



Regular Article

The freedom to choose: Theory and quasi-experimental evidence on cash transfer restrictions[☆]Jade Siu, Olivier Sterck^{*}, Cory Rodgers

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ABSTRACT

Should cash transfer programmes restrict consumer choice? For example, should food assistance delivered in cash be restricted to food and exclude temptation goods? Theoretically, restrictions induce (1) a substitution effect away from restricted goods and (2) a negative wealth effect if transfers are extra-marginal and the resale of goods is costly. The welfare impact on transfer recipients is negative. We test 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 receive unrestricted transfers. In line with theory, we find that restricted transfers increase participation in a shadow resale market and negatively affect non-food expenditure, temptation-goods spending, and subjective well-being. Consistent with theory, restrictions have no significant effect on food consumption. Our results show that policy-makers should avoid restrictions to maximise positive impacts on transfer beneficiaries, especially when extreme poverty implies that transfers are extra-marginal.

“At the heart of the economist’s love affair with cash transfers is the doctrine of absolute consumer sovereignty. Everyone is his own best judge of what should be done to maximise his own utility”.

[Lester C. Thurow (1974)]

Billions of dollars of cash-based assistance¹ are distributed each year with restrictions² on the types of goods that can be purchased (Alderman et al., 2017; Gentilini et al., 2020; Girling and Urquhart, 2021).

In 2019, about half of the cash-based assistance distributed by the World Food Programme (WFP) – nearly USD 1 billion – was distributed with restrictions (by comparison, the NGO GiveDirectly distributed about USD 34 million worth of unrestricted cash transfers in 2019). In the USA, the Supplemental Nutrition Assistance Program (SNAP) distributes electronic cash transfers restricted to food. SNAP transfers reached 40 million Americans in 2018 with total benefits amounting to USD 61 billion. Restricted cash transfers are also used to encourage

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¹ 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 restrictions on what goods can be purchased.

² The term “restriction” is borrowed from early work on food assistance policies (see e.g. Southworth 1945, Olsen 1971a, Thurow 1974, Nichols and Zeckhauser 1982).

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more ecological consumption (e.g. the EcoCheque programme in Belgium).

For providers, different motives can justify the imposition of restrictions, including paternalism and interdependent preferences (Olsen, 1971b; Daly and Giertz, 1972; Garfinkel, 1973), targeting (Nichols and Zeckhauser, 1982; Blackorby and Donaldson, 1988), and political feasibility (De Janvry et al., 1991; Epple and Romano, 1996).

For beneficiaries, however, theory predicts that unrestricted transfers weakly dominate restricted transfers. The seminal work of Southworth (1945) shows that restrictions affect consumer choices if transfers are extra-marginal (i.e. binding) and the resale of unrestricted goods is costly. 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. For this reason, economists usually contend that unrestricted cash transfers are preferable. Thurow (1974) went as far as to assert that “the general economic case for cash transfers is strong enough that the burden of proof should always lie on those who advocate restricted transfers”.

Yet, Southworth’s (1945) predictions have never been empirically tested in a context where restrictions actually matter.³ Most empirical studies that compare restricted and unrestricted transfers focus on infra-marginal transfers and find little evidence of effect, in line with Southworth’s prediction that only extra-marginal transfers should affect consumption patterns (see e.g. Hoynes and Schanzenbach 2009, Hastings and Shapiro 2018 on US food stamps and Hidrobo et al. 2014, 2016 on cash, in-kind, and voucher transfers in Ecuador).⁴ To the best of our knowledge, only the pioneering study of Aker (2017) assesses the relative impact of cash transfers and vouchers in a context where transfers are extra-marginal; yet, her study finds no difference in food consumption, asset ownership, and other measures of well-being, in part due to the fact that households receiving vouchers were able to resell part of what they purchased at low or no cost.⁵

Our paper fills this important gap by providing robust empirical evidence on the net effect of cash transfer restrictions in a context where restrictions matter because cash transfers are extra-marginal and the resale of unrestricted goods is costly. We emphasise that studying extra-marginal transfers is more important, both from an academic and 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 in humanitarian contexts, where most cash-based interventions are expected to be extra-marginal.⁶

Our paper addresses three main questions. 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?

