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Article

From Workfare to Economic and Sociopolitical Stability? Evidence from a Randomized Trial in Eastern Congo

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Abstract

Did a workfare program in Eastern Congo have a lasting impact on economic and sociopolitical outcomes? Men and women in Eastern Congo were randomly assigned to 2,674 four-month job offers, or to the job offer plus a savings incentive, hard-skills training, or both. Eighteen months after the program, labor market and savings outcomes have improved, but there is no change across 12 other economic and sociopolitical outcome families. Regarding labor market outcomes, the most intensive treatment—the job offer plus the savings incentive and hard-skills training—outperforms treatments with only one add-on. This indicates that the savings incentive and hard-skills training, when combined, can create a synergistic impact greater than the sum of their individual effects. The results are mainly driven by female beneficiaries, who start at much lower levels of labor market participation and earnings than men.

JEL classification: C93, E24, J2, O16

Keywords: workfare, urban poor, savings, training, gender, Democratic Republic of Congo, field experiment

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1. Introduction

In recent years, the development community has increasingly embraced workfare programs (Subbarao et al. 2013). These programs typically last only several months and self-select the poor through a low wage offer (Ravallion 2019). Even though of short duration, their ambition extends beyond immediate poverty alleviation. It is believed that the program's poverty alleviating effects can persist if participants invest a portion of their earnings in a productive activity (income effect) and/or on-the-job learning enhances their skills (skill acquisition effect), leading to sustainable increases in self-employment or employability (Gehrke and Hartwig 2018). Another ambition is to foster sociopolitical stability, especially in fragile and conflict-affected countries. Here, the assumption is that the workfare program stimulates prosocial behavior and diverts individuals from illicit activities (Blattman and Annan 2016; Stoop, Verpoorten, and Van der Windt 2019).

A recent metastudy by Bagga et al. (2023) concludes, however, that workfare programs' short-run effects on employment and earnings fade in the medium term, suggesting the need for revising program design. This study therefore investigates a program in which a random group of beneficiaries received, in addition to workfare, a savings incentive, hard-skills training, or both. The goal is to evaluate whether these enhancements have lasting effects on economic and sociopolitical outcomes across 14 pre-registered outcome families: labor market, savings and debt, other household earnings, household expenditures, household wealth, child schooling, remittances, social capital and participation, Prosocial behavior, authority, political attitudes, crime, women's empowerment, and psychological well-being.

The workfare program was implemented in the five largest cities in Eastern Congo, where the vast majority lives in extreme poverty in a context plagued by crime and violence (Raleigh et al. 2010; Mvukiyehe et al. 2016). It offered beneficiaries work for five days per week, for four months, at the daily minimum wage of 3 USD.[1] Across 25 neighborhoods, 13,805 individuals registered for the program. Subsequently, two public lotteries were organized in each neighborhood—one for men and one for women—to randomly allocate individuals to the 2,674 available job offers. The program was implemented following a two-by-two factorial design with the savings component and/or hard-skills training component in addition to the job offer. In total, 25 projects—1 in each neighborhood—were implemented between November 2016 and December 2018. To measure program impact, a household survey was conducted with a targeted 7,524 respondents. These data were collected between May 2019 and August 2019; on average, 18 months after program end.

This study makes four contributions to the literature. First, it investigates whether a savings incentive can strengthen the workfare impact. Savings can provide a safety net against shocks, reducing the likelihood of falling (back) into poverty (Deaton 1991). Savings can also encourage long-term planning, inciting participants to invest in income-generating activities and assets (Karlan, Ratan, and Zinman 2014). Studies that evaluate the impact of savings incentives in isolation document positive effects on savings, but mixed findings on downstream outcomes such as assets, education, or health (cf. review in Dupas et al. 2018). While a number of previous workfare programs have offered a savings incentive, at times only because wage payments were mandated to be deposited into a household bank account (e.g., Ravi and Engler 2015), no study has investigated this add-on experimentally. In this study, a random subset of beneficiaries received a bank account and the option to save 1 USD (out of the 3 USD daily workfare pay), incentivized by a 1 USD extra-pay, directly placed in their account and available only by the end of the project. The data show that, on average 18 months after project end, beneficiaries assigned to the savings incentive—compared to those that received only the workfare program—have significantly more bank savings. There is, however, little evidence of impact on other outcomes.

1 In 2018, 3 USD per day had a local purchasing power of 6 USD (World Bank 2020a).

Second, this study investigates whether hard-skills training increases the effectiveness of workfare programs. Previous work has found that only well-tailored and well-targeted trainings can improve economic outcomes, but even then rarely pass a cost-benefit test (e.g., Blattman and Ralston 2015; Maitra and Mani 2017; McKenzie 2017). While several workfare programs provided technical, vocational, or business trainings (e.g., Galasso et al. 2004), only one workfare study has experimentally examined the additional impact of such training. Bertrand et al. (2021) uncovered that incorporating either a four-week basic entrepreneurship training or job-search skills training resulted in a small but enduring earnings increase for youth. Here, the present study investigates the additional impact of a 12-week hard-skills training. A random subgroup of program beneficiaries was trained by the Congolese public agency for workforce training on specific professional skills—such as construction and driving lessons for men and sewing, cooking, and hairdressing for women. Although beneficiaries could choose from a menu of training options, carefully selected based on a city's needs assessment, the data provide no evidence that adding this training enhanced the impact of the workfare program on labor market outcomes or on any of the other outcome families.

Third, the factorial design makes it possible to assess the joint effect of the savings and hard-skills training components. Individuals who enjoyed both treatments, in addition to the job offer, may use their savings to finance a business start-up that capitalizes on their newly acquired skills, for instance by purchasing tools to launch and sustain entrepreneurial initiatives. This may be important when capital constraints are limiting business start-up and growth (De Mel, McKenzie, and Woodruff 2008). The savings can also provide a buffer to help safeguard the business's viability in the face of shocks, such as unexpected expenses, a temporary economic downturn, or other unforeseen challenges. Finally, savings alleviate financial stress, potentially leading to more well-calculated business choices (Mani et al. 2013). This study's findings point to such a synergistic impact, with better labor market outcomes for those beneficiaries who received both the savings and hard-skills training component instead of only one. Suggestive evidence indicates that this may be driven by the beneficiaries' ability to purchase productive assets. For instance, the data reveal a pronounced increase in sewing machine ownership, which is a clear complement to the most popular training: sewing.

Finally, the study design makes it possible to learn about differential gender effects because of the separate public lotteries for men and women in each neighborhood. There are good reasons to believe that the benefits of the workfare program are larger for women than for men. At baseline, a lower share of women are employed; the workfare program is thus less likely to crowd out women's private labor supply (e.g., Bertrand et al. 2021). Furthermore, the impact of a savings incentive that provides access to a formal bank account may also be larger for women if they save less securely at home than men (Ashraf, Karlan, and Yin 2010; Dupas and Robinson 2013). In addition, starting from a lower knowledge and skills base, female beneficiaries may benefit more from training programs (e.g., Chinen et al. 2017). These benefits may in turn boost women's empowerment (Pankaj and Tankha 2010), although such empowerment may also trigger backlash in the form of increased gender-based violence (Amaral, Bandyopadhyay, and Sensarma 2015). Results indicate that the improvements in labor market outcomes are mainly driven by female beneficiaries. However, across all other outcome families, including women's empowerment, there is no evidence for improvement.

