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# Three essays on the economic and social impact of employment programs in Sub-Saharan Africa: evidence from Côte d'Ivoire.

Alicia Marguerie

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Thèse de doctorat



# Three essays on the economic and social impact of employment programs in Sub-Saharan Africa: evidence from Côte d'Ivoire.

Thèse de doctorat de l'Institut Polytechnique de Paris  
préparée à Ecole nationale de la statistique et de l'administration économique

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Thèse présentée et soutenue à Washington en Visio Conférence, le 17/12/2020, par

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# Résumé et mots clés

## Résumé long

En Afrique subsaharienne, les programmes d'emploi font partie intégrante des stratégies de réduction de pauvreté. Dans les pays fragiles, ils sont d'autant plus stratégiques qu'ils pourraient contribuer à la stabilisation sociale. Outre la création d'emplois, l'un des principaux défis en matière d'emploi est l'amélioration de la productivité des personnes travaillant dans des activités indépendantes. C'est le cas pour 80% des travailleurs subsahariens. Toutefois, il existe peu d'études documentant leurs effets économiques et sociaux et étudiant les caractéristiques optimales de ces interventions.

En s'appuyant sur des évaluations d'impact (essais contrôlés randomisés), cette thèse apporte de nouveaux éclairages sur l'efficacité de deux programmes d'emploi en Côte d'Ivoire qui ont pour but d'aider les groupes vulnérables à faire la transition vers un emploi plus productif : un programme de travaux publics (chapitre 1) et un programme de soutien au micro-entrepreneuriat (chapitres 2 et 3). Les deux programmes ciblent les jeunes qui sont plus vulnérables face au sous-emploi. Leur objectif commun est de faciliter leur accès à des emplois plus productifs en leur apportant des moyens de subsistance.

Le **chapitre 1** intitulé “*Do Workfare Programs Live to Their Promises? Experimental Evidence from Côte d'Ivoire*”, est une collaboration avec Marianne Bertrand, Bruno Crépon et Patrick Premand qui examine l'impact d'une intervention de travaux publics en Côte d'Ivoire à l'aide d'un essai contrôlé randomisé (ECR). Le programme de travaux publics (THIMO) a été mis en œuvre dans 16 zones urbaines (y compris Abidjan). Il cible les jeunes de 18 à 30 ans disposés à travailler pendant six mois à l'entretien des routes, rémunérés au salaire minimum journalier. Les jeunes en zones urbaines ont souvent la possibilité soit de trouver des opportunités de travail

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salarié (principalement informel), soit de démarrer leur propre activité (non agricole). Dans les deux cas, ils n'ont souvent pas les compétences nécessaires pour effectuer ces activités, pour rechercher efficacement un emploi ou pour démarrer et gérer une activité indépendante. Ils peuvent également être confrontés à des contraintes financières, qu'il s'agisse d'avoir les fonds disponibles ou un accès au capital pour leur activité indépendante, ou bien qu'il s'agisse de financer une recherche d'emploi souvent coûteuse. Le programme de travaux publics leur a fourni un revenu permanent pendant six à sept mois et une expérience de travail. Certains groupes de bénéficiaires ont également reçu une formation complémentaire à l'entrepreneuriat ou à la recherche d'emploi. L'impact du programme de travaux publics (ainsi que l'impact des formations complémentaires) est évalué à l'aide d'un essai contrôlé randomisé. Parmi ceux qui postulent au programme, un sous-ensemble de 3 125 jeunes est sélectionné lors de loteries publiques organisées dans chacune des 16 villes. Par conséquent, on peut comparer les jeunes sélectionnés avec ceux non sélectionnés pour identifier l'impact causal du programme *pendant* (4 mois après le début des travaux) et *après* (15 mois après la fin du programme).

Les résultats montrent que les impacts sur le niveau d'emploi sont limités mais qu'il s'opère une substitution vers des emplois salariés dans les travaux publics et une augmentation des revenus au cours du programme. Un an après la fin du programme, il n'y a pas d'impacts durables sur le niveau ou la composition de l'emploi, et des impacts limités sur les revenus. Ces résultats suggèrent que l'auto-ciblage basé sur le salaire minimum formel n'a pas réussi à attirer uniquement les plus vulnérables. Lorsque l'auto-ciblage est utilisé, on s'attend à ce que les impacts du programme soit hétérogènes, les candidats marginaux bénéficiant peu et les candidats inframarginaux bénéficiant jusqu'au montant du transfert. L'analyse s'appuie sur des techniques de machine learning pour comprendre *qui* profite le plus du programme, à court et à moyen terme. Les résultats du machine learning confirment des différences d'impacts entre les groupes de participants pendant le programme. L'impact moyen sur les revenus totaux à court terme est 2,2 fois supérieur dans le haut de la distribution des revenus prédits (quartile supérieur) que dans le bas (quartile inférieur). Les caractéristiques des individus qui bénéficient le plus des travaux publics (quartile supérieur) suggèrent que des règles alternatives de ciblage pourraient améliorer l'efficacité du programme. Par rapport au scénario de référence avec un auto-ciblage reposant sur le salaire minimum formel, le rapport coût-efficacité s'améliorerait de 30% à 52% en ciblant uniquement les femmes, ou en ciblant les jeunes dont les revenus sont faibles au départ (avant le programme). L'analyse n'est pas conclusive quant au mécanisme de ciblage alternatif qui maximiserait la rentabilité de ce type

de programme. Cependant, elle montre clairement que s'écarter de l'auto-ciblage basé uniquement sur le salaire minimum formel améliorerait sa rentabilité.

Les **chapitres 2 et 3** se concentrent sur une intervention de micro-entrepreneuriat (PRISE) mise en oeuvre dans 147 localités (principalement des villages ruraux) dans l'ouest de la Côte d'Ivoire. Le groupe cible correspond aux plus vulnérables, en particulier des jeunes de 18 à 35 ans. La situation à laquelle ils sont confrontés est assez différente de celle des jeunes urbains décrite ci-dessus : ils dépendent principalement d'activités agricoles indépendantes. Ils n'ont pas accès au crédit pour étendre ou diversifier leurs activités. Enfin, compte tenu de plus faibles niveaux d'alphabétisation, ils manquent de certaines compétences de base utiles pour un micro-entrepreneur (par exemple, fixer les bons prix, gérer les stocks). Le programme PRISE propose une formation à l'entrepreneuriat portant à la fois sur le démarrage et sur l'expansion d'une activité indépendante, ainsi que le développement de plans d'affaires de base. En plus de la formation, pour soutenir l'accumulation de capital de ces activités, le programme propose trois alternatives : une subvention en espèces (sans conditions), une subvention équivalente avec une condition de remboursement (50% de la subvention pour être remboursé) et un groupe d'épargne renforcé (Association Villageoise d'Epargne Communautaire, AVEC).

Le **chapitre 2**, intitulé "*Economic inclusion, capital constraints and micro-entrepreneurship : experimental evidence from Côte d'Ivoire*" est basé sur un travail réalisé avec Patrick Premand évaluant les impacts économiques directs du programme de micro-entrepreneuriat PRISE. L'évaluation d'impact repose sur un essai contrôlé randomisé mis en oeuvre dans 207 localités, qui ont toutes participé à une loterie publique. Les localités ont été randomisées selon trois modalités de soutien au capital. 30 localités ont reçu une injection de capital sous la forme d'une subvention en espèces (sans conditions). 64 localités ont reçu une subvention équivalente avec une condition de remboursement de 50%. 53 localités n'ont pas reçu de capital mais ont été formées à la création d'AVEC auprès desquelles elles pourraient ensuite contracter des prêts de petit montant. Les 30 localités restantes sont des localités témoins dans lesquelles le programme n'a pas été mis en oeuvre. Dans chaque localité, y compris les localité de contrôle, les personnes intéressées ont postulé au programme avant que la loterie ne soit réalisée. Les personnes les plus vulnérables ont été identifiées à l'aide d'un score de vulnérabilité, calculé sur la base des informations fournies, et d'un seuil. Les impacts directs du programme sont mesurés en comparant les individus sélectionnés dans les localités traitées avec leurs individus équivalents dans les localités témoins (en

simulant le même seuil). Nous étudions l'efficacité des trois interventions offrant une formation à l'entrepreneuriat et trois instruments alternatifs pour assouplir les contraintes de capital. 5 220 personnes font l'objet d'une enquête de suivi 15 mois après la fin du programme afin de mesurer ses impacts.

Nous montrons que bien que les participants s'engagent plus dans des activités indépendantes génératrices de revenus, l'intervention n'est pas suffisante pour augmenter significativement les revenus. Les impacts sur les revenus varient peu selon les modalités d'intervention. Cependant, la dynamique de l'épargne et de l'accumulation d'actifs diffère. Toutes les interventions induisent des investissements dans des activités économiques opérant à plus grande échelle, avec des impacts relativement plus élevés pour les interventions avec subvention. Cependant, une grande partie de la subvention est épargnée par les bénéficiaires plutôt qu'investie. Nous interprétons cela à la lumière du contexte rural et post-conflit, où les besoins de lissage de la consommation et les motifs de précaution pour l'épargne peuvent être particulièrement répandus. En effet, contrairement aux programmes de "graduation" (ultra-poor) et autres interventions d'inclusion économique, aucun soutien à la consommation n'est fourni. En comparaison, l'intervention AVEC conduit à des résultats comparables pour les activités génératrices de revenus, bien qu'il n'y ait pas eu d'injection d'argent. Les AVECs sont une opportunité d'épargner dans un instrument plus efficace que les traditionnelles tontines et augmentent l'accès au crédit pour faciliter les investissements dans les activités. La composante formation peut fournir d'autres incitations à l'investissement. Nos résultats sur l'accumulation d'actifs sans injection de capital mettent en évidence le potentiel des groupes d'épargne améliorés.

Le **chapitre 3**, intitulé "*Economic and social spillovers of micro-entrepreneurship program : evidence from post-conflict Côte d'Ivoire*" examine plus en détail les conséquences sociales et économiques locales du même programme de micro-entrepreneuriat, qui a été mis en oeuvre dans des régions fragiles. La zone de mise en oeuvre du programme est caractérisée par une fragmentation ethnique importante avec des tensions entre les groupes autochtones, allochtones et allogènes. L'amélioration de la cohésion sociale était un objectif du programme d'inclusion économique. Ce chapitre analyse la manière dont les interventions ont affecté la participation aux groupes et aux événements communautaires ainsi que les niveaux de solidarité, de confiance et la perception des autres groupes. Les impacts directs sur les bénéficiaires ainsi que les impacts indirects sur les autres villageois sont pris en compte. Une autre considération fondamentale est de savoir s'il y a

des effets secondaires négatifs sur les non-bénéficiaires. En effet, les retombées économiques au sein d'un village peuvent aller dans les deux sens : soit en soutenant les activités économiques locales parmi les non-bénéficiaires, soit en les évinçant (réduction des parts de marché). L'évaluation a été conçue pour mesurer les retombées au sein des localités, c'est-à-dire l'impact de l'intervention sur les autres villageois. Nous utilisons le seuil de sélection pour comparer les individus qui n'ont pas été sélectionnés dans les villages traités avec le groupe équivalent dans les villages témoins. On peut ainsi mesurer les conséquences indirectes de l'intervention au sein des villages.

Nous montrons que l'intervention améliore le tissu social en augmentant la participation aux groupes économiques et la solidarité entre les bénéficiaires. Plutôt que d'évincer d'autres activités indépendantes, nous constatons que le programme conduit à des impacts indirects positifs sur les activités génératrices de revenus des non-bénéficiaires avec des investissements légèrement plus élevés dans leurs activités. Cependant, il n'y a pas de signe d'externalités sociales plus larges. Nous n'observons pas d'augmentation de la confiance dans la communauté au sens large ou de retombées locales sur la cohésion sociale. En outre, les impacts sociaux ne sont pas différents entre l'option avec AVECs, les subventions avec remboursement et les subventions sans conditions, ce qui suggère que le renforcement des liens sociaux (par exemple via des groupes d'épargne) n'est pas la principale source d'explication.

## **Mots clés**

Emploi, Marché du travail, Entrepreneuriat, Evaluation d'impact, Politiques publiques, Afrique subsaharienne.

# Abstract and keywords

## Abstract

Employment programs are increasingly being used as policy instruments for poverty reduction in Sub Saharan Africa. In fragile countries especially, this is a strategic instrument to restore social stability. In addition to job creation, a major employment challenge is to support the productivity of people working in independent activities. In Sub Saharan countries, 80 percent of the workforce is engaged in small independent activities. However, there is limited evidence on both the employment and social impact of employment programs, and their design features.

This thesis focuses on two types of employment programs in Côte d'Ivoire which are part of a broader panel of policy options to support productive livelihoods: a public works program (Chapter 1) and a micro-entrepreneurship support program (Chapter 2 and 3). Both programs are targeted to youth which is a vulnerable population in terms of employment. Their common objective is to support livelihoods through more productive employment.

**Chapter 1** titled *Do Workfare Programs Live Up to Their Promises? Experimental Evidence from Côte d'Ivoire*, is a joint work with Marianne Bertrand, Bruno Crépon and Patrick Premand that examines the impact of a self-targeted public works intervention in Côte d'Ivoire using a randomized controlled trial (RCT). The public works program (THIMO) was implemented in 16 relatively large urban areas (including Abidjan). It targets youth 18 to 30 years-old willing to work over a six-month period in road maintenance, paid at the minimum daily wage. Urban youth often have the option to either find wage work opportunities (mostly informal), or to start their own non-agricultural activity. In both cases, they often lack the skills needed to perform the job, to efficiently search for a job, or to start and manage an independent activity. They may also face financial constraints: funds or access to capital for their independent activity, or the means

to finance costly job search. The public works intervention provided them with a stable income for six to seven months, and a work experience. Randomized subsets of beneficiaries also received complementary training on basic entrepreneurship or job search skills. We evaluate the impact of the public works program (as well as additional training components) using a randomized-controlled trial. Out of those who apply to the program, a subset of 3,125 youth is randomly selected by public lotteries held in the 16 cities. Therefore one can compare selected youth with non-selected ones to identify the causal impact of the public works, *during* the program (after 4 months of works) and *post* program (15 months after the end of the program).

Results show limited impacts on the level of employment, but a shift towards public works wage jobs and an increase in earnings during the program. A year after the end of the program, there are no lasting impacts on the level or composition of employment, and limited impacts on earnings. These results strongly suggest that self-targeting based on the formal minimum wage did not succeed in this context in getting only the most vulnerable to benefit from the program. When self-targeting is used, heterogeneity in program impacts is expected with marginal applicants benefiting little and infra-marginal applicants benefiting up to the transfer amount. We apply machine learning techniques to further understand who benefit the most from the program, both in short term and medium term. Results from machine learning confirm large differences in predicted impacts across groups of participants during the program. The average impact on earnings in the short-term for the top of the distribution (upper quartile) is 2.2 times more than for the bottom (lower quartile). The analysis of the characteristics of those who benefit the most from public works suggests that alternative targeting rules could improve effectiveness. Compared to the benchmark scenario with self-targeting based on the formal minimum wage, the cost-effectiveness ratio would improve by 30% to 52% by targeting women only, or by targeting youths with low predicted baseline earnings. The analysis cannot decisively indicate which alternative targeting mechanism would maximize cost-effectiveness. However, it clearly shows that departing from self-targeting solely based on the formal minimum wage would improve program cost-effectiveness.

**Chapter 2 and 3** focus on a micro-entrepreneurship intervention (PRISE) implemented in 147 localities (mostly rural villages) in Western Côte d'Ivoire. The target group corresponds to the most vulnerable, with a special focus on youth 18 to 35 years-old. The situation they face is quite different from urban youth (described earlier): they rely mostly on independent agricultural activities. They lack access to credit to extend or diversify their activities. Finally, with low

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literacy levels, they lack some basic skills useful for a micro-entrepreneur (e.g. setting the right prices, managing stocks). The PRISE program provides an entrepreneurship training focused on both starting and expanding an independent activity, and supporting the development of basic business plans. On top of training, support to accumulate capital for the business was provided in three alternative forms : an unconditional cash grant, an equivalent grant with a repayment condition (50% of the grant to be reimbursed) and enhanced savings group (Village Savings and Loan Association, VSLA).

**Chapter 2**, titled *Economic inclusion, capital constraints and micro-entrepreneurship : experimental evidence from Côte d'Ivoire* is a joint work with Patrick Premand evaluating the direct economic impacts of the PRISE micro-entrepreneurship program. The impact evaluation relies on a randomized-controlled trial implemented across 207 localities, which all took part to a public lottery. The localities have been randomized across the three modalities of capital support. 30 localities received a capital injection taking the form of a cash grant. 64 localities received an equivalent grant with 50% repayment condition. 53 localities did not receive capital but were trained to create VSLA from which they could later take small loans. The remaining 30 localities are control localities in which the program was not implemented. In each locality including control, interested individuals have applied to the program prior to the lottery. Based on the baseline information they provided, the most vulnerable were identified to participate using a vulnerability score and a cut-off. We measure the direct impacts of the program by comparing selected individuals in treated localities with their equivalent individuals in control localities (by simulating the cut-off). We investigate the effectiveness of the three interventions providing entrepreneurship training and three alternative instruments to relax capital constraints. We survey 5,220 individuals 15 months after the end of the program to measure its impacts.

We show that although participants engage in more independent income-generating activities, the intervention is not sufficient to significantly increase earnings. There is little variation in impacts on earnings across the intervention modalities. However, the dynamics of savings and asset accumulation do differ. All interventions induce investments in economic activities operating at a larger scale, with relatively higher impacts for the grant interventions. However, a large share of the grant is saved by beneficiaries rather than invested. We interpret that in the light of the rural and post-conflict context, where needs for consumption smoothing and precautionary motives for savings may be particularly prevalent. Indeed, contrary to graduation programs and

others economic inclusion interventions, there is no consumption support provided. In comparison, the VSLA intervention leads to comparable outcomes regarding the income-generating activities, although there is no cash injection. VSLAs shift savings to a more efficient instrument compared to ROSCAs, and increase access to credit to facilitate investments in activities. The training component may provide further incentives towards investment. Our results on asset accumulation without capital injection highlight the potential of enhanced saving groups.

In **Chapter 3**, titled *Economic and social spillovers of a micro-entrepreneurship program : evidence from post-conflict Côte d'Ivoire*, I further examine the social and economic local consequences of the same micro-entrepreneurship program, which was implemented in fragile regions. The setting is characterized by important ethnic fragmentation with tensions between local ethnic groups, internal migrants and foreign migrants. Improving social cohesion was an objective of the economic inclusion program. We analyze how the interventions affected participation to groups and community events as well as solidarity levels, trust, and perceptions of others. We take into account both direct impacts on beneficiaries, and indirect impacts on other villagers. Another fundamental consideration is whether there are any negative side effects on non-beneficiaries. In fact, economic spillovers within a village can go in both directions, either sustaining or negatively affecting local economic activities among non-beneficiaries (crowding out existing activities). The evaluation was designed to measure spillovers within localities, in other words the impact of the intervention on other villagers. We use the selection cut-off to compare individuals who were not selected in treated villages with the equivalent group in control villages. Thereby we can measure the indirect consequences of the intervention within villages.

We show that the intervention improve the social fabrics by increasing participation in economic groups and solidarity among beneficiaries. Rather than crowding-out other independent activities, we find that the program leads to positive indirect impacts on non-beneficiaries' income-generating activities with marginally higher investments in their activities. However, there are no signs of broader social externalities. We do not observe increase in trust in the broader community or local spillovers on social outcomes. Furthermore, the social impacts are not different between the VSLA, cash-grant-with-repayment and cash grants, which suggests that enhanced social ties (e.g. via savings groups) may not be the main channel.

## Keywords

Employment, Labor Market, Entrepreneurship, Impact Evaluation, Public Policy, Sub Saharan Africa.

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# Introduction

Employment programs are increasingly part of the poverty reduction strategy in developing countries, including Sub Saharan Africa. Sub Saharan Africa represents 50.7 percent of the poor globally. While the number of people living in extreme poverty has decreased in most countries between 1990 and 2013, it has risen in many Sub-Saharan African countries ([World Bank \(2018\)](#)).<sup>1</sup> This comes at a time when the demographic pressure continues unabated, such that by 2050, the median age will be only 24 years-old (United Nations projection, [Filmer and Fox \(2014\)](#)) creating large waves of workers that enter the labor market in search of sustainable income opportunities.

To respond to these poverty and labor market challenges, employment has become a top priority. Over the past decades, poverty reduction instruments have expanded beyond basic social assistance to incorporate productive inclusion support. Productive support aims at providing people with the means to increase their earnings in their activities. This support approaches both poverty and livelihoods in a more holistic manner, providing additional services such as training, asset transfer, coaching and mentoring – recognizing the multi-dimensional aspect of poverty. The focus on *jobs* does not only support the most vulnerable, but contributes to the development of the country by raising living standards and productivity, and building social cohesion ([World Bank \(2012\)](#)).

There is a need for policies supporting self-employment and micro-entrepreneurship, even if this means supporting informal employment. This thesis focuses on such employment programs.

For Sub Saharan Africa, the employment challenge is twofold. Firstly, labor demand is still limited. Increasing labor demand is a long run challenge. This outcome relies significantly on the ability of small, medium and large firms to employ and expand. The formal private sector is

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<sup>1</sup>Estimates are based on 2013 data using PovcalNet ([World Bank \(2018\)](#).)

usually appealing to support the creation of good quality jobs, in the sense of higher productivity and decent income. However, given the predicted low growth of the formal sector, it is still unlikely to create enough opportunities for the large cohorts of young workers entering the labor market in the region. In fact, the World Bank estimates that in the coming decades only one in four youth in Sub-Saharan Africa would find a wage job, out of which only a small fraction will be in the formal sector (Filmer and Fox (2014)).

Secondly, the majority of the working population in Sub Saharan Africa rely on low productive non-wage work, combining self-employment and casual paid jobs. In fact, more than 80 percent of the workforce is engaged in small independent activities, and unemployment is not an option.<sup>2</sup>

Given this fact and the youth bulge, there is a need for more immediate policies that support employment in these activities, namely (informal) micro-entrepreneurship, which is mostly self-employment. The required short-term policies and support for them differ considerably from support to the growth of formal firms. It focuses instead on poorer youth or vulnerable groups of the population, as opposed to better educated and higher income groups. The objective of these policies is to improve the income level and stability of these populations by helping them diversify their portfolio of activities and improve their productivity.

**This thesis focuses on employment programs in Côte d'Ivoire that have aimed to help vulnerable groups transition toward more productive employment.** Despite steady developments during the 1990s, two episodes of conflict caused substantial deterioration in economic conditions and living standards between 2002-07 and 2010-11.<sup>3</sup> The per capita Gross Domestic Product (GDP) in 2012 was at least 10 percent below its level in the mid-1990s. Since the post-electoral crisis, the country has regained political stability and economic growth, reaching 8 percent of GDP growth rate in 2014. Today, the poverty rate in Côte d'Ivoire is around 28 percent (measured in 2015, considering \$1.90 a day poverty line, 2011 PPP). Many people are

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<sup>2</sup>According to Filmer and Fox (2014), 62 percent of the jobs are in family farms (independent agricultural activities) and 22 percent in non-agricultural independent activities. For the latter, this is mainly self-employment (70 percent of those micro-firms are the activity of one person only). The remaining share accounts for wage jobs, and not necessarily formal ones. Unemployment is scarce, around 5 percent in lower middle income countries of Sub Saharan Africa (Filmer and Fox (2014)). This should not be surprising when there is limited access to (or coverage of) social assistance or unemployment support.

<sup>3</sup>In fact, in 2002, uprisings in the army led to the division of the country between two areas, the North under the control of the rebellion forces and the South under the forces of the army. The disarmament that took place in 2004 was not successful and the country experienced following episodes of violence and unrest, remaining divided until the Ouagadougou Political Agreement in 2007. A post-electoral crisis followed until April 2011.

engaged in low productivity activities, with 47.5 percent working in agricultural self-employment (family farms), and 29.7 percent in non-agricultural self-employment (Christiaensen and Premand (2017)).

Côte d'Ivoire is one case of post-conflict country for which the youth employment challenge is also a stability challenge. Youth represent 60 percent of the working-age population. Every year, an estimated 350,000-400,000 of them enter the labor market with low levels of educational achievement, with only 24 percent of working-age youth having completed primary education or more (Christiaensen and Premand (2017)). Now in the recovery phase, economic policies also aim at improving stability and restoring social cohesion. A common political concern is that vulnerable youth without stable and sustainable jobs destabilize their local and national economy, as they express their frustrations. Employment programs can be important stabilizing instruments, helping to increasing incomes and welfare of the most vulnerable. In regions with high ethnic fragmentation and a history of inter-ethnic violence (such as Abidjan and Western regions in Côte d'Ivoire), there is also a need to reinforce social ties across communities.

**This thesis focuses on two types of employment programs in Côte d'Ivoire which are part of a broader panel of policy options to support productive livelihoods: a public works program (Chapter 1) and a micro-entrepreneurship support program (Chapter 2 and 3).** Both programs are targeted to youth as a vulnerable population, especially in terms of employment. Their common objective is to support livelihoods through more productive employment. Analyzing the effectiveness of those two instruments is informative of different implementation context at the country level.<sup>4</sup>

**Chapter 1** titled *Do Workfare Programs Live Up to Their Promises? Experimental Evidence from Côte d'Ivoire*, is a joint work with Marianne Bertrand, Bruno Crépon and Patrick Premand that examines the impact of a self-targeted public works intervention in Côte d'Ivoire using a randomized controlled trial (RCT). The public works program (THIMO) was implemented in 16 relatively large urban areas across Côte d'Ivoire (including Abidjan). It targets youth 18 to 30 years-old willing to work over a six-month period in road maintenance, paid at the minimum

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<sup>4</sup>Another evaluation of an apprenticeship program (Crépon and Premand (2019)) in urban and peri-urban areas is another good complement to the two programs presented in this thesis.

daily wage. Urban youth often have the option to either find wage work opportunities (mostly informal), or to start their own non-agricultural activity. In both cases, they often lack the skills needed to perform the job, to efficiently search for a job, or to start and manage an independent activity. They may also face financial constraints: funds or access to capital for their independent activity, or the means to finance costly job search. The public works intervention provided them with a stable income for six to seven months, and a work experience. Randomized subsets of beneficiaries also received complementary training on basic entrepreneurship or job search skills. We evaluate the impact of the public works program (as well as additional training components) using a randomized-controlled trial. Out of those who apply to the program, a subset of 3,125 youth is randomly selected by public lotteries held in the 16 cities. Therefore one can compare selected youth with non-selected ones to identify the causal impact of the public works, during the program (after 4 months of works) and post program (15 months after the end of the program).

