# A Firm of One's Own: Experimental Evidence on Credit Constraints and Occupational Choice

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**Abstract**

We conducted a randomized evaluation of two labor market interventions targeted to young women in urban Africa. One treatment offered participants a bundled intervention that simultaneously relieved credit and human capital constraints; a second treatment provided women with an unrestricted cash grant, but no training or other support. Both interventions had economically large, statistically significant impacts on income over the medium term, but these impacts dissipated in the second year after treatment. Our results are consistent with a model in which savings constraints prevent women from smoothing consumption after receiving large transfers even in the absence of credit constraints.

**Keywords:** youth unemployment, microenterprises, entrepreneurship, credit constraints, cash grants, training, Africa, gender

**JEL Codes:** J24, M53, O12

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# 1 Introduction

Youth underemployment is a major challenge facing developing nations, particularly in Africa (Filmer and Fox 2014). Young people are more likely to be unemployed than older adults (Kluve *et al.* 2016). In low-income countries, unemployment figures also typically underestimate the proportion of youths who cannot find productive jobs (Fares, Montenegro, and Orazem 2006). After leaving school, it often takes young adults in low-income countries several years to find gainful employment or launch a viable household enterprise; during that transition from school to the labor market, many youth are forced to rely on family members for support between stints of work in irregular, informal positions (World Bank 2006). Demographics make the problem of youth underemployment particularly acute in Sub-Saharan Africa, where more than half the population is under 25. Filmer and Fox (2014) estimate that, over the next ten years, only a quarter of the African youth entering the labor market will be able to find paid employment.

Since formal sector jobs are scarce in low-income settings, many policymakers have advocated entrepreneurship promotion programs intended to help unemployed youth generate an income through self-employment (United Nations Development Programme 2013, Franz 2014). The simplest entrepreneurship promotion programs are credit market interventions such as loans or one-off grants of money or physical capital. Economic theory suggests that such interventions can help potential entrepreneurs who have limited opportunities to save or borrow to start or expand profitable businesses, and one recent study suggests that cash grants can help unemployed youth launch businesses and increase their incomes (Blattman, Fiala, and Martinez 2014). However, a growing body of evidence on the the returns to capital among entrepreneurs suggests that credit constraints may not be the main obstacle limiting the growth of female-owned microenterprises: evaluations to date have found that, in most cases, cash grants to female entrepreneurs do not lead to sustained increases in business profits or income (De Mel, McKenzie, and Woodruff 2008, De Mel, McKenzie, and Woodruff 2009, Fafchamps, McKenzie, Quinn, and Woodruff 2011, Fiala 2014, Karlan, Knight, and Udry 2015, Blattman *et al.* 2016).[1] Taken together, these results suggest that many women who operate small businesses are "subsistence entrepreneurs" (Schoar 2010) who lack either the ability or the inclination to expand their enterprises; if this is true, access to capital (alone) is unlikely to have major impacts.

In fact, though capital drop interventions are becoming increasingly common, many youth entrepreneurship programs offer more than just capital, for example, start-up capital

[1]Recent evaluations also suggest that microfinance loans, the canonical credit market intervention intended to help subsistence entrepreneurs, do not lead to significant increases in income or, in most cases, microenterprise profits (Angelucci, Karlan, and Zinman 2015, Attanasio *et al.* 2015, Augsburg, De Haas, Harmgart, and Meghir 2015, Banerjee, Duflo, Glennerster, and Kinnan 2015, Crépon, Devoto, Duflo, and Parienté 2015, Tarozzi, Desai, and Johnson 2015).

together with skills training or ongoing business mentoring (Kluve *et al.* 2016). The theory of change underlying such multifaceted approaches is that young entrepreneurs face many different obstacles and constraints that need to be addressed simultaneously in order to launch a successful microenterprise. For example, they may lack the vocational skills needed to attract customers in competitive markets, they may not have access to the start-up capital needed to launch a business, and they may not know how to manage an enterprise successfully after it is launched. Several recent studies suggest that multifaceted programs that combine vocational education and start-up capital with life skills training may improve the income prospects of young women, in particular (cf. Adoho *et al.* 2014, Bandiera *et al.* 2014).[2]

We evaluate one such multifaceted entrepreneurship intervention: a "microfranchising" program that offered young women in some of Nairobi's poorest neighborhoods a combination of vocational and life skills training together with start-up capital and ongoing business mentoring. Like many entrepreneurship programs, the microfranchising model is premised on the idea that many youth do not have the skills and experience necessary to be competitive in the labor market, and also lack the financial and human capital needed to start a successful enterprise (for example, the ability to conduct market research and develop a business plan). The franchise treatment that we study attempts to overcome these barriers by providing motivated young women with an established business model and the specific capital and supply chain linkages needed to operate the business. The franchise treatment was designed and implemented by the International Rescue Committee (the IRC) in cooperation with local community-based organizations.[3]

We estimate the impacts of this franchise treatment on applicants via a randomized trial. We not only measure the program's impacts in relation to a control group, but also compare those impacts to the effects of a simpler cash grant intervention that relaxed the credit constraint without providing any additional training or support. We interpret our findings through the lens of a simple model of investment decisions when individuals differ in terms of their labor productivity. High productivity types who have limited opportunities to save or borrow may be unable to launch profitable businesses because they cannot accumulate the required capital. In such cases, credit market imperfections may create a poverty trap, and one-off transfers of money or capital, such as those in our study, can lead

[2]There is also evidence that multifaceted programs which combine skills training and asset transfers can improve the income-generating capacity of vulnerable adults (not just youth and not just women). Banerjee *et al.* (2015) demonstrate that one such multipronged approach, the ultrapoor Graduation Program implemented by the NGO BRAC, led to large increases in income, food security, and rates of savings. A recent meta-analysis also highlights the relative effectiveness of multifaceted entrepreneurship promotion programs (Cho and Honorati 2014).

[3]See International Rescue Committee (2016b) for an overview of the IRC's economic development programs.

to permanent increases in income. One of the key insights from the model is that credit constraints are only an obstacle to productive entrepreneurship for a subset of individual types; less productive types are unable to sustain a business in any steady state. Nonetheless, savings constraints can also affect the investment decisions and occupational choices of lower productivity types who receive one-off infusions of funding or capital; though these individuals cannot sustain businesses, they may invest in capital and launch unproductive firms because enterprise capital is a technology for saving, albeit at a negative interest rate. Thus, short-term impacts of one-off transfers on entrepreneurship should not be taken as evidence that a program relieved a credit constraint or addressed a poverty trap; the critical issue is whether impacts on income persist over the longer-term.

We find that both the franchise treatment and the grant treatment led to substantial increases in income in the year after the interventions. Point estimates suggest impacts that are both economically and statistically significant: the franchise treatment increased weekly income by 30 percent, up 1.6 US dollars from a mean of 5.5 dollars in the control group (p-value 0.035); the grant treatment increased weekly income by 3.2 dollars (p-value 0.008) or 56 percent. As expected, these impacts appear to be driven by a shift from paid work to self-employment; women assigned to either the franchise or the grant treatment are approximately 10 percentage points more likely to be self-employed (and 7 percentage points less likely to work for others) relative to those in the control group. Women assigned to the grant treatment also increased their labor supply (hours worked) substantially.

Though both interventions increased income in the relatively short-run, data from endline surveys conducted between 14 and 22 months after treatment indicate that the observed impacts on income disappeared in the second year after the program(s).[4] At endline, women assigned to either the franchise treatment or the grant treatment are more likely to be self-employed than women in the control group, but the treatments are not associated with increases in income or labor supply. In addition, we find no impacts of treatment on food security, expenditures, living conditions, or empowerment at endline. Seen through the lens of our model, these findings are consistent with the existence of savings constraints; large impacts on income and occupational choice that disappear relatively quickly make sense if enterprise capital is one of the few viable savings technologies available to young women in a poor urban area. However, our findings do not suggest that credit constraints had been preventing productive entrepreneurs from launching profitable, sustainable businesses.

This paper makes several contributions. First, we measure the impact of an active labor market program on young women in an urban area in a developing country. Here, we contribute to an active literature on active labor market programs and youth unemploy-

[4] We can reject the hypothesis that the impact on income observed at endline is equal to the positive impact observed at midline (p-value 0.046).

ment.[5] Our work is most closely related to Bandiera *et al.* (2014) and Adoho *et al.* (2014), who also evaluate multifaceted labor market interventions for young women in Sub-Saharan Africa. Our study is also related to the growing literature on the returns to capital among female entrepreneurs (cf. De Mel, McKenzie, and Woodruff 2008, De Mel, McKenzie, and Woodruff 2009, Fafchamps, McKenzie, Quinn, and Woodruff 2011, Fiala 2014, Bernhardt, Field, Pande, and Rigol 2017).

We compare the impacts of a multifaceted entrepreneurship promotion intervention to those of a one-off cash grant; this provides a natural cost-effectiveness benchmark without any of the contextual caveats that would accompany a more traditional cost-benefit analysis. Though evaluations of cash grants are becoming more common (cf. Haushofer and Shapiro 2016), the use of cash as a benchmark within program evaluation is still relatively rare. Our results, like those of Karlan, Knight, and Udry (2015), suggest that unrestricted cash grant treatments can provide an extremely useful alternative to the traditional control group (that receives no treatment).[6]

We measure both interventions' impacts over time, expanding our understanding of the dynamics of the estimated impacts. In addition, we present a model, building on previous work (cf. Fafchamps *et al.* 2011, Blattman, Fiala, and Martinez 2014, Blattman *et al.* 2016), that yields a straightforward interpretation of the estimated program impacts in relation to credit and savings constraints. Our model suggests that the patterns of impacts that we observe are more likely to be explained by savings constraints than by credit-constraint-based poverty traps. This conclusion resonates with other recent evidence that the poor, particularly poor women, have a very limited menu of savings technologies (Dupas and Robinson 2013a, Dupas and Robinson 2013b).

Finally, we capitalize on the program evaluation setting to test whether participants hold accurate beliefs about program impacts; in so doing, we provide a framework for comparing methods of belief elicitation. Our work builds directly on the contributions of Smith, Whalley, and Wilcox (2011) and Smith, Whalley, and Wilcox (2012). Like McKenzie (2016a), we find the program participants do a poor job of estimating their own counterfactual (probabilistic) outcomes. However, we extend the existing set of best practices by demonstrating that participants are quite good at estimating average treatment impacts on the population once behavioral biases are taken into account.

The remainder of this paper is organized as follows. Section 2 outlines our theoretical model. Section 3 describes our research design and the specific franchise and grant treatments that we evaluate. Section 4 presents our main results. Section 5 characterizes participants' beliefs about the impacts of the program. Section 6 concludes.

[5] See Kluve *et al.* (2016) for a recent survey.

[6] Supporting this argument, Özler (2016) has also remarked that "the interesting comparison is not against 'no support' ... it's against cost-equivalent alternative efforts."

## 2 Conceptual Framework

To understand the impacts of capital infusions and other credit market interventions, we require a framework for interpreting individual responses to these interventions. We propose a simple model of labor supply decisions in the presence of credit market imperfections, when individuals may face credit constraints and may also be unable to save. We show that high productivity individuals who are are unable to save or borrow may find themselves in a poverty trap in which they never launch a business, even though their enterprises would be profitable once launched. In this constrained environment, a large capital transfer enables these individuals to start lasting businesses. In contrast, low productivity individuals are unable to sustain an enterprise in any steady state; because these individuals cannot sustain a profitable enterprise, the fact that they are not accessing loans does not indicate a market failure. However, in a savings-constrained environment, low productivity types may open businesses after receiving a large capital transfer, using enterprise capital as a savings vehicle when other savings technologies are unavailable. These businesses are temporary (because low productivity individuals cannot sustain businesses in the steady state), and are eventually closed after the initial capital investment depreciates.

We begin by considering a simple model in which production in each period depends on labor and capital. Labor is allocated between two activities: own-enterprise production, characterized by production function $f^e(K, L^e)$, and wage labor, characterized by production function $f^w(L^w)$. Individuals allocate their labor across sectors subject to the constraint: $L^e + L^w \leq 1$. Importantly, we follow other recent work (cf. Blattman, Fiala, and Martinez 2014) in assuming that own-enterprise production requires a capital investment that exceeds some minimum scale; thus, potential entrepreneurs who are credit-constrained and unable to save cannot launch arbitrarily small businesses that could then grow over time. This minimum scale requirement creates the potential for a poverty trap. Both production functions are characterized by diminishing returns with respect to individual inputs; we assume that the enterprise production function, $f^e(K, L^e)$, is homogeneous of degree one above the minimum scale.

We make the following specific assumptions about the own-enterprise production function, $f^e(K, L^e)$:

$$
\begin{aligned}
f^e(K, L^e) &\equiv 0 \quad \forall K \le K_{min} \quad \textit{(minimum scale)} && \text{(A1)}\\
\frac{\delta^2}{\delta K^2} f^e(K, L^e) < 0 < \frac{\delta}{\delta K} f^e(K, L^e) &\quad \forall K \ge K_{min} \quad \textit{(diminishing returns)} && \text{(A2)}\\
\frac{\delta^2}{\delta L^2} f^e(K, L^e) < 0 < \frac{\delta}{\delta L} f^e(K, L^e) &\quad \forall K \ge K_{min} \quad \textit{(diminishing returns)} && \text{(A3)}\\
\frac{\delta^2}{\delta L \delta K} f^e(K, L^e) > 0 &\quad \forall K \ge K_{min} \quad \textit{(inputs are complements)} && \text{(A4)}\\
\lim_{L \to 0} \frac{\delta}{\delta L} f^e(K, L^e) = +\infty &\quad \forall K \ge K_{min} \quad \textit{(Inada)} && \text{(A5)}\\
\lim_{K \to K_{min}} \frac{\delta}{\delta K} f^e(K, L^e) = +\infty &\quad \textit{(Inada)} && \text{(A6)}\\
\lim_{K \to +\infty} \frac{\delta}{\delta K} f^e(K, L^e) = 0 &\quad \textit{(Inada)} && \text{(A7)}
\end{aligned}
$$

With respect to the wage labor production function, $f^w(L^w)$, we assume that standard Inada conditions hold.[7] In other words, we assume

$$
\begin{aligned}
f^w(0) &= 0 && \text{(A8)}\\
\frac{\delta}{\delta L} f^w(L^w) &> 0 && \text{(A9)}\\
\frac{\delta^2}{\delta L^2} f^w(L^w) &< 0 && \text{(A10)}\\
\lim_{L \to 0} \frac{\delta}{\delta L} f^w(L^w) &= +\infty && \text{(A11)}
\end{aligned}
$$

In each period $t$, the agent has capital $K_t$ and one unit of labor to divide between activities such that $L^e + L^w \le 1$. The agent produces using whatever allocation of labor she chooses, yielding $\mathbb{F}(K_t, L^w) = f^w(L^w) + f^e(K_t, 1 - L^w)$. The maximum level of production in a given period results from the optimal allocation of labor between the two possible sectors:

$$\mathbb{F}^*(K_t) = \max_{0 \le L^w \le 1} \mathbb{F}(K_t, L^w) \tag{1}$$

Proposition 1 characterizes the properties of $\mathbb{F}^*(K_t)$. Because of the minimum level of capital required to produce output in the own-enterprise sector, the function $\mathbb{F}^*(K_t)$ has a characteristic shape, which is shown in Figure 1. The characteristic shape of $\mathbb{F}^*(K_t)$ drives the predictions of our model.

**Proposition 1.** $\mathbb{F}^*(K_t)$, *the total production function conditional on the optimal allocation*

[7]In the Online Appendix, we show that the same argument can be extended for a constant wage rate.

*of labor across the wage labor and own enterprise sectors, has the following properties:*

1. *For all* $K_t \leq K_{min}$, $\mathbb{F}^*(K_t) = f^w(1)$; *hence, the first and second derivatives of* $\mathbb{F}^*(K_t)$ *are equal to 0 for all* $K_t \leq K_{min}$.

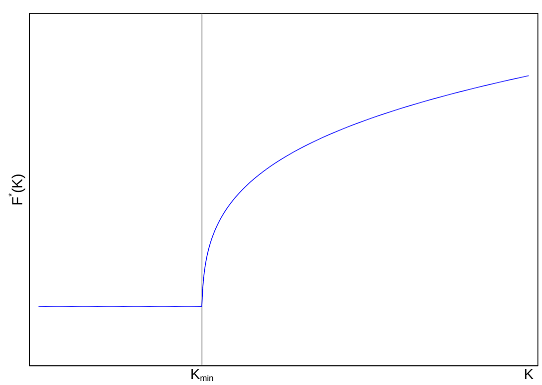
2. *For all* $K_t > K_{min}$, $\mathbb{F}^*(K_t)$ *has a positive first derivative.*

3. *For all* $K_t > K_{min}$, $\mathbb{F}^*(K_t)$ *has a negative second derivative.*

*Proof: see Online Appendix.*

Intuitively, $\mathbb{F}^*(K_t)$ is flat for $K_t \leq K_{min}$. Levels of capital below the minimum level required to operate a business, $K_{min}$, do not contribute to total output and simply depreciate; hence, for individuals who have access to a range of savings technologies, there is no reason to invest $K < K_{min}$ in the own-enterprise sector. At levels of capital exceeding $K_{min}$, $\mathbb{F}^*(K_t)$ inherits the properties of the production function in the own enterprise sector; it is always optimal to allocate one's capital and some of one's labor to the own enterprise sector and operate a business at some scale because the marginal product of capital approaches infinity as $K_t \to K_{min}^+$.

