
Temptation goods (KES)	108.5***	895	0.401	64.75
	(23.14)			
	[0.001]			
Severe food insecurity dummy	-0.0405	889	0.387	0.795
	(0.0370)			
	[0.274]			
Calories per adult equivalent (log)	0.0562	884	0.365	7.853
	(0.0407)			
	[0.205]			
Food consumption per adult equivalent (log)	-0.0255	884	0.415	4.532
	(0.0335)			
	[0.365]			
Diet Diversity Score	-0.105	1529	0.361	5.393
	(0.0829)			
	[0.223]			
Subjective well-being	0.225***	1522	0.444	2.319
	(0.0740)			
	[0.007]			

Notes: OLS estimates of intent-to-treat effects. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. q-values in square brackets * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table 3: Debts Owed to Food Retailers

	Unrestricted HH	**Restricted HH**
Owed to food retailer (%)	88	90
Average value of debt/household (cash equiv. KES)	15,257	15,392
Average value of debt/head (cash equiv. KES)	2,244	2,368
Average value of debt/head (no. of monthly transfers)	1.6	1.7

Notes: Prevalence of indebted households and the average value of debt for households who are in debt. Row 1 reports the percentage of households who are indebted to a food retailer at the time of the survey. Row 2 reports the mean value of debt for each household, expressed in KES. Households may report debt in terms of number of monthly transfers. Such entries are converted into cash equivalent (for example one month is converted to KES 1,400). Row 3 reports the mean value of debt divided by number of household members, expressed in KES. Row 3 reports the mean value of debt divided by number of household members, expressed in number of household transfers (for example, debt amount of KES 1,400 is equivalent to one monthly transfer). Column 1 reports the corresponding statistics for households receiving unrestricted cash transfers, and column 2 reports the corresponding statistics for households receiving restricted cash transfers.

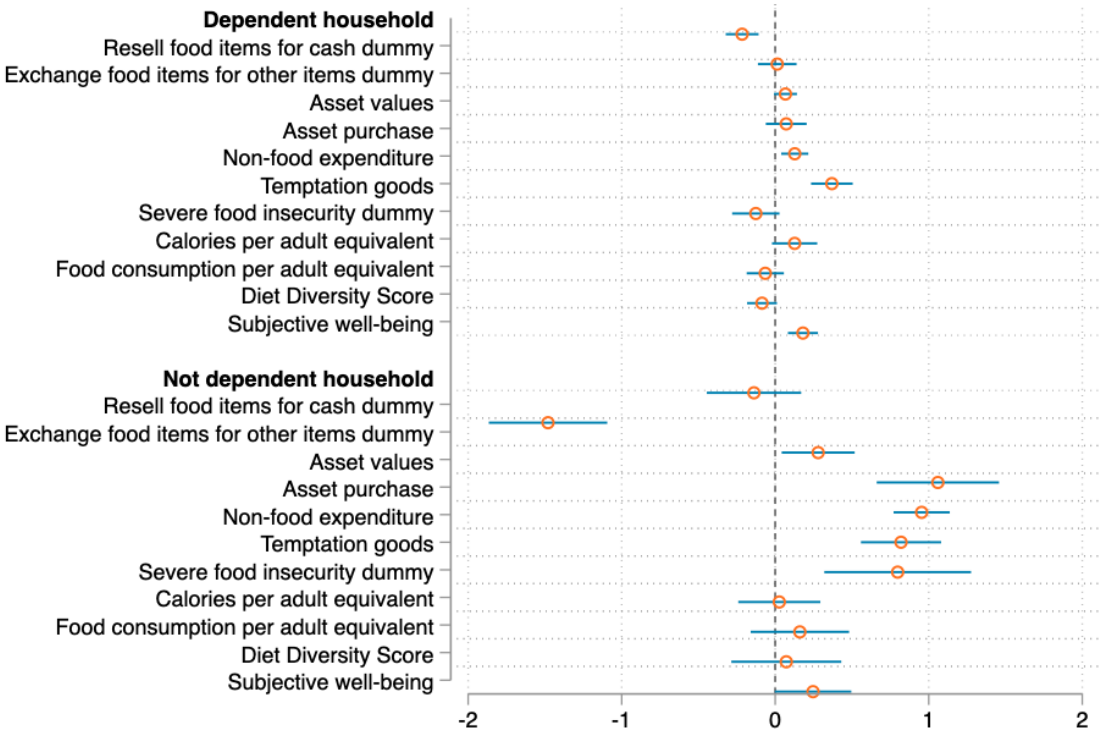

Figure 3: ITT Effects, in Standard Deviations

Notes: OLS estimates of intent-to-treat effects. 90% confidence intervals provided. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Online Appendix

A Theory

A.1 Supplementary propositions

We discuss three theoretical propositions that cannot be tested with our data.

Proposition 3 (Employed households)**.** *For employed households, the consumption level of the restricted good B is unaffected by the restriction. If $q_A^* < p_A$, the consumption level of the unrestricted good A increases, implying that they are better off as a consequence of the restriction.*

The predictions for employed households are opposite of those for assisted households. For employed households, the restriction generates a positive wealth effect due to lower prices in the resale market and a substitution effect away from good B. These effects are both favouring the consumption of good A. For good B, the substitution and wealth effects cancel out because we assumed a Cobb-Douglas utility function. With a more general utility function it is likely that the sum of wealth and substitution effects depends on the elasticity of substitution.

Proposition 4 (All households)**.** *If $q_A^* < p_A$, the total consumption of the unrestricted good A increases and the total consumption of the restricted good B decreases following the restriction.*

The impact of the restriction on household welfare is negative if $w > t$ or if $w \leq t$ and $\gamma n^a > n^e$, and positive otherwise.

The restriction creates winners (employed households) and losers (assisted households). The total effect on household welfare is negative if the gain for employed households is low compared to the loss for assisted households. This occurs if the wage of employed households is large or the number of employed households is low, implying that the welfare gains from an increase in consumption are low. The impact of the restriction on total household welfare is positive if wages are low compared to the cash transfer and if the number of employed households is large, implying that the total welfare gain for employed households is larger than the total welfare loss for assisted households.

Proposition 5 (Retailers)**.** *The total revenue of retailers is unaffected by the restriction.*

Given the emergence of a resale market in which retailers are absent, one could think that retailers would lose out from the restriction. This is not the case. The value

of the goods purchased in the retail market is identical with both modalities of cash transfers. Indeed, all goods consumed, even those purchased in the resale market, are initially sold in the retail market. However, if different retailers are selling the goods A and B, then the retailers selling the good A will gain from the restriction if $q_A^* < p_A$, while those selling good B will end up worse off.

A.2 Proofs

Proof of Proposition 1: We show that the supply in the resale market is always positive. Recall the total supply in the resale market is the following (refer to equation 7):

$$\begin{aligned} n^a s_A^a &= n^a \frac{\gamma t}{p_A(1+\gamma)} \\ &> 0 \end{aligned}$$

since $t > 0$, $p_A > 0$, $n^a > 0$, and $\gamma > 0$.

We show that the size of the resale market increases with the value of transfer. We take the first derivative of the total supply with respect to t which equals $\frac{\gamma n^a}{(\gamma+1)p_A} > 0$, since $p_A > 0$, $n^a > 0$, and $\gamma > 0$.

We show that the size of the resale market increases with the population of assisted households. We take the first derivative of the total supply with respect to n^a which equals $\frac{\gamma t}{(\gamma+1)p\_A} > 0$, since $t > 0$, $p_A > 0$, and $\gamma > 0$.

We show that the size of the resale market increases with the preference parameter. We take the first derivative of the total supply with respect to γ which equals $\frac{n^a t}{(\gamma+1)^2 p_A} > 0$, since $t > 0$, $p_A > 0$, $n^a > 0$, and $\gamma > 0$.

We show that the demand in the resale market is always positive. Recall the total demand in the resale market is the following (refer to equation 8):

$$\begin{aligned} n^e d_A^e &= n^e \frac{w}{q_A(1+\gamma)} \\ &> 0 \end{aligned}$$

since $t > 0$, $p_A > 0$, $n^a > 0$, and $\gamma > 0$.

Proof of Proposition 2: We show that the consumption level of the unrestricted good A is unaffected by the restriction. We calculate the difference in the optimal consumption level of the unrestricted good A between a household receiving restricted transfers

(equation 2) and a household receiving unrestricted transfers (equation 12):

$$\begin{aligned} c_A^{a,restrict} - c_A^{a,unrestrict} &= \frac{t}{p_A(1+\gamma)} - \frac{y}{p_A(1+\gamma)} \\ &= 0 \end{aligned}$$

since for *assisted* households, $y = t$. We show that if $q_A^* < p_A$, the restriction reduces the consumption level of the restricted good B. We compare the optimal consumption levels of a household receiving restricted transfers (equation 13) and a household receiving unrestricted transfers (equation 3):

$$\begin{aligned} c_B^{a,restrict} - c_B^{a,unrestrict} &= \frac{n^e w}{n^a p_B(1+\gamma)} - \frac{t}{p_A(1+\gamma)} \\ &= \frac{n^e w - \gamma n^a t}{\gamma n^a p_B + n^a p_B} \\ &< 0 \end{aligned}$$

since $n^e w < n^a t\gamma$ (see equation 14).

We show that the restriction lowers the well-being of assisted households. We substitute their optimal levels of good A and good B consumption into the utility function (equation 1):

$$\begin{aligned} u^{a,restricted} - u^{a,unrestricted} &= [log(c_A^{a,restrict}) + \gamma log(c_B^{a,restrict})] - [log(c_A^{a,unrestrict}) + \gamma log(c_B^{a,unrestrict})] \\ &= \gamma \left[\log(n^e w) - \log(\gamma n^a t)\right] \\ &< 0 \end{aligned}$$

since $n^e w < n^a t\gamma$ (see equation 14).

Proof of Proposition 3: We show that, if $q_A^* < p_A$, the restriction increases the consumption level of the unrestricted good A for employed households. We calculate the difference in the optimal consumption level of good A between an employed household in an economy with restricted transfers (equation 2) and an employed household in an economy with unrestricted transfers (equation 10):

$$\begin{aligned} c_A^{e,restrict} - c_A^{e,unrestrict} &= \frac{n^a t\gamma}{n^e p_A(1+\gamma)} - \frac{t}{p_A(1+\gamma)} \\ &= -\frac{n^e w - \gamma n^a t}{\gamma n^e p_A + n^e p_A} \\ &> 0 \end{aligned}$$

since $n^e w < n^a t\gamma$ (see equation 14).

We show that for employed households, the consumption level of the restricted good B is unaffected by the restriction:

$$\begin{aligned} c_B^{e,restrict} - c_B^{e,unrestrict} &= \frac{w\gamma}{p_B(1+\gamma)} - \frac{\gamma y}{p_B(1+\gamma)} \\ &= 0 \end{aligned}$$

since for employed households in an economy with restricted transfers, total income $y = w$.

We show that the restrictions increase the welfare of employed households:

$$\begin{aligned} u^{e,restricted} - u^{e,unrestricted} &= [log(c_A^{e,restrict}) + \gamma log(c_B^{e,restrict})] - [log(c_A^{e,unrestrict}) + \gamma log(c_B^{e,unrestrict})] \\ &= \log\left(\frac{\gamma n^a t}{n^e w}\right) \\ &> 0 \end{aligned}$$

since $n^e w < n^a t\gamma$ (see equation 14).