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 received USD 14 monthly, which should, in theory, be sufficient to afford 2100 kcal per day. Until mid-2019, all refugee households received mobile-money transfers restricted to food items, excluding alcohol, tobacco, and non-food items. In June 2019, the restriction was lifted for 1050 households living in one geographically bounded part of the settlement. Meanwhile, the settlement’s remaining 7000 households continued to benefit from the restricted programme. We provide evidence suggesting that the assignment to the policy change was quasi-random and exploit this natural experiment to assess the impact of cash transfer restrictions.

We use two main identification strategies to estimate the intent-to-treat (ITT) effects of the policy change. First, as balance tests suggest that households receiving restricted and unrestricted cash transfers would be similar in expectation in the absence of policy change, a simple difference between the post-treatment outcomes of households assigned to restricted versus unrestricted cash transfers should provide unbiased estimates of the ITT effects. This identification strategy assumes selection on observables.⁷ Second, we use a spatial Regression Discontinuity Design (RDD) that exploits the geographical allocation rule of the cash transfer modalities (see Fig. 1).

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.

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% of households. Ethnographic data reveal that

³ Our paper focuses on one specific aspect of the analysis of Southworth (1945): the impacts of cash transfer restrictions. Our research does not examine the other types of food subsidies discussed by Southworth (e.g. in-kind transfers) or their effects on prices. See for example Cunha (2014), Cunha et al. (2019) for excellent studies on these other related themes.

⁴ Evidence from the US shows that food stamps and SNAP transfers are infra-marginal, implying that households respond similarly to cash income and food vouchers (Hoynes and Schanzenbach, 2009; Hastings and Shapiro, 2018). In developing countries, Hidrobo et al. (2014, 2016) compare infra-marginal cash, in-kind, and voucher transfers and show that these different modalities had similar impacts on the value of food, non-food, and total consumption as well as on intimate partner violence. Cunha (2014) compares infra-marginal in-kind transfers and cash transfers in Mexico and finds only small differences in nutritional intake; Cunha et al. (2019) study the general equilibrium effects of the same program and find that in-kind transfers lead to lower prices than cash transfers.

⁵ Another limitation of existing studies is that the voucher programs differ from cash transfers in many 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 in these studies.

⁶ The importance of cash-based humanitarian assistance has more than doubled over the past five years, reaching USD 6.3 billion in 2020. About a third is provided as vouchers (Girling and Urquhart, 2021).

⁷ While results are robust to various specification changes (Section 5), we cannot definitely rule out the possibility that unobservables may bias our main results.