Figure 1: Shape of the Production Function, $\mathbb{F}^*(K_t)$




After production, the previous period's capital depreciates, so that it becomes $K_t(1-\delta)$. The agent also chooses a level of consumption, $c_t$, in period $t$. Capital in the next period is thus given by:

$$K_{t+1} = \mathbb{F}^*(K_t) - c_t + K_t(1 - \delta) \tag{2}$$

A steady state is characterized by a level of capital, $K_{ss}$, and a level of consumption, $c_{ss}$, that satisfy the following condition:

$$K_{ss} = \mathbb{F}^*(K_{ss}) - c_{ss} + K_{ss}(1-\delta) \tag{3}$$

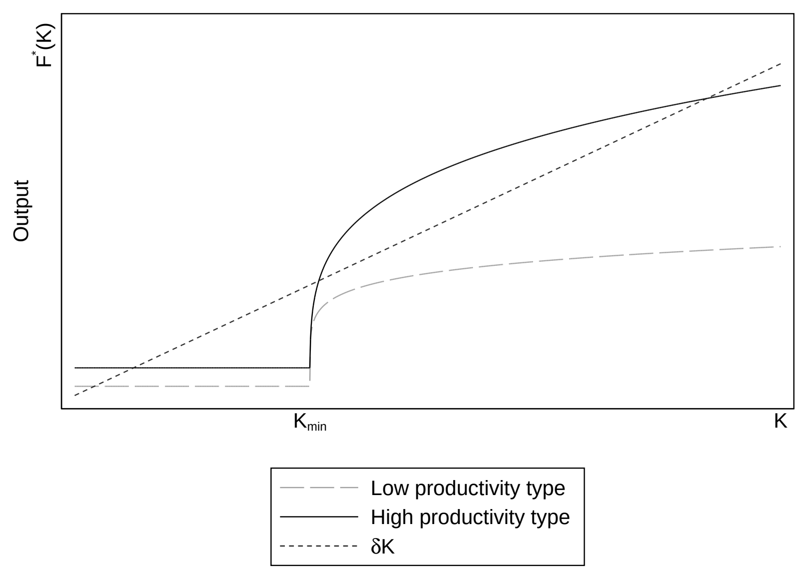
Rearranging, and because consumption cannot be negative, this becomes:

$$c_{ss} = \mathbb{F}^*(K_{ss}) - \delta K_{ss} \geq 0 \tag{4}$$

For any individual, the steady state level of capital cannot exceed the highest value of $K_t$ such that $\mathbb{F}^*(K_t) = \delta K_t$.

Because $\delta K_t$ is a ray from the origin, it may cross the production function, $\mathbb{F}^*(K_t)$, at most three times: it may cross the flat region of $\mathbb{F}^*(K_t)$ (where $0 < K_t < K_{min}$) at most once, and it may cross $\mathbb{F}^*(K_t)$ in the curved region (where $K_t \geq K_{min}$) at most twice. Examples of production functions (and their intersections with $\delta K_t$) are shown in Figure 2.

Figure 2: Examples of Production Functions




Individuals differ in terms of their productivity, which is characterized by the shape of the production function $\mathbb{F}_i^*(K_t)$. We define high productivity individuals as those that can sustain a self-employment activity in any steady state.

**Definition 1.** *Individual $i$ is a **high productivity type** if she is able to sustain a business in any steady state, i.e. if there exists $K_t$ such that $\mathbb{F}_i^*(K_t) > \delta K_t$ and $K_t > K_{min}$. A*

***latent entrepreneur*** *is a high productivity type with at least one steady state that satisfies the condition* $\mathbb{F}_i^*(K_{ss}) > f^w(1)$.

Being a high productivity type is a necessary condition for successful entrepreneurship: individuals who are not high productivity types are unable to sustain an enterprise in any steady state.[8] If high productivity individuals are sufficiently patient and they are able to save at a sufficiently non-negative interest rate, then those who prefer operating their own businesses to working (exclusively) in the wage sector will do so — they will save up the funds needed to make the initial profitable capital investment of $K_{ss} > K_{min}$ and launch their own businesses. Alternatively, high productivity types who face sufficiently low borrowing costs can borrow the funds needed to launch their businesses. However, when opportunities for saving and borrowing are limited, high productivity types who wish to launch their own enterprises may not be able to do so — creating a poverty trap.

Savings constraints also shape individual responses to cash grant interventions. When individuals are able to save, investing a transfer in enterprise capital (or in any other illiquid asset) is only attractive if the return on the investment exceeds the return on saving. However, when saving is impossible, investing in business capital and launching a small-scale enterprise may be one of the only ways to smooth positive income shocks across periods. We assume that capital stock is carried forward (minus depreciation) as long as an individual allocates at least $\epsilon > 0$ units of labor to the own-enterprise sector; we allow $\epsilon$ to be arbitrarily small.

The first key prediction of the model is that a one-off transfer to a latent entrepreneur can lead to a permanent increase in income. Individuals who have access to a zero-interest savings technology will invest enough in their businesses to transition to their preferred steady-state level of capital. In this case, income will immediately rise from $f^w(1)$ to $\mathbb{F}_i^*(K_{ss})$, and will remain there indefinitely. Consumption may also be directly impacted if individuals save and consume transferred funds without investing them in microenterprises (though these direct impacts on consumption should not be associated with changes in occupational choice).

When latent entrepreneurs are unable to save, they will invest any transfers received in their businesses.[9] If the amount of the transfer exceeds the lowest possible steady state capital stock, income rises from $f^w(1)$ to $\mathbb{F}_i^*(K_{transfer})$ and then settles toward the individual's optimal steady state value of $\mathbb{F}_i^*(K_{ss}) > f^w(1)$ over time. Thus, the short-term

[8]Whether a high productivity type prefers entrepreneurship to wage labor will depend on their preferences. For many preferences specifications, opening a business is attractive when $f^w(1) \leq max_{K_{ss}}\mathbb{F}_i^*(K_{ss})$. However, the predictions of the model do not depend on specific assumptions about the utility function.

[9]Transfer recipients may choose to consume of the transferred funds upon receipt; this does not impact the predictions of our model. $K_{transfer}$ should then be interpreted as the amount that is not immediately consumed.

impacts of capital infusions on income may be larger than the long-term impacts, but the long-term impacts on income are positive.

In contrast, for lower productivity individuals — those for whom $\delta K_t$ only crosses $\mathbb{F}_i^*(K_t)$ once, in the flat region where $K_t < K_{min}$ — a capital transfer does not have permanent impacts on income. These individuals cannot operate their own enterprises in a steady state. Even when they are able to save at a non-negative interest rate, saving money to invest in the own-enterprise sector is not an attractive proposition. Even when they are able to borrow at low interest rates, borrowing the funds to launch a business is unattractive (if one is required to eventually repay the loan).

However, when individuals who cannot sustain a profitable enterprise receive a large transfer, they may choose to invest the money in a business if they are savings constrained. Intuitively, enterprise capital is a means of saving at a negative interest rate of $\frac{\mathbb{F}_i^*(K_t)}{K_t} - \delta$. For large infusions of capital, launching a business, consuming the business income, and allowing the business to shrink over time as the capital depreciates will sometimes be preferable to immediately consuming all of the capital received. Operating that business, even if depreciation exceeds production, is still better than letting the capital depreciate without production. Thus, savings-constrained individuals who are not productive enough to sustain enterprises may operate temporary businesses if given a cash infusion. The key distinction between latent entrepreneurs and lower productivity types is that one-off infusions of capital can permanently increase the incomes of latent entrepreneurs, while such infusions of capital have impacts on lower productivity types that disappear over time.

## 3 Research Design and Procedures

We conducted a randomized evaluation of two labor market interventions targeted to young women aged 18 to 19 in three of Nairobi's poorest neighborhoods, Baba Dogo, Dandora, and Lunga Lunga.[10] Applicants to the program were stratified by neighborhood and application date and then randomly assigned to one of three treatment arms: a franchise treatment, a cash grant treatment, and a control group. This design allows us to estimate the impact of the franchise and grant treatments on those invited to the program, and to compare the impacts of the cash grant treatment — which relaxes the credit constraint but provides no other training or support — to a multifaceted program designed to address many of the obstacles to youth entrepreneurship simultaneously.

[10] Applications were solicited from women between the ages of 16 and 19; in practice, relatively few of the applicants (only 14.6 percent) were below 18 years of age when they applied. Only those women who had attained the age of legal majority were eligible to receive cash grants, so our analysis focuses on those who were in the two oldest age cohorts (randomization to treatment was stratified by age). The cash grant treatment was not announced in advance; women applied for a business training program and were then randomized into one of the three treatment arms.

## 3.1 Two Labor Market Interventions

### 3.1.1 The Franchise Treatment

Credit constraints may prevent potential entrepreneurs from launching profitable businesses. However, credit constraints may not be the only obstacle to entrepreneurial success; potential entrepreneurs — particularly young people — may also lack the market intelligence and business training needed to launch a successful enterprise (Berge, Bjorvatn, and Tungodden 2014). We evaluate a multifaceted "microfranchising" program that provided eligible applicants with an established business model and the specific training, capital, and business linkages (for example, with wholesale suppliers) needed to make the business operational. Microfranchisees supply their labor, and are free to expand their microenterprises as they see fit. Thus, a microfranchise has features in common with both a formal sector job and self-employment: while microfranchisees do not need to devise business models, they work with very little managerial supervision and considerable latitude for creativity — managing their own time and entrepreneurial effort. Thus, microfranchising strikes a middle ground between entrepreneurship and wage employment.

We evaluate a microfranchising intervention geared toward young women in Nairobi's poorest neighborhoods. The program helped young women launch branded franchise businesses, either salons or mobile food carts. The intervention combined a number of distinct elements: business skills training, franchise-specific vocational training, start-up capital (in the form of the specific physical capital required to start the franchise), and ongoing business mentoring. Several of the intervention's components are common to many entrepreneurship promotion and job skills programs; what distinguishes microfranchise programs from other interventions is the focus on a small number of specific franchise business models that are tailored to the skills and constraints of program participants (i.e. poor young women in urban Nairobi) and to local market conditions. In this case, the implementing organization (the IRC) partnered with two Kenyan businesses looking to expand their presence in slum neighborhoods — a maker of hair extensions and a poultry producer known for its fast food restaurants. The franchise partners are both relatively well-known firms (within Kenya), and their reputations added value to the franchise package that program participants received.

The first component of the franchise program was a two-week training course. In addition to a standard curriculum of business and life skills training topics, the training included modules about the two specific franchise business models. At the end of the course, participants indicated their ranking of the two franchise partners and were then matched with one of them (almost always their first choice).

After the business skills course, program participants received training from the franchise business partner with whom they had been matched. Women assigned to the salon franchise received six weeks of classroom training and then completed a two-week internship with a local salon. At the end of the internship, participants received their business start-up kits (which included branded aprons, a hair washing sink, a hair dryer, and a variety of hair cutting and styling products). For women assigned to the food cart franchise, the franchise-specific training was a one-day session where franchisees were introduced to the brand, available products, and appropriate preparation methods. Following the franchise training, program participants received business start-up kits that included a mobile cart, an apron or t-shirt displaying the company logo, and an initial stock of smoked chicken sausages.

Each franchise business launched through the program was assigned a mentor who visited the business every few weeks. Mentors helped the young women in the program get their businesses off the ground — for example, by coordinating additional training with the franchise partners, helping the businesses set up bank accounts, or assisting with financial management and record keeping.

#### 3.1.2 The Grant Treatment

Applicants assigned to the cash grant treatment were offered an unrestricted transfer of 20,000 Kenyan shillings (or 239 US dollars at the prevailing exchange rate of 83.8 shillings to the dollar).[11] Individuals assigned to the grant arm were contacted by phone and invited to meet privately with a member of the disbursement team to discuss the grant. During the meeting, individuals were told that there were no restrictions on how the grant could be used and that the grant did not need to be paid back. Disbursements to the grant recipients were timed to coincide with the launch of the microfranchise businesses.

### 3.2 Data Collection

Our analysis draws on three main sources of data. First, we administered a brief baseline survey to all eligible applicants prior to randomization. We also conducted a midline survey 7 to 10 months after the end of the intervention.[12] The midline surveys were conducted via phone. The midline included detailed questions about income-generating activities, but did not ask about a broader range of outcomes (this was not feasible in a short phone

[11] Though the US dollar value of the shilling has since declined, the exchange range was fairly constant during the grant disbursement period (from November 1, 2013 to January 13, 2014). The value of the grant was selected to make it roughly comparable to the value of the microfranchising package of training and capital; the 20,000 shilling amount is also identical to the grant size in another study of cash grants for Kenyan youth (Hicks, Kremer, Mbiti, and Miguel 2016).

[12] We also conducted an extremely brief phone survey 2 to 5 months after the intervention, but we did not ask about income-generating activities at that time. The goal of that survey was to collect better contact information than had been gathered at baseline.

survey). We conducted a more comprehensive endline survey 14–22 months after the end of the intervention.

Attrition rates are extremely low in both the midline and the endline surveys: we successfully surveyed 94.0 percent of the baseline sample at midline and 92.5 percent of the baseline sample at endline. Regressions testing for differential attrition across treatment arms are reported in the Online Appendix. Attrition is not associated with either treatment.

### 3.3 Sample Characteristics

Table 1 describes the baseline characteristics of the young women in our sample. As expected, there is little variation in age: 94.6 percent of the young women in the sample were 18, 19, or 20 years of age at baseline. 11.6 percent of women in our sample did not have a living parent at the time of the baseline survey. 16.5 percent were married or cohabitating, and 40.9 percent had given birth. The median number of years of schooling in the sample is 10; 92.4 percent of baseline respondents finished primary school, while only 41.1 percent finished secondary school.[13] 34.5 percent had done some form of vocational training prior to the program.

Only 14.6 percent of the sample was engaged in an income-generating activity (IGA) at the time of the baseline survey, but 54.6 percent had been involved in an IGA at some point in the past. The young women in the sample spent a considerable amount of time doing unpaid work at home: the median number of hours of unpaid housework (in the week prior to the baseline) was 21. Only 8.8 percent of women in the sample had a bank account at baseline, and only a third had any savings in money or jewelry. Among those with savings, the median amount of savings was (equivalent to) 8.91 US dollars.[14]

### 3.4 Compliance with Treatment

As is typical in training programs (McKenzie and Woodruff 2014), not all the women assigned to the program participated in it, and not all those who started the business

[13] The average level of education among women aged 18-20 in Nairobi is 10.6 years; 28 percent are currently married or living with a partner, and 26 percent have had a child (Kenya DHS 2014). Thus, relative to the general population of comparably-age women in Nairobi, our sample is slightly less educated, less likely to be married or cohabitating, and more likely to have had a child. These differences likely reflect the program's focus on Nairobi's poorest neighborhoods.

[14] Balance checks (i.e. tests of the hypothesis that observable characteristics are balanced across treatments) are reported in the Online Appendix. Observable characteristics were relatively balanced prior to the program. Out of 75 hypothesis tests, we find 3 differences across treatments that are significant at greater than 95 percent statistical confidence. Women assigned to the control group come from slightly larger households, and are somewhat more likely to have given birth prior to the program. Women assigned to the cash grant treatment had, on average, about half a year less schooling than those assigned to the franchise treatment and the control group. Controls for those variables that are not balanced across treatments are included in our main specifications (though results are nearly identical when controls are omitted).

training completed the program. Table 2 reports the proportion of women in the treatment and control groups who completed each stage of the program.[15] 61 percent of those assigned to the franchise treatment attended the initial two-week business training course at least once; 39 percent of those assigned to the franchise treatment completed the franchise-specific business training and launched a microfranchise. Though these modest take-up rates are not out of line with those observed in comparable training programs (McKenzie and Woodruff 2014), they have important ramifications for the interpretation of intent-to-treat estimates of program impacts (a point we return to below). Unsurprisingly, the take-up rate is extremely high in the cash grant treatment: 95 percent of those assigned to the grant treatment accepted and received the grant. We also find very little evidence of imperfect compliance with the evaluation design on the part of the implementing organization: no women assigned to the control group attended the business training, and only 1 percent were involved in starting a microfranchise.

## 4 Analysis

Our theoretical model predicts that infusions of funding will increase self-employment and income over the relatively short-term if individuals are unable to save through channels other than enterprise capital. For relatively unproductive individuals, these increases in income are temporary; they disappear as capital depreciates. Thus, impacts on entrepreneurship and income over the short-term do not indicate that capital infusions relieved a credit constraint or helped potential entrepreneurs to escape a poverty trap. In the presence of savings constraints, the key distinction between latent entrepreneurs and less productive individuals is that latent entrepreneurs can transform one-off infusions of capital into permanent increases in income. A comparison of shorter-term versus longer-term impacts indicates whether capital transfers are likely to have alleviated a poverty trap.