In sum, as measured 18 months after program end, the workfare program succeeded in improving labor market outcomes, mainly for women, and mostly when complemented with both a savings incentive and a hard-skills training. The program also led to more savings, especially when combined with a savings component, but there is no evidence that the workfare program sustainably lifted people out of poverty, nor evidence of knock-on effects on sociopolitical outcomes.

Section 2 anchors the study in the Congolese context and describes the intervention. Section 3 lays out the experimental design and empirical strategy. Section 4 presents results. Section 5 concludes.

2. Background

Economic and Sociopolitical Context

The workfare program took place in the five largest cities of Eastern Congo: Bukavu in South Kivu province; Goma, Butembo, and Beni in North Kivu province, and Bunia in Ituri (for a map of the research area, see fig. S1.1 in the supplementary online appendix, available with this article at The World Bank Economic Review website). The region is characterized by protracted conflict, widespread poverty, and large gender inequalities.

Despite the 2003 formal peace deal that put an end to the Second Congo War (1998–2003), violence in Eastern Congo has continued. During the workfare program (2016–2018), over 150 armed groups operated in the region, more than 2,100 violent events took place,[2] and an estimated 4.5 million people were internally displaced (OCHA 2018). Most armed groups recruit locally, drawing on individual material and nonmaterial motivations to fight (e.g., Stoop and Verpoorten 2021). While the groups have their hide-outs in rural areas, recruitment drives in cities are not uncommon (Raleigh et al. 2010).

In 2012, 77 percent of Congolese were estimated to live below the international 1.9 USD purchasing power parity (PPP) extreme poverty line (World Bank 2020b). During the program period, the economic climate deteriorated. At the start of the study period, in 2016, GDP per capita growth turned negative (at −0.8 percent) in the wake of slumping copper and cobalt prices. In the three-year period that followed, annual GDP per capita growth was just 1.3 percent on average (World Bank 2020c). The economic downturn affected the Congolese population through public expenditure cuts, currency depreciation, and inflation (Cassimon, Verbeke, and Verpoorten 2016).

Recent and disaggregated data for the five study cities are unavailable. The study therefore undertook a representative survey in the five study cities between June and August 2015, before the start of the program.[3] The data highlight that the typical household is poor, unemployment is rampant, savings are low, and only a minority of respondents ever enjoyed professional training. Median monthly household consumption was 149 USD PPP (World Bank 2020a), or—given a median household size of six individuals–0.83 USD PPP per day per person. Almost 6 out of 10 respondents reported being unemployed.

Men were 10 percentage points more likely to be employed than women, and employed men reported more than double the earnings of employed women. Only 11 percent of women and 14 percent of men reported having saved in the three months before the interview, with the median monthly sum set aside just 7 USD. Among respondents, 35 percent had received some professional training during their life. These trainings, which include internships and apprenticeships, are more common among men (44 percent) than women (27 percent), and among the asset rich (45 percent for richest quintile) than the asset poor (23 percent for poorest quintile).

The baseline data also confirm that the cities were highly insecure. Almost 1 out of 4 respondents mentioned armed group activity in their neighborhood in the 6 months before the survey, and 7 out of 10 respondents mentioned the occurrence of a burglary, armed robbery, murder, or other security incident.

In this context of poverty and unemployment, a workfare program that targets the poor may create a large net gain rather than crowd out existing employment, especially among women. Furthermore, both the savings and skills deficit, especially among the poorest and women, may render the saving incentives and hard-skills trainings as part of the workfare program beneficial. Amidst the high crime rates, the workfare program could provide an alternative for unlawful activities, thereby yielding significant sociopolitical benefits. Conversely, the insecure environment could also depress returns to skills training and other investments.

2 Authors' calculation based on ACLED data (Raleigh et al. 2010).

3 More details can be found in Mvukiyehe et al. (2016).

The Intervention

The workfare program was funded by the International Development Association and part of the "Productive Opportunities for Stabilization and Recovery" program in Eastern Congo (STEP, by its French acronym), which had as goal to contribute not only to the region's economic recovery but also to its sociopolitical stability. Between November 2016 and December 2018, workfare projects were implemented in 25 neighborhoods across the 5 cities.[4] The projects were implemented by local NGOs, under the supervision of a Congolese state organization (the Congolese Social Fund, FSRDC). Most projects related to public roads: resurfacing, drainage systems construction, construction of stairs to access roads, and rehabilitation of bridges. Section S2 in the supplementary online appendix provides information for each project.

Beneficiaries received employment for four months, paid at the minimum wage of 3 USD per day. Workers provided labor for eight hours per day, for five days per week. In the week before project start, all beneficiaries received a soft-skills training, which covered topics like civic education, health and safety at the work place, and financial literacy. Participants received 3 USD per day for attendance. Thus, an individual that followed the soft-skills training and worked for four months would earn a total of 275 USD. Furthermore, a subset of beneficiaries received additional components: a savings incentive and/or a hard-skills training program.

Beneficiaries assigned to the savings component were incentivized to save 1 USD a day (out of their 3 USD pay) against a 1 USD extra-pay, directly placed in their own savings account (opened at no cost) and available only by the end of the project. Beneficiaries of the savings scheme could choose each day whether to be paid 3 USD, or 2 USD directly plus 2 USD in their account. An individual working for four months and who chose to save every day, would accumulate around 365 USD.[5]

Beneficiaries of the hard-skills training were trained by the INPP, the Congolese National Institute for Professional Preparation. The menu of training options differed by city, and were chosen based on a market study of local economic conditions. Subsequently, INPP held individual meetings with training beneficiaries to help them select a skill they wanted to learn. Trainings took place at the end of the workfare project, and lasted for about 60 days. Training participants were paid 3 USD per day, and were thus able to add 180 USD to the 275 USD workfare pay, yielding a total of 455 USD.[6]

3. Experimental Design and Empirical Strategy

Treatment Assignment

Individuals were randomly assigned to treatment using public lotteries, which were organized per neighborhood and by gender. Information about the upcoming project, and how to register for it, was given through radio, churches, and a public speaker system. Individuals older than 18 years and living in the neighborhood could sign up during a two-day period. Upon registration, personal details about the individual were recorded. The lottery typically took place two days after the registration drive and was gender-specific; that is, each neighborhood had one lottery for men and another for women.