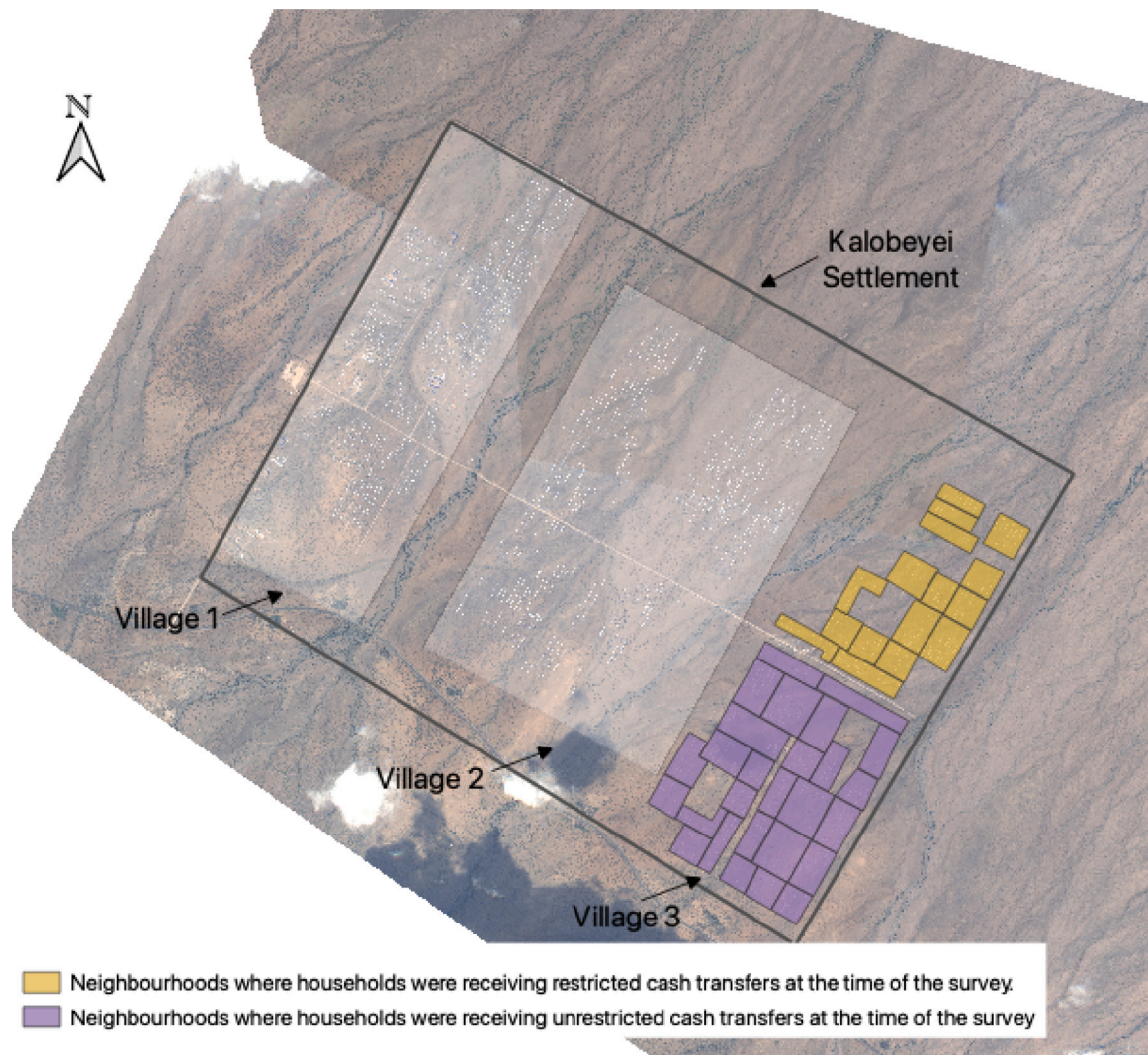


Fig. 1. Kalobeyei settlement — treatment and control areas.

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 purple 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 yellow neighbourhoods (northern part of the village) remained to be receiving restricted cash transfers. The road splitting dividing the “northern” and “southern” parts of Village 3 is named as “Jomo Kenyatta Road”. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

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. 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 (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). Our paper contributes to this literature by studying theoretically and empirically the impact of restrictions on how

cash transfers can be spent in a context where cash transfers are extra-marginal.

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).⁸ There have been comparatively fewer studies of 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.

1. Theoretical model

We study the decision-making process of households that benefit from a 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 that it explains why two different sets of prices 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 develop a theoretical model in three steps. We first introduce and solve a basic model of a 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):

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

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.⁹ We denote b_j^i the quantity of good j 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:

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

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

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 some way 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 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:

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

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:

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

⁸ For example, the effect of the inflow of refugees on the housing market (Depetris-Chauvin and Santos, 2018; Roza and Sviatschi, 2020), firm productivity (Altindag 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).

⁹ We assume that retail prices are determined at the global level and the local economy is too small to affect these prices.