Results show limited impacts on the level of employment, but a shift towards public works wage jobs and an increase in earnings during the program. A year after the end of the program, there are no lasting impacts on the level or composition of employment, and limited impacts on earnings. These results strongly suggest that self-targeting based on the formal minimum wage did not succeed in this context in getting only the most vulnerable to benefit from the program. When self-targeting is used, heterogeneity in program impacts is expected with marginal applicants benefiting little and infra-marginal applicants benefiting up to the transfer amount. We apply machine learning techniques to further understand who benefit the most from the program, both in short term and medium term. Results from machine learning confirm large differences in predicted impacts across groups of participants during the program. The average impact on earnings in the short-term for the top 25% of the predicted distribution (upper quartile) is 2.2 times more than for the bottom 25% (lower quartile). The analysis of the characteristics of those who benefit the most from public works suggests that alternative targeting rules could improve effectiveness. Compared to the benchmark scenario with self-targeting based on the formal minimum wage, the cost-effectiveness ratio would improve by 30% to 52% by targeting women only, or by targeting youths with low predicted baseline earnings. The analysis cannot decisively indicate which alternative targeting mechanism would maximize cost-effectiveness. However, it clearly shows that departing from self-targeting solely based on the formal minimum wage would improve program cost-effectiveness.

**Chapter 2 and 3** focus on a micro-entrepreneurship intervention (PRISE) implemented in 147 localities (mostly rural villages) in Western Côte d'Ivoire. The target group corresponds to the most vulnerable, with a special focus on youth 18 to 35 years-old.<sup>5</sup> The situation they face is quite different from urban youth: they rely mostly on independent agricultural activities. They lack access to credit to extend or diversify their activities. Finally, with low literacy levels, they lack some basic skills useful for a micro-entrepreneur (e.g. setting the right prices, managing stocks). The PRISE program provides an entrepreneurship training focused on both starting and expanding an independent activity, and supporting the development of basic business plans. On top of training, support to accumulate capital for the business was provided in three alternative forms : an unconditional cash grant, an equivalent grant with a repayment condition (50% of the grant to be reimbursed) and enhanced savings group (Village Savings and Loan Association, VSLA).

**Chapter 2**, titled *Economic inclusion, capital constraints and micro-entrepreneurship : experimental evidence from Côte d'Ivoire* is a joint work with Patrick Premand evaluating the direct economic impacts of the PRISE micro-entrepreneurship program. The impact evaluation relies on a randomized-controlled trial implemented across 207 localities, which all took part to a public lottery. The localities have been randomized across the three modalities of capital support. 30 localities received a capital injection taking the form of a cash grant. 64 localities received an equivalent grant with 50% repayment condition. 53 localities did not receive capital but were trained to create VSLA from which they could later take small loans. The remaining 30 localities are control localities in which the program was not implemented. In each locality including control, interested individuals have applied to the program prior to the lottery. Based on the baseline information they provided, the most vulnerable were identified to participate using a vulnerability score and a cut-off. We measure the direct impacts of the program by comparing selected individuals in treated localities with their equivalent individuals in control localities (by simulating the cut-off). We investigate the effectiveness of the three interventions providing entrepreneurship training and three alternative instruments to relax capital constraints. We survey 5,220 individuals 15 months after the end of the program to measure its impacts.

We show that although participants engage in more independent income-generating activities, the intervention is not sufficient to significantly increase earnings. There is little variation in impacts

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<sup>5</sup>The official definition of youth in Côte d'Ivoire is 18 to 35 years-old.

on earnings across the intervention modalities. However, the dynamics of savings and asset accumulation do differ. All interventions induce investments in economic activities operating at a larger scale, with relatively higher impacts for the grant interventions. However, a large share of the grant is saved by beneficiaries rather than invested. We interpret that in the light of the rural and post-conflict context, where needs for consumption smoothing and precautionary motives for savings may be particularly prevalent. Indeed, contrary to graduation programs and others economic inclusion interventions, there is no consumption support provided. In comparison, the VSLA intervention leads to comparable outcomes regarding the income-generating activities, although there is no cash injection. VSLAs shift savings to a more efficient instrument compared to ROSCAS, and increase access to credit to facilitate investments in activities. The training component may provide further incentives towards investment. Our results on asset accumulation without capital injection highlight the potential of enhanced saving groups.

In **Chapter 3**, titled *Economic and social spillovers of a micro-entrepreneurship program : evidence from post-conflict Côte d'Ivoire*, I further examine the social and economic local consequences of the same micro-entrepreneurship program, which was implemented in fragile regions. The setting is characterized by important ethnic fragmentation with tensions between local ethnic groups, internal migrants and foreign migrants. Improving social cohesion was an objective of the economic inclusion program. We analyze how the interventions affected participation to groups and community events as well as solidarity levels, trust, and perceptions of others. We take into account both direct impacts on beneficiaries, and indirect impacts on other villagers. Another fundamental consideration is whether there are any negative side effects on non-beneficiaries. In fact, economic spillovers within a village can go in both directions, either sustaining or negatively affecting local economic activities among non-beneficiaries (crowding out existing activities). The RCT was designed to measure spillovers within localities, in other words the impact of the intervention on other villagers. We use the selection cut-off to compare individuals who were not selected in treated villages with the equivalent group in control villages. Thereby we can measure the indirect consequences of the intervention within villages.

We show that the intervention improve the social fabrics by increasing participation in economic groups and solidarity among beneficiaries. Rather than crowding-out other independent activities, we find that the program leads to positive indirect impacts on non-beneficiaries' income-generating activities with marginally higher investments in their activities. However, there are no signs of

broader social externalities. We do not observe increase in trust in the broader community or local spillovers on social outcomes. Furthermore, the social impacts are not different between the VSLA, cash-grant-with-repayment and cash grants, which suggests that enhanced social ties (e.g. via savings groups) may not be the main channel.

In **Chapter 1**, we evaluate a public works program, and specifically study the existence of productive impacts in the longer run.

Public works can be seen as creating demand for unskilled workers. They are also increasingly used as productive safety nets with additional components supporting the transition to more productive employment. The underlying idea is to increase labor supply at a timely moment (e.g. economic crisis, lean season, food crisis, environmental disaster) to give the most vulnerable households an opportunity to earn (additional) income while promoting the maintenance or creation of public infrastructure useful to the community. NREGA, implemented in India, is probably the most famous example. Beyond the social assistance motives (clearly stated in the “cash-for-work”/“workfare” denomination), being “at work” theoretically allows participants to develop technical and behavioral skills thereby improving their employability. The regular income provides the opportunity to smooth consumption, and eventually to save for insurance or investment purposes. A large strand of the literature assessed the role of public works as a short-term safety net or insurance mechanism (more than an employment program) providing temporary employment and income to vulnerable populations during lean agricultural seasons (e.g. in Malawi, [Beegle et al. \(2017\)](#)) or after economic shocks (e.g. after the economic crisis in Argentina, [Galasso et al. \(2004\)](#)). General findings on program impacts on welfare and food security remain mixed.<sup>6</sup> Public works are increasingly evaluated on their potential productive impact, looking at outcomes related to independent activities performed after graduating from the program. [Rosas and Sabarwal \(2016\)](#) document that beneficiaries from a public works in Sierra Leone invest in assets and micro-enterprises shortly after the intervention (the phased-in design allows only to measure impacts up to three months after). [Deininger et al. \(2016\)](#) find effects of the India public works program on agricultural productivity.

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<sup>6</sup>[Beegle et al. \(2017\)](#) do not find significant effects on food security in Malawi in one of the few randomized control trial of a public works program so far. [Gilligan et al. \(2009\)](#) also find limited average welfare effects of the Ethiopia PNSP program, although households who received larger transfer amounts did see improvements in some measures of food security.

We also find productive investments in income-generating activities as a result of the public works. Savings remain significantly higher 15 months after the end of the program, and could be further used in the future. However, profits in those activities only marginally increase. Therefore, the public works we evaluated has no sustained impacts on earnings in the longer run.

In the two programs evaluated in this thesis and more specifically **Chapter 2 and 3**, training interventions are used to transfer entrepreneurial skills. Participants learn how to get information on their market and select an activity to launch, set prices and manage stocks, separate accounts and get familiar with basic accounting practices. Those training programs were specifically designed for low literacy levels.

Skills training programs are the most common tool for addressing failures on the supply side by developing skills relevant for the labor market, therefore increasing human capital. There is a great variety of skills training programs with some targeting the lack of managerial skills or "technical" business skills (such as accounting, stock planning), while others work on behavioral skills such as entrepreneurship spirit or personal initiative. Government-sponsored training is particularly relevant for "non firm-specific" skills, that is general skills for which firms have no incentive to pay given the risk of workers to quit or be poached. Such training programs are not provided by private providers which do not offer relevant and adequate training to most vulnerable groups in need (e.g. simplified curricula, or curricula adapted for low literacy levels) at affordable cost. In addition, even if the returns to more human capital are high, individuals might not estimate them correctly and therefore under-invest in their skills.

Evidence on the impact of standalone training programs (without capital injection or support to capital access) is mixed. [McKenzie and Woodruff \(2014\)](#) and [Jayachandran \(2020\)](#) provide comprehensive literature reviews. They note that many evaluations do not find significant and lasting impacts on revenues or profits, with some exceptions (e.g. [McKenzie and Puerto \(2017\)](#)). In fact, almost all studies find a significant improvement of management practices covered by the training in the short run, and an increase in entrepreneurial know-how. Yet, these effects are small in magnitude and often not long-lasting.<sup>7</sup> Further along the causal chain, the main issue is that in most studies they do not translate into better business outcomes or higher earnings for

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<sup>7</sup>In Ghana, [Karlan et al. \(2012\)](#) find that after one year, the effects observed after six months on managerial practices have disappeared. In Peru, [Valdivia \(2015\)](#) also observe that the additional effect of training on skills and practices fades.

the individual. Considering the high cost of training programs, standalone training interventions are not appealing policy candidates. As [Blattman and Ralston \(2017\)](#) put it : *“Policymakers who advocate skills training programs should bear the burden of proof that these programs will work”*.

For both programs (**Chapter 1 and 2**), we measure how much individuals have learned from the training (using a quiz) and whether or not they took actions to implement new practices.

In **Chapter 2 and 3**, we evaluate an economic inclusion program supporting micro entrepreneurship. It provides both training and capital injection in the form of a cash transfer.

Such intervention focuses on the demand side by supporting the creation and growth of micro-enterprises. The poor often struggle to access capital at reasonable price, even through MFI. This is despite large returns to capital, observed in several empirical studies with microentrepreneurs (e.g. [de Mel et al. \(2008\)](#) or [Fafchamps et al. \(2014\)](#)). The literature is quite unanimous regarding microcredit loans, which impacts on firms are modest and not transformational ([Banerjee et al. \(2015a\)](#), [Crépon et al. \(2015\)](#)). In fact, the impact of microcredit on business outcomes is precisely estimated to be zero along most of the distribution according to [Meager \(2019\)](#), which provides a meta-analysis of the literature using Bayesian hierarchical models. Other than microcredit, there is a large literature evaluating capital-centric interventions providing capital "in kind" (assets) or "in cash" to micro-entrepreneurs. Several papers show encouraging impacts on business performance (in Sri Lanka [de Mel et al. \(2008\)](#), in Ghana [Fafchamps et al. \(2014\)](#) or in Uganda [Blattman et al. \(2014\)](#)).<sup>8</sup>However, recent evidence suggest that those effects might not last and therefore such programs would act only as short-term "kick-starter" ([Blattman et al. \(2020\)](#)).<sup>9</sup> [Blattman and Ralston \(2017\)](#) conclude from their literature review that such interventions are more successful than standalone training programs at increasing earnings. However, improving the cost-effectiveness of capital centric interventions remain key.

Graduation programs, or similar multi-faceted micro-entrepreneurship interventions, are a related type of instrument, although focused on the vulnerable rather than micro-businesses. Those are

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<sup>8</sup>In Ghana, [Fafchamps et al. \(2014\)](#) specifically compare the effectiveness of a cash versus an asset transfer (inputs or equipment). Returns on capital are high especially for men. Results suggest stronger impacts when the transfer is "in-kind", which support the theory that behavioral bias can lead to invest less cash transfers.

<sup>9</sup>In Uganda with the YOP program ([Blattman et al. \(2014\)](#)), a large transfer to groups of youth with a project leads to investments in the capital of the activities (stock and equipment) as well as skills acquisition through vocational training. The monthly income of beneficiaries increases by 40%, even 4 years after. However, a study assessing the impacts 9 years after shows convergence between control and treatment individuals in terms or earnings ([Blattman et al. \(2020\)](#)).

economic inclusion program, which help people in improving their livelihoods. Supporting their income-generating activities by addressing the multiple constraints they face is a way to achieve this goal. Regarding support to micro-entrepreneurship, a large number of programs already address both human and capital constraints in integrated interventions. There are several evaluations of them, with good results.<sup>10</sup> Graduation interventions, pioneered by BRAC in Bangladesh, have also been introduced in that spirit. They provide in-kind capital (transfer of assets, usually livestock) and skills training, as well as consumption support (regular food or cash transfer), coaching, and facilitated access to savings. There is solid evidence on its welfare impacts across continents (Bandiera et al. (2017) in Bangladesh,; Banerjee et al. (2015b) in Ethiopia, Ghana, Peru, Honduras, India and Pakistan; Chowdhury et al. (2017) in South Sudan ; Bedoya et al. (2019) in Afghanistan ; Brune et al. (2020) in Yemen ; forthcoming Bossuroy et al. (2020) in Niger).

All **three chapters** study employment programs that also aim to help with stability. In **Chapter 3** in particular, we look in detail at the social outcomes of the intervention, both on direct beneficiaries and indirect other villagers, since the increase in social cohesion was a key goal of the program. The stability or social cohesion objective was not always explicit in the program design and in fact, the evidence that employment reduces incentives to riot or engage in violent activities is tenuous. The assumption made by many policy makers is that increased earnings and / or the requirement of the job (working hours) can deter individuals from unrest or violent activities. Blattman and Ralston (2017) discuss the possible mechanisms under such causal link – including the increase in the opportunity cost of crime when one is employed- but they also note that having a “portfolio” of activities is compatible with having both formal and illegal activities. There are few empirical papers providing evidence on this link (two notable exceptions are Blattman and Annan (2016) and Blattman et al. (2017) both reporting the results of interventions in Liberia that targeted high-risk individuals). The challenge for empirical studies is that most of

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<sup>10</sup>In Uganda, the WINGS program targets vulnerable young women who receive a \$150 grant coupled with 5 days of training and follow-up. Beneficiaries’ incomes double (measured 16 months after the program) (Blattman et al. (2016)). In Liberia, beneficiaries of the ex-combatant reintegration program receive technical training and a grant for the development of independent agricultural activities, which increases their income by 12% compared to the control group 14 months after the end of the training (Blattman and Annan (2016)). The program in Ghana evaluated by Karlan et al. (2012) compares three types of interventions: receiving a cash grant, receiving individualized and specific training by consultants, or receiving a combination of the two previous interventions. The results do not allow to argue in favor of the grant or the training since the results on the profit of the activities are insignificant for the three interventions.

the time programs do not target individuals most at risk of violence, and that it requires first an employment program successful at increasing incomes. Even fewer studies have tested whether public works can generate social externalities by offering alternative occupations to populations in fragile or post-conflict settings.<sup>11</sup> Besides, there are non-monetary incentives for violence which may not be successfully changed by an increase in income. Cognitive behavioral therapy, such as the one implemented in Liberia, offer an interesting alternative to standard monetary incentives (Blattman et al. (2017)). In the THIMO public works, the structure of a work day and the new network of co-workers can positively impact individuals, by reducing feelings of frustration, loneliness or social identity. In the enhanced savings group intervention within PRISE program studied in Chapter 2 and 3, the sense of belonging altogether to an economic group can also affect the perception of other ethnic groups. Some aspects of those employment programs, besides the material aspect, could contribute to increase stability. It is of course difficult to identify which aspect primarily drives changes. Overall, and as pointed out by Blattman and Ralston (2017), we lack empirical evidence on the causal relationship between employment and improved social cohesion or stability, especially at the individual level. This thesis contributes to this strand of research.

A final overarching question tackled in this thesis is the cost effectiveness of such employment programs. In this thesis, we take a specific cost-effectiveness angle for both evaluations. With a limited budget to be spent, in particular on social assistance and labor, policymakers must make informed decisions about which programs to fund. This is where rigorous impact evaluations, such as the ones presented here, are crucial. Both evaluations presented in **Chapter 1 and 2/3** are based on a randomized controlled trial that was part of the program design from very early on through the collaboration of our research team with government and implementing agencies. Both programs were thought as experiments or “proof of concept” that the ministry of employment would learn from. This is an important fact to highlight, since close collaboration between researchers and policymakers are crucial for evidence-based policies. Randomized controlled trials are excellent tools to combine a rigorous scientific methodology with a simple approach, helping policymakers to understand and use the evaluation results for policy-making. A cost-effectiveness analysis is especially informative. It puts in perspective the cost per individual of the program

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<sup>11</sup>Recent exceptions include Fetzer (2019) and Amaral et al. (2015). The first paper analyzes the linkages between the Indian public works program and conflict, and the second gender-based violence.

with the benefits generated (impacts on earnings or consumption for example).

In **Chapter 1**, we look at how changes in targeting rules can substantially improve the cost-effectiveness of a program, even with easy-to-implement rules. In **Chapter 3**, we show that it is important to take into account indirect effects (spillovers) when assessing the overall efficiency of a program. However, benefits can be non-monetary, and in both experiments we also measure well-being impacts using indices of psychometric variables.

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# Chapter 1

## Do Workfare Programs Live Up to Their Promises? Experimental Evidence from Côte d'Ivoire

### 1.1 Introduction

Public works programs are an important instrument in the portfolio of policy makers trying to address the social challenges of unemployment, underemployment and poverty. They offer temporary employment, typically remunerated at the minimum wage or below, for the creation of public goods, such as roads or infrastructure. In Sub-Saharan Africa, our context in this paper, labor-intensive public works programs have been adopted in response to transient negative shocks such as those induced by economic downturns, climatic shocks or episodes of violent conflicts, and often aim to offer public employment as a stabilization instrument.

While traditional welfare programs, such as cash transfers, could also be used to support the poor and vulnerable, workfare programs have some theoretical advantages that could make them superior poverty alleviation tools than welfare programs, both in the short- and medium-run. A first stated advantage of workfare programs, as highlighted by [Besley and Coate \(1992\)](#) or [Ravallion \(1991\)](#), is that they can in principle solve the difficult problem of targeting. The targeting of social protection programs is particularly complex in low-income countries because of a lack of robust data, challenges in identifying beneficiaries at the bottom of the welfare distribution, as well as weak systems and institutions, leading to potential errors of inclusion or exclusion. Public works programs very often rely on self-targeting to select transfer beneficiaries, based on the idea that only the more disadvantaged would be willing to supply labor. How well

self-targeting works in reaching the most vulnerable will, however, depend on how broadly (or narrowly) appealing the public works option is (accounting for earnings and disutility of work) compared to the alternative. In environments such as those in many developing countries where a large fraction of the population is under-employed in informal work paying below the legal minimum wage, workfare programs could appeal to a broad cross-section of the population and thus could fail to appropriately target transfers towards the most vulnerable.

A second stated advantage of workfare programs is that they may have longer-lasting positive impacts on individual beneficiaries than standard welfare programs. Regular work, even if unpleasant, may improve skills, behaviors, work habits, or well-being (such as self-esteem or mental health). Particularly relevant to post-conflict environments, engaging beneficiaries in cash-for-work rather than providing cash transfers may also operate as a social stabilization tool through an incapacitation effect: time spent working may displace risky behaviors or socially disruptive activities. All of this may translate into lasting behavioral changes that may increase labor productivity, such as by building regular work habits that might be especially difficult to learn outside of regular formal employment. Furthermore, through skill development or the signaling value of prior work experience, public works may increase the future employability or productivity of the beneficiaries. Such longer-term benefits can potentially be further enhanced by adding complementary productive interventions, such as savings facilitation or training, to the workfare experience. Whether post-program benefits of participation in public works on employment, behaviors or skills exist, however, largely remains an empirical question. And if such post-program benefits exist, it is unclear whether these benefits are tied to the distinctive “work” part of workfare programs (such as changes in work habits or behaviors), or whether they derive from more generic income support (such as by enabling saving and investing in productive activities), a channel that would also extend to traditional welfare programs. In addition, and of central interest to this paper, if such post-program benefits exist, it is unknown whether there is a trade-off between the shorter-term objectives of public work programs (better targeting cash transfers towards the most vulnerable) and any such longer-term benefits.<sup>1</sup>

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<sup>1</sup>There are other stated advantages of workfare programs. Workfare programs contribute to the creation or maintenance of public assets (e.g. better roads) which may benefit the broader community; this argument is particularly relevant in contexts where physical infrastructure was destroyed or damaged because of a crisis (e.g. climatic shocks or violent conflict). Another advantage of public work programs compared to traditional welfare programs is that they are often politically more acceptable and sustainable: political preferences for workfare programs are often linked to (valid or not) concerns about welfare dependency (and how unconditional transfers may disincentivize work) as well as a desire to generate immediate visible improvements to employment conditions.

This paper assesses the extent to which these two main promises of public works programs hold in practice, namely that self-targeting is efficient and that the programs induce immediate and lasting impacts. As such, the paper makes two main contributions. First, we analyze both the contemporaneous and post-program impacts of a randomized public work program on participants' employment, earnings and behaviors. Second, we leverage machine learning techniques to study the heterogeneity of program impacts, which is key to assess whether departing from self-targeting would improve program effectiveness. Using machine learning as a benchmark, we analyze how program performance would change under alternative self-targeting and targeting approaches.

The public works program we study was implemented by the Côte d'Ivoire government in the aftermath of a post-electoral crisis in 2010/2011, and was funded by an emergency loan from the World Bank. The stated objective of the program was to improve access to temporary employment opportunities among low-skilled young (18-30) men and women in urban or semi-urban areas who were unemployed or underemployed, as well as to develop their skills through work experience and complementary training. Participants in the public works program were employed for a period of 7 months to rehabilitate and clean road infrastructure. Program participants worked 6 hours per day, 5 days per week and were remunerated at the statutory minimum daily wage, corresponding to about \$10 PPP 2014 per day (CFA 2,500), or approximately \$223 PPP 2014 per month (CFA 55,000).<sup>2</sup>

All young men and women in the required age range and residing in one of 16 urban localities in Côte d'Ivoire were eligible to apply to the program. Because the number of applicants outstripped supply in each locality, fair access was based on a public lottery, setting the stage for a robust causal evaluation of the impacts of the program. In addition, randomized subsets of beneficiaries were also offered (i) basic entrepreneurship training to facilitate set-up of new household enterprises and entry into self-employment, or (ii) training in job search skills and sensitization on wage employment opportunities to facilitate access to wage jobs (e.g. help in identifying wage job opportunities, CV preparation, interview skills, etc.). We carried out rich surveys of youth in the treatment and control groups at baseline, during the program (4 to 5 months after the program had started), and 12 to 15 months after the program ended.

Our results on contemporaneous impacts demonstrate that the program had limited effects on

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<sup>2</sup>We use an exchange rate of USD 1=XOF 493.757 (official average exchange rate in 2014 (from IMF)) to convert CFA francs to US dollars. We use the PPP conversion factor, GDP (LCU per international \$) of 246.519 for Côte d'Ivoire in 2014 (from the World Bank).

the likelihood of employment, but induced shifts in the composition of employment. The value of the program for the modal applicant was therefore not as a way to escape unemployment but more as a way to escape under-employment in low-paying informal activities: monthly earnings are about CFA 27,083 higher in the treatment group, from a base of CFA 42,841 in the control group. While the program lifted earnings, foregone earnings are quantitatively large, with earning gains representing only about 53 percent of the transfer.<sup>3</sup> The intervention increased savings and well-being. It also induced changes in work habits and behaviors in the short-term.

Twelve to 15 months after program completion, we do not find impacts on the likelihood of employment, hours worked or the composition of employment (salaried work vs. self-employment). While we find some post-program impacts on earnings, which mostly stem from self-employment activities, they are small and not always robust to alternative specifications. Savings stock and well-being remain higher, but there are no lasting impacts on work habits or behaviors.

Since our results show that self-targeting based on the formal minimum wage failed in this context, how much would results improve if the offered wage was lowered or if the targeting criteria were adjusted? We use the distribution of predicted impacts derived from machine learning methods to answer both of these questions. First, we find that lowering the wage below the formal minimum wage would not improve program performance. As we explain in a simple theoretical framework, this is because the improvement in self-targeting is offset by lower transfer amounts and because the improvement in self-targeting is itself limited in our empirical context given the small concentration of “marginal” applicants with small predicted program impacts.

Second, we show that there are substantial differences in predicted impacts across participants during the program. The average impact on earnings in the short-term for the top 25% of the predicted distribution (upper quartile) is approximately 2.2 times more than for the bottom 25% (lower quartile). In contrast, we do not detect heterogeneity in post-program impacts on earnings and find no evidence of systematic correlation (positive or negative) between short-term and long-term impacts. We document the characteristics of those who benefit the most from public works, and assess how alternative targeting rules based on these characteristics would improve program effectiveness. Compared to the benchmark scenario with self-targeting based on the formal minimum wage, the cost-effectiveness ratio would improve by 30% to 52% by targeting

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<sup>3</sup>Datt and Ravallion (1994) and Jalan and Ravallion (2003) estimate the net income gains from public works programs in India and Argentina, finding foregone income ranging between 30% and 50%.

women only, or by targeting youths with low predicted baseline earnings. In the end, direct impacts on youths' earnings during and after the program remain substantially below program costs, especially in light of large administrative costs. The cost per participant is about 2 times the estimated direct impacts on earnings up to the endline (12 to 15 months after the program) even under improved targeting.

Despite the popularity of public works programs, experimental evidence on their overall effectiveness remains relatively limited (Subbarao et al., 2012; Gehrke and Hartwig, 2018). Existing evidence mostly comes from quasi-experimental studies on a small number of influential programs, especially from India (Murgai et al., 2015; Imbert and Papp, 2015, 2019; Muralidharan et al., 2016, 2017). Most evidence focuses on short-term economic impacts during the intervention,<sup>4</sup> and in fewer cases impacts on risky behaviors related to conflict or violence (Fetzer, 2020; Amaral et al., 2015). The effectiveness of workfare programs largely depends on whether they have productive impacts (Murgai et al., 2015), and our paper relates to a small literature that assesses whether beneficiaries of public works programs find pathways towards more productive post-program employment in wage jobs or in the informal sector. Ravallion et al. (2005) do not find significant impacts on post-program earnings in Argentina. Alik-Lagrange et al. (2017) find some persistent effects in rural areas of Colombia and suggest participants acquired new skills. Rosas and Sabarwal (2016) document investments from public works beneficiaries in assets and micro-enterprises in Sierra Leone, and Deininger et al. (2016) in agriculture in India. A few studies analyze the effectiveness of complementing public works programs with training or savings facilitation, including Galasso et al. (2004) and Almeida and Galasso (2010). Gilligan et al. (2009) report impacts of the Ethiopia public works program combined with agricultural support on adoption of agricultural technologies and off-farm small businesses. To our knowledge, no study has analyzed whether there are trade-offs between maximizing contemporaneous and post-program benefits from public works.