Proof of Proposition 4: We show that, if $q_A^* < p_A$, the restriction increases the total consumption of the unrestricted good A:

$$\begin{aligned} n^a(c_A^{a,restrict} - c_A^{a,unrestrict}) + n^e(c_A^{e,restrict} - c_A^{e,unrestrict}) &= n^e\left(-\frac{n^e w - \gamma n^a t}{\gamma n^e p_A + n^e p_A}\right) \\ &> 0 \end{aligned}$$

since $n^e w < n^a t\gamma$ (see equation 14).

We show that, if $q_A^* < p_A$, the restriction decreases the total consumption of the restricted good B:

$$\begin{aligned} n^a(c_B^{a,restrict} - c_B^{a,unrestrict}) + n^e(c_B^{e,restrict} - c_B^{e,unrestrict}) &= n^a\left(\frac{n^e w - \gamma n^a t}{\gamma n^a p_B + n^a p_B}\right) \\ &< 0 \end{aligned}$$

since $n^e w < n^a t\gamma$ (see equation 14).

The total impact of the restriction on utility is:

$$(u^{a,restricted} - u^{a,unrestricted}) + (u^{e,restricted} - u^{e,unrestricted}) = (n^e - \gamma n^a)\log\left(\frac{\gamma n^a t}{n^e w}\right)$$

with $\log\left(\frac{\gamma n^a t}{n^e w}\right) > 0$ since $n^e w < n^a t\gamma$ (see equation 14).

Proof of Proposition 5: We show that the value of the goods purchased in the re-

tail market is identical with both modalities of cash transfers. The difference in total consumption of good A by assisted households between an economy with restricted transfers and unrestricted transfers is:

$$n^a(c_A^{a,restrict} - c_A^{a,unrestrict}) = 0$$

The difference in total consumption of good A by employed households between an economy with restricted transfers and unrestricted transfers is:

$$n^e(c_A^{e,restrict} - c_A^{e,unrestrict}) = n^e \left(-\frac{n^e w - \gamma n^a t}{\gamma n^e p_A + n^e p_A} \right)$$

The difference in total consumption of good B by assisted households between an economy with restricted transfers and unrestricted transfers is:

$$n^a(c_B^{a,restrict} - c_B^{a,unrestrict}) = n^a \left(\frac{n^e w - \gamma n^a t}{\gamma n^a p_B + n^a p_B} \right)$$

The difference in total consumption of good B by employed households between an economy with restricted transfers and unrestricted transfers is:

$$n^e(c_B^{e,restrict} - c_B^{e,unrestrict}) = 0$$

The summation of the above four differences equates to zero.

B Variables

B.1 Outcomes variables

In this section, we provide details of the construction of the outcome variables.

We collected detailed transaction data of households' usage of their monthly transfer in shop visits in the 30 days preceding the survey. This includes details of which items they resold or exchanged for other items. Using the data, we construct the following outcome variables

- Resale dummy: equals to one if household resold food for cash in the past 30 days and zero otherwise.
- Exchange dummy: equals to one if household exchanged food for other items in the past 30 days and zero otherwise.

We constructed the indicators of asset ownership and purchase as follows:

- Asset value: This variable aggregates the value of assets owned by the household. For the following assets, questions were asked at a household level: radio, television, computer and tablets, refrigerator, solar panel, generator, table, chair, sofa, mattress, cupboard, and clock. For the following assets, questions were asked at the individual level: mobile phone, mp3, watch, bicycle, motorcycle, and car. The individual assets were aggregated to obtain household level measures. The value of each asset is estimated as the median value reported by survey respondents during a business survey in Kakuma and Kalobeyei in 2018 (Delius and Sterck, 2019).[36] The variable is expressed in KES.
- Asset purchases: We apply the same method as in the variable *Asset value*.

Non-food expenditure variables are constructed using the values of spending on non-durable items in the past 30 days and semi-durable items in the past three months. Non-food expenditure also includes food/drinks consumption outside home. For some items, questions were asked at a household level, while for other items, questions were asked to each individual separately (see Table A.1). We aggregated the data as follows:

- Non-food expenditure: summation of expenditures of non-durables, semi-durables and food/drinks away from home converted into KES per month per household.

[36] The survey of (Delius and Sterck, 2019) did not collect information on ownership of MP3 players. To construct the asset index, We assume that the value of a MP3 is similar to the value of a mobile phone (note that only 12 households reported owning a MP3 player).

The variable used in the main table of results is expressed in levels (KES) (see Table 2). In the pre-analysis plan, this variable was described to be transformed using inverse hyperbolic sine (IHS) (see Table A.20 for the corresponding estimate).

- Temptation goods: Summation of soda, tobacco, meals away from home, alcohol, and video halls converted into monthly expenditure. This total value is transformed using IHS. The variable used in the main table of results is expressed in levels (KES) (see Table 2). In the pre-analysis plan, this variable was described to be transformed using inverse hyperbolic sine (IHS) (see Table A.20 for the corresponding estimate).

Table A.1: Non-food Expenditure Items

Categories	**Items**
Expenditure items outside home asked at an individual level (7 days recall period)	Alcohol, tobacco, soda, coffee and tea (outside home), drinking water, meals outside home
Non-durable expenditure items asked at household level (30 days recall period)	Electricity, firewood/charcoal, rent for house, repair/maintain of house, salaries for housekeeper (e.g. for cleaning, cooking, getting water), soap and toothpaste, health expenses (hospital/clinic charges, medicine, traditional healers), milling of grains
Non-durable expenditure items asked at an individual level (30 days recall period)	Transport, airtime and charging phones, hygiene products, cosmetics, cinema and video hall, barber and beauty shops, ceremonies
Semi-durable expenditure items asked at household level (3 months recall period)	Education (including registration and examination fees, tuition, uniforms, textbooks, school meals (not home-cooked)); things in the kitchen (including plates, bowls, knives, forks, spoons, pots, pan, jerrycan, plastic bucket); plugs, cables, batteries, chargers
Semi-durable expenditure items asked at an individual level (3 months recall period)	Clothes, shoes and bags, tailoring services

Food consumption and expenditure indicators are constructed using the values and volumes of individual items consumed at home. These items are: sorghum, millet, maize and maize flour, super cereal, wheat and wheat flour, rice, pasta, potato, peas,

beans, lentils, onion, green leafy vegetables, tomatoes, garlic, ginger, carrots, pumpkin, cabbage, arrow roots, banana, mango, avocado, pawpaws, pineapples, watermelon, apple, passion fruit, red meat (beef, goat, camel), chicken and poultry, fish (including omena), eggs, milk, milk powder, yogurt, oil, and sugar.

For each of the above items, we collected data on how they obtained them and the price which they paid to obtain them. The sources are: cash, home production, from nature[37], restricted cash transfer, ATM card to access unrestricted cash transfer, given for free by friends/family/neighbours, and given for free by organisations.

We aggregated the data as follows:

- Calories per adult equivalent (log): Calories intake per adult equivalent in a household is calculated by converting the list of food items consumed by the household in the past seven days into calories. Data on calories were obtained from the U.S. Department of Agriculture. The relevant statistics are in MacPherson and Sterck (2021). We then calculate adult equivalents by following Deaton and Zaidi (2002). The formula is $AE = [(1 + \beta(A - 1)) + \alpha K]^{\theta}$ where $\alpha = 0.3$, $\beta = 1$ and $\theta = 0.9$, A =the number of adults in the household, and K =the number of children in the household. We divide the value of total calories by AE and apply a log transformation.

- Food consumption per adult equivalent (log): The quantity consumed (kg/day) in a household is multiplied by the median price in KES per kilo in our data. If the household got the item from multiple sources, we used the median price in KES for those sources. We exclude sources which do not require payment (e.g. gifts). We then aggregate the values calculated for the list of food items and divide the total by number of adult equivalent AE. The total value of food consumption per adult equivalent is then log-transformed.

The indicators of food insecurity at the household level and diet diversity at the individual level are constructed as follows:

- Diet diversity: Individual diet diversity is a summation of the number of groups of food items an individual has consumed in the past seven days. We follow the grouping suggested in Kennedy et al. (2011). The groups are: 1) Maize, rice sorghum, millet, wheat flour, chapatti and other cereals, 2) cassava, potatoes and sweet potatoes, 3) beans, peas and lentils, groundnuts, cashew nuts and other nuts, 4) vegetables, 5) fruits, 6) beef, goat, pork, poultry and other meat, 7) fish

[37]There were reports of individuals picking leaves in the bushes.

and shellfish, 8) eggs, 9) milk, yoghurt and other dairy products, 10) sugar, sugar products and honey, 11) oils, fats and butter, and 12) spices, salt, and fish powder.

- Food insecurity dummy is constructed using the Household Food Insecurity Access Scale (HFIAS) score, which is an aggregation of occurrences and frequencies of hunger, reduced consumption and worries of insufficient food (Coates et al., 2007). The variable is valued as one if the household is categorised as "severely food insecure" and zero otherwise.

The subjective well-being indicator is constructed using responses to the question "All things considered, how satisfied are you with your life as a whole these days?" (5-point Likert scale). Answers range from 1 "very unsatisfied" to 5 "Very satisfied".

Table A.2: Summary Statistics of Outcome Variables

Variable	Obs	Mean	Std. Dev.	Min	Max	P50
Resell food items for cash dummy	893	.641	.48	0	1	1
Exchange food items for other items dummy	893	.753	.432	0	1	1
Asset value (KES)	896	5370.061	7536.264	0	116600	3600
Asset purchase value (KES)	896	502.651	1573.483	0	14200	0
Non-food expenditure (KES)	896	982.466	1321.803	0	13085.72	533.75
Temptation goods (KES)	895	74.226	292.063	0	3385.714	0
Severe food insecurity dummy	889	.739	.439	0	1	1
Calories per adult equivalent (log)	884	7.899	.43	5.969	9.926	7.901
Food consumption per adult equivalent (log)	884	4.569	.446	2.631	6.292	4.557
Diet Diversity Score	1529	5.517	1.441	1	11	5
Subjective well-being	1522	2.518	1.363	1	5	2

B.2 Treatment variables

Table A.3: Actual Treatment versus Intent-to-treat Groups

	Assigned to unrestricted transfers	Assigned to restricted transfers
Received unrestricted transfers	300	71
Received restricted transfers	0	525

Notes: Number of households surveyed. "Assigned with unrestricted transfers" refers to the households who are registered by the WFP to be living in the area with unrestricted transfers. "Assigned with restricted transfers" refers to the households who are registered by the WFP to be living in the area with restricted transfers. "Received unrestricted transfers" refer the households who reported receiving unrestricted transfers. "Received restricted transfers" refer the households who reported receiving restricted transfers.

