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Cash transfers and micro-enterprise performance: Theory and quasi-experimental evidence from Kenya[☆]

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ABSTRACT

Theoretically, the welfare effects of cash-based assistance depend on how businesses respond to the demand shock and on resulting effects on prices. Such market effects have been largely overlooked in the literature. In this study, we examine the business and price effects of cash-based assistance to refugees in Kenya. Monthly restricted cash transfers worth 3 to 13 dollars were provided to 400,000 refugees in the form of digital money exclusively usable for food purchases at licensed shops. We show that licensed businesses have much higher revenues (+175%) and profits (+154%) and charge higher prices than unlicensed businesses. In line with theory, the restricted cash transfer program created a parallel retail market in which a limited number of businesses enjoy high market power. The theoretical and empirical results provide a cautionary tale highlighting the drawbacks of setting up a less competitive, parallel market to distribute cash-based assistance.

There is mounting empirical evidence regarding the positive and persistent effects of various modalities of cash-based assistance on direct recipients (Honorati et al., 2015; Bastagli et al., 2016).[2] By contrast, theoretical and empirical research on the indirect effects of cash-based assistance on local businesses is extremely limited. To the best of our knowledge, only one contemporary study – by Egger et al. (2022) – explores this question, finding that large one-time cash grants to poor households in Kenya generated positive effects on the revenue of local businesses, but had no significant effects on their profits. While large one-time grants like the ones provided by Egger et al. are promising tools to promote local development (Haushofer and Shapiro, 2016; Gazeaud et al., 2023), this type of program is still relatively rare because of high cost.[3] Small but regular transfers through cash, mobile money, or vouchers are more widespread, reaching billions of beneficiaries of social and humanitarian assistance (Honorati et al.,

2015; Gentilini et al., 2020; Girling and Urquhart, 2021). The effect of small but regular transfers on local businesses and markets is largely unknown.

This gap in the literature is problematic for at least two reasons. First, cash-based assistance can boost business creation and stimulate existing businesses, leading to positive impacts beyond direct recipients. These positive spillovers on non-recipient households should be accounted for to understand the full impact of cash-based assistance (see e.g. Angelucci and De Giorgi 2009, D'Aoust et al. 2018, Egger et al. 2022). Second, the direct effect of cash-based assistance on recipients depends, in the first place, on how businesses react to the demand shock, which in turn depends on the market structure and the features of the program. Understanding theoretically and empirically how businesses and markets respond to cash-based interventions is therefore

crucial, as this reaction ultimately determines effects on household welfare. This is the objective of our paper.

Our contribution is theoretical and empirical. First, we build a theoretical model of the effects of a cash-based intervention on prices, business outcomes, and household welfare. The predicted impacts of transfers on households and businesses depend on the degree of competition but also on the characteristics of the cash-based intervention. The case of unrestricted cash transfers in a perfectly competitive market is simple: apart from a possible period of adjustment in the short run, businesses do not benefit from transfers as prices are equal to marginal costs. Benefits are entirely reaped by transfer recipients. Markets are however rarely perfect, especially in developing countries. Regulations and credit constraints often act as entry barriers in the formal sector. Prices are rarely indicated. Transportation costs can be very large, as roads are often non-existent or in poor conditions. The presence of fixed costs means that the assumption of non-increasing returns to scale is often inaccurate. We build a Salop circle model to study the effects of cash transfers in the presence of market imperfections. With entry barriers and transportation costs, we find that businesses indirectly benefit from cash transfers, even after the adjustment period in the short run.

We also extend the model to study the case of restricted cash transfers that can only be spent at a limited number of licensed shops. Restricted cash transfer programs are widespread, taking the form of digital cash transfers or vouchers (Siu et al., 2023). Girling and Urquhart (2021) estimate that, 29 percent of cash-based humanitarian assistance in 2020 – nearly 2 billion USD – was provided with restrictions on how and where the transfers can be spent. In the US, the Supplemental Nutrition Assistance Program (SNAP) is distributing electronic cash transfers restricted to food, reaching 40 million Americans in 2018 with total benefits amounting to 61 billion USD. Restricted cash transfers are also used to promote eco-friendly consumption, as seen in programs like the EcoCheque program in Belgium.[4] With restricted transfers, we show that a two-tier market structure with two different sets of prices may emerge: low prices in the cash market, which is more competitive, and high prices in the new, restricted, market for digital cash transfers or vouchers.

Our empirical analysis assesses the business impacts of a large-scale program of restricted cash transfers implemented by the World Food Programme (WFP) in Kenya. In 2018, about 400,000 refugees living in camps or settlements in Kenya were receiving monthly digital transfers worth 3 to 13 dollars.[5] These transfers were restricted to food items and can only be spent at licensed shops. To ensure only licensed vendors could sell to beneficiaries, WFP used a mobile money platform only accessible to that group of vendors. Licenses were allocated by WFP following a competitive process in two steps. First, applicants had to fill an application form, in which they had to describe the characteristics of their businesses and commit to certain standards of conduct. Second, a multi-stakeholder committee with representatives from humanitarian organizations and the Government of Kenya allocated the licenses based on the data in the application forms. They deliberately selected a diversity of shop owners in terms of gender, origin, and location of their shops.

We assess the impact of being allocated a license on the business and household outcomes of applicants. First, we provide evidence

suggesting that the allocation of licenses was quasi-random, conditional on the data considered by the selection committee. In particular, the *unconfoundedness assumption* is plausible in our study, because: (1) we have a detailed understanding of how the licensing process took place; (2) we have access to all the data available to the selection committee; (3) licensed and unlicensed businesses are operating in the same economic environment and have been administered the same surveys; (4) placebo tests confirm that treatment status is uncorrelated with predetermined characteristics proxying for entrepreneurial ability; and (5) proxies for business size and capacity are insignificant when estimating the propensity score, showing that the selection committee did not systematically select the most (or least) successful businesses. As the unconfoundedness assumption is rhetorically and statistically plausible (McKenzie, 2021), experimental and matching methods should yield similar unbiased impact estimates (Heckman et al., 1997; Dehejia and Wahba, 2002; Diaz and Handa, 2006). We match successful and unsuccessful applicants using data from the application process, and estimate the medium-term effects of obtaining a license to accept the digital cash from the transfer on the revenue, profit, and productivity of businesses and on some aspects of their operations, including prices. In addition, we estimate medium-term effects on household consumption, asset ownership, and total household income of applicants.

We find that the applicants selected to get a license massively benefit from the cash transfer program. Licensed applicants have monthly business revenues that are 3784 USD higher on average than unlicensed applicants (+175%).[6] The effect of licenses on profits is also positive and large. Applicants who received a license have monthly business profits that are 685 USD higher on average than unlicensed applicants (+154%). Licensed applicants also have more employees and higher labor productivity, they sell a larger variety of commodities, and their households have higher living standards than the control group.

These massive effects are partly explained by the fact that successful applicants are more likely to have a business (+24 percentage points), but also that licensed businesses are much more successful than unlicensed businesses. We estimate that the effect of getting a license on profits is higher than 526 USD per month (+86%) for businesses that would exist even in the absence of cash transfer program. This estimate, which is a lower-bound, is extremely large, about 18 times the average monthly wage of paid employees (about 29 USD) and 39 times the value of monthly food assistance per refugee (about 13 USD). Importantly, we find no significant effect on sales for hard cash, suggesting that negative spillovers between licensed and unlicensed businesses are limited.

The large profits observed in the food retail sector provide evidence of market imperfections in Kakuma and Kalobeyei. These market imperfections have been magnified by the digital cash transfer program. Using data from a household survey, we find that households are charged higher prices for purchases paid with digital cash transfers compared to purchases with hard cash. The digital cash transfer program has generated two parallel markets. On the one hand, the market for hard cash transactions is relatively competitive, with about 1400 shops offering low prices to attract consumers. On the other hand, the new market for digital cash transfers is restricted to 252 licensed vendors which can charge higher prices. Because of market imperfections, licensed businesses capture part of the benefits of the cash transfer program. Our analysis suggests that most refugees would gain from policies addressing these market imperfections.

More generally, our findings caution against the establishment of a less competitive, parallel market for distributing cash-based assistance. Organizations implementing cash-based interventions should identify and address market imperfections to limit rent-seeking by businesses

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[4] Impact evaluations on the direct effects of restricted cash transfers have been implemented in many settings, including in Ecuador (Hidrobo et al., 2014, 2016), Lebanon (WFP, 2014a), Jordan (WFP, 2014b), the Democratic Republic of Congo (Aker, 2017), Senegal (Savy et al., 2020), and Kenya (Siu et al., 2023).

[5] Our research focuses on the Kakuma refugee camp and the Kalobeyei settlement in Northern Kenya which hosted more than 180,000 refugees at the time of data collection. For security reasons, we could not undertake data collection in the Dadaab refugee camp, where the same program was also implemented.

[6] We use the exchange rate at the time of the survey to convert KES into USD: 0.0096.

and maximize positive impacts on cash transfer recipients. How to do that effectively is a topic for future research.

Our contribution to the literature is at least threefold. First, we contribute to the vast literature on cash transfers (Bastagli et al., 2016), by assessing indirect impacts of a large-scale program of restricted cash transfers on local businesses. A series of recent studies have identified indirect effects on non-recipient households and on prices (Angelucci and De Giorgi, 2009; Haushofer and Shapiro, 2018; D'Aoust et al., 2018; Cunha et al., 2018; Egger et al., 2022; Filmer et al., 2021). To the best of our knowledge, only Egger et al. (2022) examines indirect effects on local businesses, finding positive impacts on their revenue but no significant impact on their profits. We highlight two key differences between their analysis and ours. First, their program is conceptually different. It involved one-time large cash grants of 1000 USD given to over 10,500 poor households, generating a large and sudden fiscal shock in local communities. In contrast, our digital cash transfer program provides small but regular transfers to more than 400,000 refugees registered in Kenya's camps and settlements over multiple years. Our program is also restricted to food and to certain shops. The indirect impacts of the two programs on businesses may sharply differ, because (1) regular and one-time transfers generate different spending patterns (Haushofer and Shapiro, 2016), (2) cash transfer restrictions affect how and where transfers are spent (Siu et al., 2023), and (3) regular transfers are more predictable, so businesses and markets may adjust differently. Second, the policy relevance of the two programs also differ. While large one-time transfers are effective tools to get people out of poverty traps (Haushofer and Shapiro, 2016; Balboni et al., 2021; Gazeaud et al., 2023), this type of program is still relatively rare because of high cost per recipient. By contrast, small regular cash transfers are widely used throughout the world for social and humanitarian assistance (Honorati et al., 2015; Gentilini et al., 2020; Girling and Urquhart, 2021). Social and humanitarian assistance often comes with restrictions to encourage certain purchases or behaviors (Siu et al., 2023). Therefore, understanding the impacts of regular and restricted cash transfers is, in our opinion, at least as important as studying the impact of one-time large unconditional cash transfers.

Second, we add to the literature on micro-enterprise performance in developing countries. The bulk of this literature investigates how supply-side constraints – and in particular financial and human capital constraints – affect business outcomes. A series of recent experimental papers study the effects of grants to business owners (De Mel et al., 2008; Fafchamps et al., 2014; McKenzie, 2017; Bernhardt et al., 2019), business training (McKenzie and Woodruff, 2014) or a combination of the two (Blattman et al., 2014; Berge et al., 2015). A nascent literature shows that social capital (Cai and Szeidl, 2018; Fafchamps and Quinn, 2018) and managerial capital (Bruhn et al., 2018) also partly explain the large heterogeneity in firm performance. By contrast, our research focuses on an intervention affecting the demand side of the market. We show that the demand shock induced by cash transfers can affect prices and business outcomes, especially in the presence of market imperfections. The retailers with access to this increased demand flourish, but do not necessarily drive other businesses out of the market.

Finally, our paper makes two methodological contributions. First, we develop a new bounding approach to circumvent the sample selection problem arising because business outcomes are only observed for applicants with a business, and business ownership itself may depend on the treatment. Our approach, which is inspired by the work of Lee (2009) and Attanasio et al. (2011), provides a lower-bound estimate of the average treatment effect on "always-traders", i.e. businesses that would exist regardless of the cash transfer program. Second, we propose a new method to deal with variables that have dispersed tails and zero-valued observations. For such variables, the common practice in applied economics is to apply the inverse hyperbolic sine (IHS) transformation in order to limit dispersion, facilitate the interpretation of

results, and reduce the influence of outliers (Bellemare and Wichman, 2019). We identify three fundamental issues affecting the IHS transformation of variables with zero- or negative-valued observations: it is (1) non-invariant to linear transformations, (2) difficult to interpret, and (3) largely ignoring the interesting differences between positive-valued observations. We propose a quantile transformation that addresses these issues.

The remainder of this paper is organized as follows. Section 1 develops a theoretical model of the effects of cash transfers on businesses and households. Section 2 describes the context of the study and the cash transfer program. Section 3 describes the data. Section 4 presents the empirical strategy. Section 5 discusses the main results. Section 6 shows that results are robust to various checks and specification changes. Section 7 examines impacts on prices and discusses market imperfections. Section 8 concludes.

1. Theory

We propose a general framework to study the indirect effects of a cash transfer program on local retailers. We first outline a series of theoretical predictions in the case of perfect competition. We then build an extended Salop circle model to study the effects of cash transfers in the presence of market imperfections. Finally, we extend this general model to examine the effects of a digital cash transfer or voucher program that is restricted to a set of licensed shops.