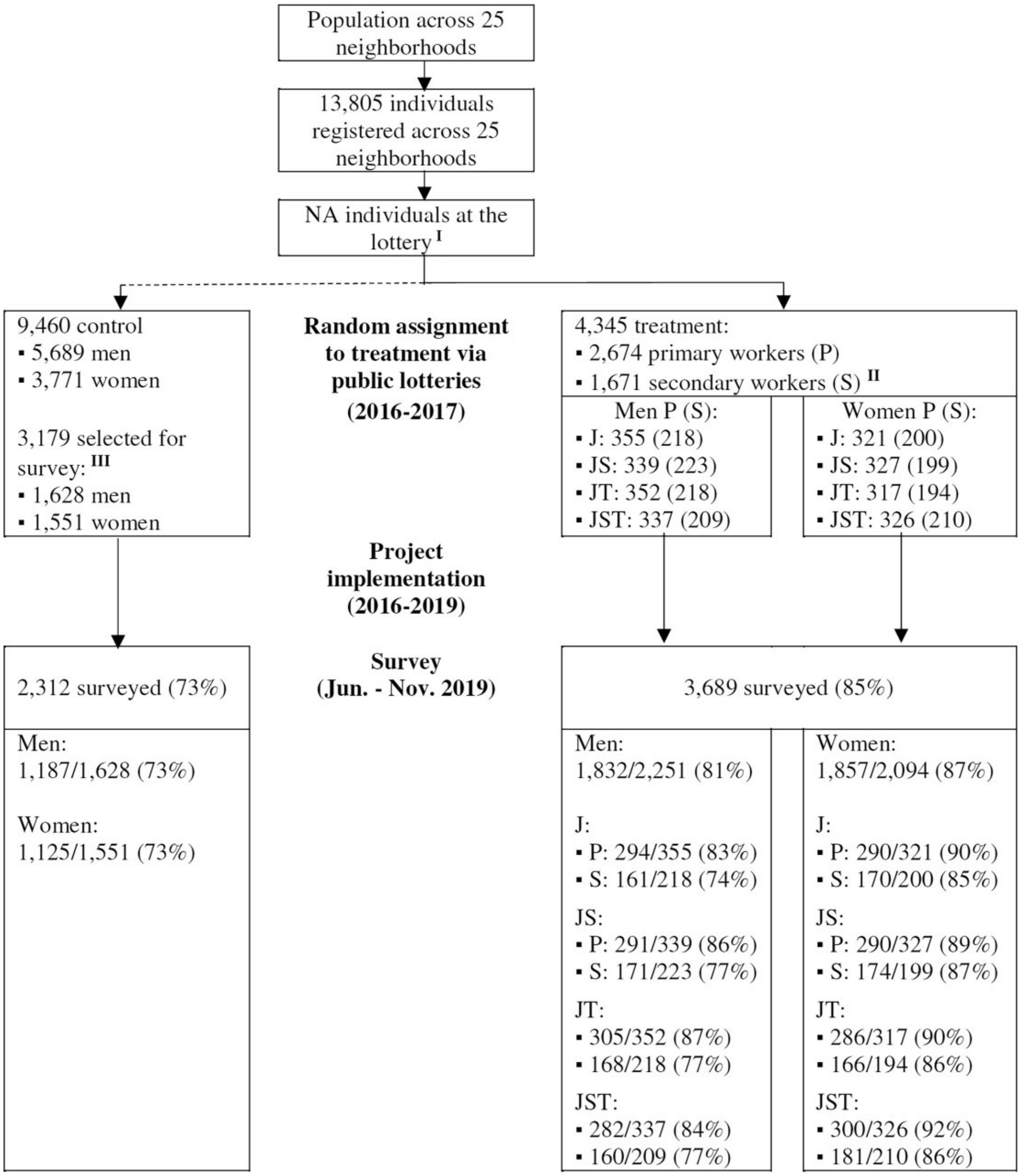
A Consort diagram summarizes the randomization and data collection (fig. 1). The four treatment arms are indicated as follows: J for job offer only, JS for job offer plus savings incentive, JT for job offer plus training, and JST for job offer plus savings incentive and training. In total, 13,805 individuals—5,865 women and 7,940 men—registered for 2,674 available jobs. In total, 4,345 individuals were randomly assigned to one of the four treatment arms; 2,674 were selected as "primary workers," and the remaining

4 The city's mayor chose which neighborhoods received a project.

5 The market deposit interest rate was 4 percent and inflation reached 30 percent in 2017 (IMF 2021).

6 Those assigned to both the savings and the trainings components were thus able to accumulate 545 USD.

Figure 1. CONSORT Diagram.



Source: Authors' calculations based on administrative and survey data.

Note: Lottery and survey were stratified by gender. [I] Attendance was not recorded at the lottery. Thus, all individuals registered but not selected for treatment are part of the control sampling frame. [II] Secondary workers were selected, among others, because primary workers may start other (re)productive activities or fail the mandatory physical test. [III] The initial target was the same number as primary workers. Additional funds made it possible to survey more individuals in Goma and Bukavu, thus targeting 3,179 control individuals.

1,671 as "secondary workers." The secondary workers were informed that they were on the wait list, and would be contacted—following the random order of selection—if a primary worker would drop out. Drop-outs were expected to occur because primary workers could fail to pass the mandatory physical test or start another (re)productive activity between the lottery and project start.

Individuals registered but not selected for treatment served as the control sampling frame. Among them, 3,179 individuals were selected for the survey.[7] Lottery attendance was not recorded; the control sampling frame thus includes those that did not attend the lottery. Lotteries, however, took place only two days after registration, and field coordinators indicate almost full lottery attendance. Section S3 in the supplementary online appendix shows that the lottery created control and treatment groups that are comparable in terms of observable baseline characteristics collected at registration.

Data Collection and Outcome Measures

The study builds on a large household survey targeting the 3,179 control individuals plus all 4,345 individuals selected during the lottery. The data collection took place from June to November 2019, on average 18 months after the workfare project ended, but with substantial variation across neighborhoods.[8] In total, 3,689 surveys (85 percent) were conducted among treatment individuals (fig. 1). The attrition rate, at 15 percent, is similar among the different treatment arms—J, JS, JT, and JST—ensuring the validity of the impact comparison across arms. Response rates are somewhat lower among men (81 percent) than among women (87 percent). The attrition rate in the control group, however, is considerably higher, at 27 percent, for both men and women. This attrition rate is higher than in other workfare studies, but not unusual in urban settings where people often exhibit greater mobility (Bagga et al. 2023). It is also typical to observe more attrition among control individuals, because they may have a lower willingness to answer survey questions, and they may be harder to track as they are more likely to seek options outside their neighborhood (Greenberg and Barnow 2014).[9]

The survey collected data for 14 outcome families, which are composed of 77 individual variables (section S4 in the supplementary online appendix presents definitions and summary information for each variable). The first two outcome families relate to the program's proximate economic benefits: *Labor market:* Whether the individual currently has a job, the weekly hours worked, and the monthly earnings: *Savings and debt:* Whether the individual has formal savings and the amount of formal savings, as well as the use and amount of formal borrowing.

The next five families relate to more indirect economic effects. These are *Other earnings:* The share of household members other than the respondent that work, and their combined earnings; *Household expenditures* on food and nonfood items; *Household wealth* composed of several categories of asset ownership; *Child schooling,* which also includes information on child labor; and *Remittances* sent and received.

Finally, seven outcome families capture sociopolitical factors. *Social capital and participation* includes membership to associations, participation in collective action, and trust; *Pro- and anti- social behaviors* measures own or friends' involvement in illicit activities and acts of violence; *Authority and service provision* probes about one's relation with and perception of local and national political authorities; *Political attitudes and participation* measures the frequency of discussing politics, voting behavior, and contact with government agents and civil society actors; *Crime, conflict, and violence* captures exposure to crime;

7 The control sample was block randomly selected. First, treatment and control individuals from the same lottery, that is, neighborhood and gender, were grouped together. Next, blocks were created based on individuals' age (younger than 30 or not), literacy, and migrant status. To account for this block randomization, the study controls for literacy, youth, and migrant status in a robustness check (section S3 in the supplementary online appendix); results are very similar to those reported in the main text.

8 The minimum period between survey and project end is five months, whereas the maximum is 26 months (section S2 in the supplementary online appendix). The study leverages this variation below.

9 Section S3 in the supplementary online appendix presents further details on attrition, a test for attrition (Ghanem, Hirshleifer, and Ortiz-Beccera 2024), and robustness checks to differential attrition by—leveraging the study's design—focusing solely on those lotteries with low levels of differential attrition, as well as by using the bounding approach proposed by Lee (2009). These robustness tests are discussed in more detail below.

Women's empowerment is measured by autonomous decision making, perceptions on female political participation and gender-based violence; and *Psychological well-being* is based on standard instruments that measure life satisfaction and mental health.