B.3 Control variables

Table A.4: Definition of Control Variables Analysed at Household Level

Variable	Definition	Household level aggregation
No. of adults	Number of adults in the household	Collected at household level
No. of people	Number of members in the household	Collected at household level
Age of HH head	Age of head of household	Collected at household level
HH head is female	= 1 if head of household is a female, 0 otherwise	Collected at household level
HH head is married	= 1 if head of household is married, 0 otherwise	Collected at household level
HH head is polygamous	= 1 if head of household is polygamous, 0 otherwise	Collected at household level
HH head is from Equatorial	= 1 if head of household is from Equatorial region of South Sudan, 0 otherwise	Collected at household level
HH head is from Uppernile	= 1 if head of household is from Upper Nile region of South Sudan, 0 otherwise	Collected at household level
Has income-generating activity before fleeing	= 1 if household had income-generating activities before displacement, and 0 otherwise.	Summation within household
Ration size	Ration size of the household	Collected at household level
Cash & assets upon arrival	Total value of assets and cash brought to Kalobeyei (KES). See Section B for the list of assets.	Collected at household level
Farmed before fleeing	= 1 if household has farmed before, 0 otherwise	Collected at household level
Father's education	Highest education level of household members' father	Maximum within household
Mother's education	Highest education level of household members' mother	Maximum within household
Total hh income (last 30d)	Total income in the past 30 days (KES)	Summation within household
Remittance	Total remittance received in the past 3 months (KES).	Summation within household

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Variable	Definition	Household level aggregation
Relationship with the leader	= 1 if the person is a leader, 2 if the person is a household member of the leader, 3 if the person is a friend/family member of the leader, 4 if the person is an acquaintance of the leader, 5 if the person does not know the leader.	Maximum within household
Farming in Kalo	= 1 if household participated in farming in Kalobeyei,	
0 otherwise	Collected at household level	
Harvested dum	= 1 if harvested crops in Kalobeyei in the past 3 months	
0 otherwise	Collected at household level	
Ethnicity: lotuka	= 1 if head of household is of Lotuka ethnicity,	
0 otherwise	Collected at household level	
Ethnicity: didinga	= 1 if head of household is of Didinga ethnicity,	
0 otherwise	Collected at household level	
Ethnicity: lopit	= 1 if head of household is of Lopit ethnicity,	
0 otherwise	Collected at household level	
Soap transfers	Cash received to buy soap in the most recent transfer (KES), or if given bars of soap, we take the cash-equivalent of that bar.	Collected at household level
Free school meals	Number of people living in the household who are receiving free meal at school	Collected at household level
Free Uji	= 1 if household received super cereal+ in the last month, 0 otherwise	Collected at household level
Free plumpy	= 1 if household received Plumpy'Sup in the last month, 0 otherwise	Collected at household level
Free school uniform	= 1 if household received free school material or uniform, 0 otherwise	Collected at household level
Transfer for vulnerable	= 1 if household received additional money last month because the household has a vulnerable or disabled member, 0 otherwise.	Collected at household level
Credit from NGO	= 1 if a household member received a loan from an NGO, 0 otherwise.	Collected at household level
Received livestock	= 1 if a household member received a poultry/chicken from an NGO, 0 otherwise.	Collected at household level

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Variable	Definition	Household level aggregation
Dist to market (m)	Distance from household to the nearest market (m), measured as crow flies	Collected at household level
Dist to road (m)	Distance from household to the Kenyatta road (m), measured as crow flies	Collected at household level
Dist to Kakuma (m)	Distance from household to the nearest point in Kakuma camp (m), measured as crow flies	Collected at household level

Table A.5: Definition of Control Variables Analysed at Individual Level

Variable	Definition	Household level aggregation
Education	Highest level of education completed.	Maximum within household
Vocational	Equals 1 if person has vocational training in Kalobeyei, 0 otherwise.	Maximum within household
Swahili level	= 0 if not at all, 1 if a bit, 2 if well, 3 if very well	Maximum within household
English level	= 0 if not at all, 1 if abit, 2 if well, 3 if very well	Maximum within household
Work exp in Kalo	= 1 if person has work experience in Kalobeyei, 0 otherwise	Summation within household
Work experience	Total years of work experience	Maximum within household
No of relatives in camp	Number of friends and family members outside Kalobeyei/Kakuma who have sent the person money or goods	Summation within household
Health shocks	No. of times in the past 12 months the person has been admitted to a hospital suddenly. Winsorised at 99 percentile.	Summation within household

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Variable	Definition	Household level aggregation
Other hh shocks	Number of times the person has experienced the following in the past 12 months: - Theft/burglary - Robbery - Violence - Personal threats Winsorised at 99 percentile.	Summation within household
Mental health	= 1 if person scores larger or equal to 10 in the Patient Health Questionnaire (PHQ-9) Depression Test Questionnaire, 0 otherwise	Maximum within household
Physical health	= 1 if answered 'Severe difficulty' or 'Extreme difficult' for either of the two questions: 1) How much difficulty did you have in standing for long periods such as 30 minutes? 2) How much difficulty did you have in walking a long distance such as akilometer?	Maximum within household
VSLA participation	= 1 if person is currently participating in self-help group, savings group or VSLA	Maximum within household

Table A.6: Summary statistics of control variables

Variable	Obs	Mean	Std. Dev.	Min	Max	P50
No. of adults	895	1.722	.88	1	8	2
No. of people	895	6.632	2.825	1	21	6
Age of HH head	895	31.755	9.412	18	75	31
HH head is female	895	.68	.467	0	1	1
HH head is married	895	.713	.453	0	1	1
HH head is polygamous	895	.32	.467	0	1	0
HH head is from Equatorial	894	.973	.162	0	1	1
HH head is from Uppernile	894	.022	.148	0	1	0
Had a job before fleeing	896	.544	.708	0	4	0
Ration size	889	6.772	2.95	0	22	6
Cash and assets at arrival (IHS)	896	3.002	4.249	0	12.256	0
Farmed beflore fleeing	892	.859	.348	0	1	1
Highest father's education	870	2.621	4.631	0	16	0
Highest mother's education	885	.435	1.829	0	14	0
Has a job	896	.105	.331	0	2	0
Remittance	896	785.824	3488.29	0	40000	0
Relationship with leader	895	1.987	.478	1	5	2
Farming in Kalo	885	.312	.464	0	1	0
Harvested (last 3 months)	885	.216	.412	0	1	0
Ethnicity: Lotuka	896	.578	.494	0	1	1
Ethnicity: Didinga	896	.229	.42	0	1	0
Ethnicity: Lopit	896	.166	.373	0	1	0
Money to buy soap (KES)	882	1931.866	1402.659	0	12500	1750
Free school meals	892	4.092	2.407	0	14	4
Free Uji	892	.318	.466	0	1	0
Free plumpy	892	.063	.243	0	1	0
Free school uniform	892	.095	.294	0	1	0
Transfer for vulnerable	892	.003	.058	0	1	0
Credit from NGO	892	0	0	0	0	0
Poultry distribution	895	.128	.335	0	1	0
Dist to market (m)	896	569.352	329.858	34.017	3633.12	544.545
Dist to road (m)	896	843.912	443.322	50.347	1685.773	839.103
Dist to Kakuma (m)	896	4808.405	548.769	3627.175	8977.008	4861.498
Education	1529	3.833	4.146	0	16	3
Vocational	1529	.114	.318	0	1	0
Swahili level	1529	.069	.253	0	1	0
English level	1529	.315	.465	0	1	0
Has a job	1528	.062	.24	0	1	0
Work experience in Kalobeyei	1529	.71	3.71	0	48	0
Work experience	1528	19.132	46.576	0	408	0
No of relatives in camp	1527	1.101	2.417	0	32	0
Health shocks	1522	.284	.71	0	4	0
Other HH shocks	1529	1.169	2.104	0	12	0

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Variable	Obs	Mean	Std. Dev.	Min	Max	P50
Mental health	1529	.229	.42	0	1	0
Physical health	1518	.184	.388	0	1	0
VSLA participation	1529	.01	.099	0	1	0

C Balance checks

Table A.7: Balance Table of Pre-determined Variables

Variable	(1) Registered in control area Mean/SE	(2) Registered in treated area Mean/SE	(3) Total Mean/SE	T-test P-value (1)-(2)	Normalized difference (1)-(2)
No. of adults	1.644	1.757	1.719	0.116	-0.129
	(0.060)	(0.040)	(0.034)		
No. of people	6.532	6.727	6.661	0.413	-0.069
	(0.182)	(0.154)	(0.119)		
Age of HH head	31.864	31.679	31.741	0.786	0.020
	(0.511)	(0.452)	(0.345)		
HH head is female	0.722	0.665	0.684	0.127	0.122
	(0.032)	(0.019)	(0.017)		
HH head is married	0.715	0.718	0.717	0.948	-0.005
	(0.028)	(0.021)	(0.017)		
HH head is polygamous	0.337	0.312	0.320	0.422	0.054
	(0.025)	(0.018)	(0.015)		
HH head is from Equatorial	0.985	0.970	0.975	0.227	0.090
	(0.008)	(0.009)	(0.007)		
HH head is from Uppernile	0.012	0.025	0.021	0.190	-0.088
	(0.006)	(0.008)	(0.006)		
Had an income-generating activity before fleeing	0.545	0.527	0.533	0.815	0.024
	(0.056)	(0.049)	(0.037)		
Ration size	6.609	6.897	6.800	0.243	-0.098
	(0.194)	(0.153)	(0.121)		
Cash and assets at arrival (IHS)	3.012	3.082	3.058	0.873	-0.017
	(0.319)	(0.302)	(0.226)		
Farmed beflore fleeing	0.898	0.839	0.859	0.148	0.170
	(0.027)	(0.030)	(0.022)		
Father's education	2.615	2.561	2.579	0.898	0.012
	(0.353)	(0.227)	(0.191)		
Mother's education	0.305	0.480	0.421	0.132	-0.097
	(0.089)	(0.075)	(0.058)		
N	300	596	896		
Clusters	43	84	127		
F-test of joint significance (p-value)				0.544	
F-test, number of observations				896	

Notes: Standard errors clustered at compound level. Survey weights applied. **, **, and * indicate significance at the 1, 5, and 10 percent critical levels. Missing values in variables are replaced with the group mean.

Table A.8: Balance Table of Programmatic Variables

Variable	(1) Registered in control area Mean/SE	(2) Registered in treated area Mean/SE	(3) Total Mean/SE	T-test P-value (1)-(2)	Normalized difference (1)-(2)
Money to buy soap (KES)	1917.792	1957.110	1943.855	0.781	-0.028
	(94.525)	(105.738)	(76.808)		
Free school meals	3.976	4.183	4.113	0.290	-0.086
	(0.146)	(0.131)	(0.100)		
Free Uji	0.316	0.317	0.317	0.981	-0.002
	(0.030)	(0.027)	(0.020)		
Free plumpy	0.065	0.059	0.061	0.733	0.025
	(0.015)	(0.010)	(0.008)		
Free school uniform	0.082	0.101	0.094	0.491	-0.064
	(0.021)	(0.018)	(0.014)		
Transfer for vulnerable	0.004	0.004	0.004	0.964	-0.004
	(0.004)	(0.003)	(0.002)		
Poultry distribution	0.175	0.107	0.130	0.059*	0.202
	(0.032)	(0.017)	(0.016)		
Farming in Kalo	0.298	0.305	0.303	0.911	-0.015
	(0.049)	(0.035)	(0.028)		
Harvested (last 3 months)	0.178	0.222	0.207	0.338	-0.106
	(0.035)	(0.029)	(0.022)		
VSLA participation	0.014	0.016	0.016	0.793	-0.019
	(0.007)	(0.006)	(0.004)		
N	300	596	896		
Clusters	43	84	127		
F-test of joint significance (p-value)				0.543	
F-test, number of observations				896	

Notes: Standard errors clustered at compound level. Survey weights applied. **, **, and * indicate significance at the 1, 5, and 10 percent critical levels. Missing values in variables are replaced with the group mean.