We highlight three differences between our model and existing frameworks (see Cunha et al. 2018, Filmer et al. 2021 for recent conceptual frameworks). First, our model assumes price competition while existing frameworks assume quantity competition a la Cournot; price competition seems more realistic in the context of food retailers. Second, our model goes beyond impacts on prices and examines impacts on sales, profits, and consumer welfare. Finally, our model considers both unrestricted and restricted cash transfers, both modalities being widely used by development and humanitarian actors to deliver cash transfers (Siu et al., 2023).

1.1. Perfect competition

The perfect competition model relies on a series of strong assumptions, including (1) a large number of firms and buyers, (2) profit maximization of sellers, (3) rational buyers, (4) homogeneous products, (5) no barriers to entry/exit, (6) firms are price takers, (7) perfect information about prices and product characteristics, (8) zero transaction costs and zero transportation costs, (9) perfect mobility of factors, and (10) non-increasing returns to scale.

In a competitive market, equilibrium prices are determined by the intersection of the demand and supply curves. At equilibrium, retailers make zero economic profit. The direct effect of a new program of cash transfers in such competitive market is to shift the demand curve to the right. As a result, prices increase in the short run, leading to supernormal profits for existing retailers. Attracted by supernormal profits, new retailers enter the market, driving profits down to zero. In summary, a new cash transfer program is expected to generate a short-run economic boom for existing retailers, with increased prices and supernormal profits. In the long-run, the entry of new retailers pushes prices down to their equilibrium levels and profits converge back to zero. Apart from the period of adjustment in the short run, retailers do not benefit from a cash transfer program in a competitive market. Benefits are entirely reaped by the transfer recipients.

1.2. Imperfect competition

Retail markets in most developing-country contexts are broadly satisfying the assumptions (1) to (4) listed above. Streets and markets are usually packed with numerous shops and street vendors that are selling broadly similar products. The situation seems somewhat different when it comes to assumptions (5) to (10). While barriers to entry are usually minimal in the informal economy (e.g. for street vendors), regulations and credit constraints are often limiting entry in the formal sector. Prices are rarely indicated, and price negotiation is frequent, especially for bulk purchases. Transportation costs can be very large, especially when roads are in poor conditions. As a consequence, the law of one price rarely holds. The presence of transportation costs and the immobility of some factors, especially infrastructure, also imply that shop localization is an important factor determining business performance. The assumption of non-increasing returns to scale is often wrong because of the presence of fixed costs.

A useful framework that represents these conditions is the Salop circle model (Salop, 1979), in which a continuum of consumers have to pay transportation costs and a set of equidistant retailers face fixed entry costs. Salop circle models typically assume that each consumer buys at most one unit from a unique retailer. While this assumption simplifies the resolution of the model, it is inconvenient to study the impact of cash transfers because cash transfers precisely aim at generating new purchases. We therefore extend the Salop circle model by assuming that consumers have a budget b that is used to purchase goods and to pay transportation costs. The budget b comes from two sources: a wage w and a cash transfer t, such that $b = w + t$. We can study the effects of the cash transfer by examining the comparative statics of the model with respect to t. We focus on a model with a fixed number n of equidistant retailers (we endogenize n by allowing free entry in the market in Appendix A).

A continuum of consumers are placed around a circle of circumference 1. Consumers maximize their consumption of a unique variety of good. Each consumer i has a budget $b = w + t$ which is spent in two ways. First, to purchase $q_{i,j}$ units of good at a retailer j at price p_j. Second, to pay transportation costs $\tau d_{i,j}$, where τ is the unit cost of transportation and $d_{i,j}$ is the distance between the consumer i and its retailer j.[7] The budget constraint of a consumer i visiting shop j is given by $b = w + t = q_{ij} p_j + \tau d_{ij}$.

A fixed number n of equidistant retailers use price competition to maximize their profit. The marginal cost of production is constant and denoted c. Retailers face a fixed cost of entry e. The price proposed by a retailer j is denoted p_j. Given symmetry, all shops will propose the same price at equilibrium, implying that consumers visit their closest shop. We assume that customers' budget is large enough to cover transportation costs to their nearest shop:

$$b = w + t > \frac{\tau}{2n} \tag{1}$$

This condition is necessary to have some competition between retailers. If condition (1) is not satisfied, retailers are monopolists as customers' budget only allows them to visit one shop at most.

Long-term equilibrium. In this setting, we show that the equilibrium price p is given by (see Appendix A for details):

$$p = \frac{4(w+t)^2 n^2}{[2(w+t)n - \tau]^2} c \tag{2}$$

The equilibrium price p is decreasing with the value of the cash transfer t. This shows that, in the presence of market imperfections,

a cash transfer program may actually reduce prices, despite higher demand. With an increased budget, consumers have a higher incentive to look for lower prices than to reduce transport costs. In turn, this leads to higher competition between retailers, who reduce prices to attract customers.[8]

At equilibrium, the profits of retailers are given by:

$$\pi = \frac{\tau[4(w+t)n - \tau]^2}{16(w+t)^2 n^4} - e \tag{3}$$

Profits are increasing with the value of the cash transfer t. In the presence of entry barriers and transport costs, cash transfers increase the profits of retailers, despite the fact that increased competition leads to a lower equilibrium price. Indeed, the increased number of units sold more than compensate for the price reduction. This implies that retailers capture part of the benefits of cash transfers in the presence of market imperfections.[9]

In our model, consumers maximize consumption. Average consumption is given by:

$$\bar{q} = \frac{(w+t) - \frac{\tau}{4n}}{p} = \frac{[2(w+t)n - \tau]^2[(w+t) - \frac{\tau}{4n}]}{4(w+t)^2 c n^2} \tag{4}$$

Consumers' welfare is increasing with the value of the cash transfer t. Cash transfers are therefore expected to increase consumers' welfare, even in the presence of market imperfections. Interestingly, the derivative of q_i with respect to t is larger than $1/p$. In other words, a one-unit increase in the value of the cash transfer t increases consumption more units than what one can buy with one unit of money. In the presence of transportation costs and entry barriers, cash transfers yield a double dividend: consumers not only have more money to buy goods, they also benefit from lower prices due to increased competition between retailers.[10]

We now show that this double dividend disappears if supply cannot fully adjust in response to the demand shock.

Constrained supply and short-term adjustment. The theoretical results above assume that retailers instantaneously adjust their supply in response to the demand shock induced by the cash transfer program. This assumption does not need to be true in the presence of market imperfections, especially in the short run. If, in response to a new cash transfer program, retailers cannot adjust their supply in the short run, then the short-term equilibrium price is such that the total demand equals the constrained supply (see Appendix A for details):

$$p^{st} = \frac{4w^2 n^2 [4n(w+t) - \tau]}{(4nw - \tau)(2nw - \tau)^2} c \tag{5}$$

The derivative of p^{st} with respect to t is positive: retailers respond to increased demand by raising their prices. In fact, retailers capture all the benefits of the cash transfer program in the short run, if supply is sticky. The impact of cash transfers on consumers is null as the quantity of goods exchanged is unchanged.

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[7] Because of transportation costs, consumers visit one shop at most. As Salop (1979), we assume that transportation costs do not depend on the quantity transported. We obtain similar theoretical predictions if we instead assume that transportation costs are the sum of two components: one that depends on distance and one that depends on quantity.

[8] Comparative statics with respect to τ and n are discussed in Appendix A.

[9] The cross-derivative of profits with respect to t and n is negative. Consequently, the impact of a cash transfer program on retailers' profit is larger if the number of retailers n is low, that is, if competition is limited. If n approaches infinity, cash transfers have no impact on profit. Similarly, the cross-derivative of profit with respect to t and τ is positive. The impact of a cash transfer program on profit is larger if transportation costs are large.

[10] The cross-derivative of average consumption $\bar{q}$ with respect to t and n is positive. Consequently, the impact of a cash transfer program is largest if the number of retailers n approaches infinity, that is, if the market is perfectly competitive. Similarly, the cross-derivative of average consumption $\bar{q}$ with respect to t and τ is negative. The lower transportation costs are, the larger the impact of a cash transfer program on consumers.

1.3. Voucher and mobile money programs

For practical reasons, cash transfers can often be spent in selected shops. This is typically the case for programs that rely on vouchers or mobile money, including the program we will evaluate below. The organizations implementing these programs usually select a limited number of retailers that get licensed to accept vouchers or digital payments. Such programs create a new, parallel, market characterized by entry barriers and a restricted number of competitors. In such settings, the effect of transfers on prices is ambiguous. On the one hand, we have seen that cash transfers can stimulate competition in imperfect markets. On the other hand, entry barriers protect licensed businesses who have access to the new market. A likely outcome is a two-tier market structure in which licensed businesses use two different sets of prices.

We model this type of program by superimposing two Salop circles. As before, we denote t the amount of the cash transfer and w the money that consumers get from other sources; m is the number of shops licensed to sell in the new market for cash transfers, and n is the total number of shops that are operating in the hard-cash market. We assume that consumers go shopping twice, once to spend the cash transfer money t and once to spend money from other sources w; these transactions are independent, implying that consumers have to pay transportation costs twice.[11] Equilibrium prices in each market are therefore determined by Eq. (2), where w and t are set to 0 respectively. In this setting, prices in the new market for cash transfers are larger than prices in the old market if $tm < wn$. This inequality is satisfied if the number of retailers that have access to the new market is limited and if the value of the cash transfers is low in comparison to the money that consumers get from other sources. These conditions are expected to be satisfied in most contexts, including in the empirical study below.

We highlight three implications when these conditions are satisfied and there are market imperfections; these predictions will be tested in the empirical part of the paper.

- **Prediction 1:** All shops generate super-normal profits.
- **Prediction 2:** The impact of vouchers or mobile money transfers on the profits of licensed businesses is positive as they benefit from higher sales and from market protection in the new market.
- **Prediction 3:** Retail markets are characterized by two prices: a lower price in the cash market, which is more competitive, and a higher price in the new market for vouchers or mobile money.

2. Background

The Kakuma refugee camp and the Kalobeyei settlement are located in northern Kenya. The Kakuma refugee camp was established in 1992 following the arrival of 10,000 refugees, mainly unaccompanied minors, who were fleeing war-torn Sudan. In 2018, when we collected endline data, the camp was accommodating about 145,000 refugees, mainly from South Sudan, Sudan, Somalia, Burundi, Democratic Republic of Congo, and Ethiopia. The Kalobeyei settlement was opened in 2016 to provide room for the still increasing number of refugees in the region. It is located 3.5 km to the west of the Kakuma refugee camp and was home to about 38,000 refugees. At both sites, refugees had access to similar facilities (Betts et al., 2018a). However, the newer site in Kalobeyei was designed as an integrated settlement which is promoting refugee self-reliance and is also open to the host population.

While the Refugee Act of 2006 grants refugees access to work permits, different constraints – e.g. movement restrictions – prevent them from exercising this right in practice (Betts and Sterck, 2022). As a result, refugees are compelled to engage in informal work, which

is tolerated within the camps and settlements. NGOs and international organizations seeking to employ refugees cannot do so formally and consider them as "volunteers" receiving incentive payments. At the time of our research, employment levels were low, especially among recent arrivals. Data from 2016 and 2018 shows that only 24% of adults in Kakuma and 10% of adults in Kalobeyei had an income generating activity, and only a minority of households received remittances (Betts et al., 2018b; MacPherson and Sterck, 2021). Most of those working were employed by NGOs or an international organizations. Both sites have high population density. Their markets are quite similar to those of medium-sized urban economies in low-income countries. A business census conducted by the Norwegian Refugee Council (NRC) in September 2018 counted a total of 2250 businesses in the Kakuma refugee camp and 450 in the Kalobeyei settlement, half of which are food vendors. The other main types of businesses are shops selling clothes, restaurants, bars, and barbers or hairdressers. Businesses have to pay a yearly fee to obtain a business permit from the Turkana county government. Although Kenyans can also have businesses in Kakuma camp and Kalobeyei settlement, the vast majority of businesses are owned by refugees.

2.1. The Bamba Chakula cash transfer program

The majority of the population in Kakuma camp and Kalobeyei settlement is reliant on food assistance from WFP (Betts et al., 2018a). After providing in-kind food rations for many years, WFP introduced a program of digital cash transfers in 2015, called Bamba Chakula (BC), which translates to "get your food" in Swahili. WFP variously describes BC as a restricted cash transfer program, a voucher program, or an electronic or digital cash transfer program. The BC system, which is based on the M-Pesa platform, provides all registered refugees with a monthly mobile money transfer.[12] The digital money can only be spent on food items at 188 licensed BC shops in the Kakuma refugee camp and 64 licensed shops in the Kalobeyei settlement. Using cash transfers restricted to food items was required by the Kenyan authorities due to concerns that unrestricted cash transfers could be diverted to finance terrorist activities. Furthermore, retailers in the camps were concerned about the security implications of handling large amounts of hard cash. In order to receive their transfer each month, refugees have to verify their presence in the camp by providing their fingerprint in a so-called "proof of life" session. The whole BC transfer for a household is made to one designated household member and depends on the household size and location.

At the time of our survey, refugees based in the Kakuma refugee camp were receiving about 30% of their monthly ration as BC transfer, while the rest was provided in-kind. In the Kalobeyei settlement, virtually all food aid was distributed through the BC program (Betts et al., 2018a).[13] With the combination of the cash transfers and in-kind rations, all refugees in Kalobeyei and Kakuma should be able to consume 2100 kcal per day at local market prices. Given widespread poverty, food assistance is extramarginal for a majority of households.