Estimation of the Mean Treatment Effects

The analysis estimates:

$$Y_{ij} = \beta_0 + \beta_1 J_{ij} + \beta_2 JS_{ij} + \beta_3 JT_{ij} + \beta_4 JST_{ij} + \alpha_l + \varepsilon_{ij} \tag{1}$$

where Y_{ij} is the outcome measure of individual i in neighborhood j.[10] J_{ij} is a binary variable indicating whether the individual participated in the baseline workfare program, receiving only the job offer. JS_{ij}, JT_{ij}, and JST_{ij} indicate whether the individual received a job offer plus the savings component, the hard-skills training component, or both, respectively. This empirical setup allows each cell in the factorial design to be compared to the control group. Lottery fixed effects, α_l, totaling 50 neighborhood-gender lotteries, are included because they are the randomization blocks.

As outlined above, the study examines the impact of the intervention on 77 individual variables. To reduce the number of statistical tests and to avoid over-rejection of the null hypothesis due to multiple inference (Anderson 2008), a commitment was made in advance to adopt a mean effects approach and combine the individual measures into 14 outcome families (following Kling, Liebman, and Katz 2007). The outcome families and variables were preregistered.[11] In addition, sharpened q-values are reported to account for the false discovery rate for tests across many prespecified outcomes.

4. Results

After introducing the sample and discussing take-up, this section shows results of the workfare program, and explores differences in impact by gender.

The Sample

Section S6 in the supplementary online appendix reports descriptive information for control respondents by gender. As expected, due to separate lotteries by gender, there is gender parity among respondents (51.4 percent of respondents are male). The typical man is 33 years old, the household head, married, literate, and has received 9 years of formal education. The women in the sample are less likely to be the household head (27 instead of 70 percent), less likely to be literate (72 versus 87 percent), and have received about 2 years less education (7 versus 9 years).

Over half of the men (59 percent) reported having an income-earning activity. Among them, 38 percent are self-employed. Women are less likely to have an income-earning activity (47 versus 59 percent) and—when they do—they are much more likely to be self-employed (61 versus 38 percent). Finally, 25 percent of men and 18 percent of women in the control group received a hard-skills training since program start. Men most often reported trainings for painter/plasterer (15 percent) and mechanic (14 percent); while seamstress (43 percent) and cook (11 percent) were the most popular trainings among women.

Program Take-Up

The projects were implemented between January 2017 and January 2019. Administrative data on who took up the job offers are unavailable. However, self-reported data from the household survey are in line with treatment assignment (details in section S3 in the supplementary online appendix). Among those assigned as either primary or secondary workers, about 52 percent of men and 58 percent of women

10 Lacking neighborhood population size, it is not possible to estimate population treatment effects.

11 Section S5 in the supplementary online appendix lists deviations from the pre-analysis plan.

report that they were offered a job by FSRDC or an NGO working on behalf of FSRDC. As expected, take-up rates are stronger when focusing solely on primary workers; 61 percent for men and 68 percent for women. Nevertheless, these take-up rates are on the low side compared to other workfare programs, which typically score above 80 percent (Bagga et al. 2023).[12] As expected given random assignment, very similar shares were offered a job across treatment arms. Among those who were offered a job, virtually all (98 percent) accepted.

Respondents assigned to the treatment arms that include the savings component were, as expected, significantly more likely to report having a bank account. Men in the JS treatment were 14 percentage points more likely to have a bank account than those in the control group (13 percent); JS women were 17 percentage points more likely to have a bank account than the control women (8 percent).[13]

Those assigned to the treatment arms that included the training component were more likely to report that they received training. Men assigned to JT are 27 percentage points more likely to have had training compared to 15 percent for control men; these numbers are 37 percentage points for JT women, compared to 18 percent for control women. The most popular trainings were sewing and weaving (26 percent), driving lessons (16 percent), cooking and catering (14 percent), and hairdressing and aesthetician (9 percent). Other trainings were car repair, masonry, soap making, agriculture and farming, woodworking, using a computer, wiring, plumbing, home appliance repair, and welding.

In what follows, intention-to-treat effects are presented, combining the information from both primary and secondary workers. These give unbiased estimates but may underestimate the impact of the program, given that not everybody assigned to treatment was actually treated.[14]

Labor Market Outcomes

The labor market outcome family includes three individual variables: whether the respondent has a job, the number of hours worked per week, and monthly earnings. Table 1 presents the estimated impact of each treatment arm for these three variables and for their mean effect; the latter is presented in standard deviations (SD); p-values and sharpened q-values are mentioned in parentheses and square brackets, respectively.

The results show that only for the most intensive treatment, JST, the estimated impact is positive and significant across all three variables and their index. Eighteen months after median project end, beneficiaries in that treatment arm are 5.9 percentage points more likely to have a job compared to the control of 53 percent ($p < 0.05$). JST respondents also work 3.5 more hours per week ($p < 0.01$) and earn 11.7 USD more in a month ($p < 0.05$), compared to control respondents' 23 hours and 56 USD, respectively. These effects are sizeable. Sharpened q-values are all below 0.02, reducing concerns that these are false positives.

The tests of difference between treatment arms, reported in the bottom panel of the table, indicate that JST outperforms JS and JT. Indeed, the estimated effect for any of these add-ons in isolation is considerably smaller in size than that of their combined effect; focusing on the mean effects these differences are statistically significant (both at $p < 0.05$). This suggests that a shortage of start-up (or back-up) financing and a lack of skills act as mutually reinforcing constraints in this study context. This may be attributed

12 Take-up rates reported by control men and women are 14 and 8 percent, respectively. This finding may be due to the self-reported nature of the data, whereby respondents may have confounded other job offers with FSDRC program jobs. However, leakage of jobs may have taken place, as the high poverty and unemployment rate in the research context may create fierce competition and possibly corruption, even for access to low-paying jobs (Moshanos 2019).

13 Not all individuals assigned to JS and JST are expected to have a bank account at the time of the survey, which is 18 months after program end, because there is a cost to maintaining a bank account; this cost was no longer covered by the program after program end.

14 Compliance was larger among primary than secondary workers. The analysis is repeated, looking solely at the primary workers. As expected, results are stronger (see section S3 in the supplementary online appendix).

Table 1. Labor Market

	Has a job (1)	Weekly hours worked (2)	Monthly earnings (3)	Mean effect (4)
J	0.054***	1.743*	1.226	0.073*
	(0.005)	(0.088)	(0.779)	(0.052)
	[0.016]	[0.124]	[0.464]	[0.090]
JS	0.024	0.655	−4.233	0.014
	(0.205)	(0.520)	(0.332)	(0.702)
	[0.213]	[0.464]	[0.285]	[0.464]
JT	0.041**	1.322	−1.581	0.046
	(0.034)	(0.194)	(0.717)	(0.218)
	[0.068]	[0.213]	[0.464]	[0.213]
JST	0.059***	3.533***	11.712***	0.140***
	(0.002)	(0.001)	(0.007)	(0.000)
	[0.010]	[0.004]	[0.019]	[0.004]
Mean control	0.532	23.280	56.159	0.000
JS = J	(0.182)	(0.362)	(0.286)	(0.183)
JT = J	(0.541)	(0.724)	(0.584)	(0.543)
JT = JS	(0.468)	(0.575)	(0.603)	(0.467)
JST = J	(0.828)	(0.134)	(0.041)	(0.131)
JST = JS	(0.120)	(0.016)	(0.002)	(0.004)
JST = JT	(0.407)	(0.063)	(0.009)	(0.034)
Observations	6001	6001	6001	6001

Source: Authors' analysis based on survey data from primary and secondary workers.
Note: Fixed effects at lottery level. Mean effect magnitudes in standard deviations. Mean of the family measure in the control areas is zero by design; p-values in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (two-tailed). Sharpened q-values in square brackets control the false discovery rate for tests across prespecified outcomes. Tests of difference between treatment arms use the same empirical model only changing the comparison group, and report p-values.

to the dominance of self-employment (75 percent), for which success requires not only a specific skill set but also capital. Alternatively, the JST effect could be due to an income effect; the savings and skills components add 85 and 180 USD of income, respectively, in addition to the 275 USD earned in the baseline treatment. This alternative explanation matches the results less well, however, as one would then expect JS and JT beneficiaries to perform better than J beneficiaries, which is not the case.