The cash grant intervention is exactly the type of unrestricted financial transfer described by our model. If the cash grant impacts occupational choice and income in the relatively short-term, analysis of longer-term impacts allows us to assess the extent to which the capital infusion relieved a poverty trap. Of course, if low productivity individuals are not savings constrained, there is little reason for them to knowingly launch an unproductive enterprise. In that case, an infusion of capital could increase consumption, savings, or assets (though possibly only over the relatively short-term), but would not impact occupational choice.

We model the impact of an infusion of capital, but our analysis compares two distinct

[15] The table is based on administrative data from the implementing NGO and the franchise partners, though self-reports line up with administrative records.

interventions. An important question is whether an equivalently-valued intervention that offers enterprise capital in a more restricted form (including some in the form of human capital) has comparable impacts. Women assigned to the franchise treatment who did not wish to start a business and were not savings-constrained had the option of selling the physical capital that they received through the program, though we would expect the market value of, for example, a mobile food cart to be well below the cost of providing the entire microfranchise package of training and mentoring plus capital. Thus, if low productivity individuals who are not savings constrained participated in the program, we would not expect them to launch businesses, and the impacts on (e.g.) consumption might be relatively small. Alternatively, if credit and savings constraints are the main obstacles to successful entrepreneurship (and business training and mentoring add little value), we might expect the impacts of the franchise treatment to be smaller than the impacts of the grant treatment (because much of the program spending paid for training that, by assumption, would not be the relevant barrier to entrepreneurship for these individuals). On the other hand, the training and mentoring provided through the franchise program might impact participants' productivity, increasing the fraction of high productivity types. If this were the case, we would expect the impacts of the franchise treatment to be more persistent than those of the grant treatment — though they might initially be smaller in magnitude, depending on the initial mix of types in the population and the value of the capital transferred to franchise program participants.

We test these predictions using data from two rounds of surveys: midline surveys that were conducted between 7 and 10 months after the interventions and endline surveys that were conducted 14 to 22 months after the interventions. Both the midline and endline surveys contain detailed data on involvement in income-generating activities. The endline survey also includes a range of measures of consumption, expenditure, and well-being — which might be impacted by treatment if participants saved or consumed the value of the capital they received without launching a small business.

## 4.1 Estimation Strategy

In our main analysis, we report intent-to-treat (ITT) estimates of the impacts of the franchise treatment and the cash grant treatment on women assigned to each treatment group. Treatment assignment was random within strata, so the impacts of the interventions on any outcome $Y_i$ can be estimated via the OLS regression specification:

$$Y_i = \alpha + \beta \cdot Franchise_i + \gamma \cdot Grant_i + \delta_{stratum} + \phi_{enumerator} + \zeta_{month} + \eta \cdot X_i + \varepsilon_i \quad (5)$$

where $Franchise_i$ and $Grant_i$ are indicators for, respectively, random assignment to the franchise treatment or the grant treatment, $\delta_{stratum}$ is a randomization stratum fixed effect, $\phi_{enumerator}$ is a survey enumerator fixed effect, $\zeta_{month}$ is a fixed effect for the month the survey was administered, $X_i$ is a vector of individual controls, and $\varepsilon_i$ is a conditionally-mean-zero error term.[16,17]

We also report treatment-on-the-treated (TOT) estimates that instrument for take-up (specifically, indicators for starting the business training portion of the franchise program and receiving the cash grant). Since take-up is almost universal among those assigned to the grant treatment, ITT and TOT estimates are nearly identical. However, the TOT estimates give us a better sense of how the franchise program impacted those who chose to participate (subject, of course, to additional assumptions).

## 4.2 Labor Market Outcomes 7–10 Months after Treatment

We summarize the (relatively) short-term impacts of the franchise and grant interventions on labor market outcomes in Table 3. Both the franchise treatment and the grant treatment had a positive and significant effect on the likelihood of self-employment, though they did not increase the likelihood of involvement in any income-generating activity. Women assigned to both treatments used the capital that they received to launch businesses. Point estimates suggest an extremely large effect: 24.5 percent of women assigned to the control group were self-employed at midline; the franchise and grant treatments both increased the likelihood of self-employment by approximately 10 percentage points. Coefficient estimates suggest that both interventions also reduced the likelihood of paid work for others, though the coefficients are not statistically significant at conventional levels.[18] As expected, the franchise treatment increased the likelihood of operating a microfranchise, while the grant treatment did not (Table 3, Panel B).

Though the grant and franchise treatments had similar impacts on the likelihood of

[16]In our main specifications, we include controls for baseline household size, education level, and indicators for having given birth, having received any vocational training, or having any paid work experience prior to the baseline survey. Results are similar in magnitude and significance when these controls are omitted.

[17]We do not correct for the false discovery rate in our analysis of medium-term labor market outcomes: we consider a relatively small set of outcomes (because the midline survey did not collect data on a broader range of outcomes), none of which can be treated as statistically independent. As will become apparent in the subsequent discussion, most of these outcomes are impacted by the treatments over the medium-term; so the overall pattern of findings is unlikely to be explained by multiple testing. In our analysis of longer-term impacts, we look at a broad range of outcomes; as almost none are impacted by either treatment, there is little need to correct for the false discovery rate. However, we implement the multiple test correction procedure proposed by Benjamini and Hochberg (1995), following the procedures suggested by Anderson (2008). Results are discussed below.

[18]The coefficient estimate on the franchise treatment suggests a marginally significant negative impact on the likelihood of paid work (p-value 0.061). The coefficient on the grant treatment is not even marginally significant (p-value 0.116).

self-employment and paid work, they had distinctly different impacts on labor supply (as shown in Table 3, Panel C). The grant treatment had a large positive impact on hours worked (over the week prior to the survey). The coefficient estimate indicates that women assigned to the grant treatment worked 6.8 more hours (p-value 0.019), which represents a 38 percent increase in hours worked. In contrast, the franchise treatment did not have a significant impact on the total number of hours worked (p-value 0.607), and we can reject the hypothesis that the two treatments had comparable impacts on hours worked (p-value 0.046). As expected, both treatments increased self-employment hours substantially; these increases are partially offset by modest (and insignificant) declines in the number of hours of paid work for others. The increases in self-employment hours are both large in magnitude and statistically significant. Assignment to the franchise treatment is associated with 4.1 additional self-employment hours per week (p-value 0.002), which represents an 87 percent increase in self-employment hours. Assignment to the grant treatment is associated with 7.6 additional hours of work in self-employment per week (p-value $< 0.001$), or a 162 percent increase in self-employment hours. Thus, both treatments are associated with substantial increases in both the likelihood of self-employment and the number of hours devoted to entrepreneurial activities.

Panel D of Table 3 summarizes the impacts of the treatment on income. Neither treatment impacts the overall likelihood of reporting an income, but both the franchise treatment and the grant treatment had positive and significant impacts on income. The franchise treatment increased weekly income by 1.6 dollars (p-value 0.035); this represents about a 30 percent increase over the mean income in the control group of 5.5 dollars per week. The grant treatment increased income by 3.2 dollars a week (p-value 0.008), or 56 percent relative to the control group mean. Though the coefficient on the grant treatment is larger in magnitude than the coefficient on the franchise treatment, we cannot reject the hypothesis that the two treatments had statistically indistinguishable impacts on income (p-value 0.208). Results are similar if we focus on log transformations of income. As expected, the impacts on income are driven by extremely large (and statistically significant) increases in self-employment income that are not offset by any statistically significant changes in income from paid work. Thus, our results provide clear evidence that both the franchise treatment and the grant treatment encouraged young women to become self-employed; this shift into self employment was associated with large increases in income over the year after the interventions.

In the Online Appendix, we report instrumental variables estimates of the impact of the franchise and grant treatments on compliers (i.e. treatment-on-the-treated estimates). As expected, ITT and TOT estimates are nearly identical for the grant treatment, since 95 percent of those assigned to treatment received the grant. We can never reject the hypothesis that the TOT impacts of the franchise and grant treatments are identical. Thus, the evidence does not support the hypothesis that the franchise treatment had larger impacts on compliers than the grant treatment. The one important difference between our ITT and our TOT results is that we can no longer reject the hypothesis that the two treatments had different impacts on hours worked (p-value 0.140), though the point estimate suggests a much larger TOT effect for the grant treatment (7.1 additional hours versus 1.9 additional hours). Both the ITT and TOT effects of the treatments on income and occupational choice are statistically indistinguishable.

### 4.3 Labor Market Outcomes 14–22 Months after Treatment

In Table 4, we examine labor market outcomes 14 to 22 months after treatment. Looking across the range of outcomes related to occupational choice (Panels A and B), hours worked (Panel C), and income (Panel D), a clear pattern emerges: the impacts on hours and income that we observed at midline disappeared completely by the time of the endline survey. Looking at income, we see that neither treatment is associated with a significant increase in income at endline, and the point estimates for both treatments are negative. Moreover, at least for the grant treatment, the lack of significance is not simply the result of noise: in the Online Appendix, we report specifications that pool data from the midline and endline surveys; we are able to reject the hypothesis that the impacts of the grant treatment on income are identical across the two survey rounds (p-value 0.046). There is also no evidence that either treatment had a significant impact on hours worked (in the last week) 14 to 22 months after treatment. The coefficients on both the franchise treatment and the grant treatment are small and not statistically significant.

Looking across the range of labor market outcomes, the clear pattern that emerges is that, by the time of the endline survey, impacts on hours and income had disappeared; however, impacts on occupational choice persisted. Both the franchise and the grant treatments increased the likelihood of self-employment at endline. The franchise treatment caused an 11.8 percentage point increase in the likelihood of self-employment (p-value 0.001) while the grant treatment led to a 12.9 percentage point increase in the likelihood of self-employment (p-value 0.003). Both effects are large in magnitude relative to the rate of self-employment in the comparison group, which is 24.3 percent. Both the franchise treatment and the grant treatment are also associated with large increases in self-employment hours and, to some extent, increases in income from self-employment (we observe significant impacts on log self-employment income, but not on the level of self-employment income).

Thus, the overall picture at endline is that the impacts of both the franchise treatment and the grant treatment are confined to the domain of occupational choice. Both treatments shift young women into self-employment, but have no overall impact on income or labor supply.[19] In the Online Appendix, we show that the franchise treatment increased the likelihood of working in the salon or beauty sector at endline; otherwise, neither the franchise treatment nor the grant treatment had a significant impact on occupational sector at endline.[20] We also find no evidence of impacts on labor market churning: women assigned to treatment are not more likely to have either started or closed a business between midline and endline, nor are they more likely to have left a job or started a new job.

### 4.4 Impacts on Other Outcomes

Though the impacts of the labor market interventions we evaluate dissipated over time, an important question is whether the treatments might have had longer-term impacts on other outcomes. As discussed above, women who are not savings constrained and are not productive entrepreneurs might save the funds that they received through the cash grant intervention; thus, the grants might increase consumption or expenditure without impacting income (except at the moment that the grant is disbursed) or occupational status. Alternatively, women might use grant money or resulting temporary increases in income to purchase durable assets that would improve their living conditions or quality of life over the relatively long-term. A third possibility is that the experience of receiving training and/or launching a business impacted self-confidence or empowerment. In any of these cases, we might expect the labor market interventions to have persistent impacts on overall welfare, even if labor market impacts are temporary.

In the Online Appendix, we estimate the impacts of the franchise and grant treatments on a range of outcomes: household assets, food security, expenditures, living arrangements and conditions, savings, time use, self-esteem, and empowerment. We find almost no evidence that the treatments had long-run impacts on any of these outcomes.[21] There is no evidence that the treatments improved women's living conditions or food security or increased their expenditures, nor is their any evidence of improvements in self-esteem or

[19] One somewhat anomalous finding is that assignment to the franchise treatment is associated with a significant increase in the likelihood of reporting any income-generating activity. Though the increase is relatively large in magnitude (the coefficient estimate suggests a 7.6 percentage point increase in the likelihood of involvement in any IGA), it is difficult to interpret since the franchise treatment does not lead to increases in the total number of hours worked or the likelihood of reporting any income over the seven days prior to the survey.

[20] The impact of the franchise treatment on the probability of working in the salon or beauty sector is robust to the multiple hypothesis testing procedure proposed by Benjamini and Hochberg (1995) (corrected q-value 0.010).

[21] In the Online Appendix, we report the estimated impacts of the franchise and grant treatments on 81 different outcomes. The estimated impacts of the franchise treatment on the likelihood of working in the salon sector or having done any vocational training are significant at the 99 percent level after implementing the multiple hypothesis testing correction proposed by Benjamini and Hochberg (1995). Those assigned to the grant treatment are also more likely to have paid school fees for someone else's child in the year after receiving the grant (Benjamini-Hochberg q-value 0.01). No other outcomes are significantly related to either treatment with adjusted q-values below 0.05.

empowerment.[22] Thus, the evidence does not provide any meaningful support for the hypothesis that the interventions had temporary impacts on income but impacted overall welfare in a more permanent manner.

### 4.5 Comparing Implementation Costs

The two treatment arms of our study allow for natural cost comparisons, complementing our overall estimates of each program's impacts. Costs in the cash grant arm are relatively straightforward. The cash grant itself was worth 239 US dollars. Because compliance was slightly below 100 percent, the average disbursement per respondent in the cash grant arm was 228 dollars. Besides simply transferring the money, administrative tasks supporting this arm included having field team members meet participants twice (once to explain the no-strings-attached grant, once for the actual transfer); confirming, via fingerprint reader, that the individuals our team met with were indeed the intended recipients; and data, accounting, and other indirect costs. These administrative tasks cost a total of roughly 82 dollars per intended recipient. Thus, the total cost of the cash grant arm, per intended recipient, was roughly 310 dollars.

Costs in the microfranchising intervention are more complicated. We begin with all costs that the IRC incurred implementing the program over three fiscal years. This study evaluates only the final calendar year of the program, but other participants were involved in the prior calendar year, and setup costs were required beforehand to make the program possible. Once we arrive at a total cost figure (the numerator), we divide by the total number of participants across all program years (the denominator). We face a number of decisions in both arriving at a total cost figure and in arriving at the number of participants, so we report upper and lower bounds on our cost estimates.[23]

One of the smallest cost items in the IRC budget is international staff support costs. We exclude this for simplicity. A larger cost is internationally hired staff in Kenya, including portions of the country director's time. Our upper bound includes these costs; our lower bound excludes them on the basis that they are needed most intensely for the startup phase of a project. The rest of the costs (national staff time, business support, trainings, office expenses, etc.) are concentrated in the two fiscal years in which the program trained most participants, but there are some costs from the first fiscal year in which the program began and in which the first participants started training. Our upper bound includes these costs;

[22] We use a range of measures including the Rosenberg self-esteem, the Ladder of Life, and Grit scales, plus the entire range of empowerment measures used by Bandiera *et al.* (2014) and Adoho *et al.* (2014).

[23] In order to determine cost per activity, each project expense was allocated, completely or partially, to either entrepreneurship activities, cash dispersements, or other non-treatment activities, and summed to determine total cost per activity. Total values were then divided by number of clients served to get an average cost per client. See International Rescue Committee (2016a) for a detailed discussion of the costing methodology.

our lower bound includes only half of the first fiscal year's costs, on the basis that continued program operation or operation at larger scale would involve lower startup costs. The upper bound figure for the total cost of the program is roughly 763,000 dollars; the lower bound is 637,000 dollars. Either way, half of the costs come from providing trainings, including the (substantial) costs of providing refreshments for hundreds of participants each day.

These total cost estimates translate into a cost of between 616 dollars and 809 dollars per participant in the microfranchising arm.[24] However, this figure is the cost associated with the treatment on the treated — not the cost for the intention to treat. This distinction matters because while 95 percent of those assigned to the grant treatment received a grant, only 61 percent of those assigned to the microfranchising treatment actually started the training. The intervention costs per individual *assigned* to the relevant treatment are thus roughly 286 dollars for the grant arm, and between 376 dollars and 494 dollars for the microfranchising arm.

The point estimates in Tables 3 and 4 for impacts of the cash grant are generally larger than (though not statistically distinguishable from) the point estimates for the microfranchising intervention; this suggests that they are comparable in effectiveness, though the point estimates suggest that the cash grant is slightly more effective. The somewhat higher costs of the microfranchising treatment do not substantially change this picture, though they tilt it further in favor of the cash grant: point estimates for the cash grant suggest it is more cost-effective than microfranchising across a range of outcomes and follow-up durations. The difference is statistically significant at the 10 percent level for 7–10 month effects on income, but otherwise is generally not statistically significant.

A full cost-benefit analysis involves measuring the extent of the benefits that accrued to participants over time. We only measure the benefits at two points in time: 7–10 months after treatment, and 14–22 months after treatment. The effects we find are statistically significant at the first of these follow-ups, but not at the second. We arrive at a lower bound on the benefits by multiplying the shorter-term impacts on income by the period between the start of the program and the survey, assuming that the impacts disappeared immediately after the 7–10 month follow-up; this is, in essence, the area of a rectangle 7–10 months wide and as tall as the impact estimate. A reasonable upper bound extends

[24] The number of participants in the microfranchising program was carefully recorded by the local partner organizations that helped run the training sessions. Over the duration of the program, there were 898 participants in these sessions: 297 in the first program year, and 601 in the second. Women launching businesses were encouraged to involve others in their enterprises, but in the first year, records only indicated 45 additional participants of this type. This leads to the lower bound figure of $898 + 45 = 943$ participants. We were unable to obtain detailed records of any others involved in new enterprises in the second year, but we can extrapolate that it is proportional to the number of participants, so roughly twice the number in the second year as in the first. This leads us to an upper bound estimate of $898 + 45 + 91 = 1034$ participants overall.

these impacts (the width of the rectangle) until just before the 14–22 month follow-up.[25] Using these approaches, and the coefficients on income in Table 3, the microfranchising intervention had total income benefits of between 60 dollars and 116 dollars; the cash grant had total income benefits of between 128 dollars and 247 dollars.