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<sup>4</sup>Several papers have assessed the role of public works as a short-term safety net or insurance mechanism providing temporary employment and income to vulnerable populations during lean agricultural seasons or after economic shocks. Findings regarding program impacts on welfare and food security are mixed. Galasso and Ravallion (2004) document how a workfare program in Argentina attenuated the negative welfare effects of an economic crisis, and Ravi and Engler (2015) find beneficial impacts of India's workfare scheme on consumption and food security. On the other hand, Beegle et al. (2017) do not find significant effects on food security in Malawi in one of the few randomized control trial of a public works program so far. Gilligan et al. (2009) also find limited effects of the Ethiopia PNSP program, although households who received larger transfer amounts did see improvements in some measures of food security. A few studies have estimated the impact of public works programs on school enrollment and child labor (Li et al., 2013; Islam and Sivasankaran, 2015; Shah and Steinberg, 2019), also with mixed results.

Our paper further complements the literature on the targeting of social programs, a topic that has garnered substantial policy and research interest (for recent reviews, see [Hanna and Olken \(2018\)](#); [Banerjee et al. \(2019\)](#); [Gentilini et al. \(2020\)](#)). Targeting experiments have predominantly tested how best to rank households in terms of poverty in the context of cash transfer programs, mainly contrasting community-based approaches and statistical methods such as proxy means testing (e.g. [Alatas et al. \(2012, 2016\)](#); [Premand and Schnitzer \(2020\)](#)). Questions have been raised as to whether there are trade-offs between selecting the poorest and maximizing program impacts ([Basurto et al., 2020](#)). Earlier studies of workfare programs focused on analyzing the profiles of beneficiaries and benefit incidence patterns.<sup>5</sup> Following the seminal work of [Manski \(2004\)](#), targeting has also been studied as a statistical decision problem. The approach aims to derive an assignment rule that maximizes welfare after program implementation. Heterogeneous treatment effects are estimated before being plugged into a social welfare objective function to derive the optimal assignment. This is the approach followed by [Bhattacharya and Dupas \(2012\)](#) in a context, close to ours, in which the program allocation involves a budget constraint.<sup>6</sup> In this paper, we use machine learning techniques as a benchmark to assess alternative targeting and self-targeting options.

On the methodological front, our paper relates to a growing literature applying machine learning to analyze treatment heterogeneity. Since the influential contribution of [Athey and Imbens \(2016\)](#), several recent papers have explored the application of these techniques.<sup>7</sup> In our application, (i) we assess the extent of heterogeneity in program impacts on earnings, (ii) we assess how a reduction of the offered wage would affect performance based on the distribution of predicted impacts, (iii) we document the profile of individuals with highest predicted impacts, (iv) we assess whether there are trade-offs between maximizing contemporaneous and post-program impacts, (v) we illustrate how machine learning can be used to explore mechanisms for impacts, and (vi) we compare how alternative targeting rules compare to machine learning estimates. One important aspect of our application is that we combine machine learning techniques with the statistical framework for

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<sup>5</sup>See for instance [Ravallion et al. \(1993\)](#); [Datt and Ravallion \(1994\)](#); [Jalan and Ravallion \(2003\)](#); [Alik-Lagrange and Ravallion \(2018\)](#).

<sup>6</sup>[Kitagawa and Tetenov \(2018\)](#) develop an alternative approach that avoids the intermediate step of estimating heterogeneous treatment effects and directly identifies the assignment rule. See also [Athey and Wager \(2017\)](#) for an application to observational studies.

<sup>7</sup>For a review, see [Knaus et al. \(2020b\)](#), as well as applications in a variety of context including marketing ([Ascarza, 2018](#)), cash grants for firms ([McKenzie and Sansone, 2019](#)), employment programs ([Knaus et al., 2020a](#)), financial work incentive programs ([Strittmatter, 2018](#)), summer employment programs for disadvantaged youth ([Davis and Heller, 2017, 2020](#)), or role models and educational choices ([Breda et al., 2020](#)).

inference developed in Chernozhukov et al. (2020), of which Breda et al. (2020) offer another application.

The rest of the paper proceeds as follows. Section 1.2 outlines a framework to analyze how self-targeting induces heterogeneity in public works impact. Section 1.3 describes the intervention design and data. Section 1.4 presents the empirical strategy and machine learning approach. Section 1.5 presents results on average impacts, and Section 1.6 on heterogeneity. Section 1.7 concludes. Additional material, tables and figures are presented in the Appendix.

## 1.2 Framework

### 1.2.1 Contemporaneous Impacts: A Simple Framework

In this section, we present a simple framework that clarifies how: 1) one should expect heterogeneity of program impacts under self-targeting; 2) alternative targeting approaches may improve program effectiveness; and 3) variation in the offered program compensation will impact self-selection and program effectiveness. Appendix Section E provides additional details.

Let  $\theta$  denote potential participants' hourly earnings in non-program activities,  $h$  hours worked, and  $c(h)$  the additive disutility of working  $h$  hours. Absent the workfare program, individuals have an optimal number of hours worked  $h_0(\theta)$  given by  $\theta = c'(h_0)$ , leading to earnings  $W_0(\theta) = \theta h_0(\theta)$  and utility  $U_0(\theta) = \theta h_0(\theta) - c(h_0(\theta))$ .

Assume that participation in the workfare program provides a transfer  $T$  in exchange of  $h_p$  hours of work. Further assume that the disutility of time spent in program activities is the same as of time spent in non-program activities. When an individual participates in the program she can also decide to work outside the program. We note  $W_1(T, \theta)$  her total earnings in case of program participation and  $U_1(T, \theta)$  the corresponding utility.

The impact on individual earnings before the actual decision to participate or not is defined as:

$$W_1(T, \theta) - W_0(\theta) = T - \Delta(T, \theta) = s(T, \theta). \quad (1.1)$$

where  $\Delta(T, \theta)$  represents the earnings that individuals forgo in order to participate in the program,

and  $s(T, \theta)$  is the net impact on earnings.

Individuals decide to participate in the program if the impact on their utility is positive:  $U_1(\theta) - U_0(\theta) > 0$ .

There are two cases to consider (see illustration in Appendix Figures B5). The first corresponds to a small transfer  $T$  offered for the  $h_p$  hours of work:  $T < h_p c'(h_p)$ . In such a case, only individuals with a low productivity participate and when they participate they only work in the program, leading to earnings  $W_1(\theta) = T$  and foregone earnings  $\theta h_0(\theta)$ . The productivity threshold  $\underline{\theta}$  triggering participation is given by  $T - c(h_p) = U_0(\underline{\theta})$ . Individuals with a productivity larger than  $\underline{\theta}$  do not participate.<sup>8</sup>

The second case corresponds to larger transfers:  $T > h_p c'(h_p)$ . In such a case, there are two types of participants. Individuals with a small productivity ( $\theta < \underline{\theta} = c'(h_p)$ ) only work in the program, again with earnings  $W_1(\theta) = T$  and foregone earnings  $\theta h_0(\theta)$ . Individuals with intermediate productivity ( $\underline{\theta} < \theta < \bar{\theta}$ ) participate in the program but they also work outside the program; their number of hours worked outside of program activities will be given by  $h_0(\theta) - h_p$ , as their optimal total number of hours worked is not impacted by program participation. Their earnings in the program will therefore be given by  $W_1(\theta) = T + \theta(h_0(\theta) - h_p)$ , with foregone earnings  $\theta h_p$ .<sup>9</sup> Individuals with large productivity ( $\theta > \bar{\theta} = T/h_p$ ) do not participate in the program.<sup>10</sup>

Importantly, note that in all cases above, a change in the transfer  $T$  does not change forgone earnings, so that  $\Delta(T, \theta) = \Delta(\theta)$  and hence  $s(T - x, \theta) = s(T, \theta) - x$ .

Whichever case applies, given the heterogeneity parameter  $\theta$ , there will be variation in the program impact on earnings.

Individuals decide whether or not to apply for the workfare program solely based on whether participation will increase their earnings, i.e. if  $s(T, \theta) > 0$ .<sup>11</sup> We call  $s_T \equiv s(T, \theta)$  this random

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<sup>8</sup>Notice that in such a case the impact on earnings for marginal applicants is  $T - \underline{\theta}h(\underline{\theta}) = c(h_p) - c(h(\underline{\theta})) > 0$ . There is a discontinuous increase in earnings for marginal participants.

<sup>9</sup>Given the unchanged total number of hours of work for these now marginal applicants, their change in utility is the same as their change in earnings. In such a case, the lower bound of the distribution of the impacts on earnings should be zero.

<sup>10</sup>It is possible to show that  $\underline{\theta} < \bar{\theta} \Leftrightarrow \underline{\theta} < \underline{\theta}$ .

<sup>11</sup>Marginal applicants are those who have the same utility whether they participate in the program or not. As stressed above, for marginal applicants who work outside the program, the difference in utility is the same as the difference in earnings. In such a case, there should be no discontinuity in earnings for marginal applicants. Our estimation of the distribution of individual treatment effects on earnings shows that this case is likely in our setting.

variable in the population of those who self-select into the program.

Let  $B$  be the total budget for transfers in the program and  $N_A(T)$  the number of individuals who self-select into the program when the transfer level is  $T$ . Assume, that  $B < TN_A(T)$ , that is, the program is over-subscribed and a lottery is used to allocate program slots among the pool of self-selected applicants, as in our application. The lottery success rate  $\lambda(T)$  is simply given by  $B = TN_A(T)\lambda(T)$ . The average contemporaneous program impact on earnings over those who self-select (also including those who were randomized out) is:<sup>12</sup>

$$S_{lottery}(T) = \lambda(T)E(s_T) \quad (1.2)$$

In order to increase program performance, targeting could be introduced to prioritize inframarginal applicants, or the effectiveness of self-targeting could possibly be improved with a lower transfer amount. We discuss both approaches below.

We first look at potential improvements associated with targeting. A growing literature explores the idea of improving program performance through targeting (see [Manski \(2004\)](#)). Our approach follows [Bhattacharya and Dupas \(2012\)](#), who seek to maximize outcomes under a budget constraint for transfers ( $B$  in our case). Consider randomly assigning the program to applicants, with probabilities depending on some observable characteristic  $z$ . The assignment probability would be a function  $\alpha(z)$ . The objective is to find the assignment function  $\alpha(\cdot)$  that maximizes program impacts on earnings:

$$S_{targeting}(T, \alpha) = E(\alpha(z)s_T) \text{ s.t. } TN_A(T)E(\alpha(z)) = B \quad (1.3)$$

Given  $E(\alpha(z)s_T) = E(\alpha(z)E(s_T|z))$ , the optimal assignment rule is simply  $\alpha^*(z) = 1(s_T(z) > \bar{s})$ , where  $s_T(z) = E(s_T|z)$  and  $\bar{s}$  is chosen such that  $TN_A(T)P(s_T(z) > \bar{s}) = B$ , i.e.  $P(s_T(z) > \bar{s}) = \lambda(T)$ . In this case, the average contemporaneous program impact on earnings is given by:

$$S_{targeting}(T, \alpha^*) = \lambda(T)E(s_T|s_T > \bar{s}) \quad (1.4)$$

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<sup>12</sup>Expectations are taken over the population of those who select for the transfer  $T$ , i.e. they are taken with respect to the distribution  $f(\theta|S(T, \theta) > 0)$ .

Such an assignment rule obviously dominates the assignment using a lottery. In the empirical section of the paper, we apply machine learning techniques to estimate the function  $s_T(z)$ . We then compute the gains associated with this optimized assignment rule, compared both to lottery assignment as well as alternative targeting rules (such as prioritizing women or prioritizing applicants with low self-reported or proxied earnings at baseline).

Second, we consider the effects of changing the transfer amount from  $T$  to  $T - x$ , for a fixed number of hours of work in the program ( $h_p$ ). As long as the program is oversubscribed, lowering the transfer reallocates program slots from those with lower impact to those with higher impact. While this effect is positive (both in terms of reallocation and in terms of more individuals being served), it comes at the cost of lowering the transfer which negatively affects all participants. Therefore, the change in the average impact on earnings is ambiguous. We derive the expression for the change in the average impact in appendix E.2 and show that it depends on the distribution of  $s_T$ .

Indeed, consider all the (potential) distributions achieving the same average impact on earnings. Absent an adjustment of the lottery success rate, a reduction in the transfer causes an equivalent reduction in average impact across all these distributions. However, given a fixed budget for the program, the lottery success rate can increase; by how much will depend on the distribution of individual program impacts rather than the average program impact. In particular, in distributions that include a large share of applicants with an impact close to zero, a large number of individuals will select-out and thus the lottery success rate will increase more. If the increase in the lottery success rate is large enough, the impact on those who can enter the program more than compensates the initial reduction in average earnings.

More generally, under such an alternative contract, the youth who self-select for the program are those for whom  $s(T - x) > 0$  or, given  $s(T - x) = s_T - x$ , those for whom  $s_T > x$ . The number of individuals who apply is given by  $N_A(T - x) = N_A(T)P(s_T > x)$ , and the number of people who can be served  $N(T - x)$  is given by  $B = (T - x)N(T - x)$ .<sup>13</sup> A lottery is again used to allocate the program among applicants as long as  $N(T - x) < N_A(T - x)$ . The rate of success of this

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<sup>13</sup>In the framework, we consider the average earnings over those who initially self-select in the program. We could consider that the social planner seeks to maximize the average of a function of earnings instead of earnings, for example the share of those whose earnings are above a given threshold  $S$ . In such a case, it is worth noting that as long as the program is oversubscribed, a reduction in the transfer increases the number of individuals who receive the transfer, which would improve this objective if the transfer is above  $S$ .

lottery  $\lambda(T - x)$  depends on  $x$  and is given in this case by  $\lambda(T - x) = N(T - x)/N_A(T - x) = B/(N_A(T)(T - x)P(s_T > x)) = \lambda(T)T/((T - x)P(s_T > x))$  (and by 1 otherwise). Clearly, the lottery rate increases with  $x$ : there are fewer individuals who still apply to the program and the amount to distribute per participant is by definition lower. The average contemporaneous program impact on earnings over those who would self-select for the full transfer  $T$  (thus including a zero impact for those who select out for the smaller transfer  $T - x$  as well as those who are randomized out) can be written as:

$$S_{\text{lottery}}(T - x) = \lambda(T - x)E((s_T - x)1(s_T - x > 0)) = \lambda(T)\frac{T}{T - x}E(s_T - x|s_T > x) \quad (1.5)$$

Unlike  $S_{\text{targeting}}$  in equation 1.4, there is no direct indication that changing the transfer amount from  $T$  to  $T - x$  would lead to an improvement in  $S_{\text{lottery}}$ . Actually, the impact on earnings after a reduction in the transfer depends on the form of the distribution of  $s_T$ . Appendix Figure B6 provides two examples that illustrate how  $S_{\text{lottery}}(T - x)$  changes with  $x$  for different forms of the distribution  $s_T$ .<sup>14</sup>

The two distributions are chosen to be symmetric around a mean impact of CFA 25,000. They have very different shapes, however.

In the first example, the distribution of individual program impacts has a mode close to zero, corresponding to a situation with a large mass of applicants with small program impacts. In such a case, when the transfer is reduced, there are many potential participants who no longer apply (i.e.,  $P(s - x > 0)$  decreases sharply). This makes it possible to substantially increase the lottery success rate and this increase is large enough to compensate for the reduction in the size of the transfer, so that the average program impact on earnings in the population of initial applicants increases.

In the second example, the mode of the distribution is located at a larger value and the density is almost zero for very low values of  $s$ , corresponding to a situation with a small mass of applicants with small program impacts. As a result, when the transfer decreases, there are very few people who do not apply for the program anymore. The lottery rate increases only by a small amount, which is not enough to compensate for the direct negative effect of the reduction in the size of the transfer. As a result, the average program impact on earnings in the population of initial

<sup>14</sup>Appendix Section E studies these relationships more formally.

applicants decreases with a reduction in the transfer amount.

The shape of the distribution of  $s_T$  therefore plays an important role in overall program performance and in  $S_{lottery}(T - x)$ , the average contemporaneous program impact on earnings for a reduced transfer. For this reason, the empirical section will study the distribution of predicted impacts on earnings. While this true density function is unknown, we can proxy for it using machine learning estimates of treatment effects on contemporaneous earnings conditional on a rich set of available covariates.

### 1.2.2 Post-Program Impacts

A first-order question in the public works literature relates to the existence and size of post-program impacts in the medium to long-term. Indeed, a growing number of public works programs also have the objective to facilitate participants' transition towards more productive occupations after the program. There is little evidence in the literature on such long-term effects, although there are several potential channels through which they could unfold. An important consideration relates to the relative allocation of short-term earning gains between consumption and savings, which will affect post-program impacts.

First, public works can help participants overcome capital constraints. Several experiments have found relatively large returns to capital for poor households (for a review, see [Blattman and Ralston \(2015\)](#)). Common instruments to make capital available to youth, such as micro credit, have not proven very effective. As such, the positive income shock induced by public works programs (but truly any income support program) could alleviate capital constraints, facilitate savings and investments.<sup>15</sup>

There are other possible mechanisms for longer-term impacts that are specific to workfare programs. Subsidized employment could be a way to improve experience, skills and productivity of participants, and ultimately increase the likelihood that they find a wage job post-program. Also, there might be behavioral effects related to program participation. For example, a workfare program requiring youths to form work habits, like getting up each morning to go to work, may induce lasting behavioral changes that will improve future employability.

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<sup>15</sup>Savings can have several potential post-program benefits, including precautionary savings to absorb future shocks, or savings to finance investments, like training or capital for income-generating activities.

On the other hand, it is also possible that participation in a public works program may have deleterious long-term impacts on participants. First, the work experience provided through the program might be of little value or only enhance skills that are not demanded in the labor market. Negative long-term impacts could also emerge because of the potential "stigmatization" of participants, with program participation sending negative signals to potential future employers. Finally, participants may give up some valuable activities or social connections in order to participate in the program, which may induce a form of capital destruction that may take time to rebuild upon exit from the program.

## 1.3 Intervention and Data

### 1.3.1 The Public Works Program

The public works intervention we study is part of an Emergency Youth Employment and Skills Development Project (PEJEDEC) set up after the 2010/2011 post-electoral crisis.<sup>16</sup> The public works program aims to provide access to temporary employment in road maintenance (such as sweeping roads or cleaning ditches) for low-skilled youths in urban areas. The program targets youths aged 18-30 in 16 localities throughout the country.<sup>17</sup> Participants are offered temporary employment for 6 hours per day and 5 days a week for a total of six months.<sup>18</sup> Participants work in teams of 25 individuals (called "*brigades*"), under the supervision of a team leader and a local supervisor. The jobs are paid CFA 2,500 (approximately \$10 PPP 2014) per workday, a wage equal to the legal daily minimum wage in the formal sector. Wages are paid monthly on bank accounts that are set-up for all participants upon enrollment. A quota of 30% of program slots was initially reserved for women.

All participants in the public works program receive a one-week basic life skills training covering issues related to HIV-AIDS, citizenship and hygiene. Some participants are also offered a complementary basic entrepreneurship training to facilitate transition into more productive self-

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<sup>16</sup>Public works programs were first introduced in Côte d'Ivoire by a post-conflict assistance project (PAPC) in the aftermath of the 2002-2007 armed conflict. The PEJEDEC public works program built on that experience. It was implemented by the national roads management agency (AGEROUTE), and supervised by BCP, the Coordination Office for Employment Programs ("Bureau de Coordination des Programmes Emploi"), under the Ministry of Labor and Social Affairs.

<sup>17</sup>Four municipalities in Abidjan (Abobo, Yopougon, Koumassi, Marcory) and 12 cities throughout the country (Yamoussoukro, Bouaké, San Pedro, Daloa, Korhogo, Abengourou, Man, Bondoukou, Gagnoa, Séguéla, Daoukro, Dimbokro).

<sup>18</sup>As explained later, the wave of the program under evaluation lasted 7 months.

employment upon exit from the program. Finally, other participants are offered a training on wage jobs search skills and sensitization to wage jobs opportunities, with the objective to facilitate transition into wage jobs upon exit from the program. Additional information on the complementary training is provided in Appendix F.

### 1.3.2 Experimental Design: Enrollment and Randomization

Four waves of the PEJEDEC workfare program were organized between 2012 and 2015, each covering 16 localities, with a similar (pre-determined) number of participant slots available for each locality in each wave. In total, 12,666 youths participated in the program. The randomized control trial focuses on the second wave of the program, which took place between July 2013 and February 2014.

The identification strategy relies on a two-step randomization process. The first step involves individual-level randomization into the program. Before the start of the second wave (and as was the case for the other waves), an intense communication campaign was organized by the implementing agency through local newspapers, local radios and public notice boards to invite interested youths to visit a registration office and apply to the program. Enrollment was open for two to three weeks in each locality. Only two eligibility criteria were applied during enrollment: applicants had to be between 18 and 30 years old, and they should not have participated in the public works program before.

Once the enrollment period had closed, public lotteries were organized in each locality (separately for men and women) to randomly select beneficiaries among the registered applicants present at the lottery.<sup>19</sup> In practice, 10,966 youths participated in the public lotteries carried out for the second wave of the program, during which 3,125 beneficiaries were selected and assigned to 125 brigades of 25 individuals each (17 men, 8 women).<sup>20</sup> Replacement of drop-outs was allowed during the first two months of the program. A waiting list was created to protect the control group, although in practice replacements were minimal.<sup>21</sup> The public lotteries were held in each

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<sup>19</sup>Public lotteries have been used continuously in Côte d'Ivoire as an assignment mechanism to allocate limited slots for jobs since the introduction of public works in the post-conflict period. The transparency of the process makes it socially acceptable and limits potential tensions. As such, the first step of the randomization protocol was already implemented as part of routine program operations.

<sup>20</sup>Beneficiaries were assigned to brigades within localities based on the number they drew in the public lottery.

<sup>21</sup>Replacements were only possible based on the waiting list, and had to be stopped when the waiting list was exhausted. This ensured that individuals in the control group were not offered the program during its implementation. In practice the waiting lists were never exhausted.

locality between the end of June and early July 2013.

The second step involves the randomization of public works brigades into groups receiving different types of complementary training that took place in the seventh and final month of the program. Specifically, brigades were randomized into three groups: (i) 45 brigades (1,225 individuals) were assigned to receiving the public works only; (ii) 40 brigades (1,000 individuals) were assigned to receiving the public works plus the complementary basic entrepreneurship training, and (iii) 40 brigades (1,000 individuals) were assigned to receiving the public works plus the wage jobs search skills training. This second randomization was stratified by locality, and was performed through a lottery held in the project office in the presence of implementing partners and a public notary in November 2013. The results of this lottery remained confidential until two weeks before the start of the complementary training (in January 2014) in order to limit potential response bias during the midline survey.

### 1.3.3 **Timeline and Data**

#### **Timeline and Surveys**

A baseline survey was conducted shortly after the lotteries. The study sample includes all the individuals selected to participate in the program after the first randomization (3,125 individuals), as well as a control group obtained from a (random) sample of 1,035 individuals drawn from the non-beneficiaries that were not on the waiting list. The data collected at baseline included information about employment and earnings. It also captured a range of other characteristics such as risk and time preferences, behavioral skills and results of tests measuring skills or manual dexterity. Attrition at baseline was very low (1.5%). The public works activities started between early and late July 2013, depending on the locality. Participants received the one-week life skills training in August 2013.

A midline survey was conducted on 3,036 individuals (2,001 beneficiaries and the control group) between the end of November 2013 and early January 2014, i.e. 4 to 5 months after the start of the program.<sup>22</sup> Both treatment and control individuals as well as the heads of their household were interviewed at midline. Attrition at midline was low (2.6%) and balanced across treatment

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<sup>22</sup>The 2,001 treated individuals are a sub-sample of the 3,125 beneficiaries stratified by locality, brigade and gender. We excluded from the midline survey brigades that had been assigned to the wage employment training. This is because their supervisors were following a management training at the time of the survey, and we were wary of any behavioral changes that could potentially affect outcomes.

and control groups.<sup>23</sup> The midline questionnaire includes very detailed modules on employment (up to three activities), earnings, time use, well-being, behaviors and list experiments to proxy risky behaviors.

The public works program ended in February 2014. It was originally expected to end in January 2014. However, as the complementary training activities only started in January, participants were given a one-month extension on their contracts, which extended the public works duration from 6 to 7 months. This ensured that all brigades of individuals selected to participate in one of the training programs could do so while being paid by the program (at the same wage) for at least part of their training, which reduced their opportunity cost of time during the training.<sup>24</sup>

An endline survey was conducted between March and July 2015, i.e. between 12 to 15 months after the program ended. The sample included the whole baseline sample of 4,160 individuals in the treatment and control groups, plus 200 individuals randomly selected to be added to the control group.<sup>25</sup> Again, both experiment subjects and household heads were interviewed. A tracking phase took place in September 2015. The final attrition rate was 6.2%, and was balanced between treatment and control groups. The endline questionnaire was based on the midline survey and enriched with retrospective information on job search, independent activities (including past projects) and an employment calendar.

## Descriptive Statistics

Table 1.1 presents descriptive statistics for the selected applicants (column (1)). Public works applicants are on average 25 years old, and 94% live in urban areas. Applicants live in households with 6 members on average, and 23% head a household. 49% did not complete primary school, and 23% only completed primary school. One third of the applicants have attended some form of vocational training, mostly informal apprenticeships. In line with the national employment situ-

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<sup>23</sup>A two-weeks tracking phase was implemented in February 2014 to limit attrition, mainly due to the mobility of control individuals. The tracking helped to reduce attrition rate from 5.4% to 2.6%. The sample for tracking was randomly selected among the treatment and control groups (stratified by locality and gender) among non-respondents who were alive, not outside Côte d'Ivoire, and excluding individuals that could not be reached since baseline.

<sup>24</sup>The complementary trainings were organized between January and mid-March and the second wave of the program ended between early and mid-February 2014 depending on the locality. 75% of the beneficiaries attended part of the second half of training after the end of their contracts. They were given a daily transport allowance of CFA 1500 (the program wage was CFA 2500) to compensate. The transportation allowance was paid ex-post in one transfer, based on the actual number of days attended. The remaining 25% were fully under contract during their training.