As part of the household survey, data were collected on asset ownership, including what one may term productive assets: irons, sewing machines, bicycles, and motorbikes. The analysis uses these data to explore the effect of the synergistic impact between savings and hard-skills trainings in addition to the workfare program. Ownership of sewing machines (and to a lesser extent irons and motorbikes) increased substantially, among JT and especially JST beneficiaries (table 2). Compared to the control mean of 0.07, JST beneficiaries own almost double as many sewing machines. This suggests that assets were acquired to complement the most popular training, which was sewing. The bottom panel shows that the JST impact is significantly stronger than that for J, JS, and JT for sewing machines ($p < 0.05$), and stronger than JS for irons ($p < 0.10$). The JST impact on motorbikes is not stronger than JS or JT. This may be because driving lessons were a less popular training than sewing. Also, in large Congolese cities there are rent-to-purchase schemes for bikes whereby individuals do not immediately own the motorbike; such contracts for sewing machines do not seem to be available.

Savings and Debt

Table 3 shows results for savings and debt: whether the respondent saved on a deposit account in the previous 12 months, the amount of formal savings, whether the respondent used formal borrowing since the start of the program, and the amount of formal debt. Columns 1 and 4 show that all treatment arms,

Table 2. Productive Urban Assets

	Owns Iron (1)	Owns sewing machine (2)	Owns Bicycle (3)	Owns motorbike (4)
J	0.041**	0.002	0.003	0.014
	(0.034)	(0.881)	(0.807)	(0.292)
	[0.171]	[0.987]	[0.987]	[0.498]
JS	0.017	− 0.017	− 0.004	0.012
	(0.383)	(0.140)	(0.725)	(0.369)
	[0.547]	[0.325]	[0.910]	[0.547]
JT	0.047**	0.027**	0.010	0.025*
	(0.015)	(0.016)	(0.362)	(0.065)
	[0.104]	[0.104]	[0.547]	[0.239]
JST	0.055***	0.060***	0.003	0.022*
	(0.004)	(0.000)	(0.749)	(0.097)
	[0.043]	[0.001]	[0.910]	[0.314]
Mean control	0.412	0.071	0.072	0.114
JS = J	(0.286)	(0.165)	(0.610)	(0.894)
JT = J	(0.797)	(0.053)	(0.571)	(0.500)
JT = JS	(0.184)	(0.001)	(0.280)	(0.417)
JST = J	(0.518)	(0.000)	(0.949)	(0.605)
JST = JS	(0.086)	(0.000)	(0.565)	(0.514)
JST = JT	(0.696)	(0.011)	(0.614)	(0.875)
Observations	6001	6001	6001	6001

Source: Authors' analysis based on survey data from primary and secondary workers.
Note: Fixed effects at lottery level. Mean effect magnitudes in standard deviations. Mean of the family measure in the control areas is zero by design; p-values in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (two-tailed). Sharpened q-values in square brackets control the false discovery rate for tests across prespecified outcomes. Tests of difference between treatment arms use the same empirical model only changing the comparison group, and report p-values.

including those that did not feature the savings incentive, have a positive and significant impact on whether the respondent formally saved and the overall family index, respectively. The effects are sizeable, with a 4.3 to 6.6 percentage points increase in the probability of having saved on a bank account, relative to the control average of 7.9 percent. In the first week of the program, all beneficiaries participated in soft-skill training sessions that included information on the benefits of saving and the pitfalls of accumulating debt, which may explain the overall positive effect across treatment arms. The treatment arms that included the saving incentive (JS and JST), however, do stand out when it comes to the amount currently in savings. Whereas those in the control group have about 15 USD in savings, those in groups JS and JST have 22 USD more in savings.[15] The other treatment arms have no impact on the amount saved formally.[16] The bottom panel corroborates the findings that the impact of JS and JST are significantly stronger than that of JT and J (all $p < 0.01$).

Other Economic and Sociopolitical Outcomes

Estimated impacts across the indices of the other 12 outcome families are not statistically significant (table 4). [17]

15 The higher savings may also result from the larger income obtained by beneficiaries in the savings treatment: 365 USD versus 275 USD if they chose to save each day. This does not entirely drive the result however, as the savings rate is higher in the JS group than in the J group, at 10.1 percent (37/365) versus 5.5 percent (15/275).

16 Note, however, that, in the specific study context, more savings in a bank account is not necessarily a good economic decision. In the study period, the Congolese franc lost 71 percent of its value, and prices rose by 66 percent (IMF 2021).

17 table S8.1 in the supplementary online appendix shows results for the individual variables that make up the outcome families.

Table 3. Savings and Debt

	Savings and debt				
	Used formal saving (1)	Amount formal savings (2)	Used formal borrowing (3)	Amount formal debt (4)	Mean effect (5)
J	0.059***	− 0.698	0.002	− 0.439	0.091**
	(0.000)	(0.839)	(0.756)	(0.875)	(0.024)
	[0.001]	[0.502]	[0.502]	[0.502]	[0.027]
JS	0.043***	22.360***	− 0.002	− 2.081	0.223***
	(0.001)	(0.000)	(0.770)	(0.453)	(0.000)
	[0.001]	[0.001]	[0.502]	[0.319]	[0.001]
JT	0.066***	2.713	− 0.017**	− 4.098	0.188***
	(0.000)	(0.429)	(0.026)	(0.139)	(0.000)
	[0.001]	[0.319]	[0.027]	[0.117]	[0.001]
JST	0.054***	22.759***	− 0.010	− 1.945	0.251***
	(0.000)	(0.000)	(0.217)	(0.484)	(0.000)
	[0.001]	[0.001]	[0.177]	[0.320]	[0.001]
Mean control	0.079	15.413	0.050	29.444	0.000
JS = J	(0.286)	(0.000)	(0.607)	(0.614)	(0.005)
JT = J	(0.656)	(0.396)	(0.030)	(0.261)	(0.041)
JT = JS	(0.129)	(0.000)	(0.097)	(0.534)	(0.452)
JST = J	(0.709)	(0.000)	(0.187)	(0.644)	(0.001)
JST = JS	(0.487)	(0.921)	(0.419)	(0.967)	(0.551)
JST = JT	(0.412)	(0.000)	(0.396)	(0.507)	(0.178)
Observations	6001	5990	6001	5943	6001

Source: Authors' analysis based on survey data from primary and secondary workers.
Note: Fixed effects at lottery level. Mean effect magnitudes in standard deviations. Mean of the family measure in the control areas is zero by design; *p*-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (two-tailed). Sharpened q-values in square brackets control the false discovery rate for tests across prespecified outcomes. Tests of difference between treatment arms use the same empirical model only changing the comparison group, and report *p*-values.