Neither intervention shows signs of the benefits exceeding the costs. However, the amount of the grant (239 dollars) falls between the upper and lower bounds of the estimated impacts on income over the year after the intervention. This suggests that grant recipients do a relatively efficient job of smoothing their income by investing grants in enterprise capital. If such one-off grants could be distributed with minimal overhead costs (as in larger programs like GiveDirectly), or the distributional benefits of making transfers to vulnerable populations justified a modest level of transaction costs, cash transfers could be socially desirable. The franchise treatment that we study achieves lower (temporary) income gains at higher cost; it is therefore reasonable to conclude that cash grants are a more efficient approach to achieving the same level of redistribution.

# 5 Participant Evaluations

Given the tremendous lengths one must go to in order to produce credible estimates of a program's impacts, an important question is whether participants themselves understand the effects of the programs in which they participate. It is not uncommon for labor market programs to survey participants *ex post*; however, Smith, Whalley, and Wilcox (2012) find that such *ex post* assessments of a program's impact are not highly correlated with objective measures of program effects. Understanding participants' beliefs about program impacts is important for two reasons. Most obviously, if — through their participation — participants obtain reasonable estimates of program impacts, this information may be a feasible, low-cost alternative to formal impact evaluation. On the other hand, if program participants do not understand a program's impacts, even after they have participated in the program, it is hard to imagine that they are making optimal decisions about whether or not to participate.

## 5.1 Empirical Approach and Practical Considerations

As Smith, Whalley, and Wilcox (2012) point out, one reason participant evaluations of programs may differ from rigorous estimates of program impacts is that participant evalu-

[25] A nearly-equivalent approach to the upper bound calculation assumes a downward ramp shape: large impacts at first, tapering linearly to zero at the 14–22 month follow-up, and with a height that is only measured at the 7–10 month follow-up. The area of the resulting triangle is just slightly larger than that of the upper bound rectangle, since the follow-up when the "height" is measured is just under halfway along the "base" of the triangle. This approach generates similar estimates of the total program impacts on income.

ation questions are often quite open-ended. For example, participants in the National Job Training Partnership Act program were asked "Do you think that the training or other assistance that you got from the program helped you get a job or perform better on the job?" (Smith, Whalley, and Wilcox 2011, p. 9). This question is obviously problematic because it is not at all clear whether better on-the-job performance should be linked to any measurable outcome (e.g. income); moreover, the link between the fraction of participants who believe that the program had a positive impact and the estimated treatment effect of the program is unclear, making it difficult to test whether participants' subjective evaluations are accurate. Smith, Whalley, and Wilcox (2012) suggest replacing such subjective evaluation questions with alternatives that (i) clearly specify the outcomes and time periods of interest, (ii) ask for continuous (as opposed to binary) responses that can be directly compared to ITT estimates, and (iii) make the counterfactual nature of the question transparent.

We follow the recommendations of Smith, Whalley, and Wilcox (2012) and ask participants in the franchise and grant treatments to estimate the counterfactual probabilities of self-employment and paid work for a reference group of women similar to themselves. Specifically, we ask women in each of the two treatment arms the question: "I would like you to imagine 100 women from [your neighborhood] who applied to the [name of treatment arm] program but who were not admitted into it. In other words, please think about 100 women similar to yourself who were not selected to the [name of treatment arm] program. Out of 100 women, how many do you think are currently running or operating their own business?" We also ask an analogous question about involvement in paid work for others. Smith, Whalley, and Wilcox (2012) suggest using this question to construct a perceived counterfactual, which can then be compared with the average outcome in the treatment group. We take a different approach, asking each participant to estimate how many of 100 women similar to themselves who "applied for and were admitted into" the program were (at the time of the survey) operating their own business (and, in a subsequent question, we ask how many were doing paid work for others). We calculate each participant's belief about the treatment effect of the program (on, for example, self-employment) by taking the difference between the perceived frequency of self-employment among women invited to participate in the program and the perceived frequency of self-employment among similar women who were not invited to participate.

We also test a second method proposed by Smith, Whalley, and Wilcox (2012): asking participants about the probability that they would be self-employed (or doing paid work for others) in the absence of the program. These individual-level beliefs about one's own counterfactual can then be combined with data on actual outcomes to construct estimates of perceived treatment effects. However, as Smith, Whalley, and Wilcox (2012) emphasize, there are several drawbacks to this approach. First, program participants may find it inherently difficult to imagine what their lives would have been like in the absence of the program. For example, psychological studies of "hindsight bias" suggest that people have a difficult time remembering the beliefs they held in the past and tend to assume that realized outcomes were always foreseeable (Fischhoff 1975, Madarász 2012). In our context, we might expect that those who have received vocational training and gained self-employment experience might have a difficult time remembering that they had not always known how to operate a business; thus, hindsight bias might inflate participants' estimates of their own counterfactual, particularly among successful microentrepreneurs. Estimates of one's own counterfactual may also be biased by the tendency to attribute one's own success to individual agency as opposed to external factors (Miller and Ross 1975). This would lead those who have benefited from business or vocational training to overstate the likelihood that they would have started a successful business in the absence of the program.

In the context of our evaluation, a third problem with questions designed to elicit beliefs about one's own counterfactual probability of self-employment (or paid work) is that they are unlikely to work well when respondents have low levels of numeracy. Though almost 92 percent of the women in our sample completed primary school, a relatively large number are not familiar with the concept of percentages. Roughly one in four cannot (correctly) answer the question: "If there is a 75 percent chance of rain and a 25 percent chance of sun, which type of weather is more likely?" While it is possible to elicit probabilistic expectations from subjects with no prior knowledge of probability, it is costly and time-consuming to do so. Instead, we asked every subject categorical questions about their counterfactual probabilities of self-employment and paid work, and collected more specific data on counterfactual probabilities from those who successfully answered the screening question described above.[26]

### 5.2 Framework for Interpreting Empirics

To facilitate comparisons between different approaches to belief elicitation, we introduce a simple conceptual framework that formalizes the measurement issues highlighted above. First, consider an outcome, $y$, and a program whose causal effect on that outcome is to increase its expected value by $\beta > 0$. Let $\gamma$ denote the expected value of $y$ in the absence of the program: $E[y_j|T_j = 0] = \gamma$.

[26] We worded the categorical question to make responses directly comparable to probability estimates. Respondents chose one of the following options: (1) *In the absence of the program, I would definitely be self-employed*, (2) *In the absence of the program, I would probably be self-employed but it is not certain*, (3) *In the absence of the program, the chances of me being self-employed or not self-employed are equal*, (4) *In the absence of the program, I would probably not be self-employed but it is not certain*, or (5) *In the absence of the program, I would definitely not be self-employed.*

We wish to know whether program participants hold accurate beliefs about $\beta$. Let

$$\tilde{\beta}_i = \tilde{\beta} + \phi_i \tag{6}$$

denote participant $i$'s belief about the impact of the program, and let

$$\tilde{E}[y_j|T_j = 0] = \tilde{\gamma} + \nu_i \tag{7}$$

be participant $i$'s belief about the expected value of the outcome of interest for an untreated individual $j$ who is outwardly similar to her. $\tilde{\beta}$ is the average belief about the impact of the program, and $\tilde{\gamma}$ is the average belief about the outcome of interest in the eligible population in the absence of the program. $\phi_i$ is the idiosyncratic component of beliefs about the impact of the program; without loss of generality, we assume that the distribution of $\phi_i$ is mean zero, and we let $\sigma_\phi$ denote its variance. $\nu_i$ can be decomposed into a mean-zero error term and a term which reflects the perceived difference between the population average of $y$ and one's own counterfactual:

$$\nu_i = \tilde{\alpha}_i \cdot \mathbb{1}(j = i) + \epsilon_i. \tag{8}$$

As discussed above, asking participants about their own counterfactuals may be problematic (for example, because of hindsight bias), and the population mean of these $\tilde{\alpha}_i$ values, $\tilde{\alpha} = E[\tilde{\alpha}_i]$ may not be equal to 0.[27] Combining and generalizing these expressions, respondents report:

$$\tilde{E}[y_j|T_j] = \tilde{\beta} \cdot T_j + \tilde{\gamma} + \tilde{\alpha}_i \cdot \mathbb{1}(j = i) + \phi_i \cdot T_j + \epsilon_i \tag{9}$$

Specifically, when asked to report the rate of self-employment among 100 potential program participants who were not invited to participate in the program, a respondent in our study reports:

$$\tilde{E}[y_j|T_j = 0] = \tilde{\gamma} + \epsilon_i. \tag{10}$$

When asked to report the rate of self-employment among 100 potential program participants who were invited to participate in the program, she reports:

$$\tilde{E}[y_j|T_j = 1] = \tilde{\beta} + \tilde{\gamma} + \phi_i + \epsilon_i. \tag{11}$$

Finally, when asked to report her own counterfactual probability of self-employment, a participant reports:

$$\tilde{E}[y_i|T_i = 0] = \tilde{\gamma} + \tilde{\alpha}_i + \epsilon_i. \tag{12}$$

The framework presented above helps to clarify the distinctions between the different

[27] This may be thought of as a "Lake Wobegon" effect.

approaches to estimating participant beliefs. First, consider an estimate of participant beliefs constructed by taking the average belief about one's own counterfactual (in our context, the counterfactual probability of self-employment) and subtracting this from the observed outcome in the treatment group. The expected value of this estimator is:

$$\begin{aligned} E[y_j|T_j = 1] - E[\tilde{E}[y_i|T_i = 0]] &= \beta + \gamma - (\tilde{\gamma} + \tilde{\alpha} + E[\epsilon_i]) \\ &= \beta + (\gamma - \tilde{\gamma}) - \tilde{\alpha} \end{aligned} \tag{13}$$

since $E[\epsilon_i] = 0$. Thus, this estimator will be biased if participants hold inaccurate beliefs about the counterfactual probability of self-employment, and it will be biased when psychological factors such as hindsight bias lead participants to overstate their own counterfactual probability of self-employment. The second estimator proposed by Smith, Whalley, and Wilcox (2012) is constructed by subtracting the mean rate of self-employment in a reference group of untreated women from the observed rate of self-employment in the treatment group. The expected value of this estimator is given by:

$$\begin{aligned} E[y_j|T_j = 1] - E[\tilde{E}[y_j|T_j = 0]] &= \beta + \gamma - (\tilde{\gamma} + E[\epsilon_i]) \\ &= \beta + (\gamma - \tilde{\gamma}) \end{aligned} \tag{14}$$

This estimator overcomes the behavioral issues inherent in estimating one's own counterfactual. However, when estimates of participant beliefs constructed in this manner diverge from actual program impacts, it is impossible to determine whether participants hold inaccurate beliefs about the impact of the program or inaccurate beliefs about the counterfactual.

The outcomes of interest in impact evaluations are often difficult to measure, and considerable effort goes into the design and pre-testing of questionnaires. Nonetheless, there is no guarantee that outcome measures derived from survey questions (for example, about labor market participation) and participant responses to belief-elicitation questions will line up, particularly in low-income settings where formal, full-time employment is relatively uncommon (and there is continuous variation in the number of hours worked, and labor supply varies substantially from week to week).[28] Impact evaluation questions designed to measure beliefs about the counterfactual may reveal systematic deviations between participants' beliefs about outcome levels and actual outcome levels; however, such measurement

[28]Smith, Whalley, and Wilcox (2012) are aware of this issue and recommend asking extremely specific questions: for example, what fraction of participants meet a well-specified criterion for employment — for example, working more than 35 hours per week — which can then be used to construct the empirical estimate of the programs impact. However, such precisely worded questions are not always feasible. In our context, we worried that any question of the form "Out of 100 women, how many spend at least $X$ hours operating their own business?" would be substantially more difficult to answer than a less specific question because few people work full-time and there is no obvious break in the distribution of hours worked at any point.

error is only problematic if it cannot be separated from the quantity of interest. To address this issue, we propose an estimate of participant beliefs that is calculated by taking the difference between beliefs about the mean outcome of interest in a reference population of treatment versus control individuals:

$$
\begin{aligned}
E[\tilde{E}[y_j|T_j = 0]] - E[\tilde{E}[y_j|T_j = 0]] \\
&= \tilde{\beta} + \tilde{\gamma} + E[\phi_i] + E[\epsilon_i] - (\tilde{\gamma} + E[\epsilon_i]) \\
&= \tilde{\beta}
\end{aligned}
\tag{15}
$$

Such an estimator allows for a direct test of the hypothesis that participants hold accurate beliefs about program impacts; moreover, collection of the relevant data necessarily also allows researchers to assess the related issue of whether participants can estimate the counterfactual — allowing for a comparison of the different approaches of belief estimation.

### 5.3 Results

Our results, which are summarized in Figure 3, suggest that participants hold remarkably accurate beliefs about program impacts. The figure compares ITT estimates of program impacts to estimates of participant beliefs about program impacts calculated by taking the difference in reference group probabilities for the treatment and control groups.[29] For example, the ITT estimates suggest that the franchise treatment increased the likelihood of self-employment by 11.9 percentage points; those assigned to the program believe that it increased the likelihood of self-employment by 12.3 percentage points. Similarly, those assigned to the cash grant treatment believe that it increased the likelihood of self-employment by 10.6 percentage points; the ITT estimates suggest a 12.9 percentage point increase. Those assigned to the franchise treatment also have remarkably accurate beliefs about the program's impact on the likelihood of paid employment. Those assigned to the cash grant treatment have less accurate beliefs about the program's impact on paid employment, though they are appropriately signed and well within the confidence interval of the estimated treatment effect. Thus, our results suggest that participants' do a reasonably good job of estimating the impact of programs that they have participated in. For the outcome most directly impacted by the treatments (self-employment), participants do a remarkably good job of estimating the program's impacts.

Figure 4 compares beliefs about the probability of self-employment and paid work to levels observed in the treatment and control groups, and compares beliefs about one's own

[29]In other words, beliefs were estimated by asking women assigned to each treatment group to estimate reference group probabilities (frequencies) for both the treatment and comparison groups. Women assigned to the control group were not asked to estimate a reference group probability for those assigned to the treatment groups since they were not familiar with the details of each treatment.

counterfactual to beliefs about a reference population of untreated women. Several patterns are apparent. First, women in the franchise treatment group underestimate the probability of paid work in both the treatment and the control group. Consequently, an estimate of the impact of the franchise program on the probability of paid work that compared counterfactual beliefs to observed levels in the treatment group would perform very poorly. Women in both the franchise and grant treatments hold more accurate beliefs about the level of self-employment (in both the treatment and control groups); however, women in both treatment arms seem to overestimate the frequency of self-employment and underestimate the frequency of paid work in both the treatment and the control groups. Thus, differences between observed outcome levels and participant beliefs appear to be systematic, suggesting that it will typically be better to estimate program beliefs by comparing beliefs about the control group to beliefs about the treatment group (rather than the observed outcome levels in the treatment group).

The figure also demonstrates that concerns that estimates of one's own counterfactual might be biased appear well-founded: the average of own counterfactual estimates is consistently higher than the estimated outcome for a reference population of untreated women. This pattern is particularly pronounced for the franchise treatment, most dramatically when participants are asked to report their own counterfactual probability of self-employment. Though participants hold accurate beliefs about the level of self-employment in both the treatment and control groups, own counterfactual estimates are so inflated that they suggest a negative impact of the program on self-employment. Thus, our evidence clearly supports the view that own counterfactual estimates are of little use in estimating treatment effects. This finding is consistent with recent work by McKenzie (2016a); he finds that program participants (business owners) do a very poor job of estimating the counterfactual. Our results support his conclusion, but suggest that an alternative approach to eliciting participants' beliefs performs substantially better.

# 6 Conclusion

We report the results of an impact evaluation comparing two labor market interventions that were offered to young, unemployed women in some of Nairobi's poorest neighborhoods. The multifaceted franchise program we evaluate provided participants with business and life skills training, vocational training, business-specific capital and supply chain linkages, and ongoing mentoring. This program was meant to simultaneously address both credit constraints and other obstacles to youth entrepreneurship. The cash grant program was a simple intervention that provided participants with an unrestricted grant of 20,000 Kenyan shillings (equivalent to 239 US dollars in 2013). Both treatments were randomly assigned (offered) to eligible applicants to the franchise program; our randomized design allows us to compare the two programs, and to compare both programs to a control group.

We find that both programs increased the likelihood of self-employment among eligible participants. In addition, both the franchise treatment and the grant treatment had large and statistically significant impacts on income in the year after the program. However, the impacts on income did not persist. By the second year after treatment, women assigned to both the franchise and grant treatments looked similar to the control group in terms of income, labor supply, food security, expenditures, living conditions, and empowerment.