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

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:

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

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:

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

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:

$$q_A^* = \frac{n^e w p_A}{n^a t \gamma}. \quad (9)$$

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:

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

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

The equilibrium quantities for assisted households are:

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

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

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:

$$n^e w < n^a t \gamma. \quad (14)$$

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 (Eqs. (2) and (3)). This type of equilibrium will occur if the employment rate and wages are high and 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 $q_A^* \geq p_A$.*

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 $q_A \leq p_A$. 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 of good A to be able to purchase 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 $t > w^a / \gamma$ (i.e. if the value of the transfer is large compared to their wage) and a resale market emerges. If $t < w^a / \gamma$ instead, assisted households are infra-marginal and the equilibrium of the economy is similar to the 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 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%), Ethiopia (13%), and Burundi (7%). The settlement was opened in May 2016 with the objective of promoting a model for refugee assistance that promotes self-reliance among the refugee and host communities.

Administratively, the settlement is divided into three villages (see Fig. 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 (Betts et al., 2018; Fix et al., 2019). In our sample, a staggering 73% of households were severely food insecure, and dietary diversity was low:¹⁰ only 3.5% of individuals ate fruit and 17% ate meat on a regular basis. Nearly half of our sample were single-adult households, of which 87% were female-headed. With an average of five children in a household, households were 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 5000 (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 was entitled to KES 1400 (USD 14) per month, which at that time was theoretically sufficient to purchase 2100 kcal per day at the prevailing market prices (MacPherson and Sterck, 2021).¹² Each month, WFP was injecting approximately USD 500,000 monthly into the local economy, which generated positive impacts on refugee beneficiaries (MacPherson and Sterck, 2021) and businesses (Delius and Sterck, 2019).

Until June 2019, food assistance was delivered to all refugee households through a cash transfer programme called Bamba Chakula. 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, partially 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, but only among 1050 households living in the southern part of Kalobeyei Village 3 (see

Fig. 1). Cash transfers are unrestricted in the sense that they can be spent on any type of good or service, including tobacco and alcohol. This programme is implemented in collaboration with Equity Bank, one of the largest Kenyan commercial banks, which 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 a 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 pin number is required to make a transaction. Both programmes use digital transfers and rely on a similar number of agents. Most Bamba Chakula traders in Kalobeyei are also Equity agents and can accept both modes of payments.¹⁴ While refugees can in theory use their MasterCard to withdraw cash, this is rarely done in practice. In our sample, 90% of households on unrestricted cash transfers never withdrew cash.

Our research aims to assess the impact of restrictions by comparing the 7000 households receiving food assistance through Bamba Chakula to the 1050 benefiting from unrestricted cash transfers on Equity bank accounts.

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 1529 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.

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 adults in households living in the selected compounds 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.

We also undertook 50 semi-structured interviews and several focus group discussions with refugees, shopkeepers, Equity Bank agents, as well as with representatives from various international organisations and NGOs.

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 3.3.¹⁵

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”.

¹⁵ 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.

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.