Program Impact By Gender

Here the analysis builds on the study's design—separate public lotteries for men and women in each neighborhood—to explore differential effects by gender; that is, differences between households where a man versus a women has participated in the program. The study focuses on the labor market and savings and debt outcomes.[18]

Table 5 shows results for labor market outcomes, with panel A (B) reporting results for men (women), and panel C assessing the differential impact across men and women.[19] First, the differences in control mean in panel C reveal that, compared to control men, control women are 12.3 percentage points less likely to have a job, work 8.5 hours less per week, and earn 41.0 USD less per month. Column 4 in panel A shows that only the JST treatment arm has a weakly significant impact on the labor index for men, but only so using p-values. This result contrasts with strong and significant effects for women for all four treatment arms (column 8, p and $Q < 0.01$). Indeed, compared to control women, women who received the workfare program plus savings and training are 8.9 percentage points more likely to have a job (up from 47 percent, $p < 0.01$), work about 5.1 hours more per week (versus 18.9 hours, $p < 0.01$), and have

18 Section S8 in the supplementary online appendix shows results for the individual variables of all outcome families for men and women, respectively.

19 That is, model (1) is re-estimated but with the addition of a gender indicator and gender interactions, and with neighborhood fixed effects. The study reports the coefficients on these interactions, which equal the difference in effects between men versus women.

Table 4. Program Impact by Treatment Arm

	Other earnings (3)	Household expenditure (4)	Household wealth (5)	Child schooling (6)	Remittances (7)	Social capital and participation (8)	Prosocial behaviors (9)	Authority (10)	Political attitudes (11)	Crime (12)	Psychological wellbeing (14)	Women's empower-ment (13)
J	− 0.055	0.008	0.028	0.036	0.012	− 0.001	− 0.015	− 0.054	− 0.033	− 0.016	0.031	− 0.036
	(0.162)	(0.828)	(0.463)	(0.406)	(0.752)	(0.983)	(0.706)	(0.149)	(0.384)	(0.666)	(0.409)	(0.358)
	[0.827]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[0.827]	[1.000]	[1.000]	[1.000]	[1.000]
JS	− 0.042	0.082**	− 0.012	− 0.003	0.053	0.047	0.026	− 0.040	0.018	− 0.073*	− 0.002	− 0.014
	(0.288)	(0.028)	(0.746)	(0.951)	(0.167)	(0.187)	(0.506)	(0.285)	(0.638)	(0.055)	(0.960)	(0.718)
	[1.000]	[0.219]	[1.000]	[1.000]	[0.827]	[0.915]	[1.000]	[1.000]	[1.000]	[0.362]	[1.000]	[1.000]
JT	0.004	0.041	0.053	− 0.006	0.087**	− 0.011	0.039	0.016	0.033	− 0.093**	0.054	− 0.047
	(0.925)	(0.267)	(0.161)	(0.881)	(0.024)	(0.757)	(0.314)	(0.677)	(0.375)	(0.014)	(0.150)	(0.220)
	[1.000]	[1.000]	[0.827]	[1.000]	[0.215]	[1.000]	[1.000]	[1.000]	[1.000]	[0.173]	[0.827]	[0.929]
JST	− 0.062	0.031	0.045	0.023	0.060	− 0.017	0.006	− 0.008	− 0.009	0.027	− 0.008	0.048
	(0.114)	(0.404)	(0.235)	(0.596)	(0.117)	(0.634)	(0.883)	(0.836)	(0.809)	(0.481)	(0.839)	(0.213)
	[0.819]	[1.000]	[0.964]	[1.000]	[0.819]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]	[0.929]
JS = J	(0.773)	(0.092)	(0.366)	(0.445)	(0.364)	(0.253)	(0.373)	(0.747)	(0.252)	(0.205)	(0.454)	(0.632)
JT = J	(0.202)	(0.447)	(0.571)	(0.405)	(0.098)	(0.806)	(0.237)	(0.112)	(0.133)	(0.085)	(0.603)	(0.795)
JT = JS	(0.322)	(0.353)	(0.140)	(0.939)	(0.452)	(0.163)	(0.770)	(0.204)	(0.722)	(0.649)	(0.203)	(0.459)
JST = J	(0.878)	(0.598)	(0.698)	(0.799)	(0.286)	(0.697)	(0.654)	(0.290)	(0.590)	(0.332)	(0.379)	(0.064)
JST = JS	(0.658)	(0.245)	(0.196)	(0.613)	(0.873)	(0.124)	(0.658)	(0.461)	(0.543)	(0.025)	(0.896)	(0.169)
JST = JT	(0.152)	(0.815)	(0.857)	(0.563)	(0.554)	(0.886)	(0.462)	(0.594)	(0.335)	(0.007)	(0.161)	(0.034)
Obs.	6001	6001	6001	4679	6001	6001	6001	6001	6001	5980	6001	6001

Source: Authors' analysis based on survey data from primary and secondary workers.

Note: Fixed effects at lottery level. Effect magnitudes in standard deviations. Mean of the family measure in the control areas is zero by design and have thus been left out. Child schooling only for those households with children younger than 14 years old; *p*-values in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (two-tailed). Sharpened q-values in square brackets control the false discovery rate for tests across pre-specified outcomes. Tests of difference between treatment arms use the same empirical model only changing the comparison group, and report *p*-values.

Table 5. Labor Market Outcomes by Gender

	A: Men				B: Women				C: Difference women versus men			
	Has a job (1)	Weekly hours worked (2)	Monthly earnings (3)	Mean effect (4)	Has a job (5)	Weekly hours worked (6)	Monthly earnings (7)	Mean effect (8)	Has a job (9)	Weekly hours worked (10)	Monthly earnings (11)	Mean effect (12)
J	– 0.018	– 1.256	– 1.547	– 0.037	0.129***	4.868***	4.263	0.202***	0.147***	6.124***	5.811	0.228***
	(0.499)	(0.398)	(0.833)	(0.487)	(0.000)	(0.001)	(0.366)	(0.000)	(0.000)	(0.003)	(0.507)	(0.003)
	[1.000]	[1.000]	[1.000]	[1.000]	[0.001]	[0.002]	[0.139]	[0.002]	[0.003]	[0.014]	[0.330]	[0.014]
JS	– 0.017	– 0.825	– 11.813	– 0.060	0.069**	2.280	3.603	0.109*	0.086**	3.105	15.416*	0.168**
	(0.511)	(0.577)	(0.106)	(0.259)	(0.013)	(0.102)	(0.444)	(0.053)	(0.024)	(0.127)	(0.078)	(0.026)
	[1.000]	[1.000]	[1.000]	[1.000]	[0.017]	[0.068]	[0.139]	[0.045]	[0.073]	[0.206]	[0.167]	[0.073]
JT	0.032	– 0.040	– 4.776	0.012	0.052*	2.854**	1.967	0.097*	0.020	2.894	6.743	0.082
	(0.221)	(0.978)	(0.509)	(0.824)	(0.066)	(0.043)	(0.679)	(0.091)	(0.609)	(0.155)	(0.440)	(0.277)
	[1.000]	[1.000]	[1.000]	[1.000]	[0.051]	[0.040]	[0.146]	[0.065]	[0.330]	[0.206]	[0.330]	[0.301]
JST	0.032	2.131	12.083	0.091*	0.089***	5.062***	11.825**	0.212***	0.056	2.931	– 0.257	0.087
	(0.228)	(0.156)	(0.104)	(0.089)	(0.001)	(0.000)	(0.011)	(0.000)	(0.143)	(0.151)	(0.977)	(0.247)
	[1.000]	[1.000]	[1.000]	[1.000]	[0.003]	[0.002]	[0.016]	[0.002]	[0.206]	[0.206]	[0.562]	[0.290]
Mean control	0.592	27.407	76.058	0.000	0.469	18.926	35.162	0.000	0.123	8.481	40.896	0.000
JS = J	(0.986)	(0.806)	(0.236)	(0.717)	(0.062)	(0.110)	(0.904)	(0.160)				
JT = J	(0.109)	(0.485)	(0.708)	(0.435)	(0.017)	(0.216)	(0.676)	(0.112)				
JT = JS	(0.111)	(0.651)	(0.412)	(0.251)	(0.594)	(0.724)	(0.765)	(0.845)				
JST = J	(0.112)	(0.056)	(0.120)	(0.043)	(0.206)	(0.904)	(0.162)	(0.874)				
JST = JS	(0.115)	(0.094)	(0.006)	(0.017)	(0.534)	(0.082)	(0.127)	(0.114)				
JST = JT	(0.990)	(0.978)	(0.509)	(0.824)	(0.248)	(0.170)	(0.069)	(0.077)				
Observations	3019	3019	3019	3019	2982	2982	2982	2982	6001	6001	6001	6001