Seen through the lens of a simple theoretical model, our findings suggest that individuals in our sample are savings-constrained; they launch unsustainable businesses to stretch out the capital infusions provided by the interventions. Our findings suggest that the training component of the franchise intervention did not increase individual productivity sufficiently to create enduring, profitable entrepreneurship. Our findings are also not consistent with the existence of a credit-constraint-based poverty trap. Of course, our results should not be taken as evidence that credit constraints *never* generate poverty traps. Recent studies by Blattman, Fiala, and Martinez (2014) and Blattman *et al.* (2016) suggest that credit constraints may well be preventing latent entrepreneurs from launching successful businesses in recently conflict-affected regions of northern Uganda. However, our findings resonate with a number of recent studies of cash grants and other credit market interventions. Studies of the return to capital among microenterprises operated by women in developing countries have consistently failed to find positive impacts on business profits, though cash grants do help *men* expand their businesses in some contexts (cf. De Mel, McKenzie, and Woodruff 2008, De Mel, McKenzie, and Woodruff 2009, Fafchamps, McKenzie, Quinn, and Woodruff 2011, Fiala 2014, Karlan, Knight, and Udry 2015).[30] Recent randomized evaluations of microfinance also suggest that access to credit has, at best, a limited impact on enterprise profits (cf. Angelucci, Karlan, and Zinman 2015, Attanasio *et al.* 2015, Augsburg, De Haas, Harmgart, and Meghir 2015, Banerjee, Duflo, Glennerster, and Kinnan 2015, Crépon, Devoto, Duflo, and Parienté 2015, Tarozzi, Desai, and Johnson 2015). Our findings also coincide with the estimated (short-term) impact of the cash grant program offered by the NGO GiveDirectly: Haushofer and Shapiro (2016) find that grants led to increased revenues from farm and non-farm enterprises, but not increased profits (see Haushofer and Shapiro 2016, Online Appendix Table 77). Taken together, these studies suggest that credit constraints are not the main obstacle preventing the poor — particularly poor women — from launching and expanding profitable, sustainable businesses.

[30] Interestingly, recent work by Bernhardt, Field, Pande, and Rigol (2017) suggests that cash grants to *women* might increase the enterprise profits of *men* who are married to grant recipients. This result is unlikely to be relevant to our study since only 16.5 percent of the women in our sample were married when grants were disbursed, and only 28.4 percent were married by the time of the endline survey.

Yet, even when they don't lead to permanent increases in income, cash grants may have important impacts. Haushofer and Shapiro (2016) find that cash transfers improved psychological wellbeing. Our results show that grants lead to economically large and statistically significant impacts on income for almost a year after treatment; it is reasonable to conclude that these increases in income were also associated with improved wellbeing within that time frame. Moreover, as in other studies of cash transfers, we see no sign of excessive spending on temptation goods (Evans and Popova 2016). Also as in other studies of cash transfers, we see that if anything, cash grants temporarily induced an increase in labor force participation, with no evidence of a decrease in either the short or long term (Banerjee, Hanna, Kreindler, and Olken 2015). Thus, our results are consistent with the view that one-off cash transfers are a simple, direct way of improving the wellbeing of the poor and vulnerable. Because grants were used to launch small-scale businesses, impacts persisted for some time, though they were not permanent.

Point estimates suggest that the cash grant was more cost effective than the franchise treatment. Other populations or subgroups could, of course, experience different benefits. Within our sample, the impacts of the franchise treatment were probably greatest among the 39 percent who actually launched businesses, relative to the 22 percent who only did some of the training but never launched businesses or the remainder of those assigned to the franchise treatment, who chose not to participate in the program. Better targeting could potentially improve impacts.[31] However, our protocol did include a reasonably high degree of screening based on non-monetary effort costs (Dupas, Hoffmann, Kremer, and Zwane 2016): everyone in our sample first filled out an application form and then visited the implementing organization's office to complete a baseline survey. Moreover, a lengthier application process would also come with its own implementation costs. Thus, given the observed pattern of impacts, the cash grant intervention appears both simpler and more cost-effective.

Our results emphasize the importance of examining relatively long-run outcomes and collecting multiple rounds of post-treatment data whenever possible. We show that while participants in our study may face credit constraints, these constraints are not acting as a poverty trap; savings constraints provide a better explanation for the patterns of outcomes that we observe. Though transforming unemployed young women into profitable entrepreneurs is a laudable policy goal, our results suggest that it may be difficult to achieve in urban contexts, where markets are active and potentially quite competitive. However, one-off cash transfers can work as a relatively cost-effective means of income support for

[31] Several recent studies find positive impacts of cash grants on potential entrepreneurs who were required to submit detailed business plans (cf. Blattman, Fiala, and Martinez 2014, McKenzie 2016b). However, the interventions we study were intended to assist poor young women with very limited work experience, many of whom might not have been able to produce detailed business plans prior to the program.

vulnerable young women; helping these vulnerable individuals may be a sufficient policy goal in and of itself.

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Table 1: Sample Characteristics at Baseline

| | **Obs.** | **Mean** | **S.D.** | **Median** | **Min.** | **Max.** |
|---|---|---|---|---|---|---|
| *Panel A. Demographics and Household Composition* | | | | | | |
| Age | 905 | 18.780 | 0.787 | 19 | 17 | 20 |
| At least one parent alive | 903 | 0.884 | 0.321 | 1 | 0 | 1 |
| Household size | 905 | 4.882 | 2.168 | 5 | 1 | 13 |
| Married or cohabitating | 905 | 0.165 | 0.371 | 0 | 0 | 1 |
| Has given birth | 905 | 0.409 | 0.492 | 0 | 0 | 1 |
| *Panel B. Educational Background* | | | | | | |
| Father's education, if known | 554 | 9.773 | 2.990 | 11 | 0 | 16 |
| Mother's education, if known | 714 | 9.036 | 2.868 | 8 | 0 | 16 |
| Years of education | 905 | 9.894 | 2.055 | 10 | 0 | 12 |
| Any vocational training | 905 | 0.345 | 0.476 | 0 | 0 | 1 |
| *Panel C. Involvement in Income-Generating Activities* | | | | | | |
| Any (paid) work experience | 905 | 0.546 | 0.498 | 1 | 0 | 1 |
| Engaged in any income-generating activities | 905 | 0.146 | 0.353 | 0 | 0 | 1 |
| Any self-employment activity | 905 | 0.050 | 0.217 | 0 | 0 | 1 |
| Any paid work for someone else | 905 | 0.099 | 0.303 | 0 | 0 | 2 |
| Hours of housework in last week | 884 | 26.072 | 15.295 | 21 | 4 | 84 |
| *Panel D. Assets, Saving, and Living Conditions* | | | | | | |
| Food insecurity index | 904 | 0.259 | 0.175 | 0.250 | 0 | 0.929 |
| Has a personal bank account | 901 | 0.088 | 0.283 | 0 | 0 | 1 |
| Has any savings (including jewelry) | 904 | 0.330 | 0.470 | 0 | 0 | 1 |
| Value of savings (in USD) | 905 | 4.938 | 14.774 | 0 | 0 | 104.886 |
| Value of savings, if any (in USD) | 248 | 18.022 | 23.709 | 8.911 | 0.593 | 104.886 |
| Owns a personal mobile phone | 905 | 0.734 | 0.442 | 1 | 0 | 1 |
| Household has electricty | 905 | 0.750 | 0.433 | 1 | 0 | 1 |
| Household has piped water | 905 | 0.490 | 0.500 | 0 | 0 | 1 |
| Household owns a television | 905 | 0.568 | 0.496 | 1 | 0 | 1 |
| Household owns a radio | 905 | 0.685 | 0.465 | 1 | 0 | 1 |
| Household asset index | 905 | -0.000 | 1.000 | -0.080 | -1.670 | 3.933 |

The food insecurity access scale is an adaptation of the measure proposed by the Food and Nutrition Technical Assistance (FANTA) Project; the measure used at baseline is based on 7 questions, and is rescaled to range from 0 (no food insecurity) to 1 (the maximum level of food insecurity). Savings balances are first deflated using CPI data from the Kenya National Bureau of Statistics to reflect prevailing prices in July 2013, when the first baseline surveys were conducted; balances are then converted to US dollars using the average exchange rate from July 2013 (84.04 Kenyan shillings to the dollar). The top 1 percent of values of the Value of savings variable are trimmed. The household asset index is calculated by taking the first principal component of the indicators for whether a respondent's household or dwelling has power, piped water, a radio, a television, a gas or electric stove, a refrigerator, a motorcycle, a bicycle, a DVD player, and a computer; the first principal component is then normalized to be mean-zero and have a standard deviation of one.

Table 2: Compliance with Treatment

| | **Control** (1) | **Franchise Treatment** (2) | **Grant Treatment** (3) |
|---|---|---|---|
| Completed baseline survey | 1.00 | 1.00 | 1.00 |
| Attended business training | 0.00 | 0.61 | 0.01 |
| Helped to start a microfranchise | 0.01 | 0.39 | 0.01 |
| Received a cash grant | 0.00 | 0.00 | 0.95 |
| Observations | 363 | 360 | 182 |

Compliance rates for the franchise treatment are calculated using administrative records (attendance sign-in sheets) from the implementing organization and its local partners. Compliance rates for the cash grant treatment are calculated from the disbursement records of the research organization. Estimates of compliance based on self-reports of program participation (recorded during the first Midline Survey) yield nearly identical compliance rates.

Table 3: Intent to Treat Estimates: Labor Market Outcomes after 7–10 Months

| | Obs. | Control Mean | Treatment Effects: Franchise Treatment | Treatment Effects: Grant Treatment | p-value: F = G |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A. Involvement in Income-Generating Activities (Previous Month)* | | | | | |
| Engaged in any income-generating activities | 851 | 0.586 | 0.019 | 0.024 | 0.918 |
| | | | (0.038) | (0.046) | |
| Any self-employment activity | 851 | 0.245 | 0.098*** | 0.101** | 0.940 |
| | | | (0.035) | (0.043) | |
| Paid work for someone else | 851 | 0.382 | -0.069* | -0.070 | 0.973 |
| | | | (0.037) | (0.045) | |
| *Panel B. Likelihood of Operating a Microfranchise (Previous Month)* | | | | | |
| Operates a microfranchise | 851 | 0.000 | 0.085*** | -0.001 | 0.000 |
| | | | (0.015) | (0.004) | |
| Operates a salon microfranchise | 851 | 0.000 | 0.050*** | -0.003 | 0.000 |
| | | | (0.012) | (0.003) | |
| Operates a food cart microfranchise | 851 | 0.000 | 0.036*** | 0.001 | 0.001 |
| | | | (0.010) | (0.003) | |
| *Panel C. Labor Supply (Previous 7 Days)* | | | | | |
| Hours worked in last week | 851 | 17.945 | 1.097 | 6.831** | 0.046 |
| | | | (2.131) | (2.903) | |
| Self-employment hours | 851 | 4.723 | 4.127*** | 7.634*** | 0.104 |
| | | | (1.353) | (2.012) | |
| Hours of paid work for someone else | 851 | 13.017 | -2.880 | -0.871 | 0.365 |
| | | | (1.787) | (2.342) | |
| *Panel D. Income Excluding Transfers (Previous 7 Days)* | | | | | |
| Labor income (in USD) | 851 | 5.476 | 1.637** | 3.153*** | 0.208 |
| | | | (0.775) | (1.179) | |
| Log of labor income (in USD) | 851 | -1.436 | 0.508** | 0.560* | 0.870 |
| | | | (0.253) | (0.317) | |
| Self-employment income (in USD) | 851 | 2.617 | 1.305** | 2.306** | 0.314 |
| | | | (0.615) | (1.001) | |
| Log of self-employment income (in USD) | 851 | -3.158 | 0.633*** | 0.705** | 0.802 |
| | | | (0.215) | (0.277) | |
| Income from paid work for someone else (in USD) | 851 | 2.901 | 0.092 | 0.489 | 0.557 |
| | | | (0.480) | (0.650) | |
| Log of income from paid work (in USD) | 851 | -2.595 | -0.087 | -0.063 | 0.931 |
| | | | (0.222) | (0.273) | |

Robust standard errors in parentheses. *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively. OLS regressions reported. All specifications include controls for baseline household size, education level, and indicators for having given birth, having received any vocational training, or having any paid work experience prior to the baseline survey, in addition to survey enumerator and survey month fixed effects. Incomes are deflated to July 2013 levels using CPI data from the Kenya National Bureau of Statistics, then converted to US dollars using the average exchange rate from July 2013 (84.04 Kenyan shillings to the dollar). The top 1 percent of values of all hours and income variables are trimmed.

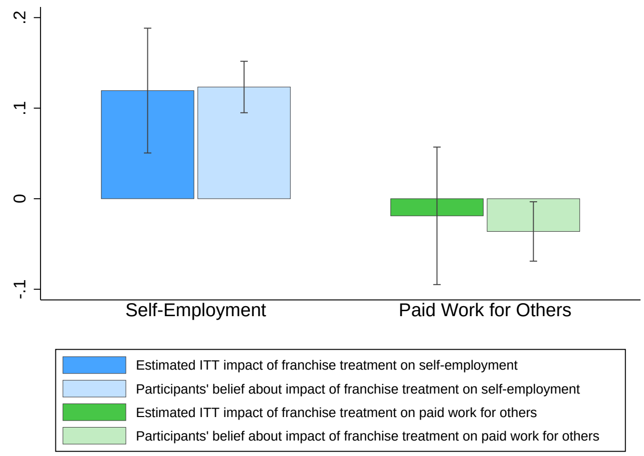
Table 4: Intent to Treat Estimates: Labor Market Outcomes after 14–22 Months

| | | | Treatment Effects | | |
|---|---|---|---|---|---|
| | Obs. | Control Mean | Franchise Treatment | Grant Treatment | p-value: F = G |
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A. Involvement in Income-Generating Activities (Previous Month)* | | | | | |
| Engaged in any income-generating activities | 837 | 0.657 | 0.076** | 0.057 | 0.655 |
| | | | (0.035) | (0.043) | |
| Any self-employment activity | 837 | 0.243 | 0.118*** | 0.129*** | 0.798 |
| | | | (0.035) | (0.043) | |
| Works for someone else | 837 | 0.497 | -0.040 | -0.063 | 0.635 |
| | | | (0.040) | (0.048) | |
| *Panel B. Likelihood of Operating a Microfranchise* | | | | | |
| Operates a microfranchise | 837 | 0.000 | 0.038*** | -0.002 | 0.001 |
| | | | (0.011) | (0.003) | |
| Operates a salon microfranchise | 837 | 0.000 | 0.028*** | -0.002 | 0.003 |
| | | | (0.009) | (0.003) | |
| Operates a food cart microfranchise | 837 | 0.000 | 0.009* | -0.000 | 0.087 |
| | | | (0.005) | (0.002) | |
| *Panel C. Labor Supply (Previous 7 Days)* | | | | | |
| Hours worked in last week | 837 | 19.130 | 1.490 | 1.223 | 0.919 |
| | | | (2.103) | (2.520) | |
| Self-employment hours | 837 | 3.509 | 3.094*** | 4.406*** | 0.427 |
| | | | (1.141) | (1.441) | |
| Hours of paid work for someone else | 837 | 15.559 | -1.758 | -3.180 | 0.538 |
| | | | (1.961) | (2.267) | |
| Hours of unpaid work in the last week | 837 | 23.364 | -0.952 | -0.995 | 0.975 |
| | | | (1.278) | (1.459) | |
| *Panel D. Income Excluding Transfers (Previous 7 Days)* | | | | | |
| Labor income (in USD) | 837 | 9.106 | -0.239 | -0.038 | 0.858 |
| | | | (1.013) | (1.198) | |
| Log of labor income (in USD) | 837 | -0.655 | 0.252 | 0.435 | 0.577 |
| | | | (0.270) | (0.326) | |
| Income from self-employment (in USD) | 837 | 2.849 | 1.022 | 1.373 | 0.679 |
| | | | (0.715) | (0.863) | |
| Log of income from self-employment (in USD) | 837 | -3.276 | 0.575*** | 0.988*** | 0.184 |
| | | | (0.221) | (0.292) | |
| Income from paid work for someone else (in USD) | 837 | 6.060 | -1.107 | -0.958 | 0.862 |
| | | | (0.765) | (0.883) | |
| Log of income from paid work (in USD) | 837 | -1.331 | -0.304 | -0.514 | 0.552 |
| | | | (0.302) | (0.351) | |

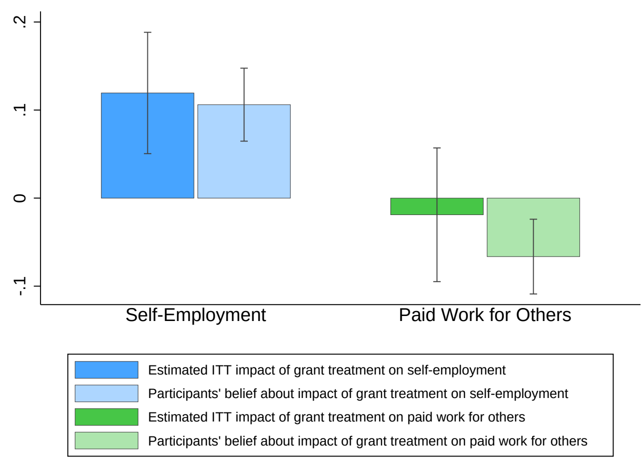
Robust standard errors in parentheses. *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively. OLS regressions reported. All specifications include controls for baseline household size, education level, and indicators for having given birth, having received any vocational training, or having any paid work experience prior to the baseline survey, in addition to survey enumerator and survey month fixed effects. Incomes are deflated to July 2013 levels using CPI data from the Kenya National Bureau of Statistics, then converted to US dollars using the average exchange rate from July 2013 (84.04 Kenyan shillings to the dollar). The top 1 percent of values of all hours and income variables are trimmed.