<sup>25</sup>The replenishment of the control group is further explained in section 4.1.

ation marked more by underemployment in low-earning occupations rather than unemployment, 78% of applicants were working prior to the program. Finally, applicants have limited financial resources, with only half having saved money over the last three months and 71% reporting being highly constrained for basic needs expenditures.

Table 1.2 compares our evaluation sample to a national sample of urban youths (between 18 and 30 years old) to provide insights into public works applicants' profiles. The gender breakdown and household asset index of public works applicants are quite similar to that of the national sample, with applicants marginally more male and from marginally poorer households. One main difference is that program applicants have lower educational attainment than the general youth population, and are much less likely to be inactive due to being in school. Because program applicants have left school, they are also more likely to be active and employed than the national population. Among the active population, the program attracts a higher share of applicants that are wage employed rather than self-employed or unemployed. As in many developing countries, a large share of youths in Côte d'Ivoire are self-employed and work for themselves. Many are underemployed as they work long hours but have low earnings. However, they are not necessarily searching for wage jobs in a traditional labor market, and unemployment tends to be higher among more educated youths (Christiaensen and Premand (2017)). Overall, Table 1.2 highlights that the program attracts youths who have left school and are already active in the labor market for wage jobs. These comparisons illustrate that the effectiveness of self-targeting is a priori unclear in contexts with widespread underemployment among low-skill youths: public works are not expected to attract the (more educated) inactive or unemployed youths, but the (less educated) youths are largely already active.

## 1.4 Empirical Methodology

### 1.4.1 Main Specifications

We estimate intent-to-treat (ITT) effects on contemporaneous and post-program outcomes for the pooled treatment via an ordinary least squares (OLS) regression:

$$Y_i = \alpha + \beta W_i + \delta X_{i,l} + \varepsilon_i \quad (1.6)$$

where  $Y$  is an outcome of interest for individual  $i$ ,  $W$  is an indicator for treatment (being assigned to the public works program at first randomization), and  $X$  is a vector of stratification variables (specifically, locality and gender).<sup>26</sup> Robust standard errors are clustered at the brigade level for treated individuals.<sup>27</sup>

To estimate post-program ITT effects by treatment arm, we use the following specification:

$$Y_i = \alpha + \beta_1 W_i + \gamma_1 (W_i \times T1_i) + \gamma_2 (W_i \times T2_i) + \delta_1 X_{i,l} + \varepsilon_i \quad (1.7)$$

where  $T1$  ( $T2$ ) is an indicator for being assigned to the complementary self-employment training (wage employment training).  $\beta$  estimates the impact of the “pure” public works, while  $\gamma_1$  ( $\gamma_2$ ) estimates the additional effect of the self-employment training (wage employment training).<sup>28</sup>

We also analyze heterogeneity in treatment effects by group  $G$  determined by a set of baseline characteristics  $Z$  (see discussion in section 6):

$$Y_i = \alpha + \beta_1 (W_i \times G_i) + \beta_2 (W_i \times (1 - G_i)) + \gamma * G_i + \delta_2 X_{i,l} + \varepsilon_i \quad (1.8)$$

We are interested in  $\beta_1$ , which estimates the impact of the pooled treatment for a specific group  $G$ .<sup>29</sup>

Table 1.1 presents baseline balance checks between treatment and control groups, with p-values for differences in column (3). Column (4) contains p-values for a test of whether differences between all treatment arms are jointly equal to zero. We focus on baseline respondents interviewed at endline.<sup>30</sup> We note that collecting the baseline survey after assignment to the program may have induced some misreporting. Despite this, there are no quantitatively meaningful differences across groups. The few imbalances that are statistically significant are of small magnitude, such as school enrollment, self-reported constraints to repay loans, having an activity or risk aversion.

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<sup>26</sup>Specification (1.6) uses probability weights to account for stratification, sampling of non-respondents during tracking surveys, and enrollment in later waves of the program (see details in Appendix I).

<sup>27</sup>We suspect within-brigade error correlation due to the interactions between treated individuals who worked together in the same brigade for several months. Some individuals moved across brigades during the program. When such movement occurred, we group the different brigades together into a "broad" brigade for clustering. The results are robust to other definitions of the brigade cluster.

<sup>28</sup>Specification (1.7) includes probability weights as in specification (1.6).

<sup>29</sup>Specification (1.8) includes probability weights as in specification (1.6).

<sup>30</sup>We use the same weights as for the estimation. We also verified that there is balance across groups for midline respondents, and for baseline respondents.

At midline, compliance to program assignment was high. Only two control individuals ended up in the program by registering in different locations. Among youth assigned to the public works, take-up was high with 93% participating more than five out of seven months. In total, youth worked an average of 141 days out of a maximum of 154 workdays.

The take-up of complementary training was lower than the take-up for the public works intervention: 72% of individuals assigned to self-employment training and 67.2% of those assigned to wage-employment training attended at least 75% of the training hours. This is in line with take-up observed in other skill training programs.<sup>31</sup> However, for each training, only 10% of individuals never attended, such that we focus on ITT estimates.

An unforeseen issue emerged at endline. A few individuals from the control group (140) were able to apply (and, for some, participate) in the third or fourth wave of the program. We account for this issue by excluding these individuals from the post-program impact analysis and assigning larger weights in the post-program analysis to control individuals who also applied in future waves but were not selected through the public lotteries.<sup>32</sup> Furthermore, we randomly select 200 additional applicants from the enrollment lists and add them to the endline sample to maintain the total size of the control group used for analysis and related statistical power.

Lastly, recent studies have shown that public works programs can have externalities on labor markets and wages through equilibrium effects. This has mainly been documented in the context of a large-scale program in India (see [Imbert and Papp \(2015, 2019\)](#), [Muralidharan et al. \(2016, 2017\)](#) or [Berg et al. \(2018\)](#)). With 12,666 beneficiaries over 4 years in 16 urban areas, the size of the program we study is small and general equilibrium effects are unlikely in our setting.

## 1.4.2 Heterogeneity Analysis with Machine Learning Techniques

The standard heterogeneity analysis from equation (1.8) relies on the estimation of average treatment effects across sub-groups using a linear interaction in a standard regression framework. The treatment variable is interacted with covariates and predicted impacts can be recovered conditional

<sup>31</sup>For instance, in a dual apprenticeship program in Côte d'Ivoire, the take-up was 75% ([Crépon and Premand, 2019](#)).

<sup>32</sup>Specifically, using administrative data from these additional program waves, we were able to identify repeat applicants, and whether they were selected or not based on the public lotteries. We identified 140 individuals from our baseline control group (i.e. 13.5%) among beneficiaries of the next waves of the program. We remove the repeat applicants who were randomly selected for the program from analysis. We then over-weight the non-selected repeat applicants. See details on weights in Appendix I

on these covariates. This approach raises the issue of selecting the dimensions of heterogeneity, which pre-analysis plans (Casey et al., 2012; Olken, 2015) and multiple hypothesis testing (List et al., 2019) can help to partly address. The main innovation of machine learning methods is that they require fewer assumptions about the source or form of this heterogeneity. Importantly, they can search for heterogeneity across high-dimensional sets of covariates without assuming a specific functional form.<sup>33</sup> Our motivations to analyze heterogeneity are to find an optimal program assignment rule in the spirit of the approach in Bhattacharya and Dupas (2012) and to study how the average contemporaneous program impact on earnings varies with the transfer. For this purpose, we are interested in identifying a non-parametric estimate of the conditional treatment effect, which requires the use of a flexible estimator.<sup>34</sup>

We are looking to estimate treatment effects for specific subgroups in the population, defined by their (observable) characteristics  $z$ . We would like to estimate the conditional average treatment effect (CATE) for some subgroups, corresponding to  $s_0(Z) = E[Y_i(1) - Y_i(0) | Z_i^K = z]$  where  $Z^K$  is a vector of  $K$  baseline covariates (features) and  $Y(k)$  the potential outcome of interest for treatment ( $k = 1$ ) and control ( $k = 0$ ).

A key challenge when using machine learning techniques to study heterogeneity is to derive confidence intervals and perform inference.<sup>35</sup> In this paper, we use the inference framework developed by Chernozhukov et al. (2020), who present a general approach using machine learning estimators as a proxy predictor to make inference on *key features* of the CATE function (rather than the whole function).<sup>36</sup> This allows us to (i) formally test for the existence of heterogeneity, (ii) compute confidence intervals around the conditional treatment effect for groups of interest (such as those at the top and bottom of the impact distribution), and (iii) compare the characteristics of the population who benefit the most or the least from the program. The approach in Chernozhukov et al. (2020) is “generic” in the sense that it applies to any machine learning algorithm used to estimate heterogeneous treatment effect, including the causal forest and generalized random forest

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<sup>33</sup>See Athey and Imbens (2016, 2017b,a) for a general discussion of machine learning techniques to analyze heterogeneous treatment effects, or Athey and Imbens (2016) on the use of regression trees.

<sup>34</sup>A large variety of estimators have been proposed in the literature, including causal forest (Wager and Athey, 2018), generalized random forest (Athey et al., 2019) or R-learner (Nie and Wager, 2017), and many others. Knaus et al. (2020a) offer a general presentation of these algorithms as well as Monte Carlo simulations to study their performance.

<sup>35</sup>The issue of detecting true heterogeneity versus noise is also discussed in Davis and Heller (2020) when using causal forest estimators.

<sup>36</sup>This contrasts with the approach in Wager and Athey (2018) who derive point-wise confidence intervals in the specific case of causal forests.

estimators proposed by [Wager and Athey \(2018\)](#) and [Athey et al. \(2019\)](#). In our own implementation, we consider several alternative machine learning algorithms (detailed in [Appendix H.2](#)). We present the main results for the best performing algorithm, and provide robustness checks in the [Appendix](#).

When applying machine learning methods, we split our data so that separate sub-samples are used to either build the model (the *auxiliary sample*, on which machine learning predictors are trained and constructed) or make inference (the *main sample*, to which the model is applied, and on which we estimate the different key features of the CATE function). In our application, this procedure is repeated 100 times on random sub-samples.<sup>37</sup> [Chernozhukov et al. \(2020\)](#) offer a procedure to aggregate results across simulations and construct valid confidence intervals and p-values.<sup>38</sup>

We test for the presence of heterogeneity by estimating the  $\beta_2$  coefficient in the following equation:

$$Y = \alpha_1 + \alpha_2 B(Z) + \beta_1(T - P(Z)) + \beta_2(T - P(Z))(S(Z) - \hat{E}(S(Z))) + \varepsilon \quad (1.9)$$

Machine learning is used to get  $S(Z)$ , a relevant *proxy* predictor of  $s_0(Z)$ , as well as  $B(Z)$ , a machine learning predictor for  $Y(0)$  (both constructed on the auxiliary sample).  $T$  is the treatment variable, and  $P(Z) = \hat{E}(T|Z)$ . We use weights  $w(Z) = \{P(Z)(1 - P(Z))\}^{-1}$ .

$\beta_1$  captures the average treatment effect (ATE) while  $\beta_2$  is the heterogeneity loading parameter (HET).<sup>39</sup> We are particularly interested in  $\beta_2$ , which offers a test for heterogeneity in treatment effect. Rejecting the null hypothesis that  $\beta_2 = 0$  means that (i) there is heterogeneity, and (ii) that our machine learning predictor is a good approximation of  $s_0(Z)$ . On the contrary, if  $\beta_2$  is not statistically different from zero, it means either that our machine learning predictor is

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<sup>37</sup>Iterations of the data-splitting process are necessary to identify how much variation is induced by specific data splits. It also ensures that each observation will be used on average both for construction and prediction (depending on the data-split), so all the information contained in survey data is used. This is especially important given our rather small sample size.

<sup>38</sup>Their procedure takes into account the uncertainty coming from both the estimation of the parameters and the data splitting process when aggregating the results (p-values, confidence interval bounds) across simulations. It takes the median of the estimated parameters over all splits, as well as the median of p-values which is then adjusted by a factor of 2. [Breda et al. \(2020\)](#) show that these adjusted p-values can be interpreted as upper bounds. Confidence intervals computed at 95% significance ( $\alpha = 0.05$ ) have to be re-adjusted for split uncertainty. After adjustment, the procedure provides confidence interval bounds at 90%.

<sup>39</sup>In this framework, the quantity  $BLP(Z) = \beta_1 + \beta_2(S(Z) - \hat{E}(S(Z)))$  can be interpreted as the best linear predictor of  $s_0(Z)$  based on  $S(Z)$ . Also  $\beta_1 = \mathbb{E}[s_0(Z)]$  is the average treatment effect (ATE) and  $\beta_2 = \frac{Cov(S(Z), s_0(Z))}{Var(S(Z))}$  is the heterogeneity loading parameter (HET).

uncorrelated with  $s_0(Z)$  (our predictor is not able to capture heterogeneity correctly), or that there is no heterogeneity. In our application, we test for heterogeneity in impacts on earnings both *during* and *post* program.

Besides detecting heterogeneity, we are also interested in the magnitude of the treatment effects along the distribution. In our application, we consider the top and bottom quartiles of the distribution, corresponding to the 25% of individuals who benefit the most and the least in terms of impacts on earnings. This is because around 25% of total applicants were selected to participate in the program we study.<sup>40</sup> We recover the parameters of interest  $E(s_0(Z)|G_k)$ , also referred as Group Average Treatment Effects (GATES), where groups are quartiles of the distribution of predicted treatment effects, through the following weighted linear projection:

$$Y = \alpha_1 + \alpha_2 B(Z) + \sum_{k=1}^4 \gamma_k (T - p(Z)) 1(G_k) + \nu \quad (1.10)$$

The projection coefficients  $\gamma_k$  are the GATES parameters. The groups are defined as  $G_k = \{S(Z) \in I_k\}$  with  $I_k = [q_{k-1}, q_k)$ , where  $q_k$  are the quartiles of  $S(Z)$ , and  $q_0/q_4 = -/\infty$ . We again use weights  $w(Z) = \{P(Z)(1 - P(Z))\}^{-1}$ . The estimated parameter  $\gamma_4$  (corresponding to the top quartile of the predicted distribution of impact, group  $G_4$ ) can be interpreted as the average treatment effect among the 25% of individuals who benefit the *most* from the program. Similarly,  $\gamma_1$  can be interpreted as the average treatment effect among the 25% of individuals who benefit the *least* from the program (group  $G_1$ ).

A natural next step is to study the characteristics of the groups of interest (i.e.  $\mathbb{E}[g(Z)|G_k]$ , where  $g(Z)$  is the vector of characteristics of an observation). In particular, we can compare baseline characteristics between the top and bottom quartile of the distribution of predicted impacts, namely groups  $G_4$  and  $G_1$ . Although machine learning methods do not allow us to exactly identify which characteristics matter the most for heterogeneous treatment effects, learning about the characteristics of those who benefit the most and the least provides insights about the variables that could be used for targeting.

In the analysis below, we will also assess how belonging to a particular heterogeneity group for a given outcome  $Y$  affects treatment effect on another outcome  $\tilde{Y}$ . In other words, we seek to

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<sup>40</sup>Chernozhukov et al. (2020) consider quintiles. We adapted the procedure to quartiles in the context of our application, as the rate of success of the lottery to assign applicants to the program is roughly 25%.

identify  $\mathbb{E}[S_{\tilde{Y}}(Z)|G_{Y,k}]$ , where  $S_{\tilde{Y}}(Z)$  is the treatment effect on variable  $\tilde{Y}$  conditional on  $Z$  and  $G_{Y,k}$  is the  $k^{\text{th}}$  heterogeneity group for the treatment effect on the variable  $Y$  conditional on  $Z$ . This is useful to determine whether there are trade-offs between optimizing selection into the program to maximize during-program impacts and post-program impacts. This is also useful in buttressing our understanding of mechanisms for longer-term impacts: for example, we can assess whether individuals that benefit most from the program in terms of during-program earnings are also those with the greatest post-program savings or post-program well-being. In practice, we can use equation (1.10) to perform this analysis, replacing  $Y$  as a dependent variable with the alternative outcome variable  $\tilde{Y}$ .

## 1.5 Results

Table 1.3 presents ITT results. We first display pooled treatment estimates from equation (1.6), both for contemporaneous impacts measured in the midline survey (4-5 months after the start of the program, while youths are still participating; Panel A) as well as for post-program impacts measured in the endline survey (12-15 months after youths have exited from the program; Panel B). We then discuss impacts by treatment arm using specification (1.7) (Panel C).<sup>41</sup> The main outcome variables are employment, type of employment, hours worked and earnings. We also present results for expenditures and savings. In the main specification, we consider continuous variables (such as hours worked and earnings, winsorized at the 97th percentile) to facilitate the interpretation of magnitudes. We also include the logarithm of the main earnings variable. Table 1.4 presents results for alternative treatment of outliers, including outcomes in logarithm and winsorized at the 99th percentile. Results are generally robust, and we discuss in the text the few cases where there are discrepancies. Table 1.5 presents results for well-being and behavior indices, as well as for time use variables proxying work habits and engagement in risky behaviors measured from list experiments.<sup>42</sup>

### 1.5.1 Contemporaneous Impacts

Table 1.3 (Panel A) presents ITT estimates on employment and hours worked during the program. Given the high share of control youths working in some form of activity at midline (85%),

<sup>41</sup>Appendix Table A11 shows similar results based on specifications that include baseline controls.

<sup>42</sup>Appendix G provides information on the definition and construction of these variables.

the impact of the public works program on the likelihood of employment is rather small (+14 percentage points). A similar pattern is observed for hours worked per week, with a small overall increase in total hours worked (by 4.9 hours) from an average of 39.2 hours per week in the control group. Employment in the public works wage jobs accounts for approximately 30 hours a week for individuals in the treatment group, so that the small increase in overall hours worked hides a large decrease in hours worked in other activities.

In contrast, the intervention had a more substantial impact on the composition of employment, with a large increase in the share of youths holding wage jobs (+48 percentage points, from a base of 49% in the control group) and a decrease in self-employment (-10 percentage points, from a base of 35% in the control group). Correspondingly, we observe a large increase in hours worked in wage employment (+15.6 hours) and a decrease in hours worked in self-employment (-5.7 hours).<sup>43</sup> This highlights that youths reorganize their portfolio of activities to participate in the program.

Table 1.3 (Panel A, columns 7-10) presents estimates of impacts on earnings during the program. Participation in public works leads to a significant net increase in earnings. The magnitude of the effect amounts to CFA 27,083 per month (or approximately US \$110 PPP 2014). The net earnings gains represent approximately 53% of the average net monthly transfer.<sup>44,45</sup> As such, the estimated effects point to substantial foregone earnings from activities that youths left or scaled down in order to participate in the program. Contemporaneous impacts on earnings stem from a strong increase in earnings from wage employment (+CFA 36,799), which offsets a significant decrease in earnings from self-employment (- CFA 5,715).

These results suggest that self-targeting did not succeed in this context in getting only the most vulnerable (e.g. those with the least outside employment opportunities) to participate in the program. A couple of factors likely explain this failure of self-targeting. First, governments typically cannot legally offer public works programs with wages below the formal minimum wage,<sup>46</sup> so a

<sup>43</sup>Youths in the treatment group also became more likely to engage in multiple activities.

<sup>44</sup>CFA 50,600 (\$205 PPP 2014) is the average amount transferred over all individuals assigned to the public works (independently of non-compliance and days not worked).

<sup>45</sup>The average treatment effect for the variable measured in  $\log$  (for which we take  $\ln(y + 1)$ ) in column (8) is 2.95 at midline. This is quite large compared to the average treatment effect for the variable in level in column (7) as a percentage of monthly income in the control group ( $27083/42841=0.63$ ). The difference is due to both the large standard error of the dependent variable and the reduction of the standard error between treatment and control group (see formula in footnote 50 below). The standard error of earnings in the treatment group (50483) is substantially smaller than in the control group (65466). This is largely explained by a lower dispersion of earnings, which are more concentrated among participants who receive the program wages and are less likely to have zero earnings.

<sup>46</sup>This is the case for many programs in West Africa, as well as in Ethiopia or India.

job that pays the statutory minimum wage could be of appeal to many in an environment where informal employment and self-employment are rampant. As in many countries in Sub-Saharan Africa, Côte d'Ivoire faces a relatively low unemployment rate, but a large share of individuals working in low-productivity self-employment or informal wage jobs without contracts.<sup>47</sup> Employment patterns in the control group illustrate this. Overall, many individuals earn less than the legal minimum wage, as the regulations are only binding for formal private companies and public administration. In this setting, the results show that the program induced a reallocation of youths' activities and substantial forgone earnings. Second, because the work was only 6 hours per day, many applicants with outside employment opportunities would still see value in applying for the public works program as they could combine it with other activities, especially those that allow for more flexible hours. Finally, while the unpleasant nature of the work may have discouraged some, it is unclear whether this work is more unpleasant than most informal activities.

In Table 1.3 (Panel A, columns 11-12), we assess contemporaneous program impacts on expenditures and savings. The observed increase in earnings CFA 27,083 per month (or approximately US \$110 PPP 2014) is associated with an increase in expenditures (CFA 14,529 per month, or \$59 PPP 2014) and savings (roughly CFA 10,000 per month, or \$41 PPP 2014). The increase in total monthly expenditures represents approximately 54% of the earnings gains. It can be decomposed in roughly equal shares between youths' own expenditures and their contribution to household expenditures. The additional expenditures are mostly for basic necessities (such as food and clothes), as well as education and training. Youths are also able to save a significant share of their net earnings gains. On average, youths in the treatment group have increased their stock of savings by approximately CFA 39,786 (\$161 PPP 2014) after about 4 months in the program. This large impact represents a 182% increase from the average stock of savings in the control group (CFA 19,250, or \$78 PPP 2014). It is also consistent with youths saving approximately 36% of their earnings gains, or 20% of the public works wages. Youths are not only more likely to save and to save larger amounts, but most of these savings are kept in formal bank accounts. These include accounts in which youths are paid their public works wages. Overall, these substantial contemporaneous increases in savings raise the possibility that youths can invest in job search or self-employment activities after program exit.

Table 1.5 documents impacts on indices of well-being and behavior, as well as work habits and

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<sup>47</sup>For additional discussion, see [Filmer et al. \(2014\)](#); [Christiaensen and Premand \(2017\)](#).

engagement in risky behaviors.<sup>48</sup> The consideration of broader well-being indicators is important as temporary public works jobs may have non-monetary benefits or costs. On the one hand, the public works activities are hard manual labor activities, which some may consider depreciating. On the other hand, there can be a certain status associated with holding a public wage job in the community, in particular a predictable and secured formal wage job. Furthermore, changes in youths' behavior are particularly relevant in a post-conflict setting such as Côte d'Ivoire, as they may point to potential program externalities on social cohesion, an issue of strong interest for local policymakers. We also investigate how participation to public works affects work habits by looking at youths' time spent in rest, leisure or work activities in the morning and in the evening.

The public works program induces substantial improvements in well-being, which increases by 0.2 standard deviation at midline (Table 1.5, Panel A, column 1). Improvements in well-being stem from a larger share of treated youths reporting feeling happy and proud, scoring higher on subscales for self-esteem, positive affect and positive attitude towards the future (see Table A12). The intervention also induces improvements in the behavior index, which increases by 0.12 standard deviation (Table 1.5, Panel A, column 2). This is driven by reductions in impulsiveness and anger (see Table A13). Youths' work habits also change as they reorganize their days to participate in the program (Table 1.5, Panel A, columns 3-8). Participants are more likely to be up and working (or travelling to work) at 6am in the morning, and much more likely to be asleep at 10pm at night. Despite changes in work habits and behaviors, we do not observe changes in an aggregate index of youths' engagement in risky behaviors measured through list experiments (Table 1.5, Panel A, column 9). The estimate is negative, pointing to a reduction in risky behaviors, but not statistically significant.<sup>49</sup>

These results highlight that the public works intervention leads to a re-organization of youths' activities that contribute to substantial forgone earnings. It also induces improvements in non-economic outcomes; while these may be associated with the observed impacts on economic outcomes, it is also possible that some youths who do not benefit substantially in economic terms may nevertheless benefit from the program in other dimensions.

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<sup>48</sup>See Appendix G for definition, and Table A12, Table A13 and Table A14 for effects on components of the indices.

<sup>49</sup>When analyzing the components of the index in Table A14, we find a decrease in the share of youths taking drugs or displaying aggressive behaviors, but find an increase in the share of youths reportedly stealing. No changes are found in other indicators such as smuggling, prostitution, or having a firearm at home.

## 1.5.2 Post-Program Impacts

Table 1.3 (Panel B) presents post-program impacts on the same outcomes. Despite strong shifts in youths' employment portfolios during the program, no post-program impacts on the likelihood of being employed, employment composition or hours worked are observed. Overall, while no negative "stigmatization" or "scarring" effects are observed, the post-program results also suggest that the public works does not bring longer-term benefits to youths in terms of employment or hours worked (columns 1-6). Despite an increase in savings during the program, post-program results show that youths are not more likely to be self-employed either.

Table 1.3 (Panel B, columns 7-10) considers earnings impacts post-program. Overall, the main results show that the public works intervention does not lead to robust changes in earnings at endline. We note that these results are slightly sensitive to the treatment of outliers. While impacts on variables in logarithm are not significant (Table 1.3, column 8 and Table 1.4, columns 1-2), small but significant impacts on post-program earnings are found in the level specification (Table 1.3, column 7 and Table 1.4, column 3): earnings increase by CFA 4,361, or about 10% compared to the control group. This increase in earnings in the level specification is driven by an increase in self-employment earnings. When looking at the characteristics of micro-enterprises that youths operate post-program, we find a relatively larger asset stock (in value) and level of investments (see columns 4-6 in Table A15), discussed further in the next section. Finding impacts on a variable in level but not in logarithm is consistent with potential heterogeneity in impacts,<sup>50</sup> which we analyze in detail below.

While the intervention does not lead to post-program impacts on employment, hours worked or robust effects on earnings, it does have sustained impacts on savings and psychological well-being. At endline, treated youths have a significantly higher savings stock by CFA 11,505. This represents nearly 25% of savings in the control group, and also approximately 30% of impacts at midline (Table 1.3, column 12). Post-program improvements in psychological well-being remain significant (0.11 standard deviation) but are also more muted than during the program (Table 1.5, column 1). They are concentrated in a narrow set of domains such as happiness, self-esteem and less present fatalism; in contrast, there are no lasting impacts on sub-scales for pride, positive

<sup>50</sup>Indeed, a first order approximation of the log function is that  $E(\ln(y)) \approx \ln(E(y)) - 0.5\text{var}(y)/E(y)^2$ . Thus  $ATE(\ln(y)) \approx ATE(y)/E(y|T=0) - 0.5(\text{var}(y|T=1)/E(y|T=1)^2 - \text{var}(y|T=0)/E(y|T=0)^2)$ . As a result, an impact can be detected in level but not in logarithm if the variance is larger in the treatment group than in the control group.

affect, or positive attitude towards the future (Table A12, Panel B).