Source: Authors' analysis based on survey data from primary and secondary workers.

Note: Columns (1) to (8) fixed effects at neighborhood level; columns (9) to (12) at the lottery level. Mean effect magnitudes in standard deviations. Mean of the family measure in the control areas is zero by design; p-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (two-tailed). Sharpened q-values in square brackets control the false discovery rate for tests across pre-specified outcomes. Tests of difference between treatment arms use the same empirical model only changing the comparison group, and report p-values.

11.8 USD more earnings per month (versus 35.2 USD, $p < 0.10$).[20] These effects imply large proportional increases compared to the control mean, of 19, 27, and 34 percent, respectively. Panel C, focusing on the mean effects in column 12, shows that the differential impact across men and women is statistically significant for the J and JS treatment arms, but not for JT and JST.

Table 6 shows results for savings and debt by gender. The control means indicate that 9.1 percent of men saved on a bank account in the previous year, compared to 6.7 percent for women; men have almost three times more savings than woman (22 USD vs. 8.7 USD); and women are more likely to have formally borrowed money (6.2 percent vs. 3.9 percent). Treatment effects for men and women are quite similar. Focusing on the mean effects, columns 5 and 10 show that the estimated impacts are positive and significant ($p < 0.05$) for all treatment arms except the workfare-only arm.[21] Although the coefficient sizes are larger for women than for men, panel C indicates that these differences are not statistically significant. As with the labor market outcomes, the proportional increases compared to the control mean for women are larger than those for men. Women in the JST group increased bank account use by 6.1 percentage points and savings by 24 USD, amounting to a 91 percent and a 279 percent increase compared to their control mean, while the respective proportional increases are 53 percent and 96 percent for JST men.

Heterogeneity by Timing of Measurement, Treatment Dose, and Context

This section discusses to what extent program impacts fade over time, and whether a higher treatment dose or a better business environment enhances the program's impact. It is based on an analysis that exploits heterogeneity across neighborhoods and compares results across subsamples, below and above the median for each dimension. Full results are shown in section S9 in the supplementary online appendix.

Bagga et al. (2023) conclude that project impacts fade over time, based on a comparison of 11 studies that vary in the timing of post-program follow-up surveys; from 1 to 66 months. Furthermore, two workfare studies that contrasted short- and medium-run effects show that any short-term labor market effects diminish over time (Alik-Lagrange et al. 2017; Bertrand et al. 2021). The present study looks at what may be considered medium-term impact, with data collected on average 18 months after project end. There is, however, considerable cross-neighborhood variation from five to 26 months between project end and data collection in a neighborhood. Section S9 in the supplementary online appendix finds no evidence that effects dissipate over time.

A longer program duration may yield a larger total income stream and potential impact (Gehrke and Hartwig 2018; Bagga et al. 2023). In the metastudy by Bagga et al. (2023), project duration varies from 2.5 to 18 months, but the median participant was offered 4 months of work. They find no evidence that project duration enhances impact. In the present study, beneficiaries received a minimum of four months' work, but many were exposed longer to the program due to delays or more complex projects (e.g., the construction of a six-ton bridge). Differences in earnings across neighborhoods can be as high as 100 USD. The study does not detect a higher impact for the subsample with a higher treatment dose (section S9 in the supplementary online appendix).

20 Section S3 in the supplementary online appendix verifies whether these results are robust to the study's differential attrition. First, the analysis leverages the fact that this study builds on 50 separate lotteries, and focuses solely on the half that have little differential attrition. Focusing on the mean effects, the study finds that the JST effect for men is larger in low-differential attrition lotteries, while for women the results remain very similar. Lower bound estimates based on Lee (2019)—which demands that the outcomes of the additional control nonrespondents are similar to treatment group observations who make up the top end of the treatment group distribution—suggest negative impacts on labor outcomes.

21 Section S3 in the supplementary online appendix shows that focusing only on those lotteries with little differential attrition, the effects on savings and debt for women are very similar, whereas they are no longer statistically significant for men. Estimated lower bounds on the treatment effects following Lee (2019) suggest no positive impact on savings and debt, except for JST women.

Table 6. Savings and Debt by Gender

	Men employed					Women employed					Difference women versus men				
	Used formal saving (1)	Amount formal savings (2)	Used formal borrow-ing (3)	Amount formal debt (4)	Mean effect (5)	Used formal saving (6)	Amount formal savings (7)	Used formal borrowing (8)	Amount formal debt (9)	Mean effect (10)	Used formal saving (11)	Amount formal savings (12)	Used formal borrow-ing (13)	Amount formal debt (14)	Mean effect (15)
J	0.044**	3.465	0.002	– 2.424	0.089	0.075***	– 4.920	0.003	1.332	0.087	0.031	– 8.385	0.001	3.756	– 0.014
	(0.014)	(0.543)	(0.849)	(0.565)	(0.116)	(0.000)	(0.196)	(0.818)	(0.714)	(0.142)	(0.212)	(0.223)	(0.953)	(0.500)	(0.866)
	[0.029]	[0.513]	[0.756]	[0.513]	[0.147]	[0.001]	[0.145]	[0.389]	[0.385]	[0.120]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
JS	0.035**	20.969***	0.001	– 0.299	0.159***	0.053***	23.645***	– 0.006	– 4.026	0.328***	0.017	2.676	– 0.008	– 3.726	0.082
	(0.047)	(0.000)	(0.882)	(0.943)	(0.005)	(0.003)	(0.000)	(0.611)	(0.268)	(0.000)	(0.487)	(0.697)	(0.624)	(0.502)	(0.306)
	[0.069]	[0.002]	[0.756]	[0.756]	[0.022]	[0.004]	[0.001]	[0.365]	[0.192]	[0.001]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
JT	0.048***	6.141	– 0.009	0.226	0.125**	0.085***	– 0.944	– 0.026**	– 8.730**	0.256***	0.038	– 7.084	– 0.018	– 8.955	0.113
	(0.007)	(0.275)	(0.370)	(0.956)	(0.025)	(0.000)	(0.806)	(0.030)	(0.017)	(0.000)	(0.131)	(0.302)	(0.254)	(0.106)	(0.162)
	[0.024]	[0.303]	[0.345]	[0.756]	[0.044]	[0.001]	[0.389]	[0.032]	[0.020]	[0.001]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
JST	0.048***	20.938***	– 0.015	1.033	0.210***	0.061***	24.271***	– 0.005	– 4.955	0.341***	0.013	3.332	0.010	– 5.988	0.046
	(0.008)	(0.000)	(0.128)	(0.808)	(0.000)	(0.001)	(0.000)	(0.691)	(0.168)	(0.000)	(0.597)	(0.628)	(0.503)	(0.282)	(0.572)
	[0.024]	[0.002]	[0.148]	[0.756]	[0.002]	[0.001]	[0.001]	[0.385]	[0.132]	[0.001]	[1.000]	[1.000]	[1.000]	[1.000]	[1.000]
Mean control	0.091	21.813	0.039	29.595	0.000	0.067	8.666	0.062	29.285	0.000	0.024	12.147	– 0.029	0.310	NA
JS = J	(0.680)	(0.009)	(0.971)	(0.669)	(0.298)	(0.271)	(0.000)	(0.524)	(0.204)	(0.000)					
JT = J	(0.862)	(0.689)	(0.360)	(0.592)	(0.590)	(0.625)	(0.370)	(0.038)	(0.018)	(0.014)					
JT = JS	(0.555)	(0.026)	(0.377)	(0.915)	(0.609)	(0.113)	(0.000)	(0.150)	(0.266)	(0.293)					
JST = J	(0.862)	(0.010)	(0.147)	(0.491)	(0.074)	(0.478)	(0.000)	(0.588)	(0.133)	(0.000)					
JST = JS	(0.560)	(0.996)	(0.156)	(0.789)	(0.447)	(0.687)	(0.886)	(0.920)	(0.824)	(0.849)					
JST = JT	(0.998)	(0.028)	(0.579)	(0.871)	(0.204)	(0.230)	(0.000)	(0.120)	(0.368)	(0.211)					
Observations	3019	3012	3019	2991	3019	2982	2978	2982	2952	2982	6001	5990	6001	5943	6001