Table A.9: Balance Table of Socio-economic Variables

Variable	(1) Registered in control area Mean/SE	(2) Registered in treated area Mean/SE	(3) Total Mean/SE	T-test P-value (1)-(2)	Normalized difference (1)-(2)
Has an income-generating activity	0.127	0.088	0.101	0.107	0.117
	(0.019)	(0.014)	(0.012)		
Remittance	766.527	760.256	762.370	0.983	0.002
	(239.764)	(178.242)	(142.610)		
Relationship with leader	2.005	1.972	1.983	0.448	0.070
	(0.033)	(0.029)	(0.022)		
Ethnicity: Lotuka	0.615	0.589	0.598	0.602	0.053
	(0.040)	(0.030)	(0.024)		
Ethnicity: Didinga	0.203	0.230	0.221	0.507	-0.062
	(0.031)	(0.025)	(0.019)		
Ethnicity: Lopit	0.163	0.181	0.175	0.561	-0.049
	(0.023)	(0.021)	(0.016)		
Education	4.518	4.805	4.709	0.427	-0.066
	(0.320)	(0.172)	(0.157)		
Vocational	0.185	0.163	0.171	0.411	0.056
	(0.021)	(0.015)	(0.012)		
Swahili level	0.093	0.106	0.102	0.621	-0.040
	(0.020)	(0.014)	(0.012)		
English level	0.335	0.432	0.400	0.011**	-0.199
	(0.032)	(0.020)	(0.018)		
Work experience in Kalobeyei	1.421	1.056	1.179	0.264	0.073
	(0.253)	(0.207)	(0.163)		
Work experience	29.734	33.030	31.919	0.557	-0.050
	(3.855)	(4.092)	(3.000)		
No of relatives in camp	1.786	1.837	1.820	0.924	-0.011
	(0.500)	(0.204)	(0.215)		
Health shocks	0.523	0.483	0.496	0.756	0.039
	(0.112)	(0.067)	(0.058)		
Other HH shocks	2.581	1.657	1.969	0.045**	0.239
	(0.416)	(0.193)	(0.192)		
Mental health	0.483	0.371	0.409	0.282	0.167
	(0.094)	(0.045)	(0.044)		
Physical health	0.312	0.242	0.265	0.247	0.160
	(0.053)	(0.031)	(0.027)		
N	300	596	896		
Clusters	43	84	127		
F-test of joint significance (p-value)				0.117	
F-test, number of observations				896	

Notes: Standard errors clustered at compound level. Survey weights applied. **, **, and * indicate significance at the 1, 5, and 10 percent critical levels. Missing values in variables are replaced with the group mean.

Table A.10: Balance Table of Geographic Variables

Variable	(1) Registered in control area Mean/SE	(2) Registered in treated area Mean/SE	(3) Total Mean/SE	T-test P-value (1)-(2)	Normalized difference (1)-(2)
Dist to market (m)	514.194	591.298	565.305	0.201	-0.234
	(49.480)	(34.491)	(28.373)		
Dist to road (m)	597.504	960.726	838.276	0.000***	-0.819
	(55.643)	(46.874)	(39.830)		
Dist to Kakuma (m)	4290.344	5066.276	4804.692	0.000***	-1.414
	(58.856)	(44.411)	(47.737)		
N	300	596	896		
Clusters	43	84	127		
F-test of joint significance (p-value)				0.000***	
F-test, number of observations				896	

Notes: Standard errors clustered at compound level. Survey weights applied. **, **, and * indicate significance at the 1, 5, and 10 percent critical levels. Missing values in variables are replaced with the group mean.

D Additional results

Table A.11: Prices in the Resale and Retail Markets

	Maize and Wheat flour			Maize			Wheat flour		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Resale market dummy	-18.14***	-19.57***	-10***	-12.18***	-13.46***	-8.833***	-27.40***	-27.75***	-30***
	(0.328)	(0.384)	(2.205)	(0.410)	(0.501)	(0.0370)	(0.405)	(0.573)	(0.114)
Control mean	61.19	61.19		49.93	49.93		78.78	78.78	
Control median			50			48.83			80
N	1985	1985	1985	1208	1208	1208	777	777	777
R-squared	0.172	0.545		0.224	0.348		0.334	0.427	
Controls	No	Yes	No	No	Yes	No	No	Yes	No

Notes: OLS estimates reported in all columns except columns 3, 6, and 9, which show LAD estimates. Dependent variable is the unit price (KES/kg) reported households. Full set of control variables, including product quantity consumed, are included but not reported in columns 2, 5, and 8. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, survey date, and product fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. OLS estimates are adjusted using survey weights. * p<0.1, ** p<0.05, *** p<0.01

Table A.12: Intensive Margins of Resale and Barter Trade

	ITT	N	R-squared	N
Value resold (prop of transfer)	-0.00939***	883	0.298	0.0351
	(0.00281)			
Value exchanged (prop of transfer)	-0.00639**	883	0.295	0.0421
	(0.00266)			

Notes: OLS estimates. Outcome variables are listed on the left. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. OLS estimates are adjusted using survey weights. * p<0.1, ** p<0.05, *** p<0.01

Table A.13: Temptation Goods: Intensive and Extensive Margins

	ITT	N	R-squared	Mean of treated	Mean of control
Soda spending dummy	0.00755	896	0.249	0.0499	0.0403
	(0.0178)				
Tobacco spending dummy	0.0627***	896	0.305	0.0554	0.0358
	(0.0125)				
Meals away from home spending dummy	0.0219*	896	0.231	0.0257	0.0364
	(0.0112)				
Alcohol spending dummy	0.0673***	896	0.259	0.134	0.0981
	(0.0231)				
Video hall spending dummy	-0.0495***	896	0.246	0.0285	0.0613
	(0.0116)				
Spending on soda/month (KES)	30.92***	895	0.279	18.20	17.04
	(10.82)				
Spending on tobacco/month (KES)	9.096***	895	0.532	7.493	6.569
	(2.425)				
Spending on meals away from home/month (KES)	27.93***	895	0.215	12.00	9.249
	(9.074)				
Spending on alcohol/month (KES)	42.49***	895	0.345	37.87	29.68
	(11.55)				
Spending on video halls/month (KES)	-1.862**	896	0.194	1.420	2.211
	(0.843)				

Notes: OLS estimates of ITT effects. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.14: Average treatment effect on the treated

	ATET	N	R-squared
Resell food items for cash dummy	-0.124***	893	0.360
	(0.0350)		
Exchange food items for other items dummy	-0.0318	893	0.301
	(0.0357)		
Asset value (KES)	674.5	896	0.454
	(456.4)		
Asset purchase value (KES)	204.7	896	0.271
	(141.5)		
Non-food expenditure (KES)	280.8***	896	0.528
	(88.48)		
Temptation goods (KES)	126.2***	895	0.392
	(26.70)		
Severe food insecurity dummy	-0.0470	889	0.387
	(0.0432)		
Calories per adult equivalent (log)	0.0654	884	0.364
	(0.0470)		
Food consumption per adult equivalent (log)	-0.0296	884	0.414
	(0.0391)		
Diet Diversity Score	-0.127	1529	0.360
	(0.100)		
Subjective well-being	0.273***	1522	0.444
	(0.0907)		

Notes: 2SLS estimates of ATET. Outcome variables are listed on the left. The first stage is reported in Table A.15. The instrument used is the ITT variable, which is defined as one if a household is registered by WFP to be living in a compound with unrestricted cash transfers, and zero otherwise. The endogenous variable is the treatment variable which defined as one if a household is receiving unrestricted cash transfers, and zero otherwise. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out.

Table A.15: Average treatment effect on the treated: First stage

	First stage	N	R-squared	Effective F statistic
Resell food items for cash dummy	0.862***	893	0.775	464.1
	(0.0194)			
Exchange food items for other items dummy	0.862***	893	0.775	464.1
	(0.0194)			
Asset value (KES)	0.859***	896	0.772	455.3
	(0.0193)			
Asset purchase value (KES)	0.859***	896	0.772	455.3
	(0.0193)			
Non-food expenditure (KES)	0.859***	896	0.772	455.3
	(0.0193)			
Temptation goods (KES)	0.859***	895	0.772	455.2
	(0.0193)			
Severe food insecurity dummy	0.861***	889	0.774	458.2
	(0.0195)			
Calories per adult equivalent (log)	0.859***	884	0.773	451.1
	(0.0196)			
Food consumption per adult equivalent (log)	0.859***	884	0.773	451.1
	(0.0196)			
Diet Diversity Score	0.825***	1529	0.757	709.3
	(0.0205)			
Subjective well-being	0.823***	1522	0.756	699.4
	(0.0206)			

Notes: First stage estimates of the 2SLS estimation of ATET. The second stage is reported in Table A.14. Effective F statistics of Olea and Pflueger (2013) are reported in the last column. The instrument used is the ITT variable, which is defined as one if a household is registered by WFP to be living in a compound with unrestricted cash transfers, and zero otherwise. The endogenous variable is the treatment variable which defined as one if a household is receiving unrestricted cash transfers, and zero otherwise. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. First stage estimates adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.16: Association between dependency on retailers and outcome variables

	Dependent HH	N	R-squared
Resell food items for cash dummy	0.375***	892	0.379
	(0.0402)		
Exchange food items for other items dummy	0.495***	892	0.337
	(0.0384)		
Asset value (KES)	76.82	892	0.458
	(454.7)		
Asset purchase value (KES)	-870.9***	892	0.280
	(222.9)		
Non-food expenditure (KES)	-69.73	892	0.536
	(90.96)		
Temptation goods (KES)	1.278	891	0.402
	(20.99)		
Severe food insecurity dummy	0.248***	887	0.395
	(0.0398)		
Calories per adult equivalent (log)	0.0285	882	0.365
	(0.0322)		
Food consumption per adult equivalent (log)	-0.0374	882	0.416
	(0.0305)		
Diet Diversity Score	-0.570***	1525	0.367
	(0.107)		
Subjective well-being	-0.205***	1518	0.444
	(0.0627)		

Notes: OLS estimates of the association between household outcomes and dependency on retailers. Variable of interest is *Dependent HH*, which is defined as one if the household was indebted to a food retailer or had an Equity ATM card or SIM card kept by a food retailer, and zero otherwise. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.17: Unit price/mean price and method

	All food items			Three common food items		
	(1)	(2)	(3)	(4)	(5)	(6)
Paid using physical cash	-0.0776***	-0.0874***	-0.136***	-0.0766***	-0.0687***	-0.0669***
	(0.0125)	(0.0120)	(0.0199)	(0.0143)	(0.0119)	(0.0120)
N	5703	5460	5460	2573	2455	2455
R-squared	0.00311	0.0392	0.0445	0.00303	0.0810	0.0827
Controls	No	Yes	Yes	No	Yes	Yes
Product FE	No	No	Yes	No	No	Yes