Finally, there is no lasting impact on any dimension of the behavior index, risky behaviors and work habits (Table 1.5). Overall, these results show that the public works program does not lead to sustained changes in behaviors or work habits, and hence is unlikely to increase productivity or employability via these behavioral channels. In other words, the public work program does not appear to live up to one of its key promises for larger longer-term impact relative to a more standard welfare program.

### 1.5.3 Post-Program Impacts by Treatment Arms

Table 1.3 (Panel C) documents post-program impacts by treatment arms. Overall, we observe little variation in impacts across treatment arms, suggesting limited value-added of the complementary skills training (micro-entrepreneurship or self-employment training and wage job search training). Specifically, post-program impacts on the likelihood of being employed, employment composition and hours worked are very consistent and not statistically different across the different treatment arms (Table 1.3, Panel C, columns 1-6).<sup>51</sup> No differences in impacts on non-economic outcomes are found between treatment arms in Table 1.5 either.

Results on total earnings also show no differential impacts for individuals assigned to complementary training (Table 1.3, Panel C, columns 7-8). The results are robust to alternative treatment of outliers (Table 1.4, Panel C, columns 1-3). Post-program impacts on self-employment earnings are positive for treated youths assigned to the entrepreneurship training when variables are expressed in level, but the finding is not robust when the variables are expressed in logarithm. In addition, we cannot reject equality of the impacts on total earnings between the public works treatment only and the public works with self-employment training, or between the treatment arms with the self-employment and wage employment training. Since there is no statistical difference in impacts on overall earnings across treatment arms, we pool treatments to conduct the finer heterogeneity analysis in the rest of the paper.

The limited value-added of the complementary training suggests that skills acquisition through

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<sup>51</sup>Hours worked are significantly larger in the jobs search training arm compared to the public works only arm. Still, the coefficient is not statistically different from 0 or from the estimate from the basic entrepreneurship training arm.

training is not a key mechanism to induce post-program impacts. Table A15 shows that the training interventions did improve knowledge as intended: knowledge on basic entrepreneurship increases by 0.11 standard deviation for the self-employment training (column 1, Panel B), and knowledge on job search skills improves by 0.26 standard deviation for the job search training (column 7, Panel B). The training also led youths to apply this knowledge in practice. For instance, the self-employment training increases the share of youths who prepared a business plan for one of their activities by 4 percentage points (column 2, panel A). Also, the wage employment training increased the share of youths who used a CV for a job search by 10 percentage points (column 8, Panel B). However, there are no impacts on job search expenditures or on the likelihood to search for a job. Overall, these changes in skills and practices are small in magnitude and do not appear sufficient to generate earnings beyond those induced by the basic public works program. Interestingly, impacts on the value of business assets (column 4) or on investments in start-up capital for self-employment activities (column 6) are driven by the public works treatment, which again points to savings and investments of public work wages as a key mechanism for post-program impacts. In sum, despite some effects on independent activities, there is no robust impact on profits or self employment earnings.

## 1.6 Heterogeneity Analysis

The public works program was oversubscribed, with the number of applicants exceeding the number of available program slots by a ratio of 4 to 1. The allocation of program slots was based on randomized assignment, which had the advantage of being fair and transparent. At the same time, the effectiveness of the program might have been improved with a finer targeting of the 25 percent of beneficiaries among applicants. Recall that the only criteria enforced at enrollment are age (18 to 30) and not being a beneficiary of a previous wave of public works.

Given the self-selection mechanism, we would expect heterogeneity in impacts among program applicants, with marginal applicants experiencing only small gains in earnings compared to inframarginal applicants with fewer employment opportunities outside the program (as outlined in section 1.2). But by how much alternative targeting might have improved program effectiveness is an empirical question that depends on the magnitude of heterogeneity and the shape of the distribution of program impacts. We now turn to this question. We focus on heterogeneity in the logarithm of total earnings since the distribution of this variable is closer to a normal distribution

than the variable in level (which has a higher dispersion even after winsorization). We also show key results for earnings in level for completeness.

### 1.6.1 Quantile Treatment Effects

Quantile regressions provide information about the lower bound of the variance.<sup>52</sup> Heterogeneous quantile treatment effects are always associated with heterogeneity in treatment effects. However, when quantile treatment effects are homogeneous, the only conclusion that can be drawn is that the lower bound of the variance is zero, unless the intervention preserves ranks. Put differently, a constant quantile treatment effect is consistent with homogeneous quantile treatment effects, but the reverse is not true.

Figure 1.1 presents quantile treatment effects on log earnings during (Panel A) and after (Panel B) the program. The horizontal axis in each panel reports the quantile and the vertical axis the estimate of the treatment effect at the corresponding quantile. The shaded area around the estimate provides the 95% confidence interval. The quantile analysis shows substantial heterogeneity in impacts on earnings during the program (Panel A). The quantile treatment effect is three times larger at the 25% quantile compared with the 75% quantile. The estimated quantile treatment effects are quite precise, suggesting the existence of true heterogeneity rather than just sampling variation. Using a Kolmogorov-Smirnov test, we can reject the null that the distributions under control or treatment are similar. In contrast, post-program quantile treatment effects are uniformly small. The dispersion is within confidence bounds (Panel B), consistent with sampling variation. Although we detect larger quantile treatment effects at the top of the distribution, we cannot reject the null of the Kolmogorov-Smirnov test that the post-program distributions are similar in treatment and control. In summary, there appears to be large heterogeneity during the program, but little heterogeneity after the program.<sup>53</sup>

The intervention is unlikely to induce churning in the distribution of contemporaneous effects, so that the heterogeneity seen in quantile treatment effects likely points to true underlying heterogeneity during the program. However, this is not necessarily the case for post-program impacts on earnings. There might be individual-level latent factors that may not contribute to the ranking

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<sup>52</sup>See Heckman et al. (1997) or Djebbari and Smith (2008). This lower bound is reached when the intervention preserves rank. In such a case, a quantile treatment effect can be interpreted as an effect at quantile. See Bitler et al. (2006, 2017) for applications.

<sup>53</sup>Figure B7 presents the post-program quantile treatment effects by treatment arm. They show similar patterns than those based on the pooled treatment.

of individual earnings in absence of the program but may contribute to the ranking of individuals' post-program earnings. For example, some individuals might be trapped at the bottom of the earnings distribution absent the program, but because of high latent returns to capital (e.g. through setting-up a highly profitable activity), these individuals might end up higher up in the post-program earnings distribution if the program allowed them to save and implement their latent project. Quantile regressions might thus fail to detect heterogeneity post-program.

## 1.6.2 Machine Learning Applications

We now present applications of machine learning techniques based on the approach described in section 1.4.2. First, we perform a statistical test to detect heterogeneity and then compare the magnitude of the conditional average treatment effects (CATE) across quartiles, with a particular focus on the bottom and top 25% of the distribution. Second, we analyze the characteristics of individuals in the bottom and top quartiles to understand how those who benefit the most differ from those who benefit the least. Third, we use the predicted distribution of conditional impacts to further understand the mechanisms between contemporaneous and post-program impacts. In particular, we look at post-program impacts on earnings conditional on being in the top quartile of the predicted distribution during program. This is helpful to understand how individuals who benefit the most during the program perform post-program; this is also helpful to assess whether there are trade-offs between maximizing contemporaneous and post-program impacts. Finally, we study the performance of alternative self-targeting (a lower offered wage) and targeting rules using the machine learning estimates as a benchmark.

### Existence and Magnitude of Heterogeneity in Impacts on Earnings

Table 1.6 (panel A) presents results from estimating equation 1.9, including a test of the statistical significance of  $\beta_2$ , the coefficient of the heterogeneity loading parameter.<sup>54</sup> We confirm finding heterogeneous impacts on earnings during the program, as  $\beta_2$  is statistically different from zero in column (1). However, we do not find evidence of heterogeneity in post-program impacts (the p-value for  $\beta_2$  is 0.97 in column (2)), which means that either there is no underlying heterogeneity, or the prediction model is not able to detect it. As for ITT estimates, we also run machine learning analysis with the outcome variable in level. Results at midline and endline are consistent

<sup>54</sup>Figure B8 presents the distribution of predicted impacts on earnings during the program and post-program, both in levels and in logarithm.

in columns 3 and 4. Results are robust to the application of a wide variety of machine learning estimators (Table A16, Panel A), or to the consideration of a larger set of covariates (Table A17, Panel A). Similar results are found when analyzing heterogeneity in post-program impacts on earnings by treatment arm in logarithm (Table A18, Panel A) or in level (Table A19, Panel A).

Panels B and C in Table 1.6 provide additional insights about the magnitude of heterogeneity by reporting Group Average Treatment Effects (GATES) by quartile, as obtained by estimating equation (1.10). Panel B displays GATES for contemporaneous program impacts on earnings in log in column (1) and in level in column (3). Panel C presents GATES for post-program impacts on earnings in log in column (2) and in level in column (4). Columns (1) and (3) of Panel B illustrate that there is substantial heterogeneity in impacts on earnings during the program. As shown in section 1.2, this is expected due to self-selection, with a fraction of marginal participants almost indifferent between being enrolled or not, and others being inframarginal. However, the magnitude of this heterogeneity is noteworthy. The average predicted impact on earnings is CFA 14,660 in the lower quartile of the distribution compared to CFA 31,671 in the upper quartile (column 3). In other words, program impacts are 2.2 times larger in the top quartile than in the bottom quartile. In contrast, columns (2) and (4) of Panel C suggest more modest heterogeneity, with no significant difference in post-program impacts between quartiles. Figure 1.2 illustrates the heterogeneity between groups, during and post-program.<sup>55</sup>

Since we do not observe heterogeneity in post-program impacts, we would further expect that it is possible to improve program effectiveness by maximizing contemporaneous program impacts without losses in post-program impacts. Figure 1.3 indeed confirms the absence of trade-off between impacts during and after the program.<sup>56</sup> A high correlation between impacts on earnings during and post program would lead to a concentration of predictions along the diagonal from the top right corner – those who have the largest impacts during and after the program – to the bottom left corner. On the contrary, the scatter plot shows that even within the top quartile of impacts during the program, the post program impacts are widely dispersed on the opposite axis. In Table 1.6, columns (2) and (4) in Panel B (respectively columns (1) and (3) in Panel C) show predicted

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<sup>55</sup>Our focus is on the difference between the top and bottom quartiles. At the bottom of each panel, Table 1.6 reports a test of equality of the GATES across the four quartiles. We cannot reject the null.

<sup>56</sup>Figure 1.3 shows predicted impacts on earnings during the program (x-axis) against predicted impacts on earnings after the program (y-axis) (for log earnings, using same machine learning estimator as in Table 1.6). The solid vertical and horizontal lines on the scatter plot correspond to the average predicted impacts during and after the program. Similarly, the horizontal (respectively vertical) dashed lines represent the 25th and 75th percentile of the distribution of predicted impacts during (respectively post) program.

impacts at endline (respectively midline) per quartile of predicted treatment effects at midline (respectively endline). They illustrate what would be the impacts on earnings post program (respectively during program) if midline impacts on earnings were maximized (respectively endline impacts were maximized). This further illustrates that there is no systematic relationship between those who benefit the most during the program and those who benefit the most after the program. We can never reject the null that coefficients are equal between groups. In other words, there is no measurable trade-off between short and medium-term impacts when trying to improve program effectiveness through finer targeting.

### Patterns of Heterogeneity

Using the Classification Analysis (CLAN) in [Chernozhukov et al. \(2020\)](#), we analyze differences in baseline characteristics between quartiles of the distribution of treatment effects in [Table 1.7](#). We focus on our two groups of interest, namely the bottom and top quartiles of the distribution of predicted impacts on earnings during the program (columns 1-2). Column (3) reports p-values for a test of equality between the lowest and highest impact groups.

[Table 1.7](#) shows clear differences in the profiles of individuals who benefit the least and the most during the program.<sup>57</sup> The share of women is significantly higher in the upper quartile of predicted impacts (53%) than in the bottom quartile (15%). Several characteristics related to financial status, expenditures, savings and earnings suggest that the lower quartile of predicted impacts was “better-off” at baseline. There is also a very large difference in both propensity to save and baseline savings stock among individuals in the bottom quartile (CFA 65,925) and those in the top quartile (CFA 6,795). Similarly, the share of participants in the bottom quartile reporting they face credit constraints is lower (43%) than the share in the top quartile (58%). The share of individuals working at baseline is substantially higher in the bottom quartile (100%, compared to 44%), as are baseline earnings. Finally, three (six) times as many individuals in the bottom quartile are engaged in self (wage) employment activities at baseline compared to the top quartile.

Participants who benefit the least during the program might still be able to save a greater share of their wages, or might be able to better invest these savings into income-generating activities.<sup>58</sup>

<sup>57</sup>As a robustness check, [Table A20](#) shows similar results for earnings in level.

<sup>58</sup>It is also possible that these individuals may particularly benefit from the opportunity to save through the bank accounts set up by the program.

It is therefore worthwhile to explore further how treatment effects on other main outcomes differ between the two groups. By doing this, we also highlight how machine learning techniques can help tease out mechanisms explaining program impacts.<sup>59</sup>

Table 1.8 presents estimates of treatment effects on the main outcomes by quartile of the (predicted) impacts on earnings during the program (panel A), and after the program (panel B). Table 1.9 analyzes post-program impacts on intermediary outcomes related to productive investments by quartile of (predicted) impacts during the program. At the bottom of each panel, we present a test of equality of treatment effects between the bottom and top quartiles. This allows us to test whether groups with high or low earnings impacts during the program invest differently in their portfolio of income-generating activities or in job searching.

Table 1.8 confirms that, in the short run, the program has strong impacts on earnings for a more vulnerable group, while also attracting less vulnerable individuals for whom impacts are much more limited. Table 1.8 (Panel A) shows that the top quartile (corresponding to the top 25% of predicted impacts on earnings during the program) also has the highest impact on the likelihood of employment and wage employment (columns (1) and (2)). This is consistent with results in Table 1.7, which showed that individuals in the top quartile were less likely to have an activity prior to the program. Total earnings are more than twice higher in the top quartile compared to the bottom quartile (columns (5) or (6)). Column (10) reveals that the savings stock increases significantly for the top quartile, but not more so than for the other groups. Given that the bottom quartile is wealthier at baseline, one could have expected that they, more than other groups, would use program transfers to increase their savings. However, we observe similar impacts on savings across quartiles of predicted impacts on earnings. Lastly, despite variations in impacts on earnings during the program, there is no difference in impacts on well-being across the different quartiles (Table 1.8, column (11)). This suggests that gains in the well-being dimension are also broadly shared.

Table 1.8 (Panel B) and Table 1.9 reveal few differences across several dimensions of post-program impacts between the top and bottom quartiles of predicted impacts during the program. We can never reject the null of equality of treatment effects at endline between quartiles of predicted impacts on earnings at midline. In other words, those who benefit the most during the program

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<sup>59</sup>Davis and Heller (2020) also use machine learning to test underlying mechanisms relying on differential treatment response from disadvantaged youth who benefited from summer jobs. They look across types of outcomes while we also analyze outcomes over time.

perform similarly as others post-program. This illustrates again the lack of a clear trade-off between contemporaneous and post-program impacts along a large number of dimensions.

For completeness, and given the key role that savings during the program would theoretically play in driving any post-program impacts, we also perform the machine learning heterogeneity analysis using saving as the outcome. Indeed, increased savings can be a catalyst for productive investments in activities, the returns of which we might not yet observe at endline. We want to verify that there is no trade-off along this dimension, as seemingly suggested by Table 1.8. Table A21 shows a similar conclusion, with no significant heterogeneity at endline.<sup>60</sup> This also means that post-program impacts on savings are broadly spread across participants.

Overall, these results reveal limited heterogeneity in post-program impacts. The most vulnerable individuals who saw the largest gains in earnings and employment during the program do not exhibit relatively larger post-program gains. On the other hand, better-off individuals who saw smaller gains in earnings during the program do not experience higher post-program impacts on investments and earnings either.

### 1.6.3 Effects of Lowering the Wage

Can the effectiveness of the program be increased by lowering the offered wage? Our estimated Conditional Average Treatment Effects (CATE) together with the framework developed in section 1.2.1 can shed light on this question.

As we discussed in the conceptual framework, the average impact of a lower transfer given a fixed budget  $B$  will depend on the distribution of individual impact in the population (see equation 1.5 and Appendix E.2). Intuitively, lowering the transfer reallocates program slots from those with lower program impact to those with higher program impact and increases the lottery success rate. While this effect is positive, it comes at the cost of lowering the transfer for all participants. If there is a large share of participants at transfer level  $T$  with close to zero program impacts, the first effect will tend to dominate. But more generally, the overall impact of a lowering of the program wage will depend on the shape of the distribution of the individual treatment effect (see Figure B6, which contrasts two examples).

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<sup>60</sup>In addition, the characteristics of those who benefit the most in terms of earnings and in terms of savings are very close: patterns in Table A22 are similar to those in Table 1.7.

While the true distribution of individual impact is unknown, we can use the distribution of our CATE estimate as a proxy. The upper panel of Figure 1.4 displays our estimate of the distribution. As the figure shows, the distribution reveals a situation closer to the second example in Appendix Figure B6. The lower bound of the support of the distribution is zero, but there is a small share of participants who experience close to zero program impact, and the distribution function increases slowly up to its mode at around CFA 27,000. This suggests that a reduction in the offered wage may not improve program effectiveness.

This is confirmed in the lower panel of Figure 1.4, where we quantify the effect of a reduction in the transfer  $T$  using equation 1.5. The figure first shows how the self-selection process is affected as the transfer amount is reduced. The reduction in the number of applicants (dotted black line) is at first slow but becomes more substantial when the reduction in the transfer amount approaches the mode of the distribution of  $s_T$ .

The figure also shows (dashed blue line) the effect of the reduction in the transfer amount on the lottery success rate. Naturally, given the fixed budget, the lottery success rate increases as the transfer amount decreases as a) the number of applicants decreases (dotted black line) and b) the size of the transfer per participant is lower. More specifically, if the original total budget for transfers in the program ( $0.25T$ , or  $B$  in the context of our model) is randomly allocated to those who still apply at the reduced transfer amount  $T - x$ , the lottery success rate is given by: Lottery rate =  $0.25T / ((T - x)P(s_T - x > 0))$ . The figure reports this lottery rate up to the value where it reaches 1 ( $\approx$  CFA 27,500). As the figure shows, given the estimated shape of  $s_T$ , the increase in the lottery success rate remains modest for a large range of reduction in the transfer amount.

Last, the solid red line shows how the average impact on earnings varies when the transfer amount is reduced. More precisely, the solid red line displays  $E((s - x)1(s - x > 0)) / (P(s - x > 0)(T - x))$ , which is  $S_{lottery}(T - x)$  from equation 1.5 normalized by  $0.25 \times 50,000$  CFA, the amount available for each initial applicant. The figure shows that the average impact on earnings would not increase when the transfer amount diminishes. The index we compute starts at 0.5; it reaches 0.3 for a reduction in the transfer of CFA 15,000 and 0.1 for a reduction in the transfer of CFA 25,000.<sup>61</sup>

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<sup>61</sup>A confidence interval is obtained following the procedure in Chernozhukov et al. (2020).

### 1.6.4 Alternative Targeting Rules and Cost-Effectiveness Ratios

The analysis so far suggests that self-targeting based on the minimum wage or a lower wage limits program effectiveness, and that departing from self-targeting could improve contemporaneous program impacts without decreasing post-program impacts. However, targeting rules based on machine learning algorithms would be too complicated and expensive to implement, relying on complex information that is not easily available. Are there alternative targeting rules that could come close to achieving the predicted impacts in the upper quartile of the machine learning estimates? This is the question we take on in the rest of this section.

Table 1.10 summarizes impacts on earnings during the program (panel A) and post-program (panel B) for specific sub-populations under alternative targeting rules. For reference, the first column displays ITT impacts on (log) earnings for the whole sample of participants selected by randomized assignment. Since maximizing impacts during the program does not reduce post-program impacts, column (2) documents the effect of selecting the observations in the top quartile (Q4) of the distribution of predicted impacts at midline. For comparison, we also report in column (3) results when selecting the bottom quartile (Q1) of the distribution of predicted impacts at midline. For those two columns, Panel A reports group averages (GATES) from the machine learning prediction of midline impacts and Panel B reports the average endline impacts for observations belonging to each quartile of the predicted impact distribution at midline.<sup>62</sup> These estimates represent the impacts on earnings that would be achieved by targeting those who benefit the most (respectively the least) in terms of earnings during the program.

We then consider a scenario where only women are targeted. Table 1.7 showed that women are over-represented in the population that benefit the most during the program. Column (4) (Table 1.10) shows that targeting only women would improve impacts during the program, with no losses in post-program impacts. In particular, average impacts on income during (after) the program would be CFA 32,097 (CFA 7,168) when targeting only women. This corresponds to a 19% improvement in average estimated impacts on earnings during the program compared to randomized assignment. The point estimate is close to the machine learning benchmark in column (2).

What about targeting on baseline earnings? If there is limited churning in the earnings dis-

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<sup>62</sup>This is similar to Table 1.6, panel B, column (2).

tribution, those with the lowest baseline earnings are likely to also have the lowest earnings at midline absent program participation. Columns (5) and (6) of Table 1.10 show results using two approaches to directly target the bottom quartile of the earnings distribution. The first approach targets individuals based on their (self) reported earnings at baseline (column (5)). The second approach uses machine learning techniques to predict (proxy) baseline earnings among program applicants using a limited set of covariates that are both easily observable and not easily manipulated, including gender, age, household characteristics and assets. We then estimate program impacts for individuals in the lowest quartile of the distribution of baseline income, either reported or predicted. This second approach (column (6)) is meant to mimic proxy means tests, which are often used to target safety nets to the poor and are more robust to misreporting than self-reported income. Columns (5) and (6) show that the contemporaneous impacts under these two approaches would come close to those predicted in the upper quartile of machine learning estimates (column (2)). Targeting individuals with reported baseline income in the bottom quartile leads to an average expected impact on income during the program of CFA 33,954 (column (5)), respectively CFA 32,824 (column (6)) when using predicted (proxied) baseline income. This is a 21%-25% improvement compared to randomized assignment. Post-program impacts are comparable to those obtained when selecting women only, and again not lower than the machine learning benchmark.

These results show that several practical targeting rules could perform better than self-targeting and improve program effectiveness. These alternative targeting rules come close to the machine learning estimates, without trade-offs between maximizing impacts during and post-program.

Panel C of Table 1.10 shows how program cost-effectiveness ratios vary by targeting rule. The average public works program costs CFA 768,708 (\$1,537.4) per beneficiary.<sup>63</sup> In our calculation, we focus on benefits captured by contemporaneous and post-program impacts on earnings under each targeting rule. Contemporaneous impacts are estimated from the midline survey and assumed constant during the 7 months of the program. Post-program impacts are considered constant from the end of the program (month 8) to the endline survey (month 21), and zero thereafter. The cost-effectiveness ratio of the existing program is presented in column (1). The discounted sum of impacts on earnings is CFA 253,920, for a cost-benefit ratio of 3.03. This means that the average

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<sup>63</sup>The total costs can be decomposed as follows: CFA 354,166 (\$717) for direct transfers to beneficiaries, CFA 255,189 (\$517) for other direct costs (material, team leaders and supervisors, basic training), CFA 108,230 (\$219) for skills training, and CFA 51,123 (\$10) are indirect management costs.

cost per beneficiary is 3.03 times higher than the average discounted direct impacts on earnings. This relatively high cost-benefit ratio is driven by the fact that net earnings gains are 53% of the average transfer amounts during the program, and that direct transfers to beneficiaries represent only 46% of overall program costs.

Note that these cost-benefit ratios are conservative. They assume zero impacts beyond what we measure at endline, around 14 months after the end of the program. Program cost-effectiveness clearly depends on the sustainability of post-program impacts. Figure B9 illustrates how long the post-program impacts would need to be sustained for the program to become cost-effective (reaching a ratio of 1 or below) based on impacts on earnings and depending on the assumptions about the dissipation of post-program impacts after the endline (respectively 0, 2 or 5% of dissipation per month). For instance, assuming no dissipation of impacts, the program would become cost-effective when targeting women if post-program impacts are sustained for 7 years, or after 4-6 years when targeting individuals with low baseline earnings (respectively predicted and reported). The time to reach cost-effectiveness increases when assuming that the impact on earnings dissipates over time. Cost-benefit ratios also do not account for non-economic benefits such as those on psychological well-being mentioned above, or other externalities from the program, such as the indirect benefits of roads rehabilitation. They still provide a benchmark to assess potential program improvements such as the implementation of alternative targeting mechanisms, in particular if we consider in a first-order approximation that non-economic benefits or externalities are similar across these potential improvements.

Columns (2) to (6) in Table 1.10 show how adjustments in targeting would affect the cost-effectiveness ratios. Columns (2) and (3) display cost-benefit ratios for individuals in the top and bottom quartiles of predicted impacts based on machine learning techniques. The cost-benefit ratio is nearly three times higher in the bottom quartile (7.94, column (3)) than in the top quartile of predicted impacts (2.44, column (2)). This illustrates the high cost of including marginal applicants. Compared to the cost-benefit ratio of 3.03 for the randomly assigned program, the cost-benefit ratio would improve to 2.33 by targeting directly women (column (5)), or around 2 by targeting individuals with low baseline earnings (between 2 and 2.2 depending on whether they are self-declared or predicted based on other proxies, columns 5-6). While the analysis cannot decisively indicate which targeting scenario would maximize cost-effectiveness given the confidence intervals around the impact estimates, it does highlight potential improvements in

cost-effectiveness ranging between 30 and 52 percent when departing from self-targeting based on the formal minimum wage.

## 1.7 Conclusion

The Côte d'Ivoire public works program we study in this paper shares many of the features of other public works programs that have been adopted throughout the developing world in response to negative economic, political or climatic shocks. It provided a few months of employment in road rehabilitation to those willing to work at the formal minimum wage. Based on a randomized control trial and rich data collected before, during and after the program, our analysis has allowed us to assess the effectiveness of the program in improving contemporaneous and post-program outcomes among participants.

Results show that program impacts on employment are limited to shifts in the composition of employment towards the public works wage jobs during the program, with no lasting post-program impacts on the likelihood or composition of employment. Public works increase earnings during the program, but post-program impacts on earnings are limited. Savings and psychological well-being improve both during and (to a less extent) post-program. However, we see no long-lasting effects on work habits and behaviors, despite improvements during the program.