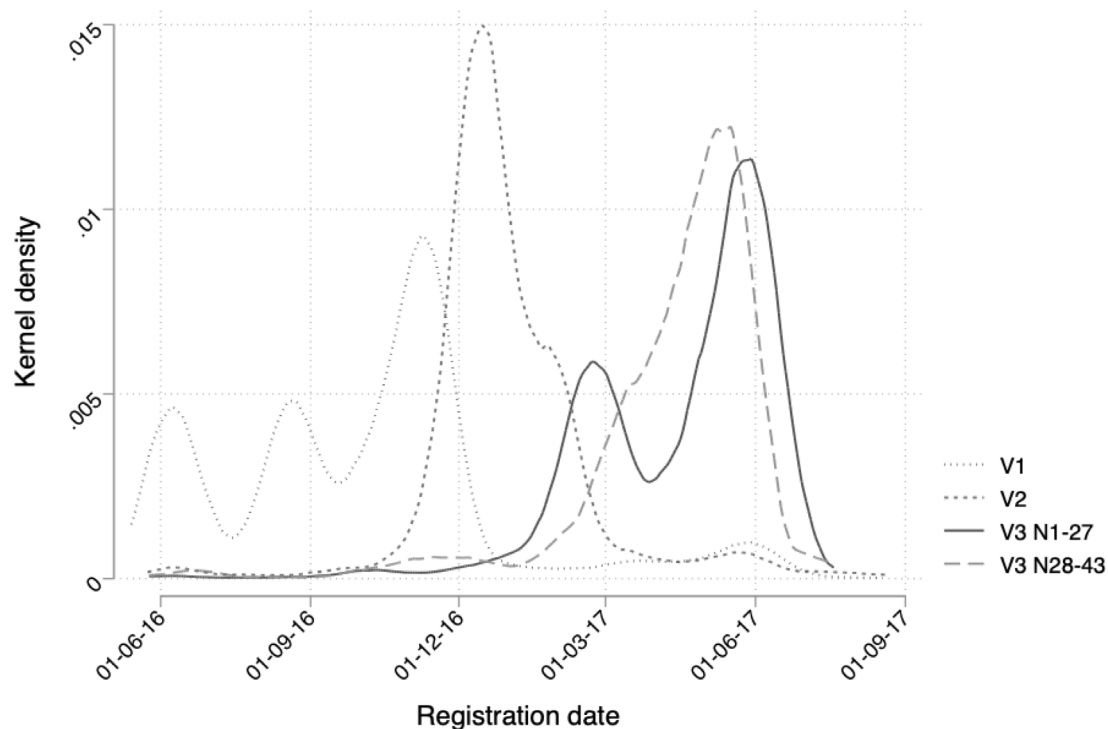


Fig. 2. Registration dates of South Sudanese refugees in Kalobeyei settlement.

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.

3.1. A natural experiment

Whether households receive restricted or unrestricted cash transfers depends on where they were placed upon arrival in the settlement (see Fig. 1). Kalobeyei is divided into three villages and each village is further divided into neighbourhoods. At the time of our research, households residing in Villages 1 and 2 and the northern side of Village 3 (neighbourhoods 28 to 43) were receiving 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. Fig. 2 illustrates how shelters were allocated based on refugees' registration date. It 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 both 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).¹⁶ 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.

- (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.
- (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.
- (2d) **Temptation goods:** This variable aggregates spending on soda, tobacco, eating out, alcohol, and attendance at 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.¹⁷ 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 respondents’ perceptions of food vulnerability and the frequency with which shortages occurred.

¹⁶ In Section 5.2, we show that these differences are driven by a few households living in remote neighbourhoods. Dropping these outlying observations does not affect the results.

¹⁷ Data on calories were obtained from the U.S. Department of Agriculture.

- (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

Our main identification strategy builds on the assumption that the switch from restricted to unrestricted transfers constitutes a credible natural experiment, i.e. that our treatment and control groups would be similar in expectation in the absence of policy change. The balance tables discussed in Section 3.1 show that this assumption is plausible. If this assumption is true, we obtain an unbiased estimate of the intent-to-treat (ITT) effect of the policy change by comparing the average outcomes of households receiving restricted versus unrestricted cash transfers. Fig. 1 suggests that the ITT effect of the policy change could also be estimated using a spatial Regression Discontinuity Design (RDD) based on the geographical allocation rule of the cash transfer modalities. In this section we detail our main identification strategy, and in Section 5.1, we describe the RDD analysis, from which we obtain similar results.¹⁸

In our main specification, we estimate the following equation using ordinary least squares (OLS):

$$y_i = \beta T_i^{\text{Register}} + \gamma' X_i + \mu_i + \tau_i + \lambda_i + \epsilon_i \quad (15)$$

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).¹⁹

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).

To account for the sampling design, we report robust standard errors clustered at the compound level.²⁰ 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

¹⁸ The spatial RDD was not pre-specified in our pre-analysis plan.

¹⁹ 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.

²⁰ Another – less conservative – approach would be to use robust standard errors, justifying this choice by the fact that the treatment was allocated at the household level when refugees were allocated to shelters within Kalobeyei. This approach – which is less restrictive – would lead to similar conclusions.