The BC purchases for a household typically include heavy bags of staple food that are difficult to carry over long distances, such that refugees often pay a boda-boda (motorbike taxi) to transport their

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[11] The assumption that transactions are independent is needed to simplify the maths.

[12] All refugee households were provided with a BC SIM card. BC businesses are required to have spare phones that customers can use to process payments. They can redeem the revenue of BC sales for cash.

[13] The combination of in-kind and cash transfers in Kakuma reflects the fact that donors – e.g. USAID – make both in-kind and cash donations to WFP. In Kakuma, the proportion of in-kind versus cash assistance depends on household size (Table A.6). Since the creation of the Kalobeyei settlement, WFP distributed cash transfers to promote refugee self-reliance and develop local economies (MacPherson and Sterck, 2021). In both Kakuma and Kalobeyei, refugees are receiving a small in-kind supplement of Corn-Soy Blend to prevent malnutrition.

goods home. Most refugees go to retail shops within the Kakuma camp and Kalobeyei settlement. A trip costs between 100 and 250 KES (between 1 USD and 2.5 USD) when buying from one of the nearest market areas, a significant cost compared to the value of the monthly transfer per refugee.

2.2. Allocation of Bamba Chakula licenses

Licenses for participation in the BC system were allocated to food retailers following a competitive selection process.[14] Four application rounds were organized: two for food retailers in the Kakuma refugee camp, one for food retailers in the Kalobeyei settlement and one specifically for Kenyan business owners intending to move to the Kalobeyei settlement. Each application round was widely advertised with the help of a public relations company. WFP held information sessions, made speaker announcements, sent enumerators to approach all shop owners, and used the network of market leaders to reach all food vendors in the respective site. On pre-specified dates, enumerators went back to the markets to fill in application forms with business owners. Help-desks were also set up to assist retailers with the application process. After the application deadline, shop visits were conducted to verify the information provided in the application forms. About 93% of BC applicants located in the Kakuma camp and Kalobeyei settlement were refugees.

Upon completion of the application phase, a spreadsheet containing each applicant's information was passed on to a multi-stakeholder committee with representatives from humanitarian organizations and the Government of Kenya for selection.[15] The committee's main goal was to select a mixed group of business owners in terms of gender, origin, and location of their shops. That way, the selection committee wanted to avoid tensions between communities and ensure that all refugees would have a BC shop close to their homes, ideally with a shop owner who shares a common language. Shops selling fruits and vegetables and who have a weighing scale were preferred.

To be eligible, traders had to be already selling food in the camp or settlement. There were no hard criteria related to shop size or capacity, as the selection committee assumed that businesses would quickly expand after obtaining a BC license. Selling items at the smallest scale, e.g. from a small blanket in the marketplace or from someone's home, was considered sufficient. Selected traders were given a grace period of three months after completion of the selection process to register their business with the county council and pay the related fees. Applicants had to commit to provide good service to refugees, to sell quality food at market prices, and to allow regular inspections and monitoring. The committee also evaluated whether all applicants were staying in Kenya legally, i.e. were registered refugees or Kenyan nationals. The three applicants excluded at this stage are not considered in our analysis.[16]

Fig. 1 illustrates the timeline of the four application rounds and provides the number of applicants and allocated licenses for each round. The first round of licenses was distributed to retailers in Kakuma Refugee Camp a couple of weeks before the launch of the BC program in Kakuma. WFP organized a second round in Kakuma four months after the launch of the BC program to expand the group of licensed businesses. When the Kalobeyei settlement opened, refugees received almost all their entire food ration as a cash transfer from day one. For this to work, food retailers with a BC license had to be available.

WFP therefore invited business people from the host community to open shops in the Kalobeyei settlement to cater for the newly-arriving refugees. Half a year after the opening of the Kalobeyei settlement, WFP started the process of selecting Kalobeyei-based business owners for BC licenses. The process was very similar to the ones that took place in the Kakuma refugee camp, with the same set of questions on the application form and the same selection criteria. More details on the BC program are provided in the policy report associated with this paper (Betts et al., 2019).

Our empirical analysis excludes Kenyan applicants for two reasons. First, their shops are mainly based in Kakuma and Kalobeyei towns, far away from the Kakuma camp and Kalobeyei settlement. At the time of the BC selection process, 93% of BC applicants in the Kakuma camp and Kalobeyei settlement were refugees, while 99% of BC applicants in Kakuma and Kalobeyei town were Kenyans. Second, for Kenyan applicants, stricter criteria were applied and the number of applicants that satisfied these criteria was so small that they all received a license. The selection process for Kenyan businesses is therefore likely to violate the overlap assumption underlying matching methods. Results are qualitatively similar when this group is included in the analysis (Table A.22).

2.3. Business development programs

Before our study, WFP had been offering two other business development programs in Kakuma and Kalobeyei. First, WFP organized business trainings in financial management, business development, food safety, and supply chain management. Second, WFP was supporting the development of business-to-business linkages between BC retailers and large wholesalers in order to reduce supply-chain inefficiencies and facilitate BC retailers' access to credit (WFP, 2018b). In Section 6.5, we test whether the impacts of BC licenses is mediated by participation in these programs.

3. Data

This section describes the data and the variables of interest.

3.1. Data sources

Our main analysis draws on two sources of data. First, we use the exact same data that the selection committee used to allocate BC licenses (see Section 2). This dataset contains all the information that shop owners provided when they applied for a BC license in 2015–2017. It will allow us to control for all factors considered in the selection process using matching methods. A description of the data is provided in Appendix B.1.

Second, we conducted a business survey in the Kakuma refugee camp and the Kalobeyei settlement in October and November 2018.[17] We aimed to interview all refugees that ever applied for a BC license using the lists of applicants provided by WFP.[18] The survey therefore covered the full population of refugee applicants and no sampling had to be done. After extensive search, we identified the location of 93.8% of applicants. Among those, 85.8% were interviewed, 11.8% had left

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[14] The description of the selection process draws on numerous discussions with WFP staff in charge of the BC program in Kakuma and Kalobeyei. We are particularly grateful to Eddie Kisach at WFP for his inputs and patience to answer all of our questions.

[15] The committee consisted of representatives of the WFP, UNHCR, Norwegian Refugee Council, the Department of Refugee Affairs, the Public Health Office, and the County Commissioner's Office.

[16] The committee did not use a score when making their selection, which is why we do not use a Regression Discontinuity Design in the empirical analysis.

[17] The questionnaire contained modules on business characteristics, business practices, and living standards. Extensive information on the characteristics of shop owners and their households was also collected. Data collection was conducted with tablets by trained enumerators. The questionnaire was translated into seven languages to ensure every shop owner could be interviewed in a language they are comfortable with, and so all enumerators conducted the interviews in their native languages. The languages included Anyuak, Somali, Kirundi, Juba Arabic, Arabic, Oromo, and Swahili.

[18] There were seven individuals that applied in both application rounds in Kakuma, but were not successful in either of them. We randomly chose one out of their two application forms when implementing matching.
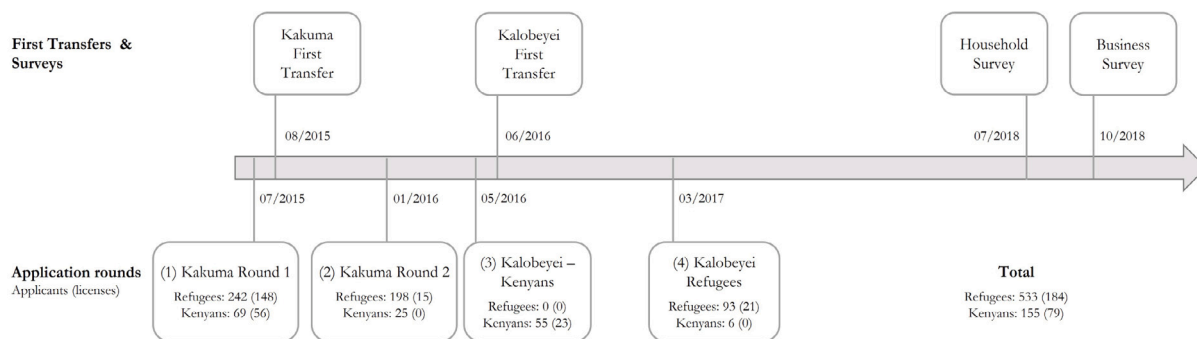
Fig. 1. Timeline of the roll-out of the Bamba Chakula program and survey data collections.

the camp permanently or temporarily or deceased, and 2.4% were found, but did not agree to be interviewed (Table A.8 in the Appendix). In total, 429 interviews were conducted with refugees that had applied for a BC license; among those, 350 still owned a business. We show that our results are robust to attrition in Section 6.4.

Beyond quantitative data collection, we conducted focus group discussions as well as 122 qualitative interviews with business owners and their clients. More details on the quantitative and qualitative surveys are provided in the policy report associated with this paper (Betts et al., 2019).

3.2. Variables of interest

Our treatment variable captures whether respondents where offered a BC license during one of the BC application rounds.[19]

We consider two categories of outcome variables: (i) business outcomes, including having a shop, revenue, profit, revenue from cash sales, number of employees, labor productivity, and the variety of goods sold, and (ii) the welfare outcomes of shop-owners' households, including a measure of food intake, an asset index, and two measures of household income. We briefly describe how these variables are constructed below (additional explanations are provided in the Appendix).

- **Shop dummy:** This dummy is equal to one if respondents had a shop selling food items in October 2018, and equal to zero otherwise.
- **Revenue:** For a list of 29 goods, interviewees were asked whether they sold them and, if so, in which units. For each selected unit, we elicited the retail and wholesale prices and the number of units sold in the past month. We estimate the revenue of shop i, by multiplying the retail price P_i^{ab} of each good a sold in unit b with the number of times this item was sold S_i^{ab}.

$$Revenue_i = \sum_{a=1}^{29} \sum_{b=1}^{B_a} S_i^{ab} P_i^{ab} \qquad (6)$$

B_a indicates the number of units elicited for good a. This measure covers the vast majority of sales in the food market, as the variety of goods available in Kakuma and Kalobeyei is limited and the 29 goods in the survey covered all regularly traded items. Similar results are obtained with a self-reported measure of revenue.

- **Profit:** The profit of shop i is calculated as the difference between revenue (Eq. (6)) and total expenses. Total expenses include inventory costs (which are constructed in the same way as revenue) as well as self-reported expenses on wages to employees, utilities, rent, maintenance and repair, rent of machinery and equipment, transportation, and telephone costs. Similar results are obtained with a self-reported measure of profit.
- **Revenue from cash sales:** We use answers to the question: "In the past month, how much were your sales of any item using cash?".
- **Employees:** The variable captures the number of people who have been working in the shop the month preceding the survey, including the shop owner.
- **Labor productivity:** We use the productivity measure suggested by Lagakos (2016) for the retail sector.[20] Productivity is defined as the value added, in terms of total revenue minus inventory costs for the good sold, per worker. Using our detailed information on prices and quantities of goods sold, we calculate the labor productivity of business i as:

$$Productivity_i = \frac{\sum_{a=1}^{29} \sum_{b=1}^{B_a} S_i^{ab}(P_i^{ab} - W_i^{ab})}{L_i} \quad , \qquad (7)$$

where P_i^{ab} and S_i^{ab} are the retail price and sales for good a in unit b as defined above, W_i^{ab} is the respective wholesale price and L_i is the number of people working in the business, including the owner.

- **The variety of goods sold:** This variable counts the number of different items sold from the list of 29 items elicited in the survey.
- **Food Consumption Score:** To measure food intake, we use the Food Consumption Score (FCS), which is a composite score that aggregates information on dietary diversity, food frequency, and the relative nutritional importance of food items. While based on a simple survey tool, this index was found to be highly correlated with more complex measures of food security and dietary diversity in a range of contexts (WFP, 2008).
- **Private assets:** We measure the value of a household's assets by aggregating the replacement value of items the household owns, from a list of assets that are likely to affect the living standard of the owner.

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[19] For three reasons, this variable might slightly differ from a variable capturing whether respondents actually trade in the BC system. Firstly, a couple of business owners do not use their license themselves, but illegally rent it out to another business. Secondly and more commonly, some non-BC shops ask businesses with a license to process some individual payments through the BC system for them. Lastly, some people lost their licenses because they moved away or because of malpractices (like renting out licenses). Overall, 29 people had lost their licenses at the time of our survey, of which 17 had left the camp or deceased and were therefore not interviewed. Based on this definition of the treatment variable, we will estimate the average treatment effect (ATE) of being allocated a BC license during one of the BC application rounds or, equivalently, the intent-to-treat (ITT) effect of BC licenses at the time of the survey.

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[20] The measure is based on the assumption that the costs of purchasing inventory are the main cost factor for retail businesses and all other expenses represent only a small fraction of total expenses. In the case at hand, expenses outside of purchasing inventory account for less than 10% of total expenses, which is in the range of what Lagakos (2016) considers as small.

Table 1
Descriptive statistics.