The program as currently implemented induces impacts on youths' earnings that are substantially below program costs. This is primarily due to the limited post-program impacts and a failure of self-selection: in an environment where informal employment is rampant, a broad cross-section of youth with outside employment opportunities self-select into public works participation. The high cost-benefit ratios also stem from the fairly high indirect cost of implementing public works programs, for instance compared to more traditional welfare programs.

We use recent machine learning techniques to document significant heterogeneity in impacts on earnings during the program, but no significant heterogeneity is found post-program. The results suggest that improvements in self-targeting or targeting are first-order program design questions, and perhaps more critical than other program design aspects related to program content, such as complementary skills training. Given the estimated distribution of individual program impacts, we show that a lower offered wage (and the subsequent change in self-targeting) would have been unlikely to improve program performance. In contrast, we show that a range of practical

targeting mechanisms perform as well as the machine learning benchmark, leading to stronger impacts during the program without reductions in post-program impacts. Still, even with this improved targeting, impacts on earnings remain substantially below program costs.

While one might be tempted to conclude from our analysis that public work programs should be de-prioritized by policy makers in favor of welfare programs with more efficient targeting procedures and lower implementation costs, it is important to remember that our cost-effectiveness analysis does not take into account all possible benefits of the program, both for the beneficiaries themselves but also for non-beneficiaries. First, we do observe lasting effects on psychological well-being and savings among beneficiaries that are not included in the cost-benefit ratios. We note, however, that the post-program effects we observe are of relatively small magnitude and it is unclear, especially given the lack of similar sustained impacts on work habits and behaviors, whether the “work” component of the workfare program is responsible for these sustained effects or whether similar effects could be achieved solely with cash transfers. Second, there might be other positive externalities associated with the program, such as a reduction in crime or illegal activities due to an incapacitation effect. While we do not find much evidence of changes in youths’ engagement in risky behaviors, neither during nor after the program, it is still possible that some externalities may arise at the level of the community and hence may be difficult to measure. Lastly, we do not quantify the societal value of the upgraded infrastructure. These two latter potential benefits are a specific feature of public work programs and might be particularly large in post-conflict environments with destroyed physical infrastructure and a high need for social stabilization. Still, these externalities would need to be very (and likely unrealistically) large for the program to be cost-effective.

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## Appendix

## A Result tables

Table 1.1 – Baseline Summary Statistics and Balance Checks

	(1)	(2)	(3)	(4)	(5)
	Mean in Treatment group (pooled)	Mean in Control group	Balance Test (p-value) (1)-(2)	Balance Test between 4 arms (p-value)	Observations
Female	0.43	0.42	0.710	0.850	3781
Live in urban area	0.94	0.93	0.337	0.579	3736
Age	24.58	24.67	0.569	0.227	3736
Nationality: Ivorian	0.96	0.97	0.167	0.265	3736
Nb of children	0.90	0.94	0.485	0.997	3736
Education					
Primary education not completed	0.49	0.49	0.944	0.839	3736
Up to primary education completed (CEPE)	0.23	0.22	0.593	0.417	3736
Up to lower secondary education completed (BEPC)	0.18	0.16	0.480	0.725	3736
Upper secondary education completed (BAC or more)	0.09	0.12	0.0724	0.576	3736
Enrolled at school	0.05	0.07	0.0584	0.681	3736
Has participated in vocational training	0.36	0.40	0.128	0.361	3733
Of which: traditional apprenticeship	0.72	0.71	0.716	0.512	1465
Household					
Household size	6.12	6.10	0.915	0.267	3735
Nb of children (< 18 years old)	2.12	2.10	0.911	0.579	3736
Is head of household	0.23	0.23	0.983	0.869	3736
Share of members working (last 7 days)	0.54	0.55	0.290	0.822	3735
Number of rooms in dwelling	3.17	3.10	0.581	0.483	3736
Household assets					
Total Nb of assets	13.53	13.5	0.960	0.404	3736
Nb of transportation assets	0.74	0.83	0.466	0.230	3736
Nb of agricultural assets	4.61	4.52	0.890	0.772	3736
Nb of household durables	1.59	1.60	0.882	0.377	3736
Nb of communication assets	6.60	6.55	0.792	0.639	3736
Savings					
Has saved (last 3 months)	0.49	0.51	0.438	0.619	3736
of which: share of savings (stock) in formal instrument	0.25	0.27	0.516	0.516	1811
Has a savings account	0.11	0.09	0.234	0.936	3736
Savings stock (CFA)	27644.3	26426.1	0.602	0.964	3685
Self-reported constraints to repay loans	0.19	0.23	0.0538	0.967	3736
Self-reported constraints to access credit	0.50	0.50	0.889	0.733	3736
Expenditures					
Nb of days without a meals (last 7 days)	0.80	0.83	0.654	0.706	3736
Self-reported constraints for basic needs expenditures	0.71	0.73	0.187	0.945	3736
Transportation expenditures (last 7 days, CFA)	1774.3	1679.5	0.495	0.724	3732
Communication expenditures (last 7 days, CFA)	1595.4	1540.8	0.721	0.739	3730
Employment					
Has an activity	0.78	0.82	0.0782	0.912	3736
Searched for a job (last month)	0.74	0.76	0.397	0.337	3736
Risk and time preferences					
Risk aversion level (scale 0 to 10, 0=very risk averse)	4.72	4.80	0.620	0.607	3736
Is risk averse (based on hypothetical lotteries)	0.74	0.69	0.0124	0.670	3736
Patience level (scale 0 to 10, 10=very patient)	3.25	3.25	0.999	0.931	3733
Preference for present (actualization rate for 1 month)	0.59	0.59	0.899	0.210	3736
Skills (% of success in answers or tasks at each test)					
Cognitive (deduction, Raven Test)	0.23	0.23	0.930	0.256	3730
Spatial vision (NV7 Test)	0.27	0.27	0.679	0.0912	3736
Dexterity (Nuts Test)	0.39	0.38	0.119	0.263	3731
Dexterity (Bolts Test)	0.33	0.34	0.243	0.246	3721

The table includes all baseline respondents interviewed at endline. Means (columns (1) and (2)) and difference in means (column (3)) estimated using endline estimation weights (see details in Appendix I). Robust standard errors clustered at (broad) brigade level. Assets and savings stock variables winsorized at the 99th percentile.

Table 1.2 – Characteristics of Applicants and National Population of Urban Youths

	Experimental Control Group	All Urban Youths (18-30)
<b>Gender</b>		
Male	55.7	50.8
Female	44.3	49.2
<b>Educational attainment</b>		
Primary education not completed	48.5	38.9
Up to primary education completed (CEPE)	23.2	18.5
Up to lower secondary education completed (BEPC)	16.6	20.0
Upper secondary education completed (BAC or more)	11.7	22.7
Total	100.0	100.0
<b>Asset Index</b> (z-score)	-0.046	0.000
<b>Occupation</b>		
Inactive, at school	2.9	23.6
Inactive, not at school	7.1	11.5
Unemployed	6.4	10.5
Wage-Employed (including informal)	47.0	25.4
Self-Employed (non agricultural)	30.1	26.0
Self-Employed in agriculture	6.5	3.1
Total	100.0	100.0

The first column displays average characteristics of program applicants. Gender, educational attainment and assets are measured at baseline. Employment status is measured at midline. (This is because the midline survey was collected between November 2013 and January 2014, which was closest to the timing of a national employment survey). The second column displays average characteristics of youths between 18 and 30 years old who live in urban areas. This is based on the 2013 National Employment Survey (collected in January 2014). The asset index is based on a principal component analysis of household assets. We include assets that are measured in both surveys: carts, wheelbarrows, bicycles, motorcycles, refrigerators, freezers, air conditioning units, fans, stoves, computers, radio stations, televisions and TV antenna, video players, cell phones, landline phones and cars. Z scores are predicted using the first component, and standardized in the national employment survey.

Table 1.3 – Impacts *during* and *post* program, economic outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Employed	Wage- Employed (in at least 1 activity)	Self- Employed (in at least 1 activity)	Total Hours worked (weekly)	Hours worked in Wage- Empl. (weekly)	Hours worked in Self-Empl. (weekly)	Total earnings in CFA (monthly)	Ln total earnings (monthly)	Earnings in Wage- Empl. in CFA (monthly)	Earnings in Self-Empl. in CFA (monthly)	Total ex- penditures in CFA (monthly)	Savings CFA (stock)
<b>Panel A: Impacts <i>during</i> the program</b> (around 4.5 months after program starts)												
Public Works Treatment (ITT)	0.14*** (0.015)	0.48*** (0.024)	-0.096*** (0.020)	4.89*** (1.25)	15.5*** (1.29)	-5.69*** (0.94)	27082.9*** (2824.9)	2.95*** (0.19)	36799.0*** (1472.5)	-5715.4*** (1214.6)	14529.3*** (1441.4)	39785.7*** (2389.2)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	0.85	0.49	0.35	39.18	20.79	11.28	42841.22	7.87	20188.33	12753.65	47233.52	19250.05
Observations	2958	2958	2958	2958	2958	2958	2912	2912	2912	2912	2945	2958
Perm. p-value: no effects	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
<b>Panel B: <i>Post</i> program impacts (pooled treatment)</b> (12 to 15 months after program ends)												
Public Works Treatment (ITT)	0.015 (0.015)	0.0068 (0.019)	0.010 (0.022)	1.34 (1.28)	-0.61 (1.14)	1.70 (1.11)	4360.6** (1906.5)	-0.037 (0.18)	-452.7 (1002.6)	4005.2** (1790.8)	1361.7 (1406.9)	11505.2*** (3136.5)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	0.86	0.51	0.36	40.49	22.06	13.26	43481.10	8.42	20706.18	18872.95	50700.71	46348.14
Observations	3934	3934	3934	3934	3934	3934	3934	3934	3934	3934	3814	3934
Perm. p-value: no effects	0.321	0.721	0.647	0.298	0.589	0.129	0.026	0.838	0.652	0.029	0.341	0.000
<b>Panel C: <i>Post</i> program impacts (by treatment arms)</b> (12 to 15 months after program ends)												
Public Works Treatment (ITT)	0.011 (0.019)	0.0081 (0.027)	0.0035 (0.028)	-0.76 (1.70)	-0.71 (1.57)	-0.12 (1.29)	2800.5 (2138.7)	-0.18 (0.22)	312.2 (1260.9)	2168.7 (1852.5)	925.7 (1536.8)	10429.5*** (3410.3)
Self-Empl. training (SET)	0.011 (0.018)	-0.018 (0.028)	0.021 (0.032)	3.42* (1.98)	0.46 (1.80)	2.77 (1.77)	4229.3 (3201.3)	0.22 (0.26)	-1591.8 (1280.0)	5595.5* (2910.0)	278.1 (1383.1)	7169.5 (4729.2)
Wage-Empl. training (WET)	0.0018 (0.019)	0.014 (0.028)	0.000048 (0.032)	3.12 (2.20)	-0.14 (1.65)	2.89 (1.90)	637.5 (2204.3)	0.24 (0.21)	-792.6 (1303.0)	135.8 (2302.8)	1077.6 (1716.7)	-3798.3 (4387.5)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	0.86	0.51	0.36	40.49	22.06	13.26	43481.10	8.42	20706.18	18872.95	50700.71	46348.14
p-value: PW+SET=0	0.238	0.672	0.375	0.168	0.870	0.153	0.018	0.878	0.289	0.004	0.459	0.000
p-value: PW+WET=0	0.489	0.350	0.906	0.198	0.534	0.113	0.215	0.818	0.704	0.437	0.285	0.142
p-value: SET=WET	0.672	0.219	0.515	0.904	0.698	0.964	0.360	0.963	0.538	0.175	0.627	0.039
Observations	3934	3934	3934	3934	3934	3934	3934	3934	3934	3934	3814	3934
Perm. p-value: PW+SET=0	0.246	0.675	0.384	0.180	0.869	0.164	0.021	0.879	0.294	0.005	0.468	0.000
Perm. p-value: PW+WET=0	0.502	0.364	0.908	0.208	0.544	0.115	0.230	0.816	0.709	0.452	0.302	0.153
Perm. p-value: SET=WET	0.688	0.230	0.513	0.907	0.705	0.967	0.364	0.963	0.548	0.178	0.639	0.042

ITT estimates in panels A and B based on specification in equation 1.6. Estimates in panel C based on specification in equation 1.7. Robust standard errors clustered at (broad) brigade level in parentheses. *Hours*, *earnings*, *expenditures*, and *savings* winsorized at the 97th percentile. For variables ( $y$ ) in logarithms we take  $\ln(y + 1)$ . Permutation tests use 10000 permutations for each hypothesis. \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Table 1.4 – Impacts *during* and *post* program, alternative definitions of earnings and savings outcomes

	Total earnings			Self-Employment earnings			Wage-Employment earnings		
	(1) Ln, winsorized at 97%	(2) Ln, winsorized at 99%	(3) Level, winsorized at 99%	(4) Ln, winsorized at 97%	(5) Ln, winsorized at 99%	(6) Level, winsorized at 99%	(7) Ln, winsorized at 97%	(8) Ln, winsorized at 99%	(9) Level, winsorized at 99%
<b>Panel A: Impacts <i>during</i> the program</b> (around 4.5 months after program starts)									
Public Works Treatment (ITT)	2.95*** (0.19)	2.95*** (0.20)	24380.2*** (5998.7)	-1.04*** (0.20)	-1.06*** (0.20)	-11303.4*** (4091.6)	5.93*** (0.25)	5.93*** (0.25)	37181.6*** (4033.5)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	7.87	7.89	54626.16	3.23	3.26	23508.66	4.62	4.64	28163.73
Observations	2912	2912	2912	2912	2912	2912	2912	2912	2912
Perm. p-value: no effects	0.000	0.000	0.000	0.000	0.000	0.007	0.000	0.000	0.000
<b>Panel B: <i>Post</i> program impacts (pooled treatment)</b> (12 to 15 months after program ends)									
Public Works Treatment (ITT)	-0.037 (0.18)	-0.028 (0.18)	7597.5*** (2380.5)	0.22 (0.23)	0.23 (0.23)	7802.7*** (2250.6)	-0.20 (0.19)	-0.20 (0.19)	-542.8 (1145.3)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	8.42	8.43	45690.44	3.56	3.57	20487.91	5.04	5.05	22036.94
Observations	3934	3934	3934	3934	3934	3934	3934	3934	3934
Perm. p-value: no effects	0.846	0.880	0.002	0.338	0.326	0.001	0.295	0.304	0.642
<b>Panel C: <i>Post</i> program impacts (by treatment arms)</b> (12 to 15 months after program ends)									
Public Works Treatment (ITT)	-0.18 (0.22)	-0.18 (0.22)	5558.1** (2665.5)	0.13 (0.27)	0.14 (0.28)	5098.3** (2288.8)	-0.22 (0.24)	-0.23 (0.24)	-21.1 (1486.7)
Self-Empl. training (SET)	0.22 (0.26)	0.23 (0.26)	5171.9 (3709.3)	0.28 (0.34)	0.29 (0.35)	7999.6** (3763.3)	-0.065 (0.26)	-0.064 (0.26)	-1327.7 (1531.2)
Wage-Empl. training (WET)	0.24 (0.21)	0.24 (0.21)	1188.7 (3179.3)	-0.0027 (0.32)	-0.0025 (0.32)	439.3 (3699.5)	0.15 (0.27)	0.15 (0.28)	-299.5 (1544.4)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	8.42	8.43	45690.44	3.56	3.57	20487.91	5.04	5.05	22036.94
p-value: PW+SET=0	0.878	0.840	0.003	0.184	0.168	0.000	0.246	0.246	0.318
p-value: PW+WET=0	0.818	0.787	0.049	0.706	0.689	0.165	0.764	0.770	0.826
p-value: SET=WET	0.963	0.972	0.392	0.462	0.451	0.173	0.456	0.452	0.476
Observations	3934	3934	3934	3934	3934	3934	3934	3934	3934
Perm. p-value: PW+SET=0	0.877	0.847	0.003	0.194	0.177	0.001	0.254	0.256	0.322
Perm. p-value: PW+WET=0	0.826	0.792	0.053	0.708	0.693	0.166	0.766	0.779	0.836
Perm. p-value: SET=WET	0.962	0.975	0.398	0.468	0.465	0.180	0.469	0.457	0.481

ITT estimates in panels A and B based on specification in equation 1.6. Estimates in panel C based on specification in equation 1.7. Robust standard errors clustered at (broad) brigade level in parentheses. For variables ( $y$ ) in logarithms we take  $\ln(y + 1)$ . Permutation tests use 10000 permutations for each hypothesis. \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Table 1.5 – Impacts *during* and *post* program, well-being, behaviors and work habits

	(1) Well-being index (z-score)	(2) Behavior index (z-score)	(3) Rest at 6 am of prev. day	(4) Leisure at 6 am of prev. day	(5) Work at 6 am of prev. day	(6) Rest at 10 pm of prev. day	(7) Leisure at 10 pm of prev. day	(8) Work at 10 pm of prev. day	(9) Risky behavior (index)
<b>Panel A: Impacts <i>during</i> the program</b> (around 4.5 months after program starts)									
Public Works Treatment (ITT)	0.20*** (0.046)	0.12*** (0.045)	-0.14*** (0.018)	-0.044*** (0.011)	0.33*** (0.032)	0.071*** (0.019)	-0.054*** (0.015)	-0.028** (0.012)	-0.18 (0.14)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	-0.03	-0.02	0.24	0.09	0.20	0.68	0.18	0.08	0.62
Observations	2934	2946	2955	2955	2955	2953	2953	2953	2956
Perm. p-value: no effects	0.000	0.006	0.000	0.000	0.000	0.000	0.001	0.015	0.340
<b>Panel B: <i>Post</i> program impacts (pooled treatment)</b> (12 to 15 months after program ends)									
Public Works Treatment (ITT)	0.11*** (0.041)	-0.012 (0.040)	0.025 (0.019)	-0.0029 (0.012)	0.0080 (0.016)	-0.000063 (0.018)	0.0011 (0.015)	0.0061 (0.011)	-0.074 (0.10)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	-0.05	0.01	0.26	0.09	0.24	0.68	0.19	0.09	0.63
Observations	3932	3933	3933	3933	3933	3933	3933	3933	3934
Perm. p-value: no effects	0.009	0.774	0.202	0.812	0.619	0.996	0.946	0.572	0.412
<b>Panel C: <i>Post</i> program impacts (by treatment arms)</b> (12 to 15 months after program ends)									
Public Works Treatment (ITT)	0.14*** (0.052)	0.025 (0.050)	0.021 (0.022)	0.0036 (0.014)	0.021 (0.021)	-0.00080 (0.023)	0.0037 (0.019)	0.0041 (0.014)	0.0025 (0.13)
Self-empl. training (SET)	-0.0068 (0.051)	-0.039 (0.045)	-0.0075 (0.032)	-0.0036 (0.013)	-0.016 (0.023)	0.0027 (0.023)	-0.0085 (0.019)	0.0030 (0.012)	-0.096 (0.13)
Wage-empl. training (WET)	-0.075 (0.047)	-0.077 (0.052)	0.022 (0.026)	-0.017 (0.014)	-0.025 (0.022)	-0.00038 (0.022)	0.00041 (0.020)	0.0033 (0.015)	-0.14 (0.15)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	-0.05	0.01	0.26	0.09	0.24	0.68	0.19	0.09	0.63
p-value: PW+SET=0	0.011	0.762	0.648	0.995	0.798	0.929	0.790	0.579	0.451
p-value: PW+WET=0	0.187	0.293	0.076	0.383	0.834	0.957	0.839	0.583	0.324
p-value: SET=WET	0.169	0.410	0.230	0.385	0.652	0.895	0.680	0.981	0.757
Observations	3932	3933	3933	3933	3933	3933	3933	3933	3934
Perm. p-value: PW+SET=0	0.011	0.774	0.656	0.996	0.807	0.930	0.796	0.587	0.474
Perm. p-value: PW+WET=0	0.198	0.295	0.076	0.402	0.842	0.959	0.840	0.597	0.329
Perm. p-value: SET=WET	0.178	0.418	0.239	0.396	0.651	0.899	0.688	0.983	0.766

ITT estimates in panels A and B based on specification in equation 1.6. Estimates in panel C based on specification in equation 1.7. The definitions of the *well-being index*, the *behavior index*, and the *risky behavior index* are discussed in Appendix G (the estimation of impacts on the *risky behavior index* is explained in table A14). Tables A12, A13, and A14 present estimates for individual components of these three indices. Robust standard errors clustered at (broad) brigade level in parentheses. Permutation tests use 10000 permutations for each hypothesis. \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Table 1.6 – Heterogeneity in impacts on earnings *during* and *post* program, machine learning results

	(1)	(2)	(3)	(4)
	Ln total earnings (Monthly)	Ln total earnings (Monthly)	Total earnings (Monthly)	Total earnings (Monthly)
	<i>Midline</i>	<i>Endline</i>	<i>Midline</i>	<i>Endline</i>
<b>Panel A: Predicted average treatment effect and heterogeneity loading parameter</b>				
ATE ( $\beta_1$ )	2.642 (2.336,2.947) [0.000]	-0.0297 (-0.466,0.397) [1.000]	24363.0 (17618.5,31150.3) [0.000]	3314.0 (-1842.7,8523.0) [0.414]
HET ( $\beta_2$ )	1.231 (0.862,1.599) [0.000]	0.111 (-0.515,0.721) [0.970]	0.390 (0.00909,0.777) [0.089]	0.416 (-1.149,5.384) [0.756]
Best ML method	Generalized Random forest	Random forest	Random forest	R-Learner
<b>Panel B: By quartile of predicted impacts on earnings <i>during</i> program (using <i>midline</i>) (GATES)</b>				
Mean in quartile 1 (0 to 25%)	1.465 (0.867,2.062)	-0.306 (-1.322,0.729)	14659.7 (1669.7,28176.4)	460.9 (-11931.0,12389.1)
Mean in quartile 2 (25 to 50%)	2.296 (1.695,2.887)	-0.311 (-1.341,0.729)	23172.3 (9865.0,36605.8)	2793.9 (-9810.5,15283.1)
Mean in quartile 3 (50 to 75%)	2.832 (2.215,3.425)	-0.0372 (-1.076,0.992)	29778.2 (16363.7,42745.5)	5658.1 (-6700.1,17794.3)
Mean in quartile 4 (75 to 100%)	3.966 (3.352,4.560)	-0.252 (-1.289,0.799)	31670.9 (18254.4,44906.7)	4490.7 (-7835.4,16887.3)
P-value all coefficients are equal	0.000	1.000	0.281	1.000
Best ML method	Generalized Random forest	Generalized Random forest	Random forest	Random forest
<b>Panel C: By quartile of predicted impacts on earnings <i>post</i> program (using <i>endline</i>) (GATES)</b>				
Mean in quartile 1 (0 to 25%)	2.860 (2.240,3.474)	-0.0834 (-0.963,0.803)	20120.2 (7275.6,32824.1)	2884.7 (-7540.5,12739.3)
Mean in quartile 2 (25 to 50%)	2.641 (2.036,3.253)	-0.0669 (-0.950,0.791)	26809.6 (14045.8,39322.7)	4020.6 (-6197.8,14214.9)
Mean in quartile 3 (50 to 75%)	2.426 (1.795,3.041)	-0.0609 (-0.915,0.796)	27443.4 (14798.2,40110.1)	3920.1 (-6313.2,14117.8)
Mean in quartile 4 (75 to 100%)	2.652 (2.020,3.277)	0.0360 (-0.823,0.886)	23655.1 (11190.1,36131.8)	2549.3 (-7586.6,13104.9)
P-value all coefficients are equal	0.100	0.888	0.477	1.000
Best ML method	Random forest	Random forest	R-Learner	R-Learner

Heterogeneity analysis based on the approach in Chernozhukov et al. (2020) (see discussion in Section 1.4.2 and Appendix H.2). Columns (1) and (3) (respectively columns (2) and (4)) focus on outcomes at midline (respectively endline). Estimates in Panel A are based on equation 1.9. P-value (in brackets) for  $\beta_1$  tests the hypothesis of no effect. P-value (in brackets) for  $\beta_2$  tests the hypothesis of no heterogeneity. GATES estimates are based on the specification in equation 1.10. Panel B (respectively Panel C) shows impacts per quartile of the predicted treatment effects at midline (respectively endline). The best predictions are reported. The chosen algorithm is the one with the highest  $\Lambda$  (lambda): the larger  $\Lambda$  gets, the stronger the correlation between  $\hat{s}_0(Z)$  noted  $S(Z)$  and  $s_0(Z)$  (see appendix table A16 for comparisons across algorithms). Predictions are estimated for each sample split, reported values are the medians across 100 sample splits. Adjusted confidence intervals at 90% in parentheses and adjusted p-values for partition uncertainty. *Earnings* variable is in CFA, winsorized at the 97th percentile. For variables ( $y$ ) in logarithms we take  $\ln(y+1)$ .