Notes: OLS estimates. Dependent variable is the unit price reported by a household divided by the mean price reported in the sample. Variable of interest is *Paid using physical cash*, which is equal to one if the price is recorded in a physical transaction, and zero otherwise. Columns 1-3 include all food items which households reported to have consumed in the seven days prior to the interview. Rows 4-6 include the three most commonly consumed food items: maize, oil, and beans. All regressions include enumerator, arrival date, and survey date fixed effects. Full set of control variables listed in table A.4, as well as quantity consumed, are included in columns 2-3 and 5-6. Product fixed effects are included in columns 3 and 6. Clustered standard errors in parentheses. Adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.18: HETEROGENEITY IN ITT EFFECTS BY DEPENDENCY ON RETAILERS

	ITT	Dependent HH In debt/card held	Interaction Dependent HH X ITT	N	R^2
Resell food items for cash dummy	-0.0627	0.405***	-0.0349	892	0.379
	(0.0840)	(0.0684)	(0.0821)		
Exchange food items for other items dummy	-0.594***	-0.0221	0.599***	892	0.344
	(0.0933)	(0.0917)	(0.0958)		
Asset value (KES)	2473.3*	1703.7	-1883.1	892	0.458
	(1266.4)	(1195.9)	(1269.5)		
Asset purchase value (KES)	1577.5***	399.7**	-1470.6***	892	0.283
	(357.7)	(200.0)	(349.3)		
Non-food expenditure (KES)	1383.6***	966.1***	-1199.0***	892	0.539
	(160.1)	(133.6)	(150.8)		
Temptation goods (KES)	234.2***	112.5***	-128.7***	891	0.402
	(45.01)	(29.24)	(39.79)		
Severe food insecurity dummy	0.318***	0.567***	-0.369***	887	0.398
	(0.115)	(0.108)	(0.116)		
Calories per adult equivalent (log)	0.0124	-0.0119	0.0468	882	0.365
	(0.0754)	(0.0781)	(0.0820)		
Food consumption per adult equivalent (log)	0.0746	0.0530	-0.105	882	0.416
	(0.0897)	(0.0870)	(0.0901)		
Diet Diversity Score	0.104	-0.368	-0.229	1525	0.367
	(0.312)	(0.305)	(0.317)		
Subjective well-being	0.313	-0.132	-0.0834	1518	0.444
	(0.191)	(0.194)	(0.197)		

Notes: OLS estimates of heterogeneous ITT effects. Outcome variables are listed on the left. Columns 1, 2 and 3 report estimates of β_1, β_2, and β_3 in equation 16 respectively. Estimates reported in column 1 (β_1) is the ITT effect sizes on outcomes for households who are not indebted. Row-wise summation of the estimates reported in Column 1 (β_1) and Column 3 (β_3) provides the ITT effect sizes for households who are indebted. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * p<0.1, ** p<0.05, *** p<0.01

E Deviations from the pre-analysis plan

The pre-analysis plan is available here. Our analyses deviate from this pre-analysis plan in several ways.

First, we report intent-to-treat (ITT) effects instead of average treatment effects (ATE). Prior to data collection, we expected perfect compliance with the switch from restricted to unrestricted cash transfers. We therefore intended to estimate the average treatment effects (ATE). Our data however show that 11.9% of the households who were registered by WFP as living in the treated area of Village 3 did not receive unrestricted cash transfers, and instead continued to receive restricted cash transfers (See Table A.3). This occurred when the adult members of households were away at the time of the modality switch or when there were technical problems with the new ATM cards. Reassuringly, we find no defiers, i.e. households receiving unrestricted cash transfers amongst households registered as living in the control area. Due to the presence of imperfect compliance, we estimate ITT effects rather than ATE. The ATE is likely to be biased by selection bias because compliers are likely to be different from non-compliers in terms of observables and unobservables. For transparency purposes, we report ATE estimates in Table A.19. Results are qualitatively similar, except for non-food expenditure, for which the coefficient becomes negative.

Second, when constructing some outcomes variables, we used a different approach than described in the pre-analysis plan.

- Assets: In the Pre-Analysis Plan, we stated that we would construct standardised summary indices following the procedure described by Anderson (2008). However, when implementing this procedure, we realised that some the weights given to assets were absurd. For example, while the monetary cost of purchasing a basic mobile phone, a sofa, a cupboard, or a bicycle in Kakuma is approximately the same, the procedure of Anderson (2008) implicitly concludes that a sofa is worth 200 mobile phones, a cupboard is worth 165 mobile phones, and a bicycle is worth 53 mobile phones. Similarly, the procedure of Anderson (2008) implicitly concludes that a generator is worth five computers while the two goods have approximately the same monetary value in the settlement (about USD 300-400). The asset index constructed with the method of Anderson (2008) gives too much weight to sofas, cupboards, clocks, and radios, and not enough weight to mobile phones, chairs, and computers, leading to meaningless results. We therefore decided to construct an asset index using the estimated value of assets.[38] Regression coefficients are statistically insignificant with the asset index constructed using the procedure of Anderson (2008) (see Table A.20 in Appendix).
- Non-food expenditure and temptation goods: In the pre-analysis plan, we proposed to transform these variables using the inverse hyperbolic sine (IHS) transformation. However, the IHS transformation does not make sense with zero-valued observations (Delius and Sterck, 2019). Bellemare and Wichman (2020) suggest that the the IHS transformation should not be used "if the data have more than one third zero-valued observations". 81% of households reported zero temptation-goods spending, implying that we cannot use the IHS transformation for this variable. We therefore use the variables in level. Results are qualitatively similar with the IHS transformation, but regression coefficients are difficult to interpret (see Table A.20).

Third, our pre-analysis plan included six outcome variables that are not discussed in the main paper

[38]The value of each asset was estimated as the median value reported by survey respondents during a business survey in Kakuma and Kalobeyei in 2018 (Delius and Sterck, 2019).

because these variables appear to be empirically irrelevant or problematic. The ITT effects on these variables are reported in Table A.21.

- Savings (IHS): This variable measures the total amount of money the individual has put away as savings. This includes savings through bank accounts, self-help groups, and merry go rounds. We hypothesised that households receiving unrestricted cash transfers would have more savings than households receiving restricted cash transfers. However, 95.5% of households reported no saving, which is why we do not discuss the variable in the main paper.
- Credit (IHS): This variable measures the total amount of unpaid debts owed to banks, NGOs, shopkeepers, and other lenders. We hypothesised that households receiving unrestricted cash transfers would have lower amount of credit than households receiving restricted cash transfers. Only 1.5% of individuals reported a loan in a bank, a micro-finance institution, or a private lender. By contrast, 89% of households are indebted to a retailer, with no significant difference between households receiving restricted and unrestricted cash transfers. Given the importance of indebtedness in Kalobeyei, we have written an entire section on debt (Section 6).
- Intra-household decision making: We consider two variables. The first variable is the percentage of decisions made by a female member of the household. The second variable is the percentage of household disagreements. We provide a list of eight household decisions to the respondent: children's schooling, children's health, food purchases, purchases of large/expensive items, who should keep the Equity ATM card or Bamba Chakula SIM card, whether to resell food, respondent's own health, and whether or not respondent can work. We hypothesised that unrestricted cash transfers would alter intra-household decision making and generate more tensions within household members. We do not report our results related to intra-household dynamics in the main body of this paper because only 53 percent of the households have more than one adult and 41 percent of the households have just one female adult.
- Payment to authorities: We consider a dummy variable equal to one if the respondent provided a response larger than zero to the question "How much did you pay in total during the last three months to the police (excluding payments for other community members)?" or the question "How much did you pay [to community leader] in total?". If the person did not ask for help, we impute as zero. We did not ask the question to interviewees who reported being a leader themselves or who are living in the same household as the leader. Those are imputed as missing. We hypothesised that individuals living in households receiving unrestricted cash transfers would make more informal payments to local authorities, including community leaders and police officers (because they have access to cash). Our data show that less than 0.7% of interviewees made payments to authorities in the three months preceding the survey (and 89% of households receiving unrestricted transfers never withdrew cash using their ATM card). For these reasons, we do not discuss this outcome in the paper.
- Income-generating activity: We constructed a dummy equal to one if the respondent has an income-generating activity (employed, self-employed, or incentive worker) in the 30 days preceding the survey, and zero otherwise. We hypothesised that unrestricted cash transfers would stimulate economic activities. However, our data show that income-generating activities are scarce in Kalobeyei. Only 5.9% of adults in our sample were working for an income at the time of our survey, with no significant difference between those receiving restricted and unrestricted cash transfers.

Finally, we stated in our pre-analysis plan that we would do adjustments for multiple inference separately for each theoretical hypothesis listed in the pre-analysis plan. We deviate from this by adjusting

across hypotheses, which is a simpler and more cautious approach. Nevertheless, the q-values computed by grouping outcomes by theoretical hypotheses are similar to those reported in our main results (Table A.22).

Table A.19: Average treatment effect

	ATE	N	R-squared	Mean of control
Resell food items for cash dummy	-0.0782***	893	0.361	0.698
	(0.0225)			
Exchange food items for other items dummy	-0.0261	893	0.301	0.784
	(0.0222)			
Asset value (KES)	429.1*	896	0.454	5117.2
	(250.1)			
Asset purchase value (KES)	130.8*	896	0.271	429.3
	(73.83)			
Non-food expenditure (KES)	-97.41*	896	0.536	1002.0
	(56.92)			
Temptation goods (KES)	55.94***	895	0.397	65.93
	(14.11)			
Severe food insecurity dummy	-0.0207	889	0.387	0.777
	(0.0211)			
Calories per adult equivalent (log)	0.0322	884	0.365	7.869
	(0.0265)			
Food consumption per adult (log)	0.00733	884	0.415	4.541
	(0.0200)			
Diet Diversity Score	-0.0260	1529	0.361	5.416
	(0.0558)			
Subjective well-being	0.194***	1522	0.445	2.378
	(0.0509)			

Notes: OLS estimates of average treatment effects. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.20: ITT effects on pre-specified outcome variables

	ITT	N	R-squared	Mean of control
Resell food items for cash dummy	-0.110***	893	0.363	0.721
	(0.0301)			
Exchange food items for other items dummy	-0.0279	893	0.301	0.799
	(0.0312)			
Asset index	-0.114	895	0.373	-0.000855
	(0.0722)			
Asset purchase index	-0.0525	895	0.244	0.00471
	(0.0744)			
Non-food expenditure (IHS)	0.675***	896	0.495	6.219
	(0.149)			
Temptation goods (IHS)	0.251*	895	0.328	0.974
	(0.151)			
Severe food insecurity dummy	-0.0408	889	0.387	0.795
	(0.0371)			
Calories per adult equivalent (log)	0.0586	884	0.366	7.853
	(0.0406)			
Food consumption per adult (log)	-0.0235	884	0.416	4.532
	(0.0334)			
Diet Diversity Score	-0.105	1529	0.361	5.393
	(0.0829)			
Subjective well-being	0.225***	1522	0.444	2.319
	(0.0740)			

Notes: OLS estimates of ITT effects. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. As specified in the Pre-Analysis Plan, the outcome variables *Asset Index* and *Asset purchase index*, as well as the control variable *Cash and assets at arrival index* (corresponding coefficient not reported in this table) are constructed using the procedure in Anderson (2008). All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * p<0.1, ** p<0.05, *** p<0.01