Figure 3: Participants' Beliefs about Impacts of Treatments

Panel A: Beliefs about Impact of Franchise Treatment

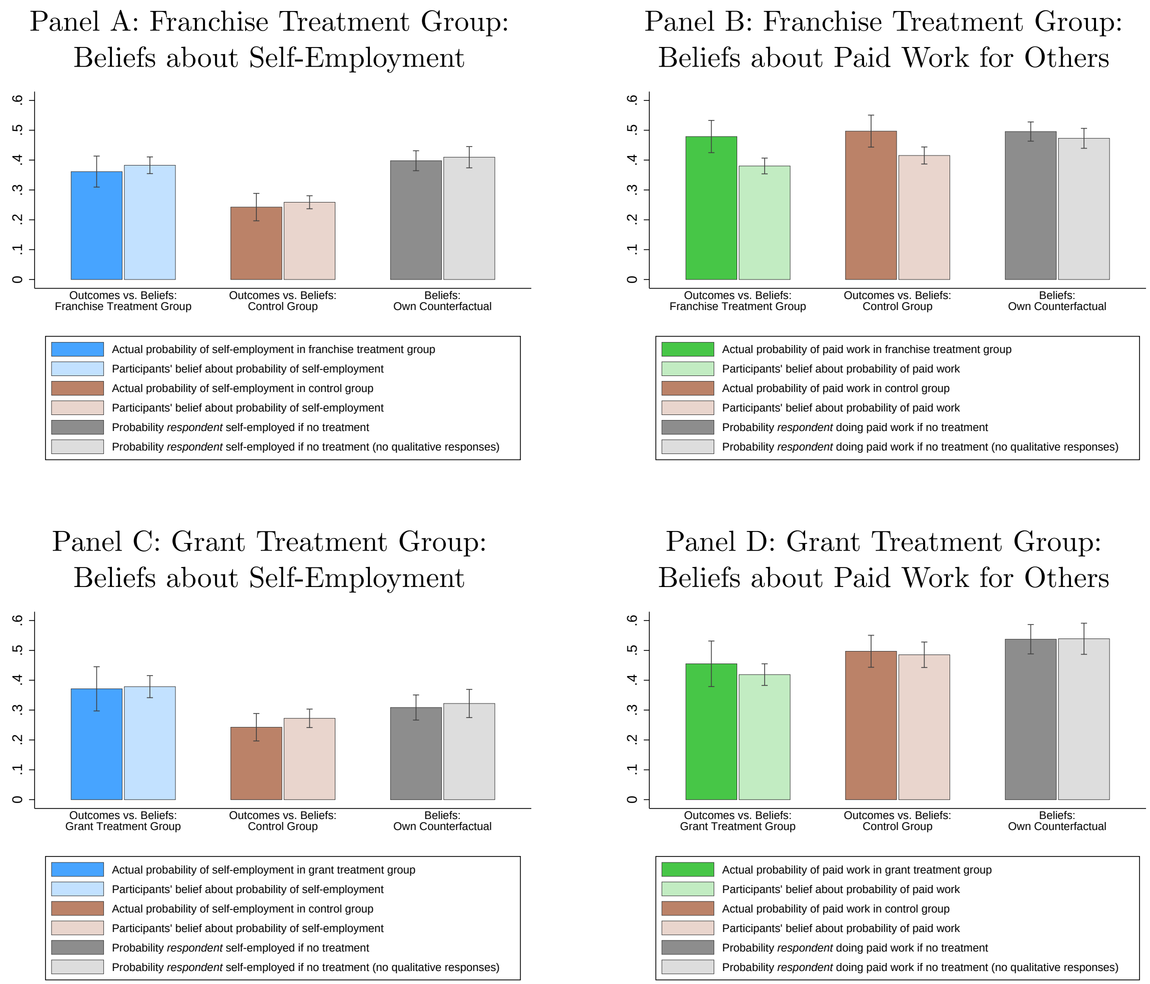



Panel B: Beliefs about Impact of Grant Treatment




ITT estimates of treatment are estimated via OLS, controlling for stratum fixed effects (we omit other controls included in our main specifications to make ITT estimates as comparable to self-reported beliefs as possible, though these controls have minimal impacts on estimated coefficients). Beliefs are estimated using estimates of the frequency of outcomes in a reference class of young women similar to oneself. For example, the estimate of the impact of the franchise treatment on the probability of self-employment is constructed using average responses to two questions: (1) "I would like you to imagine 100 women from [your neighborhood] who applied to the [name of treatment arm] program and were admitted into it, just as you were. In other words, please think about 100 women similar to yourself. Out of 100 women, how many do you think are currently running or operating their own business?" and (2) "Now I would like you to imagine 100 women from [your neighborhood] who applied to the [name of treatment arm] program and but who were not admitted into it. In other words, please think about 100 women similar to yourself who were not selected to the [name of treatment arm] program. Out of 100 women, how many do you think are currently running or operating their own business?" The difference in responses to these two questions (divided by 100) is the individual-level estimate of the average treatment effect of the program on self-employment.

Figure 4: Participants' Beliefs about Impacts of Treatments




The figure compares observed levels of self-employment and paid work in the treatment groups and the control group to beliefs about levels held by women assigned to the franchise and grant treatment arms. See Figure 3 for a description of the belief elicitation questions. The probability that a respondent would be doing paid work or in self-employment in the absence of treatment is the average response to a question about the counterfactual likelihood of involvement in the labor market.

# Online Appendix: not for print publication

## 1 Proof and extension of Proposition 1

### 1.1 Proof of Part (1)

To show Part (1) of Proposition 1, that for all $K \leq K_{min}$, $\mathbb{F}^*(K_t) = f^w(1)$, it is straightforward to proceed by contradiction. If any labor is allocated to the enterprise sector, the resulting production in the enterprise sector is zero, following Assumption A1. Thus, for any choice of the quantity of wage labor, $L^w$, the total output, $\mathbb{F}(K_t, L^w)$, is equal to $f^w(L^w)$. Because this is an increasing function of $L^w$ by Assumption A9, $\mathbb{F}(K_t, L^w) < F(K_t, 1)\ \forall L^w < 1$. Therefore, for all $K \leq K_{min}$, $L^w$ cannot be less than 1. Thus, for all $K \leq K_{min}$, no labor is allocated to the enterprise sector, all labor is allocated to the wage sector, and $\mathbb{F}^*(K_t) = f^w(1)$. In other words, $\mathbb{F}^*(K_t)$ is flat for $K \leq K_{min}$. $\square$

### 1.2 Proof of Part (2)

To show Part (2) of Proposition 1, that for $K_t \geq K_{min}$, the function $\mathbb{F}^*(K_t)$ has a positive first derivative, we reason as follows. Consider $K_t \geq K_{min}$, and $K_t' \geq K_t$. Recall that $\mathbb{F}^*(K_t)$ maximizes, over $L^w$, the value of $\mathbb{F}(K_t, L^w) = f^w(L^w) + f^e(K_t, 1 - L^w)$. Because $K_t \geq K_{min}$, and $K_t' \geq K_t$, we apply Assumption A2 ($f_k^e > 0$) implies that $f^e(K_t', 1 - L^w) > f^e(K_t, 1 - L^w)$. Thus, $\mathbb{F}(K_t', L^w) > \mathbb{F}(K_t, L^w)$. Because $\mathbb{F}^*(K_t)$ maximizes, over $L^w$, the value of $\mathbb{F}(K_t, L^w)$, it must be the case that $\mathbb{F}^*(K_t)$ is weakly greater than $\mathbb{F}(K_t', L^w)$ (which is achieved without adjusting the allocation of labor between activities). Thus, $\mathbb{F}(K_t', L^w) > \mathbb{F}(K_t, L^w)$, so $\mathbb{F}^*(K_t)$ has a positive first derivative.[32]
$\square$

### 1.3 Proof of Part (3)

To show Part (3) of Proposition 1, that the function, $\mathbb{F}^*(K_t)$ has a negative second derivative for $K_t \geq K_{min}$, it is useful to provide first a lemma, then a diagram.

**Lemma 1.** *The derivative of $\mathbb{F}^*(K_t)$ is equal to the partial derivative of $f^e(K_t, L^e)$ with respect to capital at the optimum value of $L^e$.*

*Proof.* Though this can be shown as a direct applicaton of the envelope theorem, it can also be argued succinctly as follows:

$$\mathbb{F}^*(K_t) = f^w(L^w) + f^e(K_t, L^e)$$
$$\frac{d}{dK_t}\mathbb{F}^*(K_t) = \frac{df^w}{dL^w}\frac{dL^w}{dK_t} + \frac{\delta f^e}{\delta K_t} + \frac{\delta f^e}{\delta L^e}\frac{dL^e}{dK_t}$$

But because $\frac{df^w}{dL^w} = \frac{df^e}{dL^e}$ (marginal products are equated) at the optimum, and because $\frac{dL^w}{dK_t} = -\frac{dL^e}{dK_t}$ at the constraint (since $L^e + L^w = 1$, so any movement in one is accompanied by an opposite

[32] Because of Assumption A5, the optimal allocation of labor across sectors is an interior solution in $L^e$ for all $K_t > K_{min}$. This remains true for arbitrarily large $K_t$ because of Assumption A11. Intuitively, the interior nature of the solution follows immediately from the fact that the marginal product of labor approaches infinity as labor goes to zero in either sector. If the marginal product of labor in the wage labor sector were constant, the optimal allocation of labor across sectors could involve no wage labor at some values of $K_t > K_{min}$.

movement in the other), this becomes:

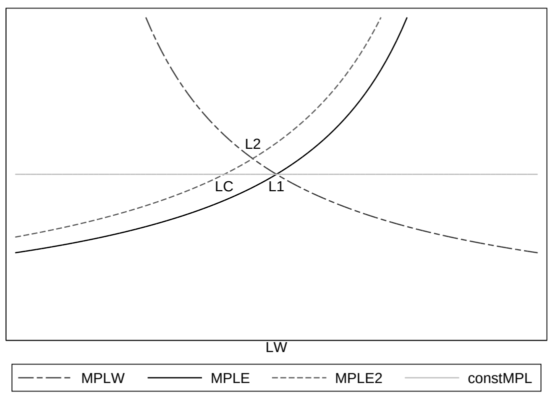
$$\frac{d}{dK_t}\mathbb{F}^*(K_t) = -\frac{df^e}{dL^e}\frac{dL^e}{dK_t} + \frac{\delta f^e}{\delta K_t} + \frac{\delta f^e}{\delta L^e}\frac{dL^e}{dK_t}$$

The first and last terms cancel, proving that:

$$\frac{d}{dK_t}\mathbb{F}^*(K_t) = \frac{\delta f^e}{\delta K_t}$$

□

Next, we provide a diagram for reference. The graph below shows (on the vertical axis) the marginal products of labor in the two sectors, as labor ($L^W$, on the horizontal axis) shifts between them. At the left side of the diagram, $L^W = 0$; at the right, $L^W = 1$.




The two curves showing the marginal product of labor in each sector, $MPL^w$ and $MPL^e$, are given their shapes by Assumptions A3 and A10. Where the marginal products are equated, the curves cross at the optimal allocation of labor, $L1$.

With reference to this diagram, Part (3) of Proposition 1 concerns changes in $K$. If $K$ increases, the change in the allocation of labor depends on the shape of $f^w$. By Assumption A4, that inputs are complements, an increase in $K$ increases the entire $MPL^e$ curve so that it becomes $MPL^e_2$, shown in dashes. As a benchmark, we now consider if, instead of a diminishing returns $f^w$, we instead had a constant returns $f^w = bL$ for some constant $b$. The graph shows how it too could intersect $MPL^e$ at the point $L1$. If the wage sector were constant returns, then when the $MPL^e$ curve shifts to become $MPL^e_2$, the new optimal allocation would be at $LC$, and the optimal marginal product of labor would remain unchanged. But with the actual $f^w$, the new optimum is at $L2$, which is a smaller shift in labor allocation: it must be the case that $LC < L2 < L1$. But because $f^e$ is homogeneous of degree one above the minimum scale, we know that the marginal products of capital and labor in the enterprise sector are functions only of the capital-labor ratio. Thus, if the marginal product of labor is higher at the new equilibrium, it is because labor did not adjust enough to preserve the capital-labor ratio. The new marginal product of capital in the enterprise sector is thus lower at $L2$ than at $L1$: the value of $\frac{\delta f^e}{\delta K_t}$ is declining in $K_t$ at the optimum. Application of Lemma 1 now implies that $\frac{d}{dK_t}\mathbb{F}^*(K_t)$ is also declining in $K_t$, so $\mathbb{F}^*(K_t)$ has a negative second derivative. □

## 1.4 Extension of Proposition 1: constant wage

If, instead of Assumption A10 (that $\frac{\delta^2}{\delta L^2}f^w(L^w) < 0$), the wage labor sector is characterized by a constant wage $\frac{\delta^2}{\delta L^2}f^w(L^w) = 0$, then Part 3 of Proposition 1 changes to: "For all $K_t > K_{min}$,

$\mathbb{F}^*(K_t)$ has a weakly negative second derivative. Specifically, there exists $\bar{K}$ such that for all $K_t$ above $K_{min}$ but below $\bar{K}$, $\mathbb{F}^*(K_t)$ has a second derivative equal to zero; and for all $K_t$ above $\bar{k}$, $\mathbb{F}^*(K_t)$ has a negative second derivative."

The proof is straightforward. By Assumtion A5, above $K_{min}$, the marginal product of labor in the enterprise sector crosses the fixed wage in the enterprise sector at some point ($L1$ in the diagram above) where a nonzero fraction of labor is allocate to the enterprise sector ($L^W > 0$). Any increase in capital shifts the $MPL^e$ curve upward, moving the optimum allocation to $LC$ in the diagram above. Because the marginal product of labor did not change, and because $f^e$ is homogeneous of degree one above the minimum scale, the capital-labor ratio did not change. Thus, as long as the $MPL^e$ curve intersects the fixed wage line, for every increase in capital, there is an exactly proportionate shift in labor from the wage to the enterprise sector. Because $f^e$ is homogeneous of degree one, this produces a proportionate shift in output. The change in output at the optimum is thus linear in capital, as long as the $MPL^e$ curve intersects the fixed wage line. At some level of capital, $\bar{K}$, the $MPL^e$ curve rises entirely above the fixed wage line in the diagram above. After this point, the optimum allocation of labor is a corner solution setting $L^W = 0$. At this point, the shape of $\mathbb{F}^*(K_t)$ is necessarily the shape of $f^e$, which by diminishing returns (Assumption A2) means it has a negative second derivative. This slight variation on the characteristic shape of $\mathbb{F}^*(K_t)$ yields the same possible numbers of crossings as before, so the definitions of latent entrepreneurial types that are used in the paper still hold. $\square$