	BC License		No License		t-test	Number
	Mean	StD	Mean	StD	*p*-value	of Obs.
Demographic characteristics						
Business Owner	0.96	0.20	0.73	0.45	0.00	429
Gender - Male	0.64	0.48	0.72	0.45	0.08	429
Age	37.79	9.25	36.20	8.79	0.08	429
Married	0.73	0.45	0.74	0.44	0.80	429
Years in Education	7.34	4.66	7.96	4.77	0.19	428
Vocational Training	0.50	0.50	0.45	0.50	0.31	429
Speaks English Well	0.28	0.45	0.35	0.48	0.11	429
Speaks Swahili Well	0.39	0.49	0.50	0.50	0.02	429
Remittance in Past 3 Months	0.12	0.32	0.12	0.33	0.85	429
Has Children in HH	0.76	0.43	0.78	0.41	0.62	429
Nationality						
Somalia	0.39	0.49	0.20	0.40	0.00	429
Sudan	0.22	0.42	0.12	0.32	0.00	429
Ethiopia	0.13	0.34	0.23	0.42	0.01	429
Burundi	0.08	0.27	0.22	0.42	0.00	429
South Sudan	0.08	0.28	0.13	0.34	0.16	429
DR Congo	0.07	0.25	0.07	0.25	0.95	429
Other	0.02	0.15	0.03	0.16	0.89	429
Business characteristics						
Age of Business in Years	5.74	3.76	4.85	3.22	0.02	349
Number of Shops	1.16	0.42	1.10	0.43	0.24	350
Business Permit	0.99	0.11	0.86	0.34	0.00	350
Number of Workers w/o Owner	2.50	1.75	1.97	1.68	0.00	350
Owner Hours Worked (Last Week)	62.61	26.11	61.71	26.67	0.75	348
Training with WFP	0.76	0.43	0.23	0.42	0.00	350
Any Written Bookkeeping	0.76	0.43	0.62	0.49	0.01	350
Bank Account	0.50	0.50	0.42	0.49	0.12	350
Business outcomes						
Revenue	589,325	689,061	311,381	461,067	0.00	349
Profit	123,007	201,303	64,621	130,492	0.00	349
Self-reported Revenue	439,617	473,436	201,276	255,368	0.00	349
Self-reported Profit	93,747	145,684	37,705	50,052	0.00	348
Cash Sales Revenue	128,769	141,944	175,991	228,488	0.02	349
Employees	2.50	1.75	1.97	1.68	0.00	350
Productivity	24,972	34,751	15,822	25,820	0.01	339
Number of Varieties	15.09	5.34	11.73	6.60	0.00	350
Household outcomes						
FCS	69.69	18.97	61.77	20.10	0.00	427
Private Asset Value	73,575	81,570	64,561	86,208	0.28	429
Non-Business Income	525	2,588	1,693	7,207	0.05	429
Total HH Income	109,102	194,609	46,581	110,436	0.00	428

Notes: The t-test tests the null hypothesis that the difference between the two means is zero. All revenue and profit variables, as well as productivity and the private asset value are reported in KES per month.

- **Non-business income:** We consider household monthly income from economic activities outside of the business.
- **Total household income:** We aggregate business monthly profits and household non-business income.

Table 1 presents summary statistics of respondents and their businesses, distinguishing whether they were offered a BC license or not.

3.3. Inverse hyperbolic sine vs. quantile transformation

The measures of revenue, profit, labor productivity, asset holding, and income have numerous zeros (for respondents without business) as well as dispersed right tails with large outliers. The profit measure is also characterized by a left tail and negative outliers.[21] The common practice in applied economics is to apply the inverse hyperbolic sine (IHS) transformation to variables that have such characteristics, in order to limit their dispersion, facilitate the interpretation of results, and reduce the influence of outliers (Bellemare and Wichman, 2019).

We argue that the IHS transformation has three fundamental flaws. First, the IHS transformation is not scale invariant, implying that different units of measurement yield different results (Aihounton and Henningsen, 2021; Mullahy and Norton, 2022; Chen and Roth, 2022).

Second, when original variables include zeros or negative values, coefficients of a regression with IHS-transformed variables cannot be converted into mean elasticities or semi-elasticities in order to be interpretable in percentage terms. Indeed, the concept of elasticity itself does not make sense with zeros as $\frac{\partial y/y}{\partial x/x}$ does not exist when y or x equals zero. With negative values, the concept of elasticity is counter-intuitive as sign of the elasticity is different than the sign of the partial derivative $\frac{\partial y}{\partial x}$ if x and y are of different sign. With zeros, or negative values, interpreting the results of a regression with IHS-transformed variables as an elasticity or semi-elasticity can be misleading.[22]

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[21] This is the case for the businesses that faced exceptional costs or misreported costs.

[22] A simple example illustrates this point (see Section 5.5 for further examples with our data). Consider a large population that has nothing, and a treatment that randomly provides an amount x to a proportion p of the population. We assume that $x > 100$ and $p > 70\%$, such that the two rules of thumb suggested by Bellemare and Wichman (2019) for applying the IHS transformation are satisfied (mean larger than 10 and few zero-valued

Third, the IHS transformation of variables that have zeros or negative values is suppressing a large amount of interesting variation. For continuous variables that have positive and zero values, the IHS transformation is almost equivalent to transforming the variable into a binary variable, with one cluster of values equal to zero and another cluster concentrated around a positive number. For example, the coefficient of correlation between the IHS of business revenue and business ownership is 0.97. For continuous variables that have negative, positive, and zero values, the IHS transformation is almost equivalent to transforming the variable into a ternary variable (a variable with three different values). For example, the coefficient of correlation between the IHS of profit and a ternary variable equal to -1, 0, and 1 for applicants with negative, zero, and positive profits respectively is 0.98. In Appendix C.2, we provide more evidence on this issue, including Monte-Carlo simulations showing that the binarization effect increases with (1) the proportion of zero-valued observations, (2) the gap between the zeros and the positive-valued observations, and (3) the scale of the variable.

Because of these three flaws, the IHS transformation is unsatisfactory. With zeros or negative values, the IHS transformation is as arbitrary and difficult to interpret as the $log(x + 1)$ transformation. A new method to transform continuous variables that have dispersed tails and zeros or negative values is therefore needed.

We propose to transform a variable into the quantiles of its distribution. The quantile-transformed variable ranges between 0 and 1 and is equal to 0.5 for the observation that has the median value, to 0.25 for the first quartile, etc. If all observations take different values, this method is equivalent to transforming the distribution into a uniform distribution.[23] The quantile transformation generalizes the Wilcoxon rank sum test, by allowing for control variables and multiple treatments in a regression framework. While the Wilcoxon rank sum test considers the ranks of observations, our approach considers the quantiles of the distribution, in order to facilitate the interpretation of effect sizes and allow for comparisons across samples.

The quantile transformation has nice properties. Contrary to the IHS transformation, the quantile transformation is invariant to changes in units of measurement of variables. The quantile transformation has an intuitive interpretation: instead of being interpreted in percentage terms (which is nonsensical with zero-valued or negative observations), it is to be interpreted in percentiles terms. The quantile transformation considers the entire distribution of the transformed variable, giving more weight to the parts of the distribution that are more dense and lower weight to isolated observations (i.e. outliers).[24]

The IHS- and quantile-transformations of our variables of interest are illustrated in Figs. A.3(a)–(f). These figures show that, for our variables, the quantile transformation is approximately concave on the positive domain and convex on the negative domain, which is what we want. It is smoother around zero than the IHS transformation.[25]

In the main analysis, we consider variables expressed in levels and quantiles. In the Appendix, we show that the sign and significance of results are similar when considering IHS-transformed variables.

4. Identification strategy

We exploit quasi-random variation in the allocation of BC licenses and compare the outcomes and practices of businesses with and without a BC license using matching methods. Our estimand captures the medium-term average effect of a business owner receiving a license to operate in the BC market versus not receiving one, conditional on a fixed number of licenses being allocated in the market. We emphasize that this estimand does not capture the market impacts of the BC program relative to an extreme counterfactual scenario without any food assistance. In fact, since most refugee households are dependent on humanitarian aid for their survival, a counterfactual scenario without any assistance is unrealistic and hence not a relevant comparison scenario in the context of this study. However, to the extent that BC generates no spillovers on non-BC businesses (this assumption is discussed below), our estimand is informative about the impacts of the BC program on BC businesses, compared to a counterfactual where all businesses only operate in the hard-cash market and food assistance is distributed using in-kind transfers. This comparison is relevant in the context at hand, as all food assistance was distributed in-kind before the launch of the Bamba Chakula program in 2015. This comparison is generally relevant in humanitarian contexts, where international organizations and NGOs have to decide how to deliver food assistance (and not whether to deliver food assistance).

4.1. Matching

Matching methods rely on three key assumptions. First, the *unconfoundedness* assumption states that, conditional on a vector of control variables X, the potential outcomes are independent of treatment status. Under unconfoundedness, treatment assignment is quasi-random and matching and experimental methods generate similar, consistent results (Heckman et al., 1997; Dehejia and Wahba, 2002; Diaz and Handa, 2006). Yet, matching methods receive bad press among economists because they have been used in contexts where unconfoundedness is unlikely to be satisfied.

We provide five rhetorical and statistical arguments suggesting that the unconfoundedness assumption is plausible in our study (McKenzie, 2021). (1) We have a detailed understanding of how the licensing process took place. (2) We have access to the exact same data available to the selection committee to allocate BC licenses. Based on extensive discussions with WFP staff, we believe that the selection committee only used this data and no other information during the selection process.[26] (3) Businesses in the treatment and control groups are operating in the same economic environment and have been administered the

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observations). We denote y the outcome variable, which is equal to x for a proportion p of the population, and zero otherwise. Theoretically, calculating the elasticity does not make sense given the presence of zeros. Empirically, if we apply the IHS transformation to y and then use the formula $exp(\beta) - 1$ to estimate the semi-elasticity (Bellemare and Wichman, 2019), we obtain that the semi-elasticity is approximately equal to $2x$. This empirical result has no clear interpretation. Repeat the same experiment with a population of individuals that have 100 initially. In this case, the elasticity is approximately equal to $x\%$, which makes perfect sense. Without zeros, regressions with IHS-transformed variables can be easily interpreted in percentage terms. This is not the case with zeros or negative values.

[23] In Stata, we use the function *egen rank* to rank observations and then divide the results by $N + 1$ to obtain the percentiles. If several observations take the same value – the zeros in our study – *egen rank* automatically assigns them the midpoint rank of the cluster, which corresponds well to the intuition that the original distribution is being transformed into a uniform distribution.

[24] Because the quantile transformation depends on the population considered, it should be re-applied to the different sub-populations when doing subgroup analysis.

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[25] Note that concave transformations (log, IHS, quantile) are inadequate to identify treatment effects that concentrated at the top of the distribution. Researchers suspecting important heterogeneous treatment effects, with effects only visible at the top of the distribution, should use quantile regression at different percentiles or endogenous stratification (Abadie et al., 2018).

[26] The area comprising Kakuma refugee camp and Kalobeyei settlement is comparable to a medium-sized city with more than 180,000 inhabitants and about 1300 food retailers. The members of the committee that assigned the licenses did not collect the applications themselves. It is therefore unlikely that the panel making the selection personally knew many of the 533 applicants. More generally, WFP had no formal interaction with refugee owned businesses before the launch of the BC program. They sourced food from large wholesalers in different parts of Kenya and from abroad, and distributed food rations in large distribution centers in the camp without any retailers being involved.

same surveys. (4) Placebo tests confirm that treatment status is uncorrelated with pre-determined characteristics proxying for entrepreneurial ability (Imbens, 2015). We estimated treatment effects on six pseudo-outcomes: years of education, a vocational training dummy, age, a dummy equal to one if the family of the applicant ever owned a shop, a dummy equal to one if applicants ever worked in another shop before starting their current business, and a dummy equal to one if applicants ever owned another shop before starting their current business. The pseudo-outcomes should not be affected by the treatment because they were determined prior to the treatment itself. If we were to find significant coefficients, it would suggest that the selection committee used information about applicants that was not reported in the application forms. As shown in Table A.17 in the Appendix, the estimated effects on the six pseudo-outcomes are low and statistically insignificant at conventional levels. This suggests that the selection committee only used the data from the application forms – which we have – to allocate the licenses. (5) Proxies for business size and capacity are insignificant when estimating the propensity score, showing that the selection committee did not systematically select the most (or least) successful businesses. Using the method of Oster (2019), we also show that selection on unobservables would have to be implausibly large to change the research conclusions (see Section 6.3 and Table A.18 for details). These various pieces of evidence suggest that the unconfoundedness assumption is plausible in our setting.

The second key assumption is the *overlap* assumption, which requires that the probability of receiving the treatment is bounded away from zero and one. In the context of assigning BC licenses, this assumption implies that every BC applicant had a chance to be selected for a license and no applicant was pre-determined to receive one for sure. This assumption is likely to be satisfied. The three application rounds for refugees did not include hard criteria that could have barred applicants from receiving a license. Table 2 shows that no variable collected during the application process perfectly predicts the success or failure of an application. In fact, the pseudo R-squared of the regression is quite low – 0.14 – suggesting that the selection process was largely driven by quasi-random bureaucratic happenstance.