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)	70	51	33		
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	37	5.9	2.4	3.5
3a	Quantity received against 1 kg 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 or 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.

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. 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.²¹

Our identification strategy assumes selection on observables. We show that results are robust to various specification changes in Section 5. Yet, we cannot definitely rule out that unobservables may bias our results, thus results should be interpreted cautiously.

4. Results

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

4.1. Resale shadow market

A sizeable resale market exists in Kalobeyei. During the month preceding the survey, as many as 70% of households on restricted cash transfers resold food and 78% exchanged food for 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). 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.²²

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 for charcoal and greens.²³ 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.11 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 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 [Proposition 2](#) of the theoretical model. We test this proposition in the next section.

²¹ 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.

²² We obtain similar results with regression analysis and control variables (Table A.11 in Appendix).

²³ 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.

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

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 * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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 constrains 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 mean of the control group. 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 than those in the area with restricted transfers. This is large, about 1.7 times the mean of the control group. 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).²⁴ We also find positive effects on asset ownership and asset purchases, but these effects 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 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 food consumption much (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 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.

²⁴ 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.

5. Robustness checks

We assess the robustness of results by using a regression discontinuity design (RDD) based on the geographical allocation rule of the cash transfer modalities (Section 5.1).²⁵ We also show that results are not driven by outlying observations (Section 5.2), by differences in registration dates (Section 5.3), by the selection or omission of certain variables and fixed effects (Section 5.4), or by spillovers (Section 5.5). Tables and Figures are presented in Appendix.

5.1. Regression discontinuity design

Fig. 1 shows that the allocation of restricted versus unrestricted cash transfers depends on where refugees were assigned to live in the settlement. This suggests that a fuzzy spatial RDD could be used to obtain unbiased estimates of the ITT effects of the policy change. We consider a running variable whose absolute value is the distance to the road that splits Kalobeyei Village 3 in two parts. The running variable is positive (resp. negative) for households living in the southern (resp. northern) part of Village 3. The relationship between the running variable and assignment to treatment, and the discontinuity in this assignment are shown in Figure A.1. We use both a non-parametric and a parametric approach to estimate the treatment effects (Jacob et al., 2012).²⁶

The non-parametric approach considers observations that lie within a small bandwidth of the cutoff, where the functional form is more likely to be close to linear (Jacob et al., 2012).²⁷ We use the robust bias-corrected approach of Calonico et al. (2014b, 2019) to estimate the local average treatment effects and consistent cluster-robust standard errors. Results without control variables and fixed effects are shown in Table A.26. The number of observations selected on each side of the cutoff is extremely low, implying that statistical power is limited (and control variables and fixed effects cannot be included). Results should therefore be interpreted with caution. We note that the estimated effects have the expected signs, and the estimated effects on the exchange dummy and on non-food expenditures are statistically significant at conventional levels.

The parametric approach yields more statistical power because all observations are exploited. Estimates without and with control variables and fixed effects are shown in Table A.24 and Table A.25 respectively. Reassuringly, the results of the RDD analysis are consistent with the results of our main specification: unrestricted cash transfers reduce the need to resell or exchange food, and increase asset purchases, non-food expenditures, expenditures on temptation goods, and subjective well-being. The impact on food consumption is ambiguous, in line with theory.

5.2. Outliers

In Section 3.1, we used balance checks to show that households receiving restricted versus unrestricted cash transfers were broadly similar before the switch to unrestricted cash transfers. 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). As shown in Figures A.2 to A.5, these differences are driven by a few outlying neighbourhoods, 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.27–A.29).

Qualitative evidence and descriptive statistics show that most households in Village 3 have similar habits and constraints regarding 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.30–A.33. If anything, results are stronger without these outliers.

5.3. Registration dates

Fig. 2 shows that registration dates are broadly similar in the treatment and control areas. 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). Most registrations in Village 3 took place from early March 2017 until mid-June 2017. In Table A.34, 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, asset ownership, and asset purchases become statistically significant at conventional levels.