## 2 Additional Tables and Figures

Table A1: Baseline Covariates, by Treatment Status

| | Control | Franchise Treatment | Grant Treatment | Differences | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | **F – C** | **G – C** | **G – F** |
| *Panel A. Demographics and Household Composition* | | | | | | |
| Age | 18.758 | 18.803 | 18.780 | 0.044 | 0.023 | −0.021 |
| | [0.802] | [0.748] | [0.832] | (0.055) | (0.069) | (0.068) |
| At least one parent alive | 0.890 | 0.878 | 0.884 | −0.013 | −0.005 | 0.007 |
| | [0.314] | [0.328] | [0.321] | (0.024) | (0.029) | (0.029) |
| Household size | 5.127 | 4.700 | 4.753 | $-0.421^{***}$ | $-0.375^{*}$ | 0.047 |
| | [2.258] | [1.986] | [2.291] | (0.154) | (0.203) | (0.197) |
| Married or cohabitating | 0.149 | 0.189 | 0.148 | 0.039 | 0.000 | −0.039 |
| | [0.356] | [0.392] | [0.356] | (0.027) | (0.031) | (0.032) |
| Has given birth | 0.364 | 0.439 | 0.440 | $0.076^{**}$ | $0.077^{*}$ | 0.002 |
| | [0.482] | [0.497] | [0.498] | (0.036) | (0.044) | (0.044) |
| *Panel B. Educational Background* | | | | | | |
| Father's education, if known | 9.596 | 9.761 | 10.142 | 0.158 | 0.519 | 0.361 |
| | [3.245] | [2.820] | [2.767] | (0.290) | (0.341) | (0.321) |
| Mother's education, if known | 8.955 | 9.137 | 9.007 | 0.162 | 0.047 | −0.115 |
| | [2.949] | [2.798] | [2.847] | (0.239) | (0.285) | (0.285) |
| Years of education | 10.033 | 9.914 | 9.577 | −0.122 | $-0.459^{**}$ | −0.337 |
| | [1.998] | [2.015] | [2.213] | (0.147) | (0.191) | (0.191) |
| Any vocational training | 0.369 | 0.319 | 0.346 | −0.050 | −0.023 | 0.027 |
| | [0.483] | [0.467] | [0.477] | (0.035) | (0.042) | (0.042) |
| *Panel C. Involvement in Income-Generating Activities (IGAs)* | | | | | | |
| Any (paid) work experience | 0.537 | 0.544 | 0.566 | 0.007 | 0.029 | 0.022 |
| | [0.499] | [0.499] | [0.497] | (0.037) | (0.045) | (0.045) |
| Engaged in any IGAs | 0.124 | 0.167 | 0.148 | 0.042 | 0.024 | −0.018 |
| | [0.330] | [0.373] | [0.356] | (0.026) | (0.031) | (0.033) |
| Any self-employment activity | 0.039 | 0.061 | 0.049 | 0.023 | 0.011 | −0.012 |
| | [0.193] | [0.240] | [0.217] | (0.016) | (0.019) | (0.021) |
| Any paid work for someone else | 0.085 | 0.111 | 0.104 | 0.025 | 0.019 | −0.007 |
| | [0.280] | [0.315] | [0.324] | (0.022) | (0.028) | (0.029) |
| Hours of housework in last week | 25.881 | 26.192 | 26.215 | 0.305 | 0.329 | 0.025 |
| | [15.716] | [15.545] | [13.961] | (1.184) | (1.337) | (1.330) |
| *Panel D. Assets, Savings, and Living Conditions* | | | | | | |
| Food insecurity index | 0.257 | 0.265 | 0.254 | 0.009 | −0.003 | −0.012 |
| | [0.185] | [0.173] | [0.162] | (0.013) | (0.015) | (0.015) |
| Has a personal bank account | 0.088 | 0.092 | 0.078 | 0.003 | −0.010 | −0.013 |
| | [0.284] | [0.289] | [0.269] | (0.021) | (0.025) | (0.025) |
| Has any savings | 0.339 | 0.336 | 0.298 | −0.002 | −0.039 | −0.037 |
| | [0.474] | [0.473] | [0.459] | (0.035) | (0.042) | (0.042) |
| Value of savings (in USD) | 4.688 | 5.225 | 4.872 | 0.543 | 0.153 | −0.390 |

*Continued on next page*

Table A1 – *Continued from previous page*

| | Control | Franchise Treatment | Grant Treatment | Differences | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | **F – C** | **G – C** | **G – F** |
| | [14.250] | [15.334] | [14.744] | (1.103) | (1.321) | (1.358) |
| Value of savings, if any (in USD) | 17.364 | 17.913 | 19.706 | 0.395 | 2.875 | 2.481 |
| | [23.138] | [24.129] | [24.400] | (3.571) | (4.227) | (4.336) |
| Owns a personal mobile phone | 0.741 | 0.731 | 0.725 | −0.013 | −0.015 | −0.002 |
| | [0.439] | [0.444] | [0.448] | (0.031) | (0.038) | (0.038) |
| Household has electricty | 0.749 | 0.758 | 0.736 | 0.009 | −0.013 | −0.021 |
| | [0.434] | [0.429] | [0.442] | (0.032) | (0.040) | (0.039) |
| Household has piped water | 0.490 | 0.494 | 0.478 | 0.003 | −0.013 | −0.017 |
| | [0.501] | [0.501] | [0.501] | (0.033) | (0.041) | (0.041) |
| Household owns a television | 0.567 | 0.575 | 0.555 | 0.007 | −0.014 | −0.021 |
| | [0.496] | [0.495] | [0.498] | (0.036) | (0.044) | (0.045) |
| Household owns a radio | 0.716 | 0.664 | 0.665 | −0.053 | −0.054 | −0.001 |
| | [0.451] | [0.473] | [0.473] | (0.034) | (0.042) | (0.043) |
| Household asset index | -0.003 | 0.013 | -0.020 | 0.012 | −0.021 | −0.034 |
| | [0.971] | [1.024] | [1.013] | (0.073) | (0.089) | (0.091) |
| Observations | 363 | 360 | 182 | | | |

Standard deviation in brackets, robust standard errors in parentheses. Columns 4 through 6 report differences in means across treatments, with significance levels estimated controlling for strata fixed effects (as in our main specifications). $^{*}$, $^{**}$, and $^{***}$ indicate significance at the 90, 95, and 99 percent confidence levels, respectively. Asset PCA is the first principal component of the set of indicators for: whether the household has electricity, whether the household has piped water, and whether anyone in the household owns or has a television, a refrigerator, a stove, a computer, a DVD player, a motorcycle, a bicycle, or a lamp.

Table A2: Attrition from the Sample

| | **OLS** (1) | **OLS** (2) | **OLS** (3) |
|---|---|---|---|
| Franchise treatment | 0.009 | 0.017 | -0.379 |
| | (0.02) | (0.028) | (0.73) |
| Grant treatment | 0.014 | 0.018 | -0.662 |
| | (0.024) | (0.034) | (0.857) |
| Age | . | 0.019 | 0.003 |
| | | (0.016) | (0.026) |
| At least one parent alive | . | 0.057 | 0.076 |
| | | (0.046) | (0.086) |
| Household size | . | -0.01 | -0.004 |
| | | (0.006) | (0.01) |
| Married or cohabitating | . | -0.019 | -0.004 |
| | | (0.043) | (0.07) |
| Has given birth | . | -0.003 | 0.023 |
| | | (0.033) | (0.058) |
| Father's education, if known | . | -0.009$^{*}$ | -0.0007 |
| | | (0.005) | (0.008) |

*Continued on next page*

Table A2 – *Continued from previous page*

| | **OLS** (1) | **OLS** (2) | **OLS** (3) |
|---|---|---|---|
| Mother's education, if known | . | 0.011** | 0.008 |
| | | (0.005) | (0.009) |
| Years of education | . | -0.003 | -0.005 |
| | | (0.008) | (0.014) |
| Any vocational training | . | 0.033 | 0.04 |
| | | (0.028) | (0.043) |
| Any (paid) work experience | . | -0.015 | 0.008 |
| | | (0.028) | (0.044) |
| Engaged in any IGAs | . | -0.065 | 0.026 |
| | | (0.042) | (0.073) |
| Hours of housework in last week | . | 0.00007 | 0.00003 |
| | | (0.0008) | (0.001) |
| Food insecurity index | . | -0.017 | -0.142 |
| | | (0.081) | (0.125) |
| Has a personal bank account | . | 0.01 | 0.032 |
| | | (0.046) | (0.074) |
| Has any savings | . | 0.023 | 0.044 |
| | | (0.028) | (0.045) |
| Household asset index | . | -0.013 | -0.029 |
| | | (0.015) | (0.024) |
| Age x franchise treatment | . | . | 0.027 |
| | | | (0.039) |
| At least one parent alive x franchise treatment | . | . | 0.003 |
| | | | (0.113) |
| Household size x franchise treatment | . | . | -0.0006 |
| | | | (0.015) |
| Married or cohabitating x franchise treatment | . | . | -0.037 |
| | | | (0.098) |
| Has given birth x franchise treatment | . | . | -0.041 |
| | | | (0.079) |
| Father's education, if known x franchise treatment | . | . | -0.024** |
| | | | (0.011) |
| Mother's education, if known x franchise treatment | . | . | 0.002 |
| | | | (0.013) |
| Years of education x franchise treatment | . | . | 0.011 |
| | | | (0.019) |
| Any vocational training x franchise treatment | . | . | 0.034 |
| | | | (0.064) |
| Any (paid) work experience x franchise treatment | . | . | -0.052 |
| | | | (0.064) |
| Engaged in any IGAs x franchise treatment | . | . | -0.129 |
| | | | (0.099) |
| Hours of housework in last week x franchise treatment | . | . | -0.001 |
| | | | (0.002) |
| Food insecurity index x franchise treatment | . | . | 0.381** |
| | | | (0.186) |
| Has a personal bank account x franchise treatment | . | . | 0.007 |
| | | | (0.104) |
| Has any savings x franchise treatment | . | . | -0.056 |
| | | | (0.063) |
| Household asset index x franchise treatment | . | . | 0.044 |
| | | | (0.035) |

*Continued on next page*

Table A2 – *Continued from previous page*

| | **OLS** | **OLS** | **OLS** |
|---|---|---|---|
| | (1) | (2) | (3) |
| Age x grant treatment | . | . | 0.046 |
| | | | (0.044) |
| At least one parent alive x grant treatment | . | . | -0.058 |
| | | | (0.128) |
| Household size x grant treatment | . | . | -0.025 |
| | | | (0.017) |
| Married or cohabitating x grant treatment | . | . | 0.038 |
| | | | (0.122) |
| Has given birth x grant treatment | . | . | -0.032 |
| | | | (0.093) |
| Father's education, if known x grant treatment | . | . | 0.012 |
| | | | (0.015) |
| Mother's education, if known x grant treatment | . | . | -0.008 |
| | | | (0.016) |
| Years of education x grant treatment | . | . | -0.009 |
| | | | (0.022) |
| Any vocational training x grant treatment | . | . | -0.086 |
| | | | (0.079) |
| Any (paid) work experience x grant treatment | . | . | 0.008 |
| | | | (0.077) |
| Engaged in any IGAs x grant treatment | . | . | -0.119 |
| | | | (0.124) |
| Hours of housework in last week x grant treatment | . | . | 0.003 |
| | | | (0.002) |
| Food insecurity index x grant treatment | . | . | -0.0003 |
| | | | (0.231) |
| Has a personal bank account x grant treatment | . | . | -0.113 |
| | | | (0.134) |
| Has any savings x grant treatment | . | . | 0.011 |
| | | | (0.078) |
| Household asset index x grant treatment | . | . | 0.032 |
| | | | (0.041) |
| Constant | $0.069^{***}$ | -0.271 | -0.05 |
| | (0.014) | (0.314) | (0.489) |
| Observations | 905 | 499 | 499 |
| $R^2$ | 0.0004 | 0.036 | 0.097 |
| F-Test: observables (p-value) | | 0.371 | 0.959 |
| F-Test: treatment-observables interactions (p-value) | | | 0.545 |

Robust standard errors in parentheses. $^{*}$, $^{**}$, and $^{***}$ indicate significance at the 90, 95, and 99 percent confidence levels, respectively. OLS regressions reported. The dependent variable in all specifications is an indicator for attrition from the sample (between baseline and endline). The last two rows of the table report p-values from associated F-tests of whether the observable characteristics and observable characteristics interacted with treatment are jointly significant in the attrition regressions in columns 2 and 3.

Table A3: Treatment on the Treated: Labor Market Outcomes after 7–10 Months

| | Obs. | Control Mean | TOT Estimates: Started Franchise Program | TOT Estimates: Received Grant | p-value: F = G |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A. Involvement in Income-Generating Activities (Previous Month)* | | | | | |
| Engaged in any income-generating activities | 851 | 0.586 | 0.032 | 0.025 | 0.908 |
| | | | (0.061) | (0.047) | |
| Any self-employment activity | 851 | 0.245 | 0.163*** | 0.105** | 0.307 |
| | | | (0.055) | (0.044) | |
| Paid work for someone else | 851 | 0.382 | -0.114* | -0.073 | 0.473 |
| | | | (0.059) | (0.046) | |
| *Panel B. Labor Supply (Previous 7 Days)* | | | | | |
| Hours worked in last week | 851 | 17.945 | 1.853 | 7.105** | 0.140 |
| | | | (3.442) | (2.953) | |
| Self-employment hours | 851 | 4.723 | 6.856*** | 7.938*** | 0.680 |
| | | | (2.180) | (2.047) | |
| Hours of paid work for someone else | 851 | 13.017 | -4.756 | -0.903 | 0.167 |
| | | | (2.893) | (2.383) | |
| *Panel C. Income Excluding Transfers (Previous 7 Days)* | | | | | |
| Reports any labor income | 851 | 0.466 | 0.093 | 0.062 | 0.607 |
| | | | (0.061) | (0.048) | |
| Income excluding transfers (in USD) | 851 | 5.476 | 2.720** | 3.278*** | 0.699 |
| | | | (1.250) | (1.198) | |
| Log income (in USD) | 851 | -1.436 | 0.842** | 0.582* | 0.525 |
| | | | (0.407) | (0.323) | |
| Self-employment income (in USD) | 851 | 2.617 | 2.167** | 2.397** | 0.843 |
| | | | (0.988) | (1.018) | |
| Log of self-employment income (in USD) | 851 | -3.158 | 1.049*** | 0.733*** | 0.382 |
| | | | (0.345) | (0.282) | |
| Income from paid work for someone else (in USD) | 851 | 2.901 | 0.154 | 0.508 | 0.675 |
| | | | (0.776) | (0.661) | |
| Log of income from paid work (in USD) | 851 | -2.595 | -0.143 | -0.066 | 0.825 |
| | | | (0.359) | (0.278) | |
| *Panel D. First-Stage F-Statistics on Excluded Instruments* | | | | | |
| Franchise treatment | | | 277.723 | | |
| Grant treatment | | | 2440.035 | | |

Robust standard errors in parentheses. *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively. OLS regressions reported. All specifications include controls for baseline household size, education level, and indicators for having given birth, having received any vocational training, or having any paid work experience prior to the baseline survey, in addition to survey enumerator and survey month fixed effects. Money amounts are deflated to July 2013 levels using CPI data from the Kenya National Bureau of Statistics, then converted to US dollars using the average exchange rate from July 2013 (84.04 Kenyan shillings to the dollar). The top 1 percent of values of all hours and income variables are trimmed.

Table A4: Intent to Treat Estimates: Occupational Sectors after 7–10 Months

| | Obs. | Control Mean | Treatment Effects: Franchise Treatment | Treatment Effects: Grant Treatment | p-value: F = G |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A. Occupational Sectors* | | | | | |
| Domestic services | 851 | 0.143 | -0.045* | -0.049 | 0.904 |
| | | | (0.025) | (0.030) | |
| Salon and beauty | 851 | 0.146 | 0.088*** | -0.073** | 0.000 |
| | | | (0.029) | (0.030) | |
| Retail and hawking | 851 | 0.090 | 0.043* | 0.112*** | 0.055 |
| | | | (0.024) | (0.035) | |
| Food service and catering | 851 | 0.026 | -0.007 | 0.006 | 0.434 |
| | | | (0.013) | (0.016) | |
| Sells prepared food or cooked snacks | 851 | 0.061 | 0.037* | 0.034 | 0.916 |
| | | | (0.020) | (0.025) | |
| White collar or professional | 851 | 0.020 | 0.002 | 0.016 | 0.373 |
| | | | (0.011) | (0.016) | |
| Janitorial work and trash collection | 851 | 0.026 | -0.020* | -0.028*** | 0.196 |
| | | | (0.011) | (0.009) | |
| Sells uncooked fruits and vegetables | 851 | 0.038 | -0.021 | 0.031 | 0.010 |
| | | | (0.014) | (0.022) | |
| Works in light industry (factory work) | 851 | 0.035 | -0.009 | 0.005 | 0.409 |
| | | | (0.014) | (0.019) | |
| Wholesale and distribution | 851 | 0.006 | 0.018* | 0.010 | 0.450 |
| | | | (0.009) | (0.009) | |
| Tailoring, sewing, and arts and crafts | 851 | 0.015 | -0.000 | -0.005 | 0.667 |
| | | | (0.010) | (0.011) | |
| Entertainment or professional sport | 851 | 0.017 | -0.015** | -0.009 | 0.353 |
| | | | (0.007) | (0.010) | |
| Construction, security, and manual labor | 851 | 0.009 | -0.005 | 0.004 | 0.246 |
| | | | (0.006) | (0.007) | |
| Farming or agricultural labor | 851 | 0.015 | -0.013** | -0.004 | 0.257 |
| | | | (0.006) | (0.011) | |
| Sex worker | 851 | 0.000 | 0.002 | -0.000 | 0.325 |
| | | | (0.002) | (0.001) | |

Robust standard errors in parentheses. *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively. OLS regressions reported. All specifications include controls for baseline household size, education level, and indicators for having given birth, having received any vocational training, or having any paid work experience prior to the baseline survey, in addition to survey enumerator and survey month fixed effects..