Third, the *Stable Unit Treatment Value Assumption* (SUTVA) requires no spillovers between the treatment and the control groups (all impact evaluation methods rely on this assumption). Three types of spillovers are theoretically possible in our study. (1) There could be negative spillovers in the hard-cash market. This would occur if refugees chose to use their BC allowances and their hard cash at the same shop, for example, to reduce transportation or transaction costs. In this case, retailers in the control group would be negatively affected by the BC program, leading us to overestimate treatment effects. Positive spillovers in the hard-cash market are also possible if control-group retailers get new clients because BC retailers increase their prices or change their practices following their participation in the BC program. In Section 5, we show that spillovers in the hard-cash market are unlikely: the cash revenue of BC shops and control-group shops are not statistically different. (2) Positive spillovers on all shops are also possible if BC businesses spend part of the extra profits from BC sales in Kakuma or Kalobeyei. Yet, our control group is unlikely to directly benefit from such spillovers: as BC business owners have access to food at wholesale prices at their own shops, it seems unlikely that they would use their profits to buy food from competitors in the control group. (3) Short-term, spillovers are conceivable as the market adjusted to the BC system from the in-kind transfers that were previously distributed

in Kakuma. For example, supply costs and quantities could have been impacted by an aggregate supply curve that is not perfectly elastic or consumers needing time to learn where to optimally buy when spending money in the cash market. However, since we are studying medium-term impacts, more than three years after the first BC transfer and 1.5 years after the most recent distribution of BC licenses, spillovers from an adjustment period in which supply catches up and consumers become familiar with the new system should not be relevant anymore. Overall, our data is not consistent with large negative spillovers. Small positive ones are possible, in which case we err on the side of caution by underestimating treatment effects.

As the *unconfoundedness*, *overlap*, and *SUTVA* assumptions are plausible in our study, we are confident that matching methods are a sensible choice to evaluate the average treatment effect of applicants receiving a BC license, conditional on a fixed number of licenses being allocated in the market.

We use three different matching algorithms to ensure our results are not driven by the choice of method. The first estimator is the widely used propensity score matching (PSM) estimator. The recent literature however shows that propensity score matching is less efficient than other estimators and more likely to yield biased estimates, because it discards a lot of valuable information by (i) only using the scalar propensity score instead of the full variation in X as basis for matching and (ii) only matching with the nearest neighbor, which might leave other arbitrarily similar observations unmatched (Imbens and Wooldridge, 2009; Huber et al., 2013). The two other matching estimators considered in this study each circumvent one of these problems.

The second estimator is the nearest neighbor distance matching (NNDM) estimator suggested by Abadie and Imbens (2006), which is seen as best practice among matching estimators that incorporate regression adjustment (Imbens and Wooldridge, 2009; Imbens, 2015). Unlike PSM, the NNDM matches observations based on the whole vector of covariates X. Following Imbens and Rubin (2015), matches are formed based on the Mahalanobis distance in the multivariate space of X. To minimize bias, we only use the single closest neighbor, match with replacement, and use regression adjustment to correct for any remaining differences in covariates after matching (Abadie and Imbens, 2006).

The third estimator is the distance weighted radius matching (DWRM) estimator proposed by Lechner et al. (2011). This estimator also uses the propensity score for matching. Instead of limiting the number of matches for each observation, the number of matches is determined by the number of similar observations in a local neighborhood. This reduces the bias of the estimator as it rules out matches that are too far apart. It also reduces the variance of the estimator by allowing multiple matches when possible. Observations within a radius r of an observation are considered, but are weighted proportionally to the absolute difference in estimated propensity scores, with smaller weights if the observation is further away. We use the procedure of Huber et al. (2015) to determine the radius r. Remaining differences in observables after matching are corrected using regression adjustment.

4.2. Unconditional and conditional average treatment effects on business outcomes

For business outcomes, we estimate two objects of interest. First, we estimate unconditional average treatment effects on all applicants, coding business outcomes as zero for respondents not operating a business (McKenzie, 2017).

Second, we estimate conditional average treatment effects on businesses that would exist in the absence of the BC program. Rigorously estimating treatment effects on businesses is challenging because of sample selection. Business outcomes are only observed for business owners, and business ownership itself is likely to be affected by the

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Even informal interactions were limited, as most staff members of humanitarian organizations do their personal shopping in Kakuma town, where the compounds of NGOs and international organizations are located. Furthermore, our research focuses on applicants, such that we can rule out any self-selection based on unobservable characteristics that determined who applied in the first place.

treatment. This selection problem is usual in studies focusing on employment or business outcomes (Lee, 2009; Attanasio et al., 2011) and is not driven by attrition or lack of randomization. Individuals can be categorized into four types: those who would have a business regardless of the BC program (the "always-traders"), those who would never have a business (the "never-traders"), those starting a business thanks to the BC program (the "compliers"), and those stopping their business because of the program (the "defiers"). Sample selection comes from the fact that business outcomes are measured for different types of individuals in the treatment and control groups. In the treatment group, business outcomes are only measured for the "compliers" and the "always-traders". By contrast, business outcomes are only measured for the "defiers" and the "always-traders" in the control group.

We consider two approaches for bounding average treatment effects on "always-traders". Both approaches assume that treatment assignment only affects sample selection in one direction, i.e there are no "defiers". This monotonicity assumption is commonly invoked in the literature on imperfect compliance (Imbens and Angrist, 1994).

The first approach is the trimming procedure for bounding average treatment effects proposed by Lee (2009). In short, the method consists of estimating the number of "compliers" in the treatment group and then trimming the upper and lower tails of the distribution of business outcomes in the treatment group by this number, yielding worst-case scenario bounds. In our data, the proportion of business owners is much larger in the treatment (96%) than in the control group (73%) because the treatment impacted the probability of having a business. Because Lee Bounds are based on extreme assumptions about sample selection, this approach yields large confidence intervals that are not very informative (Lee, 2009; Mobarak et al., 2023). We therefore propose a second approach, which relies on one supplementary assumption.

The second approach further assumes that, among BC businesses, the average outcomes of "always-traders" are at least as high as the average outcomes of "compliers". Formally, we assume:

$$E(Y|F(1) = 1, F(0) = 1, T = 1) \geq E(Y|F(1) = 1, F(0) = 0, T = 1) \quad (8)$$

where Y is the outcome of interest, T is one for BC businesses and zero otherwise, and F is a function of T which is one for existing firms and zero otherwise.[27] This assumption needs to be carefully evaluated.[28]

Theoretically, the assumption makes sense in our setting. Compliers are less successful businesses that survive thanks to BC licenses but would not exist without the program. Empirically, our data provides evidence that the assumption is plausible. BC businesses that have been created after the allocation of BC licenses (likely compliers) have lower sales and profit on average than BC businesses that have been created before the allocation of BC licenses (likely always-traders).[29]

If the assumption holds, then the difference in average business outcomes between BC and non-BC businesses provides a lower-bound estimate of average treatment effects on "always-traders". Formally:

$$E(Y|F(1) = 1, T = 1) - E(Y|F(0) = 1, T = 0)$$

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[27] With this notation, borrowed from Attanasio et al. (2011), businesses are always-traders if $F(1) = 1$ and $F(0) = 1$; they are compliers if $F(1) = 1$ and $F(0) = 0$.

[28] We note that this assumption would certainly not make sense in the context of sample selection due to attrition.

[29] To test this hypothesis, we restrict the sample to BC shops only. We regress revenue and profit variables on a dummy identifying shops that have been created after the BC application. We control for the age of businesses and application variables. The coefficients of the dummy identifying shops that have been created after the BC application are negative in all specifications, and statistically significant at the 1% level for self-reported revenue (in levels and quantiles) and at the 5% level for self-reported profit (in levels). The self-reported revenue of BC businesses that have been created after the allocation of BC licenses are 54% lower on average than self-reported revenue of BC businesses that have been created before the allocation of BC licenses, certeris paribus.

$$= [(1 - \pi)E(Y|F(1) = 1, F(0) = 1, T = 1) + \pi E(Y|F(1) = 1, F(0) = 0, T = 1)]$$
$$\quad - E(Y|F(1) = 1, F(0) = 1, T = 0)$$
$$= E(Y|F(1) = 1, F(0) = 1, T = 1) - E(Y|F(1) = 1, F(0) = 1, T = 0)$$
$$\quad - \pi[E(Y|F(1) = 1, F(0) = 1, T = 1) - E(Y|F(1) = 1, F(0) = 0, T = 1)]$$
$$\leq E(Y|F(1) = 1, F(0) = 1, T = 1) - E(Y|F(1) = 1, F(0) = 1, T = 0)$$

where π is the share of compliers among BC businesses.[30] We focus on this approach in the main text. Lee bounds are shown in the Appendix (Table A.12).[31]

4.3. Inference

Our survey targeted the entire population of BC applicants. In the absence of sampling, there is no sampling error and sampling-based inference – which aims at quantifying sampling error – is irrelevant. The main source of uncertainty comes from the quasi-random assignment of BC licenses. We therefore use randomization-based inference to test the sharp null hypothesis of no treatment effects (Imbens and Wooldridge, 2009). We apply the following procedure to estimate randomization inference p-values. We randomly re-assign treatment 1000 times using the propensity score as the probability to get a BC license, holding the number of BC license fixed per camp. We then use these 'fake' treatment dummies in order to estimate 'fake' treatment effects. P-values are given by the share of the 'fake' treatment effects that are larger in absolute value than the 'real' point estimates. We also estimate 95% Fisher intervals for treatment effects by inverting the randomization inference tests (Imbens and Rubin, 2015). For the sake of comparison, we also report p-values from sampling-based inference.[32]

5. Results

In this section, we first estimate the propensity score. Next, we look at the treatment effects of receiving a BC license on whether an applicant still has a business and, if so, on its revenue, profit, and a set of intermediate business outcomes. We also study how the receipt of a BC license affects household consumption, asset ownership, and income. When discussing the magnitude of effects, we focus on nearest neighbor distance matching (NNDM) as this matching algorithm yields the best balance between the treatment groups (see Section 6.1).

5.1. Estimation of the propensity score

We estimate the probability of getting a BC license using a logit model that includes all variables from the application process. The variables are described in Appendix B.1.

Results are presented in Column (1) of Table 2. The most important factors are the gender, the nationality, and the location of the applicants in the camp as well as whether they are selling fruits and vegetables or not. The negative coefficient on gender reflects the fact that BC licenses were seen as an opportunity to strengthen female headed businesses. The location of the shop and the nationality of the owner were critical, to ensure a fair distribution of licenses across sites and nationalities. Shop owners in Kakuma 4 were more likely to receive a license compared to those in Kakuma 1 (omitted category). The

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[30] Formally, $\pi = \frac{Pr(F(1)=1, F(0)=0)}{Pr(F(1)=1, F(0)=1) + Pr(F(1)=1, F(0)=0)}$.

[31] A third approach, empirically driven, would be to compare BC and non-BC businesses after first excluding businesses that have been created after the allocation of BC licenses (likely compliers). This approach, which heavily relies on self-reported information on business creation dates, yields similar results (Table A.13 in the Appendix).

[32] For PSM and NNDM estimators, we estimate robust Abadie–Imbens standard errors and p-values (Abadie and Imbens, 2006, 2016). For the DWRM estimator, we estimate asymptotic standard errors and calculate p-values based on bootstrapped t-statistics with 1000 replications (Huber et al., 2015).
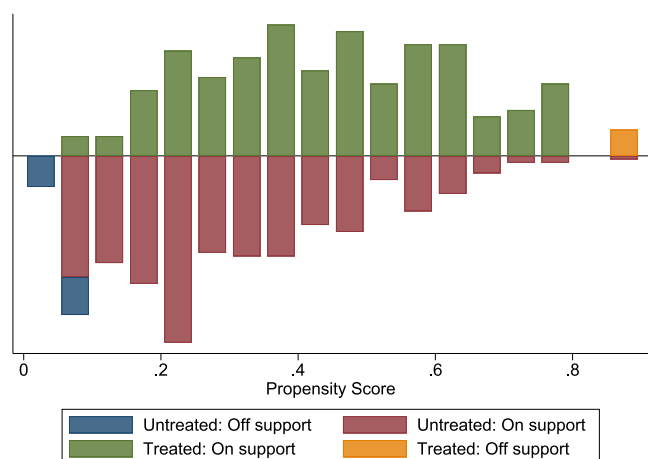
Fig. 2. Distribution of estimated propensity scores.

market in Kakuma 4 is less developed than in other parts of the Kakuma refugee camp and the Kalobeyei settlement, which explains why there were fewer applicants and a higher acceptance rate in Kakuma 4 compared to other areas. Regarding the nationalities of shop owners, the probability of receiving a license was lower for Burundian compared to Ethiopian nationals. The ownership of a weighing scale as well as selling fruit or vegetables increases the propensity of having received a license. By contrast, the possession of a business permit from the local government lowers the probability of successful application. Already having a business permit was not a requirement to be selected for a BC license; shop owners were given a three-month grace period to get one after being selected to trade in the BC system. Interestingly, proxies for business size and capacity (high capacity, permanent structure, stock levels) are insignificant in the regression, showing that these variables were not central in the selection process. This is reassuring when it comes to unconfoundedness, as it shows that most (or least) successful businesses where not systematically selected by the committee.

In Column (2) of Table 2, we add a set of variables that were not part of the application forms but proxy for cognitive and entrepreneurial ability to the model. Reassuringly, these variables do not significantly predict the outcome of the BC application process after controlling for application variables (*p*-value of omnibus χ^2 test = 0.61). This provides further evidence that unconfoundedness is a reasonable assumption in this study. The rest of the analysis therefore focuses on the benchmark model (similar results are obtained with the extended model).

Based on the benchmark model, we calculate the propensity scores of each observation. Fig. 2 shows that the propensity scores have a large region of common support, without gaps in the distribution, but with a few observations lying outside of the range of the opposite treatment group. This is typical for applications of the propensity score and is commonly solved by trimming all observations that violate the overlap assumption (Imbens and Wooldridge, 2009; Lechner et al., 2011). Trimming reduces the number of observations by 3.4% only, from 533 to 515 refugee applicants. We use the trimmed sample for all estimators — including the nearest neighbor estimator which is not based on the propensity score. That way, the results from all three methods are based on the same sample and are hence comparable. For the PSM and DWRM estimators, we re-estimate the propensity score on the trimmed sample before matching (Imbens and Rubin, 2015).