Table A.21: ITT EFFECTS ON OTHER OUTCOME VARIABLES LISTED IN OUR PRE-ANALYSIS PLAN

	Treatment effect	N	R-squared	Weighted mean of control
Savings/month (IHS)	0.0516	1529	0.361	0.372
	(0.0597)			
Loans (IHS)	0.132	891	0.314	9.245
	(0.184)			
Percentage of decisions made by female	0.0589**	472	0.620	0.830
	(0.0275)			
Percentage of household disagreements	-0.00220	1103	0.350	0.252
	(0.0505)			
Payment to authorities dummy	0.0117*	1492	0.156	0.00439
	(0.00662)			
Has income-generating activity in past 30 days	-0.00465	1529	0.384	0.0771
	(0.00591)			

Notes: OLS estimates of ITT effects. Outcome variables are listed on the left. All pre-specified outcome variables are listed and described in Section B. Outcomes on savings, income-generating activities, payments to authorities, and household disagreements are analysed at individual level. Outcomes on loans and household decision-making are analysed at household level. Outcomes related to intra-household dynamics (*Percentage of decisions made by female* and *Percentage of household disagreements*) are analysed for households with at least two adults. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.22: ITT Effect of the Switch from Restricted to Unrestricted Transfers

	ITT	N	R-squared	Mean of control
Resell food items for cash dummy	-0.107***	893	0.361	0.721
	(0.0301)			
	[0.002]			
Exchange food items for other items dummy	-0.0274	893	0.301	0.799
	(0.0308)			
	[0.232]			
Asset value (KES)	579.4	896	0.454	5085.4
	(392.1)			
	[0.081]			
Asset purchase value (KES)	175.9	896	0.271	392.3
	(121.2)			
	[0.081]			
Non-food expenditure (KES)	241.2***	896	0.537	970.5
	(76.22)			
	[0.003]			
Temptation goods (KES)	108.5***	895	0.401	64.75
	(23.14)			
	[0.001]			
Severe food insecurity dummy	-0.0405	889	0.387	0.795
	(0.0370)			
	[0.584]			
Calories per adult equivalent (log)	0.0562	884	0.365	7.853
	(0.0407)			
	[0.584]			
Food consumption per adult equivalent (log)	-0.0255	884	0.415	4.532
	(0.0335)			
	[0.584]			
Diet Diversity Score	-0.105	1529	0.361	5.393
	(0.0829)			
	[0.584]			
Subjective well-being	0.225***	1522	0.444	2.319
	(0.0740)			

Notes: OLS estimates of intent-to-treat effects. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. q-values, calculated by grouping outcome variables by theoretical hypotheses, in square brackets. * p<0.1, ** p<0.05, *** p<0.01

F Robustness checks

F.1 Outliers

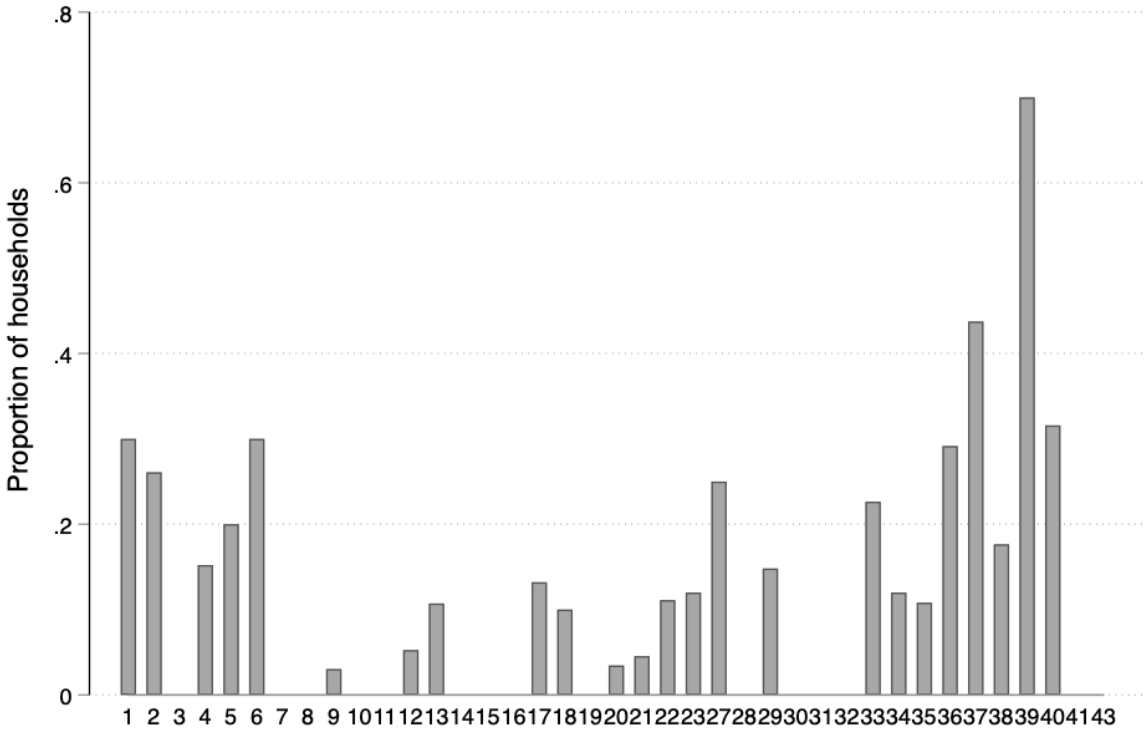

Figure A.1: Households Receiving Poultry, by Neighbourhood

Notes: Bar chart showing the proportion of households had received poultry from an NGO in Kalobeyei in each neighbourhood. Neighbourhood numbers are shown in the x-axis.

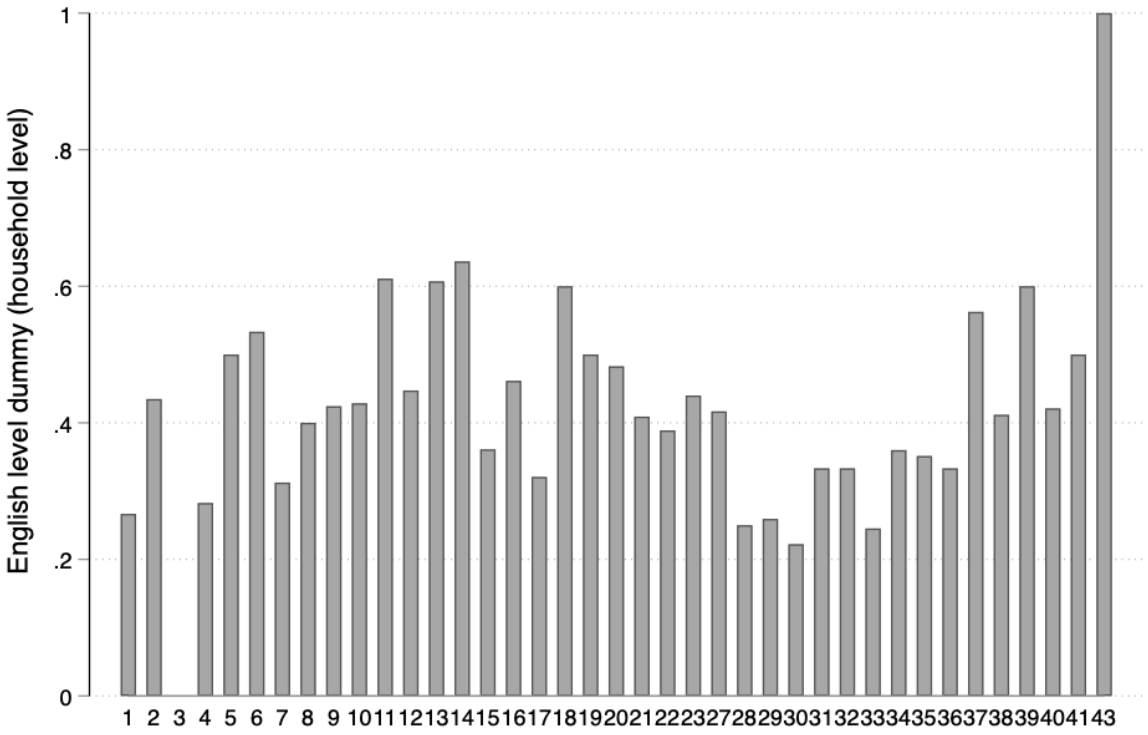

Figure A.2: Command of English Language, by Neighbourhood

Notes: Bar chart showing the proportion of households with at least one family member who have described his/her command of English language as "well" or "very well" in each neighbourhood. Neighbourhood numbers are shown in the x-axis.

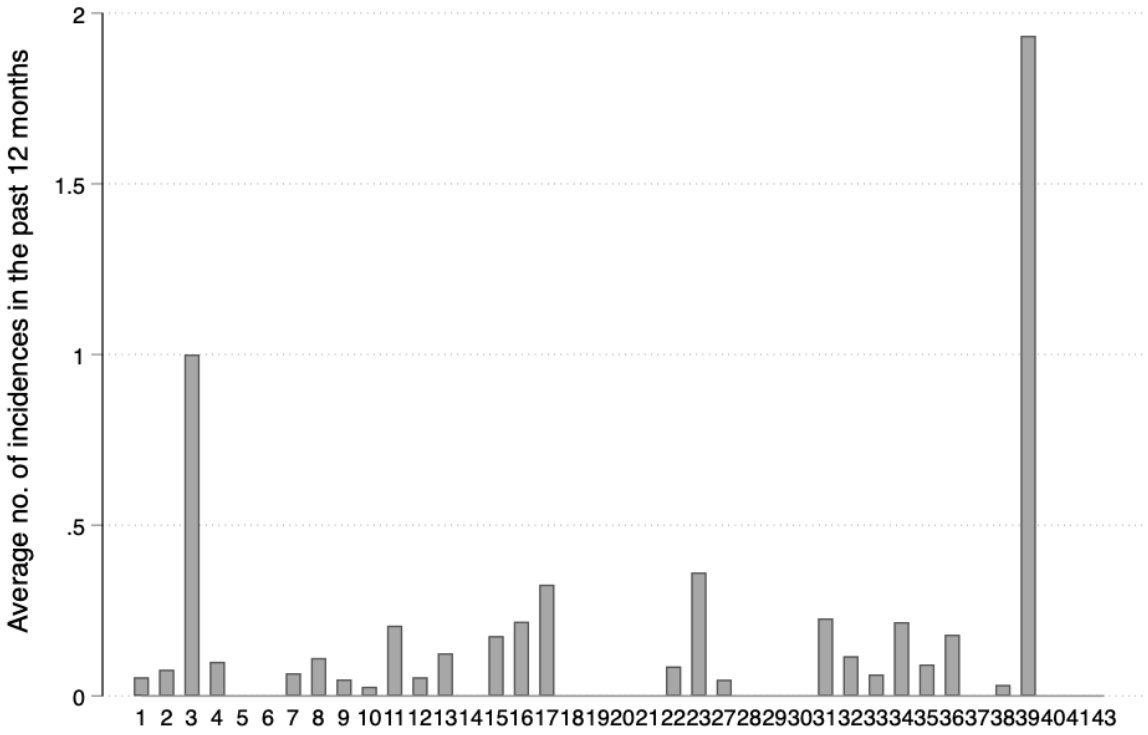

Figure A.3: Robbery Incidence, by Neighbourhood

Notes: Bar chart showing the average number of robbery incidences experienced in the past 12 months by a refugee in each neighbourhood. Neighbourhood numbers are shown in the x-axis.