5.4. Omitted variables versus overfitting

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.39. 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.²⁸ For statistically significant variables, δ that are positive and low (i.e. $0 < \delta \ll 1$) signal a possible risk.²⁹ 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 the 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 Eq. (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 Eq. (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

²⁵ While matching methods seem less appropriate in this context because the overlap cannot be satisfied (Lee and Lemieux, 2010), we note that results are similar if treatment and control households are matched using the nearest neighbour distance matching estimator suggested by Abadie and Imbens (2006) (Table A.26).

²⁶ This specification was not pre-specified in our pre-analysis plan.

²⁷ For each outcome variable, we estimated the optimal bandwidth using the mean-squared-error optimal bandwidth selector of Calonico et al. (2014a). The bandwidth we use is the mean value of the estimated bandwidths.

²⁸ 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.

²⁹ 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 ITT effects that are larger in absolute value.

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.

included in the model from 177 to 81. Results are qualitatively similar (Table A.36). 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).³⁰

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 Eq. (15) using OLS with this set of controls (Belloni et al., 2014; Urminsky et al., 2016). We implement two versions of the procedure. In Table A.37, only pre-determined variables, enumerator fixed effects, and date of interview fixed effects can be penalised by the procedure.³¹ In Table A.37, 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.

5.5. 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 Fig. 2).

In Tables A.40 and A.41, we restrict the sample to households whose distance away from the road is more than the 10th and 25th percentile of the distribution of distances respectively. 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, 87% 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.³² Second, cash withdrawal is surprisingly rare. Only 10% of households on unrestricted transfers withdrew cash using their ATM card in the month preceding our survey. Finally, only a minority of households reported preferring unrestricted cash transfers: 39% of those receiving unrestricted cash transfers prefer unrestricted cash transfers and 60% 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.

6.1. The debt trap

The prevalence of household debt is high for both treated and control households. 89% of households in our sample are indebted towards their food retailers,³³ 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. Refugee households in Kalobeyei are extremely vulnerable to shocks. 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% 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. There are moral pressures urging shopkeepers to extend credit to needy customers:

The people are hungry and the shopkeeper cannot 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)

³⁰ 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 the week of arrival fixed effects or pre-determined controls.

³¹ 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.

³² With unrestricted cash transfers, a sizeable resale market could still exist if reselling food is a coping strategy in case of shock.

³³ Altindag and O'Connell (2020) finds a similar prevalence of indebtedness among Syrian refugees in Lebanon.

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)

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% of indebted households reported that their SIM card or ATM card was held by a shop owner (compared to 65% for debt-free households). When they hand over their cards, most customers also hand over their private PIN numbers to the shop owners (97% for indebted households and 73% for households who are not indebted). Shop owners can then make withdrawals on their customers' behalf.

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 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).³⁴ Dependency on food retailers is positively associated with severe food insecurity and negatively associated with asset purchases, 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 reinforces dependency and poverty traps:

The food is sometimes rotten, especially fish and beans. Since you do not 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 because – like households receiving restricted cash transfers – they must use the resale market to access non-food items.

We modify our original empirical model described in Eq. (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 Fig. 3 (see Table A.18 in Appendix for regression results). Extreme caution should be exercised when interpreting these results 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.

³⁴ MacPherson and Sterck (2021) and Delius and Sterck (2019) find similar results with data collected in 2017 and 2018 respectively.