5.2. Business outcomes

Estimates of unconditional average treatment effects (ATEs) on business outcomes are shown in Table 3. Lower-bound estimates of conditional average treatment effects on businesses that would exist

in the absence of the BC program (the "always-traders") are shown in Table 4. The distribution of outcomes for businesses in the treatment and control groups is illustrated in Figs. A.7(a)–(h) in the Appendix.

We first estimate the ATE of BC licenses on the probability to have a business. Based on the NNDM estimator, successful BC applicants were 24 percentage points more likely to still own a business at the time of the survey. The estimates are similar in magnitude across estimation methods and all significant at 1% level. Two mechanisms explain this result. First, people that were not successful with their application are about 11 percentage points less likely to have started a business in the first place (Table A.9 in the Appendix). Although applicants were expected to already have a business, some survey respondents in both the treatment and control groups provided a starting date of their business that is after the license distribution. This suggests that some people made up a business for the application process and intended to open one in case they were successful. Second, businesses without a BC license are about 10 percentage points more likely to have closed, probably because they were not profitable enough (Table A.9 in the Appendix).

Businesses that received a BC license have higher revenues. When considering revenue in levels, the unconditional ATE is an increase of nearly 400,000 KES per month (3784 USD), corresponding to approximately 175% higher average revenues in the treatment group than in the control group. Revenues in the treatment group are on average 27 percentiles higher than in the control group. This massive increase is partly explained by the fact that applicants in the treatment group are more likely to have a business, but also that BC businesses massively benefited from the BC program. Our lower-bound estimate of the ATE on "always-traders" is about 300,000 KES per month (2912 USD). These estimates are significant at the one percent level for all estimation methods. Similar results are obtained with a self-reported measure of business revenue (Table A.10 in the Appendix). If we multiply the estimated ATE on revenues by the total number of BC shops operating in Kakuma and Kalobeyei, we find that the total monthly sales of all BC shops increased by about 1 million USD thanks to BC transfers. Logically enough, this amount is equal to the total amount of BC credit distributed monthly in the two sites (Table A.7).

The impact of BC licenses on profits is also positive and statistically significant. The unconditional ATE is 71,310 KES per month (685 USD), which represents a 154% increase in average profit compared to the control group. Profits in the treatment group are on average 23 percentiles higher than in the control group. Our lower-bound estimate of the conditional ATE on "always-traders" is also large and statistically significant. Businesses that received a BC license reported monthly profits that are about 55,000 KES (526 USD) higher on average than businesses in the control group. This difference is large: about 18 times the average monthly wage of paid employees and about 39 times the value of monthly food assistance to each refugee. This suggests that the BC program not only increased the likelihood of having a business but also the profits of businesses that would have existed even in the absence of the program. Similar results are obtained with a self-reported measure of profits (Table A.10 in the Appendix). The ratio of profit to revenue is 17.6% on average in our sample, showing that profit margins are high. We find no statistically significant difference between the profit margins of BC and non-BC businesses. These findings are consistent with the Predictions 1 and 2 of the theoretical model.

The picture for the effect of BC licenses on cash revenues is quite different. The unconditional ATE on cash revenues in levels and quantiles are close to zero and statistically insignificant. The lower-bound estimates of the conditional ATE on "always-traders" are negative and statistically insignificant in all specifications. This evidence suggests that BC shops are not more attractive for purchasing food items with cash. If anything, BC licenses negatively affect cash sales for businesses that would exist in the absence of the BC program. Based on this

Table 2
Propensity score estimation (logit).

	Propensity Score Estimation (logit models)				
	Benchmark model			Extended model	
	Coefficient (1)	SE		Coefficient (2)	SE
Gender	−0.711***	(0.244)		−0.561**	(0.283)
High Capacity	−0.304	(0.254)		−0.240	(0.291)
Permanent Structure	0.339	(0.374)		0.314	(0.431)
Weighing Scale	0.912**	(0.387)		0.842*	(0.437)
Sells Meat	0.420	(0.419)		0.0580	(0.486)
Sells Fruit/Veg	0.717***	(0.239)		0.743***	(0.282)
Sells Fish	−0.167	(0.426)		−0.0913	(0.499)
Business License	−0.545**	(0.273)		−0.736**	(0.317)
Stock Level					
- < 25 Percent	−0.411	(0.554)		−0.669	(0.661)
- 25-50 Percent	0.252	(0.522)		0.190	(0.611)
- 50-75 Percent	0.475	(0.522)		0.503	(0.613)
Location					
- Kakuma 2	−0.149	(0.344)		−0.633	(0.411)
- Kakuma 3	0.375	(0.298)		0.187	(0.349)
- Kakuma 4	1.133***	(0.373)		1.358***	(0.420)
- Kalobeyei 1	0.272	(0.573)		0.00951	(0.592)
- Kalobeyei 2	0.210	(0.455)		0.372	(0.500)
Nationality					
- Burundi	−1.316***	(0.485)		−1.164**	(0.542)
- Congo	0.117	(0.550)		0.155	(0.593)
- Somalia	0.436	(0.387)		0.969**	(0.457)
- Sudan	0.596	(0.424)		0.653	(0.475)
- South Sudan	−0.370	(0.504)		−0.133	(0.554)
- Other Nationality	0.159	(0.674)		−0.0740	(0.825)
Years in Education				0.000747	(0.0275)
Vocational Training				0.185	(0.249)
Family Shop				0.195	(0.238)
Age				0.0183	(0.0144)
Constant	−1.289*	(0.691)		−1.890**	(0.948)
Pseudo R-squared	0.138			0.173	
N	533			428	

The benchmark model considers variables from the application process, which are defined in Table A.2. The extended model further adds a set of variables from the survey that proxy for cognitive and entrepreneurial ability. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The omitted categories are Kakuma 1 for the locations, Ethiopia for the nationalities, and > 75 percent of the available space for the stock level.

evidence, we conclude that the higher revenues and profits of licensed businesses are primarily driven by BC transactions.

Importantly, this latter result provides support for the Stable Unit Treatment Value Assumption (SUTVA). The fact we find a large effect on total business revenue but no effect on cash sales suggests that the BC program did not affect the cash market much, or at least in the same way for BC and non-BC businesses. The BC program led to the creation of a new parallel market for BC money whose influence on the cash market seems quite limited.

5.3. Intermediate business outcomes

BC businesses have more employees than businesses in the control group. Both the unconditional and conditional ATE are positive and statistically significant across all matching methods. Based on the NNDM estimator, the unconditional ATE is 1.1. The lower-bound estimate of the conditional ATE on "always-traders" is 0.9, which corresponds to at least 46% more employees on average compared to the control group.

The effect of BC licenses on labor productivity is also positive and statistically significant in all specifications. Labor productivity in the treatment group is on average 14 percentiles higher than in the control group. The lower-bound estimate of the conditional ATE on "always-traders" suggests that average labor productivity is at least 11,000 KES higher (106 USD) for BC businesses compared to unlicensed businesses. This corresponds to at least a 70% increase in average value added per

worker. This is a substantial effect, knowing that the average monthly wage of paid employees at control group shops is 3000 KES (29 USD).

BC businesses also sell a larger variety of goods. The unconditional and conditional ATE are positive and highly significant across all matching methods. Based on the NNDM estimator, the unconditional ATE is 6.1, and the lower-bound estimate of the conditional ATE on "always-traders" is 3.3. These estimates are large knowing that control businesses sell on average 12 different types of goods.

5.4. Household welfare outcomes

We analyze the effect of receiving a BC license on household consumption, asset ownership, non-business income, and total household income. These outcomes are observed for all applicants (not only for business owners), which is why we focus on the unconditional ATE. Results are presented in Table 5. The distribution of outcomes for both treatment groups is illustrated in Figure A.8 in the Appendix.

Households of applicants with a BC license have significantly higher food consumption scores (FCS), suggesting that they are more food secure and have a more diverse diet. The ATE is 5.4, which represents a 8.8% higher average FCS in the treatment group than in the control group. The average FCS in the control group is 61.6, much higher than the threshold of 35 defining an acceptable score (WFP, 2008), and much higher than the average score of 39.2 measured in a representative sample of refugees living in the Kalobeyei settlement (Betts et al., 2020). This shows that business owners – both with and without

Table 3
Unconditional ATE on business outcomes.

	Propensity score matching (PSM)	Distance matching (NNDM)	Radius matching (DWRM)	N	Control group mean
Shop dummy	0.183	0.238	0.191	413	0.722
	(0.003)***	(0.000)***	(0.002)***		
	{0.000}***	{0.000}***	{0.002}***		
	[0.07;0.3]	[0.13;0.35]	[0.07;0.31]		
Revenue (Levels)	297 292.4	394 183.9	311 257.4	412	225 440.5
	(0.001)***	(0.000)***	(0.001)***		
	{0.000}***	{0.000}***	{0.021}**		
	[153004;443060]	[257926;530936]	[166419;454233]		
Revenue (Quantiles)	0.244	0.271	0.249	412	0.395
	(0.000)***	(0.000)***	(0.000)***		
	{0.000}***	{0.000}***	{0.000}***		
	[0.17;0.32]	[0.2;0.33]	[0.18;0.32]		
Profit (Levels)	61 900.9	71 309.5	63 721.4	412	46 191.0
	(0.008)***	(0.001)***	(0.006)***		
	{0.000}***	{0.001}***	{0.007}***		
	[18190;106087]	[28851;112302]	[22463;105981]		
Profit (Quantiles)	0.205	0.227	0.213	412	0.417
	(0.000)***	(0.000)***	(0.000)***		
	{0.000}***	{0.000}***	{0.000}***		
	[0.13;0.29]	[0.15;0.3]	[0.14;0.29]		
Cash Revenue (Levels)	1374.8	3942.5	2323.1	412	129 770.3
	(0.963)	(0.890)	(0.937)		
	{0.926}	{0.833}	{0.906}		
	[-52282;54227]	[-44431;51511]	[-51517;55681]		
Cash Revenue (Quantiles)	0.0517	0.0585	0.0588	412	0.472
	(0.205)	(0.119)	(0.140)		
	{0.064}*	{0.053}*	{0.123}		
	[-0.03;0.14]	[-0.01;0.13]	[-0.02;0.14]		
Employees	0.848	1.115	0.822	413	1.437
	(0.004)***	(0.000)***	(0.003)***		
	{0.000}***	{0.000}***	{0.022}**		
	[0.36;1.33]	[0.65;1.59]	[0.34;1.29]		
Productivity (Levels)	13 907.7	17 189.0	15 114.5	402	11 321.1
	(0.008)***	(0.000)***	(0.006)***		
	{0.000}***	{0.000}***	{0.090}*		
	[5910;21610]	[9426;24686]	[7656;22467]		
Productivity (Quantiles)	0.191	0.212	0.204	402	0.414
	(0.000)***	(0.000)***	(0.000)***		
	{0.000}***	{0.000}***	{0.000}***		
	[0.11;0.27]	[0.14;0.28]	[0.12;0.28]		
Number of Varieties	4.890	6.094	4.722	413	8.556
	(0.000)***	(0.000)***	(0.000)***		
	{0.000}***	{0.000}***	{0.000}***		
	[2.9;6.8]	[4.2;8.1]	[2.7;6.7]		

Notes: Outcomes are set as 0 for applicants without business. P-values are reported in parentheses for randomization-based inference and in curly brackets for sampling-based inference. In square brackets, we report 95% Fisher intervals, which are estimated by inverting the randomization inference tests (Imbens and Rubin, 2015). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

BC licenses – tend to be among the richest refugees in Kakuma and Kalobeyei.

The picture is similar for asset ownership. Households with BC licenses have more assets than households in the control group. P-values range between 0.15 and 0.18 when the asset index is expressed in level and between 0.003 and 0.012 when considering the quantile transformation of the asset index. Considering the NNDM estimator, we find that the value of assets of successful applicants is on average 11 percentiles higher than for the control group.

Getting a BC license is not associated with a crowding-out of other income opportunities. Households with and without a BC license are similar in terms of income from other sources. Effects are small and not statistically significant in all specifications. In fact, 90% of households have no other income source than their main business. Consequently, our measure of total household income and of business profit are highly correlated (coefficient of correlation = 0.93). The estimated effect of BC licenses on total household income is therefore very similar to the effect we find on profit. Households who received a BC license have total monthly incomes that are about 66,000 KES higher (637 USD) on average than households in the control group. This effect is massive

compared to the average monthly wage of paid employees in control shops (3000 KES or 29 USD).

Overall, the BC licenses have a positive effect on the living standards of successful applicants' households in terms of food intake, asset ownership, and household income. The absence of effect on non-business income and the large effect on total household income suggest that unsuccessful applicants who do not have a business were not able to start a different, similarly lucrative, activity.

5.5. Results with IHS-transformed variables

Results with IHS-transformed variables are shown in Table A.11. We use the formula $exp(\beta) - 1$ to approximate semi-elasticities (Bellemare and Wichman, 2019). Some semi-elasticities are absurdly large: +5241% for revenue, +4195% for profit, +1212% for cash revenue, +1988% for labor productivity, and +1877% for total household income. These results do not make sense, illustrating the risk of interpreting regressions with IHS-transformed variables in percentage terms when the original variables include zero-valued observations.