Table 1.7 – Baseline characteristics of the bottom and top quartiles of the distribution of predicted impacts on (ln) earnings *during* program

	(1) Mean in 1st quartile	(2) Mean in 4th quartile	(3) Test (1)-(2) (p-value)
<b>Individual characteristics</b>			
Female	0.15	0.53	0
Live in urban area	0.94	0.96	0.608
Age	25.22	24.29	0
Nb of children	0.78	0.86	0.316
<b>Education</b>			
Years of education	11	9.69	0.026
Primary education not completed	0.40	0.48	0.071
Has participated in vocational training	0.52	0.28	0
<b>Household characteristics and assets</b>			
Household size	5.91	6.45	0.060
Is head of household	0.39	0.090	0
Total Nb of assets	0.63	0.43	0
Nb of transportation assets	0.83	0.61	0.030
Nb of agricultural assets	5.39	3.39	0.041
Nb of household durables	1.96	1.63	0.031
Nb of communication assets	7.57	6.68	0.011
<b>Employment</b>			
Has an activity	1	0.44	0
Is Wage-Employed	0.55	0.09	0
Is Self-Employed	0.52	0.14	0
Nb of activities	1.38	0.46	0
Total Earnings (monthly, CFA)	49673.3	653.4	0
Searched for a job (last month)	0.61	0.52	1
<b>Savings, constraints and expenditures</b>			
Has saved (last 3 months)	0.72	0.25	0
Of which: share of formal savings (cond. on savings)	0.33	0.17	0.001
Has a savings account	0.20	0.06	0
Savings Stock (CFA)	65924.9	6795.1	0
Self-reported constraints to repay loans	0.20	0.18	0.513
Self-reported constraints to access credit	0.43	0.58	0
Self-reported constraints for basic needs expenditures	0.66	0.71	0.257
Transportation expenditures (last 7 days, CFA)	3041.8	1021.0	0
Communication expenditures (last 7 days, CFA)	3221.0	655.9	0
<b>Cognitive skills and risk preference</b>			
Cognitive (deduction, Raven Test)	0.24	0.23	0.738
Dexterity (Nuts Test)	0.37	0.39	0.124
Dexterity (Bolts Test)	0.34	0.33	0.458
Positive affect (CES-D scale, Nb positive days)	6.45	6.09	0.084
Positive attitude towards the future (ZTPI scale)	29.33	29.11	0.676
Is Risk averse (based on hypothetical lotteries)	0.71	0.70	1
Patience (scale 0 to 10, 10=very patient)	3.44	3.28	0.872
Preference for present (actualization rate for 1 month)	0.58	0.58	1

Heterogeneity analysis based on the approach in [Chernozhukov et al. \(2020\)](#) (see discussion in Section 1.4.2 and Appendix H.2). Column (1) (respectively (2)) displays average characteristics of the bottom (respectively top) quartile of the distribution of predicted impacts on (ln) earnings during the program. Column (3) reports p-values for a test of equality between the top and bottom quartile. Reported results are based on the algorithm with best predictions for midline : Generalized Random forest. The chosen algorithm is the one with the highest  $\Lambda$  (lambda): the larger  $\Lambda$  gets, the stronger the correlation between  $\widehat{s}_0(Z)$  noted  $S(Z)$  and  $s_0(Z)$  (see appendix table A16 for comparisons across algorithms). Means by quartile are estimated for each sample split, reported values are the medians across 100 sample splits. Adjusted p-values for partition uncertainty. *Household assets* and *savings stock* variables winsorized at the 99th percentile.



Table 1.8 – Impacts *during* and *post* program on main outcomes, by quartile of predicted impacts on (ln) earnings *during* the program

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Employed	Wage-Empl. (in at least 1 activity)	Self-Empl. (in at least 1 activity)	Total Hours worked (weekly)	Total earnings in CFA (monthly)	Ln total earnings (monthly)	Earnings in Wage-Empl. in CFA (monthly)	Earnings in Self-Empl. in CFA (monthly)	Total expenditures in CFA (monthly)	Savings in CFA (stock)	Well-being index (z-score)
<b>Panel A: Impacts <i>during</i> the program</b> (around 4.5 months after program starts)											
Mean in quartile 1 (0 to 25%)	0.039 (-0.0078, 0.087)	0.35 (0.28, 0.42)	-0.059 (-0.16, 0.046)	0.81 (-4.39, 6.08)	14805 (1639, 27760)	1.46 (0.87, 2.06)	28834 (21922, 35807)	-6859 (-12362, -1345)	15414 (6633, 24276)	35561 (23460, 47674)	0.23 (0.025, 0.44)
Mean in quartile 2 (25 to 50%)	0.093 (0.046, 0.14)	0.40 (0.33, 0.46)	-0.11 (-0.21, -0.0051)	1.12 (-4.09, 6.35)	21937 (8698, 35146)	2.30 (1.70, 2.89)	33479 (26487, 40457)	-6486 (-12019, -936)	10622 (1709, 19388)	32056 (20219, 44174)	0.21 (0.0014, 0.43)
Mean in quartile 3 (50 to 75%)	0.13 (0.082, 0.18)	0.44 (0.37, 0.50)	-0.10 (-0.20, -0.00068)	4.15 (-1.09, 9.28)	30085 (16876, 43013)	2.83 (2.22, 3.42)	36303 (29276, 43299)	-4536 (-9914, 738)	11175 (2444, 19858)	40038 (27897, 51787)	0.11 (-0.10, 0.31)
Mean in quartile 4 (75 to 100%)	0.23 (0.19, 0.28)	0.53 (0.46, 0.60)	-0.12 (-0.22, -0.013)	8.00 (2.63, 13.3)	31604 (18150, 44869)	3.97 (3.35, 4.56)	39464 (32525, 46318)	-4154 (-9726, 1312)	18011 (9471, 26864)	40551 (28577, 52610)	0.21 (0.0047, 0.42)
P-value all interactions equal	0.000	0.002	1.000	0.166	0.228	0.000	0.164	0.911	0.663	1.000	0.984
P-value treatXQ1=treatXQ4	0.000	0.001	0.809	0.104	0.114	0.000	0.068	0.771	1.000	1.000	1.000
Mean in control for Q(1)	0.97	0.85	0.34	48.3	74622.6	10.5	50610.5	14069.4	73954.4	55428.6	0.20
Mean in control for Q(2)	0.97	0.85	0.28	45.0	65875.5	10.3	47993.5	9482.7	59611.2	45068.4	0.16
Mean in control for Q(3)	0.94	0.83	0.25	41.9	60839.7	9.89	46146.6	7528.4	55768.8	42968.5	0.14
Mean in control for Q(4)	0.91	0.79	0.21	37.0	52419.4	9.37	42025.2	5264.7	47311.8	35408.5	0.07
<b>Panel B: <i>Post</i> program impacts (pooled treatment)</b> (12 to 15 months after program ends)											
Mean in quartile 1 (0 to 25%)	-0.020 (-0.098, 0.060)	0.0019 (-0.11, 0.12)	-0.0025 (-0.12, 0.11)	-1.36 (-8.53, 5.80)	1579 (-10755, 13912)	-0.31 (-1.32, 0.73)	3157 (-3737, 10005)	235 (-9751, 10546)	3729 (-5357, 12897)	12399 (-7700, 32587)	0.16 (-0.077, 0.40)
Mean in quartile 2 (25 to 50%)	-0.018 (-0.096, 0.061)	-0.010 (-0.12, 0.10)	0.0047 (-0.11, 0.12)	-1.06 (-8.27, 6.30)	736 (-11458, 13060)	-0.31 (-1.34, 0.73)	593 (-6330, 7529)	836 (-9441, 11045)	1290 (-7596, 10055)	3689 (-16501, 23859)	0.071 (-0.17, 0.31)
Mean in quartile 3 (50 to 75%)	0.018 (-0.061, 0.096)	-0.0028 (-0.12, 0.11)	-0.018 (-0.13, 0.097)	2.32 (-4.89, 9.55)	6456 (-6066, 18844)	-0.037 (-1.08, 0.99)	830 (-6015, 7696)	5076 (-5057, 15254)	-368 (-9472, 8493)	6800 (-13642, 27063)	0.056 (-0.18, 0.30)
Mean in quartile 4 (75 to 100%)	0.012 (-0.067, 0.090)	-0.028 (-0.14, 0.087)	0.049 (-0.064, 0.16)	-0.065 (-7.30, 7.17)	2771 (-9307, 15356)	-0.25 (-1.29, 0.80)	-2661 (-9488, 4389)	5650 (-4708, 16012)	1946 (-7036, 11018)	10040 (-9921, 30277)	0.068 (-0.17, 0.30)
P-value all interactions equal	1.000	1.000	1.000	1.000	1.000	1.000	0.800	1.000	1.000	1.000	1.000
P-value treatXQ1=treatXQ4	1.000	1.000	1.000	1.000	1.000	1.000	0.397	0.855	1.000	1.000	0.979
Mean in control for Q(1)	0.94	0.64	0.40	47.6	64297.7	9.76	30622.6	27727.4	66767.3	77089.8	0.14
Mean in control for Q(2)	0.90	0.56	0.34	44.5	48049.5	8.86	23910.2	19502.6	53638.7	58768.3	0.10
Mean in control for Q(3)	0.88	0.54	0.32	42.6	45313.4	8.37	21591.9	18096.8	49538.7	52302.9	0.10
Mean in control for Q(4)	0.80	0.48	0.32	35.7	34223.3	7.31	16394.0	15165.5	43487.1	42764.8	-0.04

Heterogeneity analysis based on the approach in Chernozhukov et al. (2020) (see discussion in Section 1.4.2 and Appendix H.2). Estimates of treatment effects on the main outcomes during the program (Panel A), and after the program (Panel B) by quartile of the (predicted) impacts on earnings during the program. At the bottom of each panel, we present a test of equality of treatment effects between all quartiles respectively between the bottom (Q1) and top (Q4) quartiles. Estimation is based on a specification similar to equation 1.10 but replacing the dependent variable with the alternative (midline or endline) outcome variable. The best predictions during program are used: Generalized Random Forest. The chosen algorithm is the one with the highest  $\Lambda$  (lambda): the larger  $\Lambda$  gets, the stronger the correlation between  $\widehat{s}_0(Z)$  noted  $S(Z)$  and  $s_0(Z)$  (see appendix table A16 for comparisons across algorithms). Predictions are estimated for each sample split, reported values are the medians across 100 sample splits. Adjusted p-values for partition uncertainty. *Hours*, *earnings*, *expenditures*, and *savings* winsorized at the 97th percentile. For variables ( $y$ ) in logarithms we take  $\ln(y + 1)$ .

Table 1.9 – Impacts *post* program on intermediate outcomes, by quartile of predicted impacts on (ln) earnings *during* the program

	Investment in Self-Employed Activities				Search for Wage Jobs	
	(1)	(2)	(3)	(4)	(5)	(6)
	Total number of income generating activities	Value of productive assets in CFA (stock)	Start-up capital in CFA (main self-empl. act., stock)	Value of investments in CFA (last 3 months)	Searched for a job (last 3 months)	Total spent in job search in CFA (last 12 months)
Mean in quartile 1 (0 to 25%)	0.026 (-0.12, 0.18)	11189 (261, 22223)	14723 (-310, 29499)	538 (-1431, 2560)	0.082 (-0.038, 0.20)	1806 (-3341, 6861)
Mean in quartile 2 (25 to 50%)	0.0093 (-0.14, 0.16)	3843 (-7105, 14666)	2899 (-11880, 18088)	322 (-1654, 2301)	0.0062 (-0.11, 0.13)	-580 (-5819, 4584)
Mean in quartile 3 (50 to 75%)	-0.0043 (-0.16, 0.15)	-1325 (-12576, 9517)	2086 (-13125, 17246)	-625 (-2558, 1338)	0.025 (-0.096, 0.14)	828 (-4278, 5934)
Mean in quartile 4 (75 to 100%)	0.038 (-0.11, 0.19)	-20 (-11026, 10771)	5612 (-9583, 20764)	-138 (-2121, 1916)	0.057 (-0.065, 0.18)	887 (-4198, 5942)
P-value all interactions equal	1.000	0.350	0.893	1.000	0.956	1.000
P-value treatXQ1=treatXQ4	1.000	0.282	0.710	1.000	1.000	1.000
Mean in control for Q(1)	0.55	23944.3	43103.8	3673.3	0.57	11914.3
Mean in control for Q(2)	0.50	18478.1	30054.0	2704.7	0.55	8466.7
Mean in control for Q(3)	0.47	14974.2	26206.3	2242.7	0.55	8269.3
Mean in control for Q(4)	0.40	10781.7	19455.5	974.5	0.56	8413.1

Heterogeneity analysis based on the approach in [Chernozhukov et al. \(2020\)](#) (see discussion in Section 1.4.2 and Appendix H.2). Estimates of post-program impacts on intermediary outcomes related to productive investments by quartile of the (predicted) impacts on earnings during the program. At the bottom of each panel, we present a test of equality of treatment effects between all quartiles respectively between the bottom (Q1) and top (Q4) quartiles. Estimation is based on a specification similar to equation 1.10 but replacing the dependent variable with the alternative (endline) outcome variable. The best predictions during program are used: Generalized Random Forest. The chosen algorithm is the one with the highest  $\Lambda$  (lambda): the larger  $\Lambda$  gets, the stronger the correlation between  $\widehat{s}_0(Z)$  noted  $S(Z)$  and  $s_0(Z)$  (see appendix table A16 for comparisons across algorithms). Predictions are estimated for each sample split, reported values are the medians across 100 sample splits. Adjusted p-values for partition uncertainty. *Value of productive assets, value of investments, start-up capital and total spend in job search* winsorized at the 97th percentile.

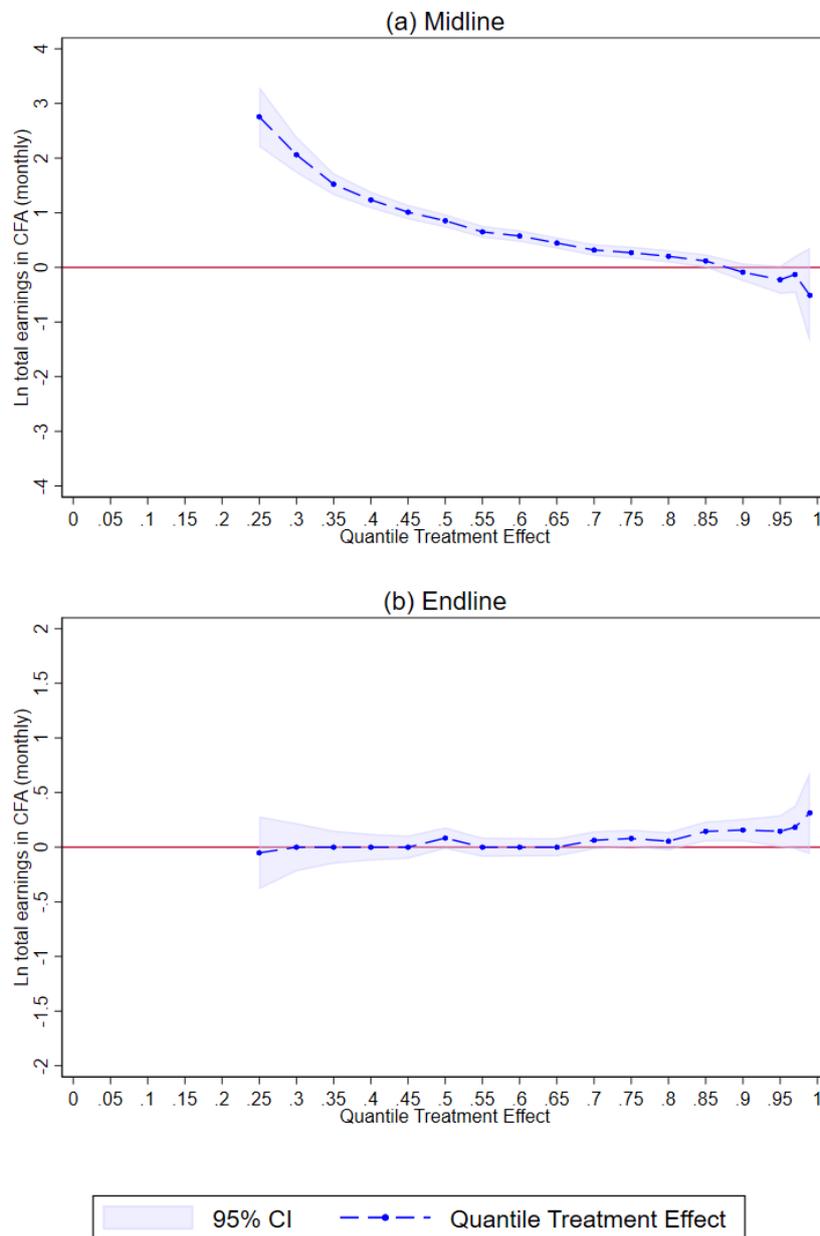
Table 1.10 – Impacts on (ln) earnings and cost-benefit ratios under alternative targeting approaches

	Random selection	Machine learning pred. by quartile of pred. impacts <i>during</i> program		Selection on baseline characteristics		
	(1)	(2)	(3)	(4)	(5)	(6)
	Treated (ITT)	Mean in quartile 4 (75% to 100%)	Mean in quartile 1 (0 to 25%)	Women	Low baseline earnings (self-declared) (bottom 25 %)	Low baseline earnings (predicted) (bottom 25%)
<b>Panel A: Impacts <i>during</i> the program</b> (around 4.5 months after program starts)						
Treatment	2.948*** (0.194)	3.966*** (0.307)	1.465*** (0.305)	3.728*** (0.349)	4.035*** (0.384)	3.657*** (0.373)
Observations	2912	2877	2877	2912	2877	2877
<b>Panel B: <i>Post</i> program impacts (pooled treatment)</b> (12 to 15 months after program ends)						
Treatment	-0.037 (0.181)	-0.252 (0.529)	-0.306 (0.525)	-0.050 (0.345)	-0.147 (0.517)	0.020 (0.405)
Observations	3934	3865	3865	3934	3736	3736
<b>Panel C: Cost-benefit Analysis</b>						
Effect <i>during</i> program in CFA [A]	27083	31671	14660	32097	33954	32824
Effect <i>post</i> program in CFA [B]	4361	4491	461	7168	7602	10646
Discounted sum of impacts ( <i>during</i> + <i>post</i> program) [C]	253920.50	287958.58	109428.85	330429.84	349837.28	386817.87
Total cost per beneficiary [D]	768708.10	768708.10	768708.10	768708.10	768708.10	768708.10
Cost-benefit ratio ( <i>during</i> + <i>post</i> program) [E]	3.03	2.67	7.02	2.33	2.20	1.99

Column (1) is the ITT estimate based on specification in equation 1.6. Columns (4-6) show the estimated  $\beta_1$  coefficient from the specification in equation 1.8. Columns (2) and (3) document the effect of selecting the observations in the top and bottom quartile of the distribution of predicted impacts at midline. Column 2 (respectively column 3) of Panel B reports group averages (GATES) endline impacts for observations in the top quartile (respectively bottom quartile) of predicted impacts at midline. (This is similar to Table 1.6, panel B, column (2)). Column (6) uses predictions of baseline earnings based on the Elastic Net method. [A] Effect on total monthly earnings *during* program in CFA, in levels, winsorized at the 97th percentile. [B] Effect on total monthly earnings *post* program in CFA, in levels, winsorized at the 97th percentile. [C] Discounted sum of impacts on total earnings from program start (month 1) up to 14 months after program ended (month 21). It is computed as  $\sum_{k=1}^7 \rho^{(k-4)} \beta^{During} + \sum_{k=8}^{21} \rho^{(k-7)} \beta^{Post}$ , with  $\beta^{During}$  (respectively  $\beta^{Post}$ ) the contemporaneous (respectively post-program) ITT estimates of impact on monthly total earnings and  $\rho$  the monthly discount factor.  $\rho = 1/(1 + \delta)^{1/12}$ ,  $\delta = 10\%$ . For column (3), we consider post-program impacts to be 0 when calculating *the discounted sum of impacts* in [C]. [E] = [D] / [C]. Robust standard errors clustered at (broad) brigade level in parentheses. \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

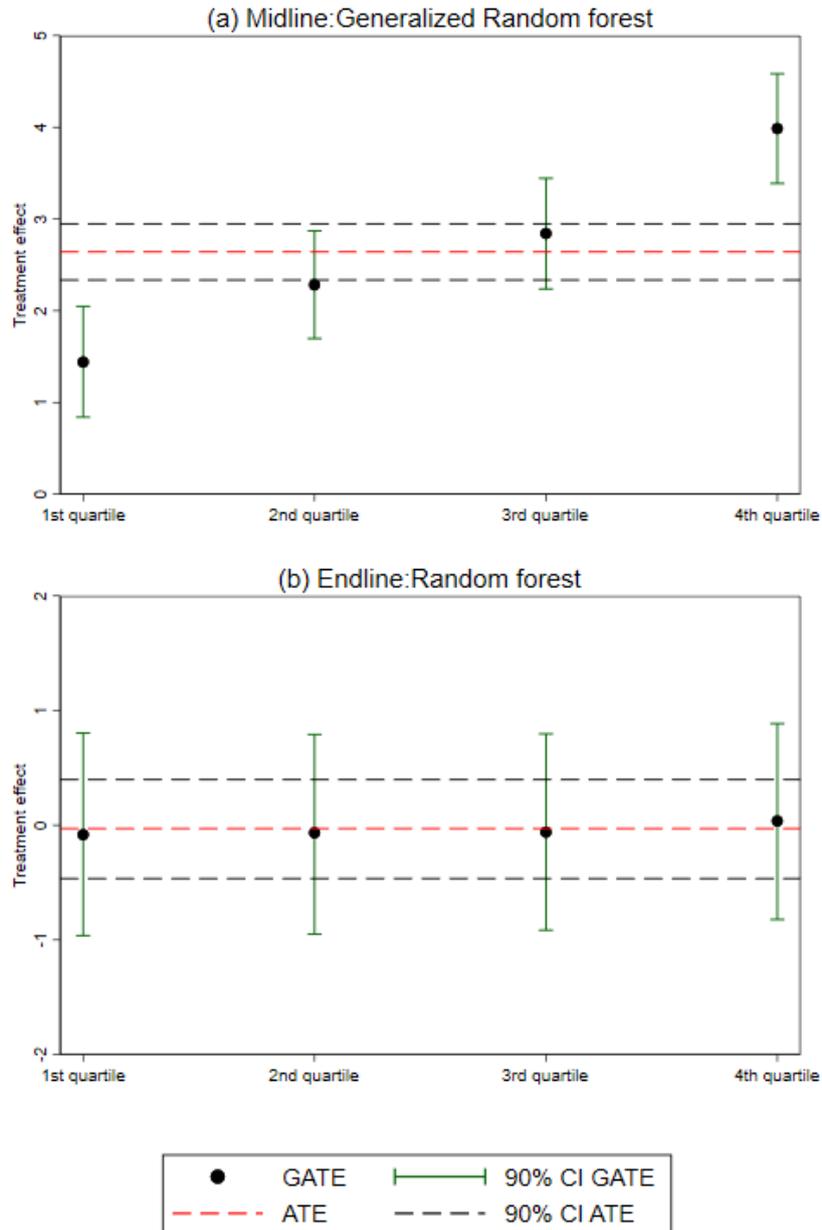
## **B Main figures**

Figure 1.1 – Quantile treatment effects for (ln) earnings *during* and *post* program

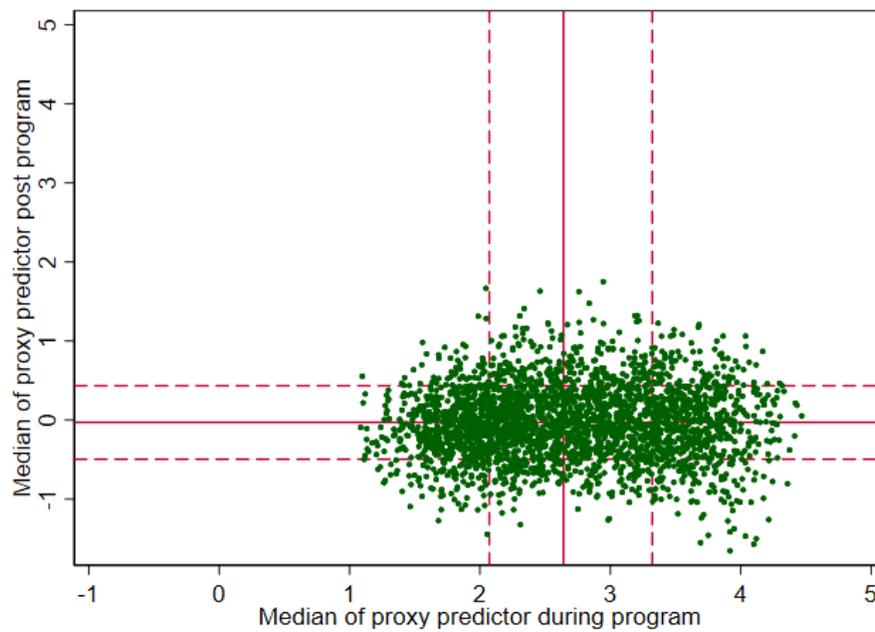


Note: Quantile treatment effects (for non-zero earnings) up to the 99th percentile. Permutation tests use 10000 permutations for each hypothesis. Wilcoxon rank-sum test permutation p-value is 0.000 for panel (a) and 0.466 for panel (b). Kolmogorov-Smirnov permutation p-value is 0.000 for panel (a) and 0.776 for panel (b). Total monthly earnings variable is in CFA. The variable is not winsorized to study the top of the distribution. Results shown up to the 99th percentile.

Figure 1.2 – Group average treatment effects (GATES) for (ln) earnings

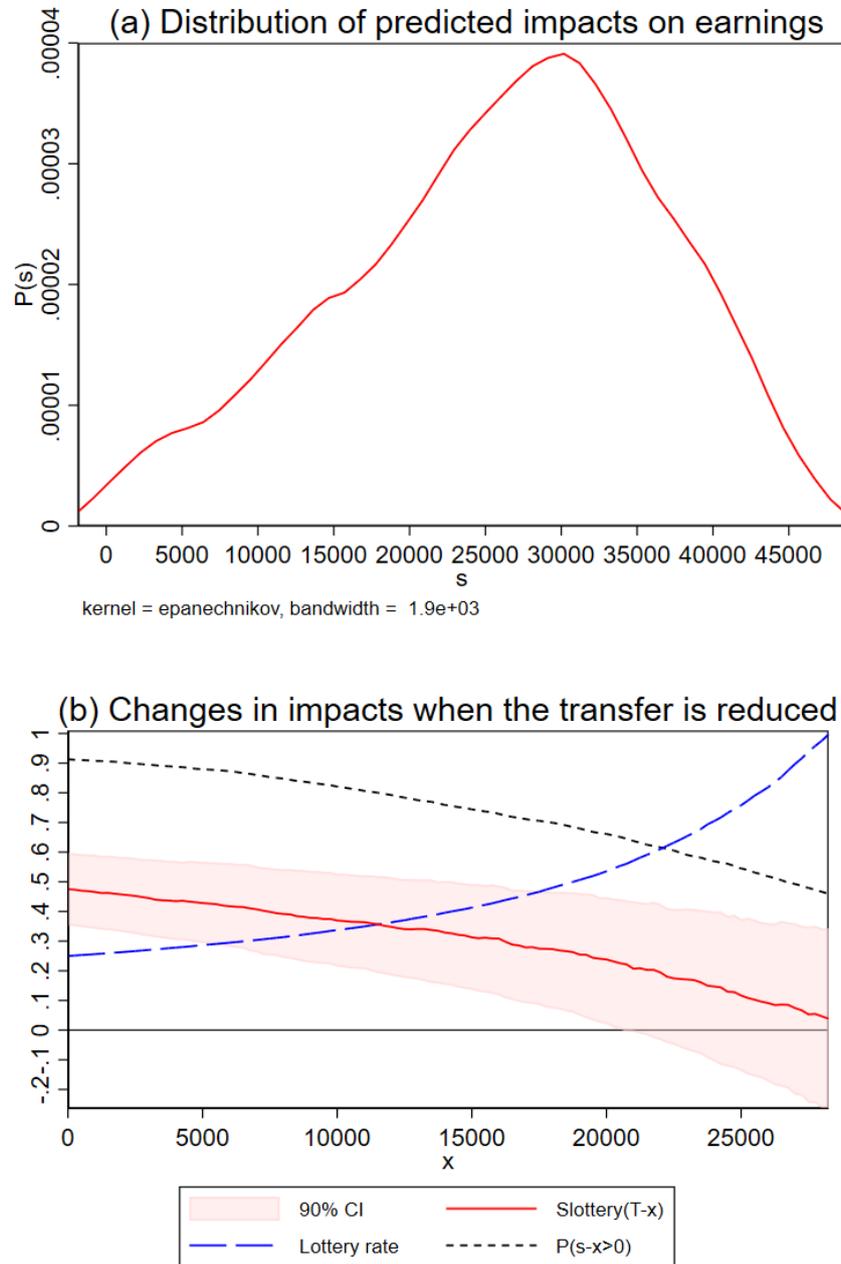


Note: Heterogeneity analysis based on the approach in Chernozhukov et al. (2020) (see discussion in Section 1.4.2 and Appendix H.2). The best predictions are reported. Choice of algorithm is based on  $\Lambda$  and is indicated at the top of each figure (see appendix table A16 for comparisons across algorithms). Total monthly earnings variable is in CFA, winsorized at the 97th percentile. For variables ( $y$ ) in logarithms we take  $\ln(y + 1)$ .