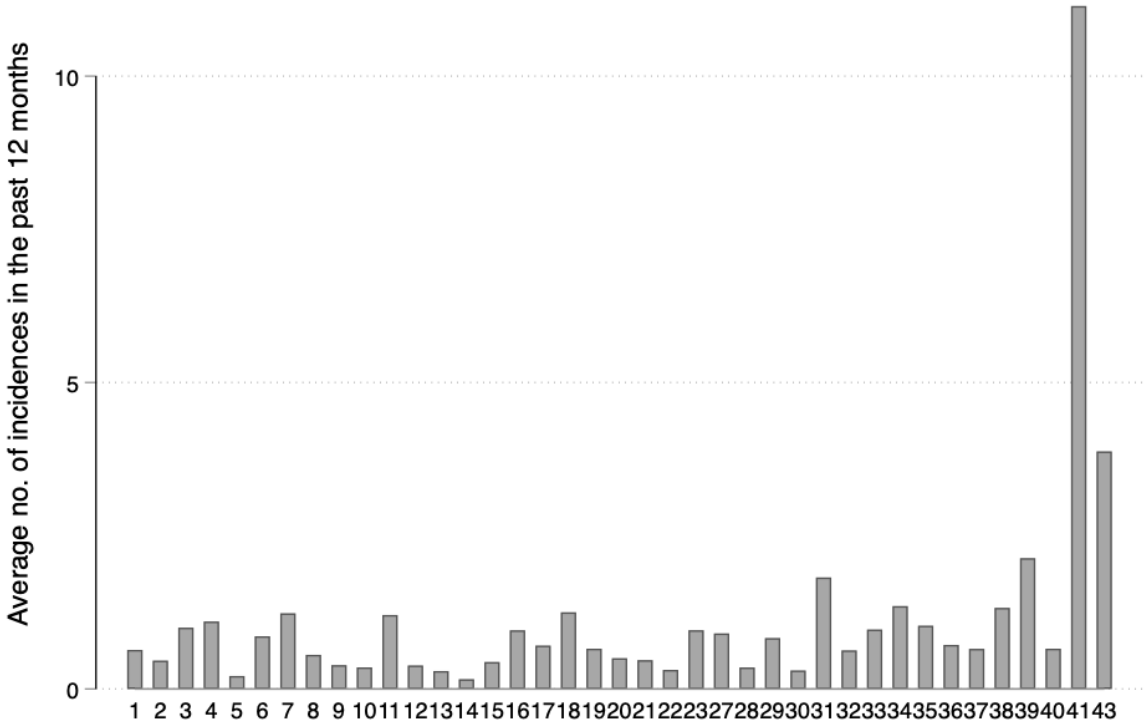

Figure A.4: Theft Incidence, by Neighbourhood

Notes: Bar chart showing the average number of theft incidences experienced in the past 12 months by a refugee in each neighbourhood.Neighbourhood numbers are shown in the x-axis.

Table A.23: ITT effects with restricted sample of households not living in Neighbourhood 39

	ITT	N	R-squared	Mean of control
Resell food items for cash dummy	-0.0995***	883	0.364	0.715
	(0.0309)			
Exchange food items for other items dummy	-0.0302	883	0.302	0.802
	(0.0313)			
Asset value (KES)	397.7	886	0.486	4687.1
	(354.2)			
Asset purchase value (KES)	205.0*	886	0.273	404.7
	(123.0)			
Non-food expenditure (KES)	231.7***	886	0.541	989.0
	(77.06)			
Temptation goods (KES)	109.0***	885	0.399	63.81
	(23.53)			
Severe food insecurity dummy	-0.0384	879	0.387	0.789
	(0.0377)			
Calories per adult equivalent (log)	0.0536	874	0.366	7.861
	(0.0415)			
Food consumption per adult equivalent (log)	-0.0296	874	0.418	4.536
	(0.0342)			
Diet Diversity Score	-0.133	1514	0.361	5.392
	(0.0840)			
Subjective well-being	0.219***	1507	0.443	2.336
	(0.0747)			

Notes: OLS estimates of intent-to-treat effects. Sample restricted to households not living in neighbourhood 39. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * p<0.1, ** p<0.05, *** p<0.01

Table A.24: ITT effects with restricted sample of households not living in Neighbourhood 41

	ITT	N	R-squared	Mean of control
Resell food items for cash dummy	-0.112***	891	0.363	0.722
	(0.0298)			
Exchange food items for other items dummy	-0.0287	891	0.301	0.801
	(0.0310)			
Asset value (KES)	547.7	894	0.454	5116.9
	(392.5)			
Asset purchase value (KES)	159.2	894	0.273	394.7
	(121.6)			
Non-food expenditure (KES)	226.8***	894	0.539	972.8
	(74.97)			
Temptation goods (KES)	107.4***	893	0.401	65.15
	(23.04)			
Severe food insecurity dummy	-0.0403	887	0.387	0.794
	(0.0369)			
Calories per adult equivalent (log)	0.0623	882	0.368	7.852
	(0.0406)			
Food consumption per adult equivalent (log)	-0.0210	882	0.417	4.531
	(0.0333)			
Diet Diversity Score	-0.120	1522	0.359	5.415
	(0.0823)			
Subjective well-being	0.226***	1515	0.442	2.335
	(0.0747)			

Notes: OLS estimates of intent-to-treat effects. Sample restricted to households not living in neighbourhood 41. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * p<0.1, ** p<0.05, *** p<0.01

Table A.25: ITT effects with restricted sample of households not living in Neighbourhood 43

	ITT	N	R-squared	Mean of control
Resell food items for cash dummy	-0.107***	890	0.361	0.721
	(0.0299)			
Exchange food items for other items dummy	-0.0229	890	0.298	0.806
	(0.0308)			
Asset value (KES)	578.0	893	0.451	4983.0
	(393.7)			
Asset purchase value (KES)	175.8	893	0.270	380.1
	(121.2)			
Non-food expenditure (KES)	226.6***	893	0.538	945.2
	(75.89)			
Temptation goods (KES)	107.5***	892	0.400	62.69
	(23.00)			
Severe food insecurity dummy	-0.0374	886	0.388	0.797
	(0.0371)			
Calories per adult equivalent (log)	0.0571	881	0.364	7.856
	(0.0408)			
Food consumption per adult equivalent (log)	-0.0268	881	0.415	4.532
	(0.0335)			
Diet Diversity Score	-0.113	1521	0.359	5.389
	(0.0828)			
Subjective well-being	0.224***	1514	0.443	2.324
	(0.0736)			

Notes: OLS estimates of intent-to-treat effects. Sample restricted to households not living in neighbourhood 43. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * p<0.1, ** p<0.05, *** p<0.01

Table A.26: ITT effects with restricted sample of households who did not withdraw cash using their Equity ATM card

	ITT	N	R-squared	Mean of control
Resell food items for cash dummy	-0.0876***	836	0.372	0.721
	(0.0299)			
Exchange food items for other items dummy	-0.0128	836	0.322	0.799
	(0.0310)			
Asset value (KES)	419.7	839	0.457	5085.4
	(402.0)			
Asset purchase value (KES)	21.38	839	0.300	392.3
	(113.8)			
Non-food expenditure (KES)	188.8**	839	0.543	970.5
	(80.13)			
Temptation goods (KES)	122.2***	839	0.419	64.75
	(23.05)			
Severe food insecurity dummy	-0.0176	832	0.394	0.795
	(0.0369)			
Calories per adult equivalent (log)	0.0408	827	0.369	7.853
	(0.0419)			
Food consumption per adult equivalent (log)	-0.0466	827	0.420	4.532
	(0.0339)			
Diet Diversity Score	-0.185**	1431	0.365	5.393
	(0.0850)			
Subjective well-being	0.226***	1425	0.449	2.319
	(0.0774)			

Notes: OLS estimates of intent-to-treat effects. Sample restricted to households who have not withdrawn cash using their Equity ATM card. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.27: ITT effects with restricted sample of households who only shopped in Kalobeyei

	ITT	N	R-squared	Mean of control
Resell food items for cash dummy	-0.121***	863	0.378	0.713
	(0.0289)			
Exchange food items for other items dummy	-0.0235	863	0.305	0.803
	(0.0308)			
Asset value (KES)	581.1	863	0.457	5050.6
	(404.6)			
Asset purchase value (KES)	114.2	863	0.281	384.1
	(120.9)			
Non-food expenditure (KES)	195.6**	863	0.551	967.4
	(76.31)			
Temptation goods (KES)	111.3***	862	0.416	66.81
	(23.01)			
Severe food insecurity dummy	-0.0551	858	0.388	0.796
	(0.0373)			
Calories per adult equivalent (log)	0.0594	853	0.367	7.849
	(0.0408)			
Food consumption per adult equivalent (log)	-0.0255	853	0.418	4.527
	(0.0340)			
Diet Diversity Score	-0.162**	1475	0.366	5.390
	(0.0827)			
Subjective well-being	0.212***	1468	0.449	2.323
	(0.0729)			

Notes: OLS estimates of intent-to-treat effects. Sample restricted to households who have only shopped in Kalobeyei. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.28: ITT EFFECTS WITH RESTRICTED SAMPLE OF HOUSEHOLDS WHO ONLY SHOPPED IN ONE SHOP

	ITT	N	R-squared	Mean of control
Resell food items for cash dummy	-0.121***	829	0.378	0.716
	(0.0321)			
Exchange food items for other items dummy	-0.0294	829	0.311	0.799
	(0.0330)			
Asset value (KES)	714.7*	832	0.465	5054.8
	(418.7)			
Asset purchase value (KES)	291.2**	832	0.284	408.4
	(127.6)			
Non-food expenditure (KES)	303.2***	832	0.542	990.4
	(73.44)			
Temptation goods (KES)	98.87***	831	0.408	64.91
	(19.92)			
Severe food insecurity dummy	-0.0726*	825	0.395	0.797
	(0.0379)			
Calories per adult equivalent (log)	0.0576	820	0.379	7.849
	(0.0405)			
Food consumption per adult equivalent (log)	-0.0179	820	0.427	4.528
	(0.0341)			
Diet Diversity Score	-0.0801	1428	0.375	5.373
	(0.0915)			
Subjective well-being	0.232***	1422	0.460	2.297
	(0.0741)			

Notes: OLS estimates of intent-to-treat effects. Sample restricted to households who have only shopped in only one shop. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.29: ITT effects with restricted sample which excludes indebted households who have changed the shop from which they bought food since June 2019

	ITT	N	R-squared	Mean of control
Resell food items for cash dummy	-0.133***	808	0.379	0.711
	(0.0326)			
Exchange food items for other items dummy	-0.0539	808	0.305	0.801
	(0.0329)			
Asset value (KES)	599.0	811	0.472	5273.0
	(428.0)			
Asset purchase value (KES)	228.8*	811	0.304	413.1
	(128.2)			
Non-food expenditure (KES)	279.7***	811	0.537	968.7
	(86.85)			
Temptation goods (KES)	95.10***	810	0.425	67.30
	(23.40)			
Severe food insecurity dummy	-0.0840**	804	0.399	0.799
	(0.0395)			
Calories per adult equivalent (log)	0.0559	799	0.405	7.838
	(0.0425)			
Food consumption per adult equivalent (log)	-0.0276	799	0.447	4.525
	(0.0356)			
Diet Diversity Score	-0.0859	1379	0.376	5.405
	(0.0835)			
Subjective well-being	0.254***	1372	0.478	2.267
	(0.0770)			