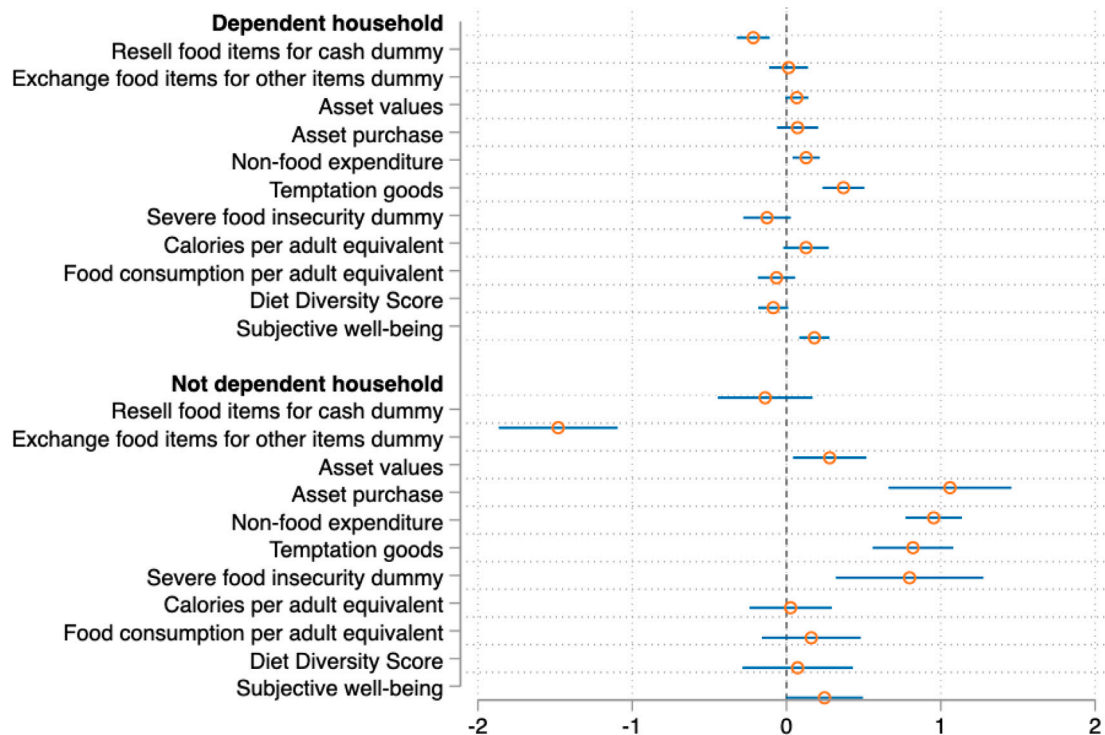


Fig. 3. ITT effects, in standard deviations.

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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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.

The theoretical work of Southworth (1945) shows that restrictions affect consumer choices if transfers are extra-marginal (i.e. binding) and resale of unrestricted goods is costly. If both conditions are satisfied, restrictions induce a substitution effect away from restricted goods and a negative wealth effect, leaving consumers worse off. To the best of our knowledge, our paper provides the first empirical test of these predictions in a context where both conditions are satisfied, implying that restrictions matter.

Our paper studies the impact of the change from restricted to unrestricted transfers in the Kalobeyei settlement in Kenya. We draw three main conclusions from the analysis of this natural experiment. 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, especially in contexts where cash transfers are large compared to other income sources. This note of caution is particularly relevant in humanitarian contexts where most people have high dependency on material assistance, such that minor differences in transfer modalities can have important real-life consequences for recipients. Organisations often opt for restricted cash transfers in order to encourage the consumption of certain goods. Our study provides evidence that 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 seek a deep understanding of markets and social systems to ensure that the full benefits of their selected cash transfer modalities can be realised. Particular attention should be paid to creditor–debtor relationships, as debt dependency can severely constrain debtors and undermine the impacts of cash transfers.

CRediT authorship contribution statement

Jade Siu: Methodology, Quantitative data collection, Data curation, Analysis, Writing – original draft, Writing – review & editing, Visualization. **Oliver Sterck:** Conceptualization, Methodology, Project administration, Supervision, Quantitative data collection and analysis, Writing – original draft, Writing – review & editing. **Cory Rodgers:** Qualitative data collection, Data curation, Analysis, Writing – review & editing.

Data availability

The data belongs to WFP, which allowed the authors to share the replication files once the manuscript is accepted for publication. Please contact the authors by email to request access to these files.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.jdeveco.2022.103027>.

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