Table 4
Lower-bound estimate of conditional ATE on business outcomes of "always-traders".

	Propensity score matching (PSM)	Distance matching (NNDM)	Radius matching (DWRM)	N	Control group mean
Revenue (Levels)	386 492.2	303 281.7	385 961.1	335	312 627.4
	(0.000)***	(0.000)***	(0.000)***		
	{0.000}***	{0.000}***	{0.004}***		
	[220250;555943]	[145550;454892]	[223951;551149]		
Revenue (Quantiles)	0.261	0.219	0.259	335	0.402
	(0.000)***	(0.000)***	(0.000)***		
	{0.000}***	{0.000}***	{0.000}***		
	[0.17;0.35]	[0.14;0.3]	[0.18;0.34]		
Profit (Levels)	63 695.4	54 824.4	61 688.2	335	64 054.9
	(0.018)**	(0.037)**	(0.019)**		
	{0.000}***	{0.008}***	{0.014}**		
	[10627;116764]	[3746;104979]	[10855;112623]		
Profit (Quantiles)	0.178	0.174	0.180	335	0.428
	(0.000)***	(0.001)***	(0.000)***		
	{0.000}***	{0.000}***	{0.000}***		
	[0.08;0.27]	[0.08;0.26]	[0.09;0.27]		
Cash Revenue (Levels)	−18500.0	−24515.3	−21932.0	335	179 957.7
	(0.600)	(0.456)	(0.537)		
	{0.355}	{0.267}	{0.389}		
	[-80668;45699]	[-85395;34035]	[-86753;44960]		
Cash Revenue (Quantiles)	−0.0391	−0.0560	−0.0476	335	0.531
	(0.401)	(0.182)	(0.303)		
	{0.313}	{0.108}	{0.350}		
	[-0.13;0.05]	[-0.15;0.03]	[-0.14;0.04]		
Employees	0.625	0.907	0.657	336	1.989
	(0.030)**	(0.003)***	(0.025)**		
	{0.002}***	{0.000}***	{0.024}**		
	[0.07;1.17]	[0.37;1.44]	[0.11;1.22]		
Productivity (Levels)	16 675.1	11 066.8	16 230.4	325	15 955.4
	(0.009)***	(0.017)**	(0.011)**		
	{0.000}***	{0.001}***	{0.212}		
	[7761;25516]	[2229;19542]	[7642;24893]		
Productivity (Quantiles)	0.152	0.137	0.147	325	0.439
	(0.001)***	(0.007)***	(0.002)***		
	{0.000}***	{0.000}***	{0.001}***		
	[0.06;0.24]	[0.05;0.23]	[0.06;0.24]		
Number of Varieties	2.976	3.327	3.160	336	11.85
	(0.004)***	(0.000)***	(0.002)***		
	{0.000}***	{0.000}***	{0.002}***		
	[1.2;4.7]	[1.5;5.1]	[1.3;5]		

Notes: Outcomes are set as missing for applicants without business. P-values are reported in parentheses for randomization-based inference and in curly brackets for sampling-based inference. In square brackets, we report 95% Fisher intervals, which are estimated by inverting the randomization inference tests (Imbens and Rubin, 2015). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6. Robustness checks

Results are robust to various checks and specification changes. In Section 6.1, we show that the matching algorithms achieve a well balanced sample in terms of all relevant observable characteristics. In Section 6.2, we show that selecting matching variables using the data-driven algorithm of Imbens and Rubin (2015) does not improve balance, which confirms that focusing on the variables that the selection committee used during the selection process is the best strategy. In Section 6.3, we apply the method of Oster (2019) and find that selection on unobservables would have to be improbably large to change research conclusions. We also find similar results when focusing on observations within the interquartile range of the distribution of propensity scores. In Section 6.4, we estimate Lee bounds for all treatment effects to demonstrate that our results are not driven by attrition. Finally, in Section 6.5, we show that a significant part of the effect of BC licenses is independent of participation in other business development programs proposed by WFP.

6.1. Evaluation of the matching quality

Ultimately, the aim of matching on observables is to create a balanced sample in which the treatment groups are similar with respect to the matching variables. To analyze whether this was achieved, we

compare the standardized differences (SD) between the treatment and control groups for all matching variables in the full sample, after trimming and after matching. Several publications suggest that better balance is needed if any of the SDs between the treatment and control groups exceeds 0.25 (Rubin, 2001; Imbens and Wooldridge, 2009). Results are shown in Table A.14 in the Appendix for the full sample.[33] The three matching algorithms substantially improve the balance, and NNDM matching yields the best results. Half of the SDs are below 0.05 and none is above 0.25. For the other two methods, there is also no standardized difference above 0.25. Overall, these results show that matching indeed leads to a well balanced sample.[34]

For the estimations based on the propensity score, Sianesi (2004) proposes an additional method to evaluate the matching quality. Before matching, the selection variables should have some predictive power with respect to the assignment to treatment. However, re-estimating the propensity score on the matched sample, using weights generated

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[33] Balance improvements are similar with the sample of business owners (Table A.15 in the Appendix).

[34] For DWRM matching, the *Stata* post-estimation command *pstest* allows us to estimate t-tests for equality of means using the weights from the matching procedure. Results reported in Table A.14 show that matching reduces the number of statistically significant t-tests from 13 to 1.

Table 5
Unconditional ATE on household outcomes.

	Propensity score matching (PSM)	Distance matching (NNDM)	Radius matching (DWRM)	N	Control group mean
Food Consumption Score	5.953 (0.035)** {0.001}*** [0.34;11.5]	5.437 (0.042)** {0.009}*** [0.02;10.66]	5.602 (0.040)** {0.039}** [0.18;11.01]	411	61.63
Private Assets (Levels)	16 250.3 (0.146) {0.037}** [-6720;39586]	14 137.1 (0.181) {0.095}* [-6589;35260]	15 801.2 (0.157) {0.104} [-6421;37534]	413	66 282.1
Private Assets (Quantiles)	0.110 (0.008)*** {0.000}*** [0.03;0.19]	0.111 (0.003)*** {0.000}*** [0.04;0.18]	0.107 (0.012)** {0.008}*** [0.03;0.18]	413	0.475
Non-Business Income (Levels)	755.8 (0.377) {0.383} [-1294;2914]	−140.2 (0.854) {0.768} [-1722;1456]	763.1 (0.379) {0.672} [-1159;2763]	413	1747.6
Non-Business Income (Quantiles)	0.0140 (0.559) {0.575} [-0.03;0.06]	−0.00619 (0.791) {0.709} [-0.05;0.04]	0.0130 (0.580) {0.754} [-0.03;0.06]	413	0.518
Total HH Income (Levels)	51 209.6 (0.016)** {0.000}*** [10172;92552]	66 363.3 (0.002)*** {0.002}*** [24630;108181]	53 755.2 (0.013)** {0.010}** [13734;92994]	412	45 798.6
Total HH Income (Quantiles)	0.186 (0.000)*** {0.000}*** [0.1;0.27]	0.205 (0.000)*** {0.000}*** [0.13;0.28]	0.195 (0.000)*** {0.000}*** [0.12;0.27]	412	0.426

Notes: P-values are reported in parentheses for randomization-based inference and in curly brackets for sampling-based inference. In square brackets, we report 95% Fisher intervals, which are estimated by inverting the randomization inference tests (Imbens and Rubin, 2015). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

by the matching algorithm, no systematic difference between selection variables should be left and the pseudo-R^2 should be low. Table A.16 in the Appendix shows that this is indeed the case, with the pseudo-R^2 decreasing from 0.14 to 0.03 after radius matching. The likelihood ratio test for the joint significance of regressors in the logit estimation cannot be rejected, indicating no systematic difference in the selection variables after matching.

6.2. Selection of matching variables

The variables included in the estimation of the propensity score were chosen based on extensive knowledge about the selection process. For many applications of matching methods this is not possible, because the relevant process cannot be observed, so data driven ways to select the matching variables are common. We applied the algorithm suggested by Imbens and Rubin (2015) for variable selection, in order to check whether we missed important variables or important interaction terms. We consider all variables of the extended model in Table 2 as well as all interactions between shop characteristics and all interactions with a camp dummy. Only three interaction terms were selected for inclusion by the algorithm, but their inclusion in the propensity score estimation did not improve the balance after matching.[35] As the aim of this exercise is to improve balance, we did not include them in our main analysis. Including the interactions in the list of matching variables yields qualitatively the same results.

6.3. Selection on unobserved variables

We use the method proposed by Oster (2019) to assess the sensitivity of results to omitted variables. This method estimates how strong

the selection on unobservables needs to be to change the research conclusions. To implement this method, we compare the results of OLS regressions of each outcome on treatment status with and without controlling for variables from the application process. Results are shown in Table A.18. The statistic δ indicates how much larger the selection on unobservables would have to be compared to the selection on observables for the true effect to be zero.[36] Reassuringly, all but one estimated δ are much larger than one, indicating that selection on unobservables would have to be much larger than selection on observables to have a true effect equal to zero. The other estimated δ is negative, suggesting that controlling for unobservables would lead to larger effects. We conclude that unobservables are unlikely to drive our results.

Recognizing that observations with very high or very low propensity scores may be different, we also examine whether results are robust to limiting the range of propensity scores considered in the analysis. In Table A.19, we find very similar results when focusing on applicants falling within the interquartile range of the distribution of propensity scores.

6.4. Attrition

The rate of attrition between the BC application rounds and our business survey is 19.5% (Table A.8 in the Appendix), which is large in absolute terms, but not surprising in view of the high mobility of refugee populations (Betts et al., 2023). Out of the 19.5% of attrited applicants, 11% had left the camp permanently or temporarily or deceased, 2.3% did not agree to be interviewed, and 6.2% were not found. The rate of attrition is larger in the control group (24.4%)

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[35] The selected interactions were the following: (Stock Level 50%–75%)*(Sells Meat); (Business Permit)*(Sells Fish); (Kakuma Dummy)*(Stock Level 50%–75%).

[36] 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.

compared to the group that was offered a BC license (10.3%). The difference is large and statistically significant at the 1% level.[37]

We apply Lee Bounds to assess whether selection bias due to differential attrition may drive our results (Lee, 2009). For this purpose, we re-estimate the treatment effects after trimming the upper or lower tails of the distribution of outcome variables for the treatment group. Results are presented in Table A.20 in the Appendix. Using Imbens and Manski (2004) confidence intervals, we reject the null hypothesis that the lower bound is equal to zero for shop ownership, revenue (in levels and quantiles), profit (quantiles), the number of employees, productivity (quantiles), the number of varieties, total income (quantiles), self-reported revenue (in levels and quantiles), and self-reported profit (in levels and quantiles). As Lee Bounds are based on extreme assumptions about selection into attrition (Lee, 2009; Mobarak et al., 2023), we interpret these results as strong evidence that our findings are not driven by selection bias due to attrition.

6.5. Confounding effect of other programs

Our preferred interpretation of results is that the BC program created a massive demand shock that benefited BC businesses. This interpretation could be biased if BC businesses were also more likely to benefit from other business development programs that impacted their business outcomes. At the time of our survey, WFP had been offering two additional programs to BC businesses. First, WFP organized training courses in financial management, business development, food safety, and supply chain management. Second, WFP was supporting the development of business-to-business linkages between BC businesses and four wholesalers, labeled *preferred wholesalers* by WFP. BC businesses were encouraged to work with the *preferred wholesalers* to reduce supply-chain inefficiencies, negotiate prices, and get privileged access to credit (WFP, 2018b; Betts et al., 2019). Participation in these programs could mediate the impact of BC licenses on business performances and thereby invalidate our interpretation of results.

We use the method of Acharya et al. (2016) to carry out a formal mediation analysis. We consider two possible mediators – participation in the trainings and in the preferred wholesaler agreements facilitated by WFP[38] – and use pre-determined characteristics proxying for entrepreneurial ability as intermediate controls. We estimate the Average Conditional Direct Effect (ACDE) of BC licenses, i.e. their effect when the mediators are fixed and can therefore not drive the effects of BC licenses on the outcomes of interest. If the ACDE is significantly smaller than the original treatment effect, it suggests that a significant share of the impact of BC licenses is channeled through the possible mediators.[39] If the ACDE remains important and statistically significant, it means that a significant share of the impact of BC licenses is independent of the considered mediators.

Results of the mediation analysis are presented in Table A.21 in the Appendix. The ACDE remain large and statistically significant for all business outcomes. For two household outcomes – the FCS and the asset index – the ACDE on become statistically insignificant at conventional levels. Overall, the mediation analysis shows that a significant part of the effect of BC licenses is independent of the considered mediators,

which is consistent with our interpretation of results as a massive demand shock that benefited BC businesses. Our preferred interpretation is also consistent with the absence of observed effects on cash revenue. Participation in business development programs, if effective, should affect all outcomes of interest, including cash revenue. This is not what we observe. By contrast, BC licenses are expected to mostly affect revenue and profit from BC sales, in line with our findings.

7. Prices and market imperfections

The model presented in Section 1 predicts that market power is higher in the BC market, implying that BC businesses can charge higher prices for BC sales (Prediction 3). We test this hypothesis and discuss implications.