Table A5: Pooled Intent to Treat Estimates: Labor Market Outcomes over Time

| | **Main Effect** | | **× Second Year** | |
|---|---|---|---|---|
| *Treatment:* | **Franchise** | **Grant** | **Franchise** | **Grant** |
| | **(1)** | **(2)** | **(3)** | **(4)** |
| Engaged in any income-generating activities | 0.016 | 0.021 | 0.066 | 0.040 |
| | (0.038) | (0.046) | (0.051) | (0.063) |
| Any self-employment activity | 0.096$^{***}$ | 0.105$^{**}$ | 0.022 | 0.020 |
| | (0.035) | (0.043) | (0.049) | (0.060) |
| Paid work for someone else | -0.072$^{*}$ | -0.079$^{*}$ | 0.039 | 0.024 |
| | (0.037) | (0.044) | (0.054) | (0.065) |
| Hours worked in last week | 0.863 | 6.759$^{**}$ | 1.244 | -5.628 |
| | (2.129) | (2.897) | (2.992) | (3.830) |
| Self-employment hours | 4.177$^{***}$ | 7.907$^{***}$ | -1.111 | -3.770 |
| | (1.361) | (2.000) | (1.795) | (2.457) |
| Hours of paid work for someone else | -3.142$^{*}$ | -1.202 | 2.009 | -1.807 |
| | (1.794) | (2.337) | (2.652) | (3.271) |
| Labor income (in USD) | 1.594$^{**}$ | 3.203$^{***}$ | -1.586 | -3.319$^{**}$ |
| | (0.774) | (1.184) | (1.269) | (1.664) |
| Log of labor income (in USD) | 0.514$^{**}$ | 0.564$^{*}$ | -0.222 | -0.120 |
| | (0.253) | (0.318) | (0.367) | (0.451) |
| Self-employment income (in USD) | 1.310$^{**}$ | 2.429$^{**}$ | -0.272 | -1.173 |
| | (0.619) | (0.999) | (0.949) | (1.302) |
| Log of self-employment income (in USD) | 0.620$^{***}$ | 0.731$^{***}$ | -0.032 | 0.233 |
| | (0.216) | (0.278) | (0.308) | (0.400) |
| Income from paid work for someone else (in USD) | 0.043 | 0.445 | -0.934 | -1.394 |
| | (0.487) | (0.652) | (0.903) | (1.103) |
| Log of income from paid work (in USD) | -0.105 | -0.125 | -0.132 | -0.332 |
| | (0.224) | (0.273) | (0.374) | (0.445) |

Robust standard errors in parentheses. $^{*}$, $^{**}$, and $^{***}$ indicate significance at the 90, 95, and 99 percent confidence levels, respectively. OLS regressions reported. Sample includes both midline and endline data (pooled). All specifications include controls for baseline household size, education level, and indicators for having given birth, having received any vocational training, or having any paid work experience prior to the baseline survey, in addition to survey enumerator (by survey round) and survey month fixed effects. Incomes are deflated to July 2013 levels using CPI data from the Kenya National Bureau of Statistics, then converted to US dollars using the average exchange rate from July 2013 (84.04 Kenyan shillings to the dollar). The top 1 percent of values of all hours and income variables are trimmed. Columns 1 and 2 report the overall impact of each treatment, pooled across survey rounds; and Columns 3 and 4 report interactions between treatments and an indicator for the endline survey.

Table A6: Intent to Treat Estimates: Occupational Sector and Other Outcomes after 14–22 Months

| | | | **Treatment Effects** | | |
|---|---|---|---|---|---|
| | **Obs.** | **Control Mean** | **Franchise Treatment** | **Grant Treatment** | **p-value: F = G** |
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** |
| *Panel A. Occupational Sectors* | | | | | |
| Domestic services | 837 | 0.198 | -0.021 | 0.014 | 0.374 |
| | | | (0.031) | (0.039) | |
| Salon and beauty | 837 | 0.166 | 0.114*** | -0.002 | 0.002 |
| | | | (0.031) | (0.035) | |
| Retail and hawking | 837 | 0.121 | 0.009 | 0.048 | 0.274 |
| | | | (0.026) | (0.034) | |
| Food service and catering | 837 | 0.056 | 0.022 | -0.013 | 0.116 |
| | | | (0.020) | (0.021) | |
| Sells prepared food or cooked snacks | 837 | 0.053 | 0.013 | -0.000 | 0.560 |
| | | | (0.019) | (0.022) | |
| White collar or professional | 837 | 0.047 | -0.022 | -0.005 | 0.272 |
| | | | (0.014) | (0.019) | |
| Janitorial work and trash collection | 837 | 0.041 | -0.008 | -0.003 | 0.741 |
| | | | (0.015) | (0.018) | |
| Sells uncooked fruits and vegetables | 837 | 0.027 | 0.013 | 0.016 | 0.894 |
| | | | (0.014) | (0.018) | |
| Works in light industry (factory work) | 837 | 0.024 | 0.002 | 0.010 | 0.690 |
| | | | (0.013) | (0.017) | |
| Wholesale and distribution | 837 | 0.024 | -0.003 | -0.005 | 0.853 |
| | | | (0.011) | (0.014) | |
| Tailoring, sewing, and arts and crafts | 837 | 0.021 | 0.001 | 0.026 | 0.177 |
| | | | (0.011) | (0.018) | |
| Entertainment or professional sport | 837 | 0.012 | -0.003 | -0.013* | 0.120 |
| | | | (0.009) | (0.007) | |
| Construction, security, and manual labor | 837 | 0.009 | -0.003 | 0.001 | 0.548 |
| | | | (0.005) | (0.007) | |
| Farming or agricultural labor | 837 | 0.000 | 0.012* | 0.018* | 0.624 |
| | | | (0.006) | (0.010) | |
| Sex worker | 837 | 0.000 | 0.002 | -0.000 | 0.329 |
| | | | (0.002) | (0.001) | |
| *Panel B. Labor Market Churning* | | | | | |
| Closed a business between midline and endline | 812 | 0.183 | 0.010 | 0.024 | 0.715 |
| | | | (0.032) | (0.040) | |
| Left a paid job between midline and endline | 812 | 0.241 | -0.051 | -0.056 | 0.894 |
| | | | (0.033) | (0.040) | |
| Started a business between midline and endline | 812 | 0.177 | 0.039 | 0.047 | 0.850 |
| | | | (0.031) | (0.037) | |
| Started a new paid job between midline and endline | 812 | 0.387 | -0.055 | -0.058 | 0.941 |
| | | | (0.039) | (0.047) | |
| *Panel C. Occupational Sector* | | | | | |
| Years of education | 837 | 10.198 | -0.032 | -0.083 | 0.605 |

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Table A6 – *Continued from previous page*

| | Obs. (1) | Control Mean (2) | Treatment Effects: Franchise Treatment (3) | Treatment Effects: Grant Treatment (4) | p-value: F = G (5) |
|---|---|---|---|---|---|
| | | | (0.092) | (0.092) | |
| Curently enrolled in school | 837 | 0.101 | -0.014 | -0.016 | 0.934 |
| | | | (0.022) | (0.026) | |
| Has done any vocational training | 837 | 0.568 | $0.292^{***}$ | 0.035 | 0.000 |
| | | | (0.033) | (0.045) | |
| Has done business skills training | 837 | 0.098 | $0.149^{***}$ | 0.001 | 0.000 |
| | | | (0.028) | (0.029) | |
| Business skills score (scaled 0 to 5) | 837 | 1.036 | 0.129 | -0.103 | 0.037 |
| | | | (0.095) | (0.109) | |
| Has done salon skills training | 837 | 0.213 | $0.289^{***}$ | 0.003 | 0.000 |
| | | | (0.034) | (0.039) | |
| Salon skills score (scaled 0 to 9) | 837 | 4.580 | 0.136 | $-0.485^{***}$ | 0.000 |
| | | | (0.128) | (0.159) | |
| Has done tailoring training | 837 | 0.062 | 0.003 | 0.018 | 0.564 |
| | | | (0.019) | (0.026) | |
| Tailoring skills score (scaled 0 to 8) | 837 | 1.325 | -0.021 | 0.035 | 0.610 |
| | | | (0.092) | (0.112) | |
| Has done computer training | 837 | 0.237 | $-0.069^{**}$ | 0.003 | 0.032 |
| | | | (0.027) | (0.034) | |
| Seconds required to complete typing test | 835 | 100.935 | 5.298 | $13.055^{**}$ | 0.145 |
| | | | (4.385) | (5.285) | |
| *Panel D. Household Composition and Living Arrangements* | | | | | |
| Household size | 837 | 4.716 | -0.082 | 0.133 | 0.289 |
| | | | (0.169) | (0.205) | |
| Married or cohabitating | 837 | 0.269 | 0.012 | -0.040 | 0.208 |
| | | | (0.034) | (0.041) | |
| Had an additional child (after program) | 837 | 0.145 | $0.061^{**}$ | 0.045 | 0.663 |
| | | | (0.030) | (0.037) | |
| Lives with own child | 837 | 0.453 | $0.071^{**}$ | $0.091^{**}$ | 0.624 |
| | | | (0.032) | (0.040) | |
| Lives in Nairobi | 837 | 0.891 | -0.023 | -0.049 | 0.416 |
| | | | (0.024) | (0.031) | |
| *Panel E. Household Assets and Living Conditions* | | | | | |
| Household has electricity | 837 | 0.849 | -0.023 | -0.041 | 0.645 |
| | | | (0.028) | (0.037) | |
| Household has piped water | 837 | 0.470 | 0.039 | 0.068 | 0.488 |
| | | | (0.035) | (0.043) | |
| Household has flush toilet | 837 | 0.388 | 0.056 | $0.076^{*}$ | 0.618 |
| | | | (0.034) | (0.041) | |
| Household owns a TV | 837 | 0.598 | -0.042 | -0.020 | 0.631 |
| | | | (0.039) | (0.046) | |
| Household owns computer | 837 | 0.080 | $-0.051^{***}$ | $-0.040^{**}$ | 0.486 |
| | | | (0.017) | (0.019) | |

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Table A6 – *Continued from previous page*

| | Obs. (1) | Control Mean (2) | Treatment Effects: Franchise Treatment (3) | Treatment Effects: Grant Treatment (4) | p-value: F = G (5) |
|---|---|---|---|---|---|
| Owns a personal mobile phone | 837 | 0.891 | -0.011 | -0.062* | 0.134 |
| | | | (0.025) | (0.034) | |
| Owns a personal SIM card | 837 | 0.950 | -0.011 | -0.010 | 0.984 |
| | | | (0.019) | (0.023) | |
| *Panel F. Consumption, Expenditures, and Savings* | | | | | |
| Food Insecurity Access Scale (out of 27) | 837 | 9.571 | -0.224 | 0.903 | 0.072 |
| | | | (0.512) | (0.621) | |
| Women's Dietary Diversity Score (scaled 0 to 9) | 805 | 4.745 | 0.156 | 0.049 | 0.484 |
| | | | (0.122) | (0.145) | |
| Expenditures on self and children (in USD) | 837 | 7.837 | 0.050 | -0.528 | 0.477 |
| | | | (0.735) | (0.845) | |
| Log of expenditures on self and children (in USD) | 837 | 1.266 | 0.172 | 0.045 | 0.365 |
| | | | (0.114) | (0.143) | |
| Spent money on tea, soda, or sweets in past week | 837 | 0.663 | 0.020 | 0.017 | 0.956 |
| | | | (0.036) | (0.043) | |
| Spent money on alcohol in past week | 837 | 0.059 | -0.036** | -0.025 | 0.514 |
| | | | (0.016) | (0.019) | |
| Transfers (in USD) | 837 | 2.725 | -0.006 | 0.286 | 0.524 |
| | | | (0.367) | (0.470) | |
| Log of transfers (in USD) | 837 | -0.859 | 0.216 | 0.168 | 0.849 |
| | | | (0.200) | (0.251) | |
| Savings (in USD) | 837 | 49.211 | -9.496 | 2.118 | 0.199 |
| | | | (7.967) | (9.783) | |
| Change in savings relative to last year | 837 | -0.139 | -0.169** | -0.156* | 0.875 |
| | | | (0.072) | (0.084) | |
| Paid school fees for self or own child in 2014 | 837 | 0.107 | -0.003 | 0.019 | 0.469 |
| | | | (0.024) | (0.030) | |
| Paid school fees for someone else's child in 2014 | 837 | 0.071 | 0.009 | 0.124*** | 0.001 |
| | | | (0.021) | (0.034) | |
| *Panel G. Time Use on Week Day Prior to Survey* | | | | | |
| Hours of income-generating activities | 837 | 2.746 | 0.283 | 0.498 | 0.623 |
| | | | (0.356) | (0.420) | |
| Self-employment hours | 837 | 0.364 | 0.287* | 0.431** | 0.534 |
| | | | (0.163) | (0.201) | |
| Hours of paid work for others | 837 | 2.382 | -0.004 | 0.067 | 0.862 |
| | | | (0.341) | (0.398) | |
| Hours of unpaid household work | 837 | 5.544 | 0.142 | -0.055 | 0.512 |
| | | | (0.258) | (0.299) | |
| Hours of unpaid work in a business | 837 | 0.393 | -0.085 | 0.023 | 0.421 |
| | | | (0.121) | (0.141) | |
| Hours of job search | 837 | 0.086 | -0.007 | 0.026 | 0.670 |
| | | | (0.055) | (0.074) | |
| Hours commuting or in transit | 837 | 0.166 | -0.062 | 0.265* | 0.029 |

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Table A6 – *Continued from previous page*

| | Obs. (1) | Control Mean (2) | Treatment Effects: Franchise Treatment (3) | Treatment Effects: Grant Treatment (4) | p-value: F = G (5) |
|---|---|---|---|---|---|
| | | | (0.050) | (0.155) | |
| Hours of leisure | 837 | 10.260 | -0.431 | -0.722** | 0.362 |
| | | | (0.276) | (0.314) | |
| Hours of education or training | 837 | 0.595 | 0.071 | -0.037 | 0.571 |
| | | | (0.167) | (0.181) | |
| Hours of religious observance, visiting the sick | 837 | 0.154 | 0.089 | 0.022 | 0.481 |
| | | | (0.096) | (0.092) | |
| *Panel H. Indices Capturing Empowerment, Self-Esteem, etc.* | | | | | |
| Rosenberg self-esteem scale (0 to 30) | 837 | 19.130 | 0.363 | -0.348 | 0.056 |
| | | | (0.310) | (0.370) | |
| Ladder of Life wellbeing scale (scaled from 0 to 10) | 837 | 6.491 | 0.139 | -0.000 | 0.280 |
| | | | (0.110) | (0.126) | |
| Grit (scaled from 1 to 5) | 837 | 2.006 | -0.004 | 0.006 | 0.737 |
| | | | (0.022) | (0.029) | |
| *Panel I. Empowerment Measures Used in Bandiera et al (2015)* | | | | | |
| Gender Empowerment Index (scaled 0 to 100) | 837 | 48.352 | -0.740 | 2.171 | 0.171 |
| | | | (1.749) | (2.096) | |
| Business Confidence Index (scaled 0 to 100) | 837 | 71.915 | 0.589 | -2.267* | 0.015 |
| | | | (0.942) | (1.175) | |
| Suitable age for a woman to marry | 837 | 24.828 | -0.357* | -0.205 | 0.530 |
| | | | (0.207) | (0.230) | |
| Suitable age for a man to marry | 837 | 28.281 | -0.328 | 0.085 | 0.182 |
| | | | (0.263) | (0.281) | |
| Desired age of marriage for daughter | 788 | 26.101 | -0.226 | -0.300 | 0.759 |
| | | | (0.206) | (0.238) | |
| Desired age of marriage for son | 813 | 28.856 | -0.283 | 0.073 | 0.209 |
| | | | (0.242) | (0.280) | |
| Suitable age for a woman to have a child | 837 | 24.891 | -0.294 | -0.161 | 0.672 |
| | | | (0.251) | (0.291) | |
| Number of children desired | 837 | 2.757 | 0.039 | 0.057 | 0.824 |
| | | | (0.069) | (0.084) | |
| Number of boys desired | 835 | 1.494 | -0.061 | 0.044 | 0.086 |
| | | | (0.054) | (0.065) | |
| Desired proportion of male children | 835 | 0.537 | -0.030** | 0.021 | 0.003 |
| | | | (0.014) | (0.018) | |
| *Panel J. Empowerment Measures Used in Adoho et al (2014)* | | | | | |
| Self Confidence Index (scaled from 1 to 6) | 837 | 4.257 | 0.066 | -0.094 | 0.133 |
| | | | (0.087) | (0.106) | |
| Respondent has her own money | 837 | 0.805 | 0.081*** | 0.066* | 0.660 |
| | | | (0.028) | (0.036) | |
| Controls money she earns from IGAs | 644 | 0.956 | 0.012 | 0.013 | 0.963 |
| | | | (0.017) | (0.022) | |
| Needs permission to spend earnings | 837 | 0.050 | 0.009 | 0.006 | 0.912 |

*Continued on next page*

Table A6 – *Continued from previous page*

| | Obs. (1) | Control Mean (2) | Treatment Effects: Franchise Treatment (3) | Treatment Effects: Grant Treatment (4) | p-value: F = G (5) |
|---|---|---|---|---|---|
| | | | (0.018) | (0.022) | |

Robust standard errors in parentheses. *, **, and *** indicate significance at the 90, 95, and 99 percent confidence levels, respectively. OLS regressions reported. All specifications include controls for baseline household size, education level, and indicators for having given birth, having received any vocational training, or having any paid work experience prior to the baseline survey, in addition to survey enumerator and survey month fixed effects. Money amounts are deflated to July 2013 levels using CPI data from the Kenya National Bureau of Statistics, then converted to US dollars using the average exchange rate from July 2013 (84.04 Kenyan shillings to the dollar). The top 1 percent of values of all hours and income variables are trimmed. The estimated impacts of the franchise treatment on the likelihood of working in the salon sector (see Panel A) or having done any vocational training (see Panel C) are significant at the 99 percent level after implementing the multiple hypothesis testing correction proposed by Benjamini and Hochberg (1995). Those assigned to the grant treatment are also more likely to have paid school fees for someone else's child in the year after receiving the grant (Benjamini-Hochberg q-value 0.01; see Panel F). No other outcomes are significantly related to either treatment with adjusted q-values below 0.05.