Source: Authors' analysis based on survey data from primary and secondary workers.

Note: Columns (1) to (10) fixed effects at neighborhood level; columns (11) to (15) at the lottery level. Mean effect magnitudes in standard deviations. Mean of the family measure in the control areas is 0 by design; p-values in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (two-tailed). Sharpened q-values in square brackets control the false discovery rate for tests across prespecified outcomes. Tests of difference between treatment arms use the same empirical model only changing the comparison group, and report p-values.

Finally, the analysis explores the context. Compared to other programs, the present study took place in a context with particularly bad security and weak rule of law. In the average neighborhood, 23 percent of survey respondents mention that an armed group had been active in the neighborhood in the six months preceding the survey. There is, however, much variation across neighborhoods: in the least-affected neighborhood, less than 1 percent of respondents mentioned the presence of armed actors, while this proportion is 70 percent in the most-affected neighborhood. Section S9 in the supplementary online appendix shows that while estimated impacts are consistently lower for the subsample with the most armed group activity, the differences are not statistically significant.

5. Conclusion

Workfare programs have evolved significantly over the past two decades. While initially designed to provide a safety net to help households smooth consumption in the face of negative shocks, a new generation of workfare programs seeks to sustainably lift people out of poverty (Bagga et al. 2023). To achieve this more ambitious goal, workfare programs have expanded beyond only providing employment, and include trainings and—in a handful of cases—savings incentives. But, so far, no study had explored the impact of an additional savings incentive and only one study experimentally had investigated the impact of an additional basic entrepreneurship or job search skills training (Bertrand et al. 2021). To fill this gap, this study assessed the effects of a randomized workfare program on economic and sociopolitical outcomes, wherein random subsets of participants received either an extra savings incentive, hard-skills training, or both.

Results indicate that the workfare program significantly increased labor market participation and earnings for women, but not men, 18 months after its end. The savings incentive boosted the amounts saved on a bank account for both men and women. For the pooled sample, including both men and women, the most intense treatment, that is, the job offer plus savings incentive and hard-skills training (JST), outperforms treatments with only one add-on, which suggests that—in the present study context with a dominance of self-employment rather than wage employment—a capital injection amplifies the impact of a hard-skills training. However, even for the JST treatment, there is no evidence for knock-on effects on other economic or sociopolitical families.

Attrition rates were higher in the control (27 percent) than in the treatment group (15 percent). Reassuringly, the results for women and the labor market results for men are similar when focusing solely on those neighborhoods with little differential attrition. The results, however, are not robust to assuming that the outcomes of the additional control nonrespondents are similar to treatment group observations who make up the top end of the treatment group distribution (Lee 2009). In other words, the positive results observed in the treatment group would not hold up if the nonrespondents in the control group would have outcomes as good as, or better than, those of the best-performing treatment respondents.

In terms of quantified benefits, monthly earnings increased by 12 USD for JST. This is a substantial increase from a household perspective (compared to the control mean of 56 USD), but does it make the JST program cost-effective? Administrative data regarding the costs incurred by FSDRC for project implementation are unavailable, including expenses for training, bank account setup, program oversight, and beneficiary recruitment. Similarly, information on the benefits of infrastructure works, economic multiplier effects, and labor market spillovers, such as wage increases, is lacking. Therefore, the cost-benefit analysis is limited to the money disbursed to beneficiaries and the measured individual-level impact 18 months post-program. The money disbursed to the JST beneficiaries amounted to 545 USD: 275 USD labor earnings, an extra 90 USD if they chose to save every day, and an extra 180 USD for the 60-day training. Assuming a stable increase in monthly earnings of 12 USD and applying a 10 percent discount rate, it would take four years to earn back the money disbursed to beneficiaries.

How do the study's findings compare with those of other workfare studies? The recent review by Bagga et al. (2023) serves as a benchmark, encompassing 11 experimental studies on workfare programs. From this review, it is observed that positive impact estimates regarding employment and earnings are common for studies that focus on short time horizons after program end, but rare in those looking beyond 12 months post-program. The fact that the present study detects such positive outcomes 18 months post-program for the most intense treatment could possibly be attributed to the synergistic impact of the savings and hard-skills training. It could also be driven by the setting's low baseline employment as well as the specific targeting of women who start with even lower labor force participation and earnings, which—also according to the metastudy—boosts the workfare impact on labor market outcomes. In terms of cost-effectiveness, the metastudy concludes that the impact on earnings tends to be small relative to the value of the monthly program earnings, on average 7 percent. The program described in this article does better, at 19 percent (12 USD compared to 63 USD monthly program earnings). Most studies that find positive effects on labor market outcomes, including this one, also detect a boost in productive assets, indicating that these may indeed drive sustained program impacts by fueling self-employment activities. Almost no studies detect sizeable impacts on women's empowerment, psychosocial well-being, or sociopolitical outcomes. Taken together, the most significant new lesson to draw from this study is that it is valuable to further identify how and to what extent program add-ons can contribute to the transition from temporary workfare to more stable employment opportunities.

Finally, to what extent do the results from this study generalize beyond the present study site? The five case study cities are very similar on a number of key dimensions to African (mega-)cities, such as the "urbanization without industrialization" process and the large concentration of the population living in slums (Lall, Henderson, and Venables 2017). On the other hand, the five cities are poorer and less secure than the average African city, and they suffer from a particularly poor provision of public goods and services (Mvukiyehe et al. 2016). It is possible that a similar workfare program in cities with equally low poverty and unemployment levels, but a better business climate, would yield better outcomes. Conversely, in a better institutional environment, market failures are less severe and workfare programs will need to more carefully target the most vulnerable and most constrained to have an impact. This article shows that one way to do so is by targeting women.

Data Availability

The datasets needed to run those files are publicly available at the below website (also referenced in the manuscript): https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/CZ2JAA.

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