We study the effect of the BC program on retail prices using survey data from a representative sample of households whose members arrived in Kakuma and Kalobeyei after March 2015.[40] The survey was undertaken in July and August 2018, two months before the business survey. In Kalobeyei, we interviewed 704 households from South Sudan, Burundi, and Ethiopia. These nationalities were selected as the most sizable communities living in Kalobeyei, comprising 93% of the population of the settlement. Households were randomly selected from a satellite image of the settlement. In Kakuma, we interviewed 611 recently-arrived South-Sudanese households, which were selected from UNHCR's registration list. The survey was administered by trained enumerators in Kirundi, Dinka, Juba-Arabic, Nuer, and Somali languages.

The questionnaire included detailed questions on consumption and expenditures. For 18 categories of food, the household member preparing the food was asked whether any household member ate or drank the commodity in the seven days preceding the survey. For positive answers, follow-up questions were asked about the quantity consumed, how they purchased or obtained the food (BC, money, gift, own production), and how much they paid for it.

We use these data to study whether BC purchases are priced differently than cash purchases. In our main specification, we use a simple OLS regression in which our dependent variable of interest is the price paid per kilo divided by the average price paid per kilo of BC transactions for that good. Our main variable of interest is a dummy equal to one for cash transactions and equal to zero for BC transactions. We also do a series of robustness tests to show that results are not driven by mismeasurement problems, differences in product quality, or unobservables at the household level. First, we control for a dummy equal to one for transactions that occurred in Kalobeyei and zero for transactions in Kakuma, and for a measure of the quantity purchased. Second, we control for product and household fixed effects. Third, we explore whether results are robust to trimming the price variable; the top and bottom 1% of prices are set to missing before constructing the dependent variable. Fourth, we exclude outliers, defined as observations with a standardized residual larger than two in absolute value. Fifth, we restrict the sample to staple food (cereals, potatoes, beans, and oil), as these products do not vary much in quality and are consumed by most households. Finally, we consider a median regression.

Results are presented in Table 6. We find that cash purchases are significantly cheaper than BC purchases, in line with the Prediction 3

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[37] We used t-tests and the application data to compare baseline attributes of attritors and non-attritors. Only two out of 25 t-tests are statistically significant at the 10 percent level (nationality dummies for Burundi and Somalia), which is what one would expect by chance. However, a F-test of joint significance is statistically significant at the 1% level, suggesting attrited households are different. We find no significant difference in the propensity scores in the two groups (p = 0.9857).

[38] Data on participation in these programs was kindly provided by WFP.

[39] A significant difference between the ACDE and the original treatment effect could also be due to the mediator being itself a consequence of the outcomes of interest (e.g. if successful businesses are more likely to participate in business trainings or preferred wholesaler agreements).

[40] The data is the second wave of a panel survey. Details about the sampling strategy, the data, and the context are provided in MacPherson and Sterck (2021). Similar results are obtained with the first wave of data. We note that data from the business survey cannot be used to study whether BC and cash sales are priced differently. Offering different prices for BC and cash sales goes against WFP's rules. Therefore, questions related to prices did not distinguish payment modalities to avoid undermining respondents' trust and data quality. As a result, we do not know whether BC applicants reported the price of BC sales, the price of cash sales, or some weighted average of the two.

of the theoretical model. The difference is sizeable. Prices are 16 to 30% lower with cash, depending on the specification. This suggests that, because of market imperfections, BC retailers have a higher market power, which enables them to charge higher prices for BC sales. During qualitative interviews, respondents also reported that prices tend to be much higher at BC shops. One South-Sudanese refugee complained in the following terms: *"Prices of Bamba Chakula traders are extremely high but, since we are restricted, we have no choice"*. Another South-Sudanese refugee expressed his frustration: *"Bamba Chakula shop price is the worst price I ever imagined [...] Non-Bamba Chakula traders, their price is friendly to us"*. A Somali-Ethiopian refugee reported that price differences are large: *"Non-Bamba Chakula shops are cheaper. There is a big difference. In Kakuma, the sugar costs 2500 KES with cash but 3000 KES with Bamba Chakula"*. Another South-Sudanese refugee provided more examples: *"The problem with Bamba Chakula is that the Bamba Chakula traders are increasing prices. For example, five liters of cooking oil is 800 KES in Bamba Chakula shops. In non-Bamba Chakula shops, five liters of cooking oil is 600 KES. Prices at Bamba Chakula shops are not the same. One bag of sorghum, for instance, is 1100 KES in non-Bamba Chakula shops and, in Bamba Chakula shops, the sack is 1500 KES"*.

Price differences suggest that BC transfers have generated a two-tier market structure in which BC businesses enjoy higher market power. At least three market imperfections explain this outcome. First, the number of BC shops is restricted by WFP. At the time of our survey, only 252 BC licenses had been allocated; about 1200 food retailers were excluded from the BC market (Table A.7).[41]

A second factor limiting competition is the scarcity of transportation options and the poor quality of roads. Only 2% of respondents to the household survey had a bicycle, 0.7% had a motorcycle, and 0.5% had a car. Public transport within and between sites is non existent. As a result, transportation costs are high. Most households use a boda-boda (motorbike taxi) for their shopping. A trip typically costs between 100 and 250 KES, depending on the distance and the quantity of goods transported. This is a significant cost compared to the value of monthly food assistance per person (1400 KES). Our theoretical model in Section 1 shows that competition is reduced in the presence of transportation costs.

A third factor is price collusion. Some retailers reported meeting on a regular basis to agree on a common set of prices. The meetings are organized in the different markets by a market coordinator, a role which was created by WFP. One trader explained the purpose of these meetings as follows: *"Most of the time we discuss the prices, because the prices vary in the camp, especially for sugar, sweet potatoes, beans, and others; so that is why we do discuss in case of any change. The meetings are useful because when we talk, we know the prices to use all of us. It helps us to have the same prices; otherwise the customers will see the differences between different shops, which is not good"*.

These factors explain why prices of BC purchases are higher than cash purchases and, more generally, why food retailers are able to make substantial profits. These profits are made at the expense of cash transfer recipients, who would be able to purchase 16 to 30% more food with their cash transfer if BC prices were equivalent to hard-cash prices.

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[41] During the qualitative survey, a Somali-Ethiopian refugee directly associated the limited competition in the BC market to the higher prices *"Actually, because of limited number of Bamba Chakula agents, they are increasing the price of items. Since they are not that so many, the demand and supply are not balanced"*. A Bamba Chakula trader from Kakuma was recognizing the problem in the following terms: *"If the traders are many, they will help the community as competition will increase. But if the number of Bamba Chakula shops is small, it will not be good and people will face problems. I am not saying this because I am a trader, but it is better to increase the number of traders to benefit the people"*.

8. Conclusion

In this paper, we showed that the impact of cash-based assistance crucially depends on the modality of transfer, the market structure, and the reaction of businesses to the demand shock. If markets are perfectly competitive, the recipients of unrestricted cash transfers capture all the benefits of the transfers. Apart from a possible period of adjustment in the short run, prices do not change and businesses make no profit. When markets are imperfect, however, businesses may be able capture part of the benefits of unrestricted cash transfers by offering prices that are above the marginal cost. Cash transfer programs that are restricted to certain shops – e.g. programs using vouchers or digital money – can lead to a two-tier market with two different sets of prices: low prices in the cash market, which is more competitive, and high prices in the new, restricted, market for vouchers or digital cash transfers.

Our empirical analysis illustrates this scenario in the context of the Kakuma refugee camp and the Kalobeyei settlement in Kenya. In these two sites, WFP is implementing a program of mobile money transfers called Bamba Chakula (BC). This program is restricted to food items and to certain shops that are licensed by WFP. Restricted cash transfer programs, like BC, are frequently used by governments, development organizations, and humanitarian agencies to restrict recipients' choices (Siu et al., 2023). For example, WFP distributed 2.1 billion USD in different forms of cash based transfers to 27.9 million people in 2019; about half of that amount was distributed with restrictions (WFP, 2018a). A third of cash-based humanitarian assistance is provided as vouchers (Girling and Urquhart, 2021). In developed countries, restricted cash transfer programs are also used to address poverty (e.g. SNAP in the US) and to encourage eco-friendly consumption (e.g. EcoCheque programme in Belgium).

We used matching methods to compare the outcomes of licensed and unlicensed businesses. Our results are consistent with the existence of market imperfections that led to the creation of a two-tier market structure. Applicants who received a BC license have business revenues that are 3784 USD higher on average than unlicensed applicants (+175%). The aggregate effect on the revenues of all licensed applicants is approximately equivalent to the total amount of money injected in the economy (about 1 million USD monthly). The effect of BC licenses on profits is also massive. Applicants who received a BC license have business profits that are 685 USD higher on average than unlicensed applicants (+154%). These massive effects on revenue and profits are partly explained by the fact that successful applicants are more likely to have a business, but also that BC businesses are much more successful than businesses without a license. Licensed businesses have profits that are 526 USD higher than control group businesses (+86%). This difference is extremely large, about 18 times the average monthly wage of paid employees (about 29 USD) and 39 times the value of monthly food assistance per refugee (about 13 USD). More generally, profits in this industry are large, which is consistent with the existence of market imperfections.

We find that prices of purchases with cash are 16 to 30% lower on average than purchases with BC mobile money. We also find that the BC program has large positive effects on the number of employees, labor productivity, and the variety of products sold at BC businesses. Households of BC business owners have better diets, more assets, and higher household income. Several market imperfections explain our results, including the restrictions limiting the number of retailers selling in the BC market and the high transportation costs.

Our paper illustrates the importance of understanding the impacts of cash transfers on markets and businesses. The large profits and higher prices in the market for cash-based assistance suggest that a limited number of businesses are using their privileged status to capture part of the benefits of the program, at the expense of transfer recipients. Our theoretical model is useful to discuss the external validity of findings (Deaton, 2010). In the presence of market imperfections,

Table 6
Effect on prices.

	Dependent variable: prices, expressed in % of the mean BC price of each product							
	OLS	OLS	OLS	OLS	OLS, trimmed prices	OLS, without outliers	OLS, staple food	Median regression
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cash dummy	−0.256***	−0.231***	−0.238***	−0.200**	−0.227**	−0.161***	−0.299***	−0.171***
	(0.026)	(0.027)	(0.051)	(0.086)	(0.095)	(0.019)	(0.053)	(0.017)
Kalobeyei		0.086***	0.086***		0.111***	0.020**	0.049	−0.000
		(0.030)	(0.031)		(0.015)	(0.010)	(0.048)	(0.010)
Quant (% mean)		−0.060***	−0.061***	−0.166***	−0.051***	−0.039***	−0.040***	0.000
		(0.011)	(0.012)	(0.042)	(0.010)	(0.006)	(0.008)	(0.003)
Product FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Household FE	No	No	No	Yes	No	No	No	No
Observations	5690	5690	5690	5690	5461	5430	3700	5690
R-squared	0.0083	0.017	0.023	0.29	0.013	0.22	0.0063	

Notes: heteroskedasticity-robust standard errors in parentheses in Columns (1) to (6). Standard errors in Column (7). In Columns (3) to (7), product fixed effects are included in the regressions. Household fixed effects are further added in Column (4). In Column (5), the top and bottom 1% of prices are set to missing (trimming) before constructing the dependent variable. In Column (6), we exclude outliers, i.e. observations with a standardized residual larger than two in absolute value. In Column (7), we only consider products that are consumed by more than 50 households in each site. In Column (8), we consider a median regression. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

our model predicts that businesses would also benefit indirectly from cash transfers at the expense of direct recipients if the program was unrestricted. All businesses would then indirectly benefit from transfers. Price effects might however be different, as prices might actually decrease if unrestricted cash transfers increase competition. Empirical findings would most likely be very different in perfectly competitive markets, as – aside from a possible period of adjustment in the short run – businesses would not indirectly benefit from cash transfers.

The direct policy implication of our work is that organizations implementing cash-based interventions should identify and address market imperfections to limit rent-seeking and maximize positive impacts on cash transfer recipients, considering market imperfections that exist before the implementation of the cash transfer as well as imperfections that may be introduced through the design of the program. Our findings also illustrate the risks and drawbacks of establishing a parallel market with reduced competition for the distribution of cash-based assistance. As the living conditions of most beneficiaries of cash transfer programs are extremely precarious, any improvements in their living standards can have massive welfare effects.

We conclude this paper by discussing the limitations of our analysis and suggesting several avenues for future research. First, there are several aspects of the theoretical model that could not be tested in the empirical analysis. According to the theory, various scenarios are possible, depending on the market structure and the characteristics of the cash transfer program. Our empirical evidence is consistent with one of the scenarios: for digital cash transfer or voucher programs, a two-tier market structure with different sets of price is likely to emerge. Evidence from other contexts and modalities of transfer is needed to assess the empirical relevance of other theoretical scenarios. Second, our empirical analysis focuses on medium-term effects. We use survey data that was collected up to three years after businesses received their licenses. Future research would be needed to understand how markets adjust in the short run to a new cash transfer program. Finally, our data does not offer enough statistical power to study gender dynamics and heterogeneous treatment effects across sites. The literature has shown that interventions targeted at businesses can have a differential effect on businessmen and businesswomen (Bernhardt et al., 2019). Our model predicts different outcomes depending on the degree of competition. Such heterogeneous effects would be interesting to explore with data from other contexts. More generally, research on how to address market imperfections in contexts where social and humanitarian assistance is delivered is needed.

Data availability

The data used in this paper is owned by the WFP. The de-identified data used in the main analysis is available for replication purposes, upon reasonable request addressed to the corresponding author of the paper.

Appendix A. Supplementary material

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jdeveco.2023.103232.

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