Notes: OLS estimates of intent-to-treat effects. Sample excludes indebted households who have changed the shop from which they buy food since June 2019. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

F.2 Registration dates and distance to WFP compound

Table A.30: ITT effect with restricted sample of households who arrived into Kalobeyei between 1 March 2017 and 21 June 2017

	ITT	N	R-squared	Mean of control
Resell food items for cash dummy	-0.179***	693	0.368	0.721
	(0.0369)			
Exchange food items for other items dummy	-0.0858**	693	0.345	0.799
	(0.0339)			
Asset value (KES)	1077.6**	695	0.485	5085.4
	(474.7)			
Asset purchase value (KES)	318.3**	695	0.299	392.3
	(141.0)			
Non-food expenditure (KES)	256.4***	695	0.578	970.5
	(97.10)			
Temptation goods (KES)	142.4***	694	0.489	64.75
	(28.08)			
Severe food insecurity dummy	0.00243	691	0.413	0.787
	(0.0445)			
Calories per adult equivalent (log)	0.0835*	686	0.373	7.862
	(0.0482)			
Food consumption per adult equivalent (log)	-0.0102	686	0.424	4.540
	(0.0408)			
Diet Diversity Score	-0.0591	1189	0.357	5.450
	(0.110)			
Subjective well-being	0.176*	1182	0.460	2.358
	(0.0940)			

Notes: OLS estimates of intent-to-treat effects. Sample restricted to households who arrived into Kalobeyei between 1 March 2017 and 21 June 2017. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

F.3 Omitted variables versus overfitting

Table A.31: ITT effects controlling for distance to Equity Bank

	ITT	N	R-squared	Mean of control
Resell food items for cash dummy	-0.140***	893	0.362	0.721
	(0.0309)			
Exchange food items for other items dummy	-0.0141	893	0.301	0.799
	(0.0325)			
Asset value (KES)	730.4*	896	0.454	5085.4
	(390.4)			
Asset purchase value (KES)	431.6***	896	0.276	392.3
	(135.4)			
Non-food expenditure (KES)	382.5***	896	0.539	970.5
	(80.41)			
Temptation goods (KES)	122.3***	895	0.401	64.75
	(25.37)			
Severe food insecurity dummy	-0.0506	889	0.387	0.795
	(0.0411)			
Calories per adult equivalent (log)	0.114**	884	0.368	7.853
	(0.0502)			
Food consumption per adult equivalent (log)	-0.00340	884	0.416	4.532
	(0.0399)			
Diet Diversity Score	-0.139	1529	0.361	5.393
	(0.0856)			
Subjective well-being	0.170*	1522	0.444	2.319
	(0.0867)			

Notes: OLS estimates of intent-to-treat effects. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. Distance from household to Equity Bank is also included. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. q-values in square brackets * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.32: ITT effects without pre-determined control variables, survey date and enumerator FEs

	ITT	N	R-squared	Mean of control
Resell food items for cash dummy	-0.124***	893	0.137	0.721
	(0.0418)			
Exchange food items for other items dummy	-0.0538	893	0.139	0.799
	(0.0329)			
Asset value (KES)	1740.8***	896	0.329	5085.4
	(420.5)			
Asset purchase value (KES)	333.2**	896	0.149	392.3
	(151.6)			
Non-food expenditure (KES)	387.2***	896	0.435	970.5
	(80.12)			
Temptation goods (KES)	82.43***	895	0.314	64.75
	(18.29)			
Severe food insecurity dummy	-0.0514	889	0.137	0.795
	(0.0428)			
Calories per adult equivalent (log)	0.0555	884	0.141	7.853
	(0.0405)			
Food consumption per adult equivalent (log)	0.0414	884	0.148	4.532
	(0.0399)			
Diet Diversity Score	0.303***	1529	0.202	5.393
	(0.115)			
Subjective well-being	0.147	1522	0.118	2.319
	(0.133)			

Notes: OLS estimates of ITT effects. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Control variables include programmatic, socio-economic, and geographic controls, as well as week-of-arrival fixed effects. Their corresponding coefficients are not reported in this table. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.33: ITT effects using variables chosen by double-LASSO, with week-of-arrival fixed effects, programmatic, socio-economic, and geographic controls first partialled out

	ITT	N	R-squared
Resell food items for cash dummy	-0.135***	893	0.258
	(0.0366)		
Exchange food items for other items dummy	-0.0697**	893	0.188
	(0.0331)		
Asset value (KES)	1485.2***	896	0.331
	(448.7)		
Asset purchase value (KES)	196.3	896	0.155
	(158.3)		
Non-food expenditure (KES)	256.6***	896	0.442
	(79.42)		
Temptation goods (KES)	69.72***	895	0.315
	(20.12)		
Severe food insecurity dummy	-0.0366	889	0.273
	(0.0375)		
Calories per adult equivalent (log)	0.0494	884	0.213
	(0.0423)		
Food consumption per adult equivalent (log)	-0.0404	884	0.279
	(0.0362)		
Diet Diversity Score	0.0718	1529	0.257
	(0.0999)		
Subjective well-being	0.239**	1522	0.376
	(0.0931)		

Notes: OLS estimates of ITT effects. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Control variables are chosen using the double-LASSO method (Belloni et al., 2014; Urminsky et al., 2016). Their corresponding coefficients are not reported in this table. The control variables are chosen from the list of pre-determined variables listed in Table A.7, as well as enumerator and survey date dummies. To implement the double-LASSO, week-of-arrival fixed effects, programmatic, socio-economic, and geographic controls are firstly partialled out. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * p<0.1, ** p<0.05, *** p<0.01

Table A.34: ITT effects using variables chosen by double-LASSO, with no control variables first partialled out

	ITT	N	R-squared
Resell food items for cash dummy	-0.0290	893	0.187
	(0.0401)		
Exchange food items for other items dummy	-0.0613*	893	0.102
	(0.0323)		
Asset value (KES)	463.0	896	0.204
	(392.2)		
Asset purchase value (KES)	212.7	896	0.0497
	(131.9)		
Non-food expenditure (KES)	260.1***	896	0.404
	(85.13)		
Temptation goods (KES)	34.83**	895	0.0130
	(15.77)		
Severe food insecurity dummy	-0.0522	889	0.188
	(0.0397)		
Calories per adult equivalent (log)	0.102**	884	0.0919
	(0.0410)		
Food consumption per adult equivalent (log)	0.0742*	884	0.0967
	(0.0384)		
Diet Diversity Score	0.0995	1529	0.197
	(0.103)		
Subjective well-being	0.299***	1522	0.349
	(0.0792)		

Notes: OLS estimates of ITT effects. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Control variables, as well as week-of-arrival, enumerator and survey dates fixed effects, are chosen using the double-LASSO method (Belloni et al., 2014; Urminsky et al., 2016). Their corresponding coefficients are not reported in this table. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$

Table A.35: UNOBSERVABLE SELECTION

	ITT	N	R-squared	Delta
Resell food items for cash dummy	-0.107***	893	0.361	1.004
	(0.0301)			
Exchange food items for other items dummy	-0.0274	893	0.301	0.374
	(0.0308)			
Asset value (KES)	579.4	896	0.454	1.653
	(392.1)			
Asset purchase value (KES)	175.9	896	0.271	1.182
	(121.2)			
Non-food expenditure (KES)	241.2***	896	0.537	-4.269
	(76.22)			
Temptation goods (KES)	108.5***	895	0.401	-8.028
	(23.14)			
Severe food insecurity dummy	-0.0405	889	0.387	0.379
	(0.0370)			
Calories per adult equivalent (log)	0.0562	884	0.365	0.890
	(0.0407)			
Food consumption per adult equivalent (log)	-0.0255	884	0.415	-0.417
	(0.0335)			
Diet Diversity Score	-0.105	1529	0.361	-0.514
	(0.0829)			
Subjective well-being	0.225***	1522	0.444	0.622
	(0.0740)			

Notes: OLS estimates of ITT effects. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. The column δ provides the statistic which captures the influence of unobservables on treatment relative to the influence of observables on treatment for the treatment effect β to equal to zero. To calculate this statistic, we use Oster (2019)'s assumption that $R^2max = R^2 \times 1.25$. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * p<0.1, ** p<0.05, *** p<0.01

F.4 Spillovers

Table A.36: ITT effects using restricted sample (distance > 10th percentile)

	ITT	N	R-squared
Resell food items for cash dummy	-0.0998***	804	0.367
	(0.0343)		
Exchange food items for other items dummy	-0.0351	804	0.316
	(0.0352)		
Asset value (KES)	1231.8**	807	0.471
	(502.5)		
Asset purchase value (KES)	219.0	807	0.299
	(153.4)		
Non-food expenditure (KES)	225.1**	807	0.546
	(97.79)		
Temptation goods (KES)	93.88***	807	0.416
	(27.65)		
Severe food insecurity dummy	-0.00896	801	0.416
	(0.0415)		
Calories per adult equivalent (log)	0.00574	797	0.309
	(0.0349)		
Food consumption per adult (log)	-0.0520	797	0.346
	(0.0338)		
Diet Diversity Score	-0.189**	1397	0.355
	(0.0933)		
Subjective well-being	0.367***	1390	0.469
	(0.0697)		

Notes: OLS estimates of ITT effects. Sample restricted to households whose distances to the road are higher than the 10th percentile of the distance distribution. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * p<0.1, ** p<0.05, *** p<0.01

Table A.37: ITT effects using restricted sample (distance > first quartile)

	ITT	N	R-squared
Resell food items for cash dummy	-0.123**	670	0.414
	(0.0503)		
Exchange food items for other items dummy	-0.0294	670	0.335
	(0.0469)		
Asset value (KES)	1598.9**	672	0.472
	(731.0)		
Asset purchase value (KES)	11.10	672	0.346
	(190.6)		
Non-food expenditure (KES)	310.2**	672	0.582
	(149.8)		
Temptation goods (KES)	142.8***	672	0.514
	(36.41)		
Severe food insecurity dummy	0.0192	669	0.453
	(0.0574)		
Calories per adult equivalent (log)	0.0649	666	0.310
	(0.0546)		
Food consumption per adult (log)	-0.0167	666	0.331
	(0.0501)		
Diet Diversity Score	-0.0740	1167	0.390
	(0.137)		
Subjective well-being	0.254**	1162	0.513
	(0.108)		

Notes: OLS estimates of ITT effects. Sample restricted to households whose distances to the road are higher than the first quartile of the distance distribution. Outcome variables are listed on the left. All outcome variables are listed and described in Section B in Appendix. All outcomes in this table are analysed at the household level, except the outcomes *Subjective well-being* and *Diet Diversity Score*, which are analysed at the individual level. Full set of control variables are included but not reported. The list of control variables are listed in Table A.4 in Appendix. All regressions include enumerator, arrival date, and survey date fixed effects. Missing values are dummied out. Clustered standard errors in parentheses. Adjusted using survey weights. * $p<0.1$, ** $p<0.05$, *** $p<0.01$