Figure 1.3 – Predicted impact on (ln) earnings *during* vs *post* program

Note: Heterogeneity analysis based on the approach in [Chernozhukov et al. \(2020\)](#) (see discussion in Section 1.4.2 and Appendix H.2). Median across 100 simulations. Solid lines represent the ATE. Dashed lines delimit bottom 25% and top 25% of the distribution. Predictions using Generalized Random Forest for impacts during program. Predictions using Random Forest for post program impacts. Total monthly earnings variable is in CFA, winsorized at the 97th percentile. For variables ( $y$ ) in logarithms we take  $\ln(y + 1)$ .

Figure 1.4 – Change in impact on total earnings when the transfer is reduced



Panel (a) presents the estimated distribution of our ML estimate of CATE using the best method: Random Forest (see appendix Table A16 for comparisons across algorithms). Panel (b) presents simulations of changes in impacts induced by a reduction in the transfer by  $x$ , assuming the distribution of individual impact on earnings in Panel (a) (see conceptual framework in section 1.2). The dotted black line shows the share of initial applicants who still apply when the transfer is reduced by  $x$ . The dashed blue line shows the lottery success rate when the transfer is reduced by  $x$  (Lottery rate =  $0.25T/((T-x)P(s_T - x > 0))$ ). The figure reports this lottery rate up to the value where it reaches 1 ( $\approx$  CFA 27,500). The solid red line shows the average impact on earnings  $E((s-x)1(s-x > 0))/(P(s-x > 0)(T-x))$  when the transfer amount is reduced by  $x$ ,  $S_{lottery}(T-x)$  appearing in equation 1.5, normalized by  $0.25 \times 50,000$ , the amount available for each initial applicant.

## C Additional Tables

Table A11 – Estimated impacts *during* and *post* program on economic outcomes, with baseline controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Employed	Wage- Employed (in at least 1 activity)	Self- Employed (in at least 1 activity)	Total Hours worked (weekly)	Hours worked in Wage- Empl. (weekly)	Hours worked in Self-Empl. (weekly)	Total earnings in CFA (monthly)	Ln total earnings (monthly)	Earnings in Wage- Empl. in CFA (monthly)	Earnings in Self-Empl. in CFA (monthly)	Total ex- penditures in CFA (monthly)	Savings in CFA (stock)	Well-being index (z-score)
<b>Panel A: Impacts <i>during</i> the program</b> (around 4.5 months after program starts)													
Public Works Treatment (ITT)	0.14*** (0.01)	0.48*** (0.02)	-0.09*** (0.02)	5.06*** (1.26)	15.68*** (1.29)	-6.87*** (1.03)	27485.77*** (2608.25)	2.92*** (0.19)	36799.02*** (1463.69)	-5567.13*** (1196.97)	14431.49*** (1317.32)	40035.35*** (2303.55)	0.19*** (0.05)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes
Mean in Control	0.85	0.49	0.35	39.69	21.40	12.67	42841.22	7.87	20188.33	12753.65	47233.52	19250.05	-0.03
Observations	2958	2958	2958	2958	2958	2958	2912	2912	2912	2912	2945	2958	2934
<b>Panel B: <i>Post</i> program impacts (pooled treatment)</b> (12 to 15 months after program ends)													
Public Works Treatment (ITT)	0.02 (0.02)	0.00 (0.02)	0.02 (0.02)	0.74 (1.40)	-1.58 (1.26)	2.47** (1.23)	5155.88*** (1902.00)	0.00 (0.19)	-665.49 (1079.42)	4783.59*** (1852.73)	2387.97 (1466.46)	10143.10*** (3316.16)	0.12*** (0.04)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	0.86	0.51	0.36	41.00	22.50	13.80	43481.10	8.42	20706.18	18872.95	50700.71	46348.14	-0.05
Observations	3934	3934	3934	3934	3934	3934	3934	3934	3934	3934	3814	3934	3932
<b>Panel C: <i>Post</i> program impacts (by treatment arms)</b> (12 to 15 months after program ends)													
Public Works Treatment (ITT)	0.01 (0.02)	0.00 (0.03)	0.02 (0.03)	-1.58 (1.78)	-1.82 (1.65)	0.53 (1.44)	3406.18 (2116.14)	-0.14 (0.22)	69.13 (1319.73)	3071.78 (1901.47)	2131.18 (1596.09)	8598.92** (3644.89)	0.15*** (0.05)
Self-Empl. training (SET)	0.01 (0.02)	-0.02 (0.03)	0.02 (0.03)	3.59* (2.01)	0.74 (1.83)	2.58 (1.80)	4226.69 (3044.51)	0.21 (0.24)	-1668.02 (1287.18)	5203.42* (2826.96)	-207.36 (1422.57)	8143.34* (4479.69)	-0.01 (0.05)
Wage-Empl. training (WET)	0.00 (0.02)	0.01 (0.03)	0.00 (0.03)	3.42 (2.36)	-0.03 (1.65)	3.41 (2.16)	947.80 (1963.81)	0.24 (0.19)	-796.06 (1313.08)	210.73 (2173.59)	881.90 (1599.02)	-3206.44 (4242.97)	-0.08* (0.05)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	0.86	0.51	0.36	41.00	22.50	13.80	43481.10	8.42	20706.18	18872.95	50700.71	46348.14	-0.05
Observations	3934	3934	3934	3934	3934	3934	3934	3934	3934	3934	3814	3934	3932

ITT estimates in panels A and B based on specification in equation 1.6. Estimates in panel C based on specification in equation 1.7. The set of baseline controls differs for each outcome. Variables are selected from a pool of 1312 covariates using post-double selection lasso. Control variables include information about individual characteristics, education, household composition, experience of violence, household expenditure, asset ownership, and access to infrastructure. Robust standard errors clustered at (broad) brigade level in parentheses. Hours, earnings, expenditures, and savings winsorized at the 97th percentile. \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Table A12 – Estimated impacts *during* and *post* program on well-being index components

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Well-being index (z-score)	Self-esteem (Rosenberg scale) (z-score)	Positive Affect (CES-D sub scale) (z-score)	Positive attitude towards the future (ZTPI sub scale) (z-score)	Present fatalism (ZTPI sub scale) (z-score)	Happiness in daily activities (z-score)	Pride in daily activities (z-score)
<b>Panel A: Impacts <i>during</i> the program</b> (around 4.5 months after program starts)							
Public Works Treatment (ITT)	0.20*** (0.05)	0.14*** (0.04)	0.18*** (0.04)	0.086** (0.04)	0.021 (0.04)	0.15*** (0.05)	0.15*** (0.05)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	-0.03	-0.02	-0.05	-0.01	0.03	0.01	-0.00
Observations	2934	2951	2958	2951	2955	2950	2949
Perm. p-value: no effects	0.000	0.001	0.000	0.035	0.606	0.002	0.001
<b>Panel B: <i>Post</i> program impacts (pooled treatment)</b> (12 to 15 months after program ends)							
Public Works Treatment (ITT)	0.11*** (0.04)	0.10** (0.04)	0.041 (0.04)	0.061 (0.05)	-0.093** (0.04)	0.076* (0.04)	0.053 (0.04)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	-0.05	-0.05	-0.03	-0.01	0.05	-0.02	-0.02
Observations	3932	3933	3932	3933	3933	3933	3933
Perm. p-value: no effects	0.010	0.022	0.314	0.197	0.041	0.074	0.201
<b>Panel C: <i>Post</i> program impacts (by treatment arms)</b> (12 to 15 months after program ends)							
Public Works Treatment (ITT)	0.14*** (0.05)	0.12** (0.05)	0.063 (0.05)	0.094* (0.05)	-0.052 (0.05)	0.11** (0.05)	0.089* (0.05)
Self-Empl. training (SET)	-0.0068 (0.05)	-0.044 (0.06)	-0.0078 (0.05)	-0.11* (0.06)	-0.11** (0.05)	-0.018 (0.05)	-0.025 (0.05)
Wage-Empl. training (WET)	-0.075 (0.05)	-0.019 (0.05)	-0.062 (0.05)	0.0034 (0.05)	-0.020 (0.05)	-0.082* (0.05)	-0.087* (0.05)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	-0.05	-0.05	-0.03	-0.01	0.05	-0.02	-0.02
p-value: PW+SET=0	0.011	0.165	0.275	0.816	0.001	0.059	0.177
p-value: PW+WET=0	0.187	0.065	0.977	0.046	0.201	0.598	0.972
p-value: SET=WET	0.169	0.703	0.232	0.038	0.073	0.138	0.216
Observations	3932	3933	3932	3933	3933	3933	3933
Perm. p-value: PW+SET=0	0.014	0.175	0.277	0.820	0.000	0.064	0.188
Perm. p-value: PW+WET=0	0.201	0.066	0.977	0.052	0.211	0.609	0.970
Perm. p-value: SET=WET	0.178	0.706	0.239	0.040	0.081	0.141	0.224

ITT estimates in panels A and B based on specification in equation 1.6. Estimates in panel C based on specification in equation 1.7. The definition of the *well-being index* and variables entering the index is detailed in Appendix G. *Present fatalism* enters as an inverted measure in the index (a negative impact in column (5) is associated with a positive impact on the index in column (1)). Robust standard errors clustered at (broad) brigade level in parentheses. Permutation tests use 10000 permutations for each hypothesis. \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Table A13 – Estimated impacts *during* and *post* program on behavior index components

	(1)	(2)	(3)	(4)	(5)
	Behavior index (z-score)	Conduct problems (SDQ sub scale) (z-score)	Pro-social behavior (SDQ sub scale) (z-score)	Impulsiveness (DERS sub scale) (z-score)	Anger in daily activities (z-score)
<b>Panel A: Impacts <i>during</i> the program</b> (around 4.5 months after program starts)					
Public Works Treatment (ITT)	0.12*** (0.04)	-0.031 (0.04)	0.023 (0.04)	-0.095** (0.04)	-0.13*** (0.04)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes
Mean in Control	-0.02	0.01	-0.00	0.02	-0.01
Observations	2946	2957	2956	2954	2950
Perm. p-value: no effects	0.005	0.459	0.569	0.034	0.003
<b>Panel B: <i>Post</i> program impacts (pooled treatment)</b> (12 to 15 months after program ends)					
Public Works Treatment (ITT)	-0.012 (0.04)	0.013 (0.04)	-0.0032 (0.04)	0.0050 (0.04)	0.014 (0.04)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes
Mean in Control	0.01	-0.01	-0.01	-0.01	-0.03
Observations	3933	3933	3933	3933	3933
Perm. p-value: no effects	0.772	0.767	0.941	0.894	0.731
<b>Panel C: <i>Post</i> program impacts (by treatment arms)</b> (12 to 15 months after program ends)					
Public Works Treatment (ITT)	0.025 (0.05)	0.034 (0.05)	0.0066 (0.06)	-0.051 (0.04)	-0.013 (0.05)
Self-Empl. training (SET)	-0.039 (0.04)	-0.062* (0.04)	-0.054 (0.06)	0.054 (0.04)	0.012 (0.05)
Wage-Empl. training (WET)	-0.077 (0.05)	-0.0041 (0.05)	0.024 (0.07)	0.12*** (0.04)	0.072 (0.05)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes
Mean in Control	0.01	-0.01	-0.01	-0.01	-0.03
p-value: PW+SET=0	0.762	0.530	0.317	0.939	0.978
p-value: PW+WET=0	0.293	0.574	0.546	0.147	0.222
p-value: SET=WET	0.410	0.197	0.066	0.165	0.229
Observations	3933	3933	3933	3933	3933
Perm. p-value: PW+SET=0	0.767	0.534	0.322	0.940	0.980
Perm. p-value: PW+WET=0	0.300	0.580	0.549	0.155	0.236
Perm. p-value: SET=WET	0.418	0.212	0.079	0.170	0.240

ITT estimates in panels A and B based on specification in equation 1.6. Estimates in panel C based on specification in equation 1.7. The definition of the *behavior index* and variables entering the index is detailed in Appendix G. *Conduct problems*, *impulsiveness* and *anger in daily activities* enter as inverted measures in the index (a negative impact in columns (2), (4) or (5) is associated with a positive impact on the index in column (1)). Robust standard errors clustered at (broad) brigade level in parentheses. Permutation tests use 10000 permutations for each hypothesis. \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Table A14 – Estimated impacts *during* and *post* program on risky behaviors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Stealing	Assaulting someone	Believing smuggling is necessary (to earn a living)	Prostituting	Threatening someone	Taking illicit drugs	Smuggling stolen objects	Ties with a smuggling network	Keeping fire arms at home
<b>Panel A: Impacts <i>during</i> the program</b> (around 4.5 months after program starts)									
Public Works Treatment (ITT)	0.09*	-0.11**	0.00	0.01	-0.02	-0.11***	-0.05	0.01	0.01
	(0.05)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Mean in Control	0.05	0.18***	0.10***	0.07*	0.11***	0.03	0.05*	0.02	0.01
	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value: no design effect in list A	1.000	1.000	0.446	0.008	0.156	0.015	0.314	0.485	0.152
p-value: no design effect in list B	1.000	1.000	0.655	0.373	1.000	0.425	0.645	0.107	0.856
Impact in list A	0.09	-0.19	-0.07	0.03	-0.11	-0.08	-0.14	0.04	-0.01
Impact in list B	0.08	-0.03	0.08	-0.02	0.07	-0.15	0.04	-0.01	0.03
p-value: Impact A= Impact B	0.930	0.097	0.126	0.694	0.070	0.501	0.046	0.587	0.677
Observations	2956	2956	2956	2956	2953	2955	2954	2954	2955
<b>Panel B: <i>Post</i> program impacts (pooled treatment)</b> (12 to 15 months after program ends)									
Public Works Treatment (ITT)	0.03	-0.01	0.01	-0.01	-0.01	0.01	-0.09***	0.00	0.00
	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Mean in Control	0.04	0.09***	0.08***	0.10***	0.08***	0.03	0.09***	0.06**	0.05**
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value: no design effect in list A	0.083	–	0.317	–	1.000	0.157	0.063	0.510	0.981
p-value: no design effect in list B	–	1.000	1.000	0.083	0.214	0.171	0.612	0.425	0.611
Impact in list A	0.01	0.04	0.02	-0.02	-0.01	-0.04	-0.09	0.07	0.05
Impact in list B	0.05	-0.07	-0.00	-0.00	-0.01	0.06	-0.08	-0.07	-0.04
p-value: Impact A=Impact B	0.592	0.108	0.752	0.743	0.945	0.144	0.915	0.058	0.190
Observations	3933	3933	3933	3933	3933	3932	3933	3933	3933
<b>Panel C: <i>Post</i> program impacts (by treatment arms)</b> (12 to 15 months after program ends)									
Public Works Treatment (ITT)	0.01	0.03	0.03	-0.05	0.00	0.02	-0.06	0.01	0.01
	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Self-Empl. training (SET)	0.03	-0.08	-0.04	0.10***	0.02	-0.05	-0.07*	0.01	-0.02
	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.04)
Wage-Empl. training (WET)	0.01	-0.06*	-0.02	0.01	-0.06	0.01	-0.01	-0.03	0.01
	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Mean in Control	0.04	0.09***	0.08***	0.10***	0.08***	0.03	0.09***	0.06**	0.05**
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value: PW+SET=0	0.275	0.344	0.662	0.197	0.534	0.498	0.001	0.708	0.681
p-value: PW+WET=0	0.539	0.419	0.820	0.316	0.194	0.450	0.072	0.571	0.668
p-value: SET=WET	0.667	0.689	0.613	0.008	0.077	0.188	0.185	0.416	0.388
Observations	3933	3933	3933	3933	3933	3932	3933	3933	3933

Variables measured using a double list experiment (Droitcour et al., 1991), whereby each respondent was assigned to a list A with sensitive items and a list B without sensitive items, or vice versa (see Appendix G). Difference-in-means estimation (Miller, 1984) was used to estimate the mean in control and treatment effects. In Panels A and B, the specification  $Y_i = \alpha + \gamma_1 L_i + \gamma_2 W_i + \gamma_3 (L_i \times W_i) + \delta_i X_{i,l} + \varepsilon_i$  was used; where  $\gamma_1$  is the mean in the control group,  $\gamma_3$  is the treatment effect, and  $X_{i,l}$  is a vector of stratification variables. Similarly, in Panel C we used  $Y_i = \alpha + \gamma_1 L_i + \gamma_2 W_i + \gamma_3 (W_i \times T1_i) + \gamma_4 (W_i \times T2_i) + \gamma_5 (L_i \times W_i) + \gamma_6 (L_i \times W_i \times T1_i) + \gamma_7 (L_i \times W_i \times T2_i) + \delta_i X_{i,l} + \varepsilon_i$ ; where  $\gamma_1$  is the mean in the control group,  $\gamma_5$  the effect of "pure" public works, and  $\gamma_6$  ( $\gamma_7$ ) the additional effect of self-employment training (wage employment training). The test for the presence of design effects is based on the likelihood ratio test (Blair and Imai, 2012). The null hypothesis is no design effect. We report Bonferroni adjusted p-values. Weights are used for estimation but not for the design effect test (because it is not supported). The dash symbol indicates that the test statistics could not be processed due to a lack of variance in estimated probabilities:  $P(C = 4, S = 1) = P(C = 4, S = 0) = 0$ . However, none of the point estimates of joint probabilities were negative in such cases. \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Table A15 – Estimated impacts *post* program on skills, investments in self-employed activities and search for wage jobs

	Investment in self-employed activities					Search for wage jobs				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Self-Empl. Quiz (z-score)	Prepared a business plan	Total number of income generating activities	Value of productive assets in CFA (stock)	Value of investments in CFA (last 3 months)	Start-up capital in CFA (main self-empl. act., stock)	Wage-Empl. Quiz (z-score)	Used a CV for job search	Total spent in job search in CFA (last 12 months)	Searched for a job (last 3 months)
<b>Panel A: <i>Post</i> program impacts (pooled treatment) (12 to 15 months after program ends)</b>										
Public Works Treatment (ITT)	0.12*** (0.04)	0.024** (0.01)	0.048* (0.03)	4159.1*** (1529.97)	347.4 (292.63)	8712.6*** (2071.94)	0.12*** (0.04)	0.047*** (0.01)	-468.4 (823.02)	0.026 (0.02)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	-0.07	0.05	0.48	12570.82	1910.81	22198.36	-0.09	0.15	9026.08	0.52
Observations	3934	3934	3934	3934	3934	3934	3934	3934	3934	3934
Perm. p-value: no effects	0.005	0.021	0.076	0.011	0.248	0.000	0.004	0.002	0.582	0.225
<b>Panel B: <i>Post</i> program impacts (by treatment arms) (12 to 15 months after program ends)</b>										
Public Works Treatment (ITT)	0.073 (0.06)	0.013 (0.01)	0.040 (0.03)	4878.0** (1956.43)	580.0 (404.36)	8282.5*** (2620.46)	0.019 (0.05)	0.016 (0.02)	-411.6 (915.44)	0.023 (0.03)
Self-Empl. training (SET)	0.11* (0.06)	0.041*** (0.01)	0.0058 (0.04)	-661.3 (2282.23)	-428.1 (383.22)	2534.1 (3330.95)	0.048 (0.05)	-0.0064 (0.02)	-324.3 (863.67)	-0.0023 (0.03)
Wage-Empl. training (WET)	0.033 (0.05)	-0.0058 (0.01)	0.021 (0.04)	-1576.7 (2040.88)	-296.7 (450.91)	-1187.7 (3306.09)	0.26*** (0.05)	0.10*** (0.02)	146.5 (942.35)	0.011 (0.03)
Strata f.e. control	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	-0.07	0.05	0.48	12570.82	1910.81	22198.36	-0.09	0.15	9026.08	0.52
p-value PW+SET=0	0.001	0.000	0.217	0.046	0.625	0.000	0.162	0.546	0.449	0.458
p-value PW+WET=0	0.026	0.568	0.076	0.064	0.494	0.018	0.000	0.000	0.797	0.300
p-value SET=WET	0.107	0.001	0.695	0.648	0.764	0.236	0.000	0.000	0.591	0.746
Observations	3934	3934	3934	3934	3934	3934	3934	3934	3934	3934
Perm. p-value PW+SET=0	0.001	0.000	0.223	0.051	0.631	0.000	0.172	0.561	0.464	0.467
Perm. p-value PW+WET=0	0.029	0.575	0.081	0.067	0.502	0.018	0.000	0.000	0.804	0.304
Perm. p-value SET=WET	0.113	0.000	0.699	0.660	0.770	0.246	0.000	0.000	0.599	0.747

ITT estimates in panels A and B based on specification in equation 1.6. Estimates in panel C based on specification in equation 1.7. *Value of productive assets, value of investments, start-up capital and total spend in job search* winsorized at the 97th percentile. Robust standard errors clustered at (broad) brigade level in parentheses. Permutation tests use 10000 permutations for each hypothesis. \*  $p < .1$ , \*\*  $p < .05$ , \*\*\*  $p < .01$ .

Table A16 – Comparison of Machine Learning algorithms to predict impacts on earnings *during* and *post* program

	Estimates in logs					Estimates in levels				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Elastic net	Generalized Random forest	Gradient boosting	R-Learner	Random forest	Elastic net	Generalized Random forest	Gradient boosting	R-Learner	Random forest
<b>Panel A: Predicted average treatment effect and heterogeneity loading parameter <i>during</i> program</b>										
ATE ( $\beta_1$ )	2.631 (2.9,2.3) [0.000]	2.642 (2.9,2.3) [0.000]	2.643 (2.9,2.3) [0.000]	2.635 (2.9,2.3) [0.000]	2.634 (2.9,2.3) [0.000]	24577.6 (31323.2,17831.5) [0.000]	24351.6 (31214.6,17669.0) [0.000]	24238.5 (30976.0,17471.1) [0.000]	24549.6 (31423.5,17789.9) [0.000]	24363.0 (31150.3,17618.5) [0.000]
HET ( $\beta_2$ )	0.980 (1.3,0.6) [0.000]	1.231 (1.6,0.9) [0.000]	0.420 (0.6,0.2) [0.000]	0.920 (1.2,0.6) [0.000]	0.849 (1.1,0.6) [0.000]	0.405 (0.9,-0.07) [0.157]	1.070 (2.1,-0.05) [0.121]	0.288 (0.7,-0.03) [0.160]	0.429 (1.3,-0.2) [0.353]	0.390 (0.8,0.009) [0.089]
$\Lambda$	0.9	1.0	0.7	0.9	0.9	6007.2	6401.7	5988.1	4621.1	6785.3
<b>Panel B: Predicted average treatment effect and heterogeneity loading parameter <i>post</i> program</b>										
ATE ( $\beta_1$ )	-0.0551 (0.4,-0.5) [1.000]	-0.0508 (0.4,-0.5) [1.000]	-0.0484 (0.4,-0.5) [1.000]	-0.0570 (0.4,-0.5) [1.000]	-0.0297 (0.4,-0.5) [1.000]	3217.5 (8428.4,-1918.3) [0.439]	3527.3 (8737.3,-1706.6) [0.374]	3546.3 (8685.8,-1630.9) [0.358]	3314.0 (8523.0,-1842.7) [0.414]	3474.5 (8642.7,-1641.7) [0.370]
HET ( $\beta_2$ )	-0.0581 (0.6,-0.7) [1.000]	0.156 (1.6,-1.3) [1.000]	0.0376 (0.3,-0.2) [1.000]	-0.00219 (3.1,-2.8) [1.000]	0.111 (0.7,-0.5) [0.970]	0.0837 (0.6,-0.5) [1.000]	0.391 (1.8,-1.0) [1.000]	0.0220 (0.4,-0.3) [1.000]	0.416 (5.4,-1.1) [0.756]	0.0800 (0.5,-0.4) [1.000]
$\Lambda$	0.10	0.1	0.1	0.1	0.2	1625.5	1742.1	1679.7	2182.6	1746.7

Heterogeneity analysis based on the approach in Chernozhukov et al. (2020) (see discussion in Section 1.4.2 and Appendix H.2). Estimates are based on equation 1.9. P-value (in brackets) for  $\beta_1$  tests the hypothesis of no effect. P-value (in brackets) for  $\beta_2$  tests the hypothesis of no heterogeneity. Panel A (respectively Panel B) shows estimates of  $\beta_1$  and  $\beta_2$  at midline (respectively endline). The  $\Lambda$  (lambda) statistic is displayed at the bottom of each panel: the larger  $\Lambda$  gets, the stronger the correlation between  $\widehat{s}_0(Z)$  noted  $S(Z)$  and  $s_0(Z)$ . Predictions are estimated for each sample split, reported values are the medians across 100 sample splits. Adjusted confidence intervals at 90% in parentheses and adjusted p-values for partition uncertainty. Earnings variable is in CFA, winsorized at the 97th percentile. For variables ( $y$ ) in logarithms we take  $\ln(y + 1)$ .

Table A17 – Heterogeneity in impacts on earnings *during* and *post* program, machine learning results for an extended set of covariates

	(1) Ln total earnings (Monthly)	(2) Ln total earnings (Monthly)	(3) Total earnings (Monthly)	(4) Total earnings (Monthly)
	<i>Midline</i>	<i>Endline</i>	<i>Midline</i>	<i>Endline</i>
<b>Panel A: Predicted average treatment effect and heterogeneity loading parameter</b>				
ATE ( $\beta_1$ )	2.311 (1.918,2.714) [0.000]	-0.136 (-0.723,0.449) [0.928]	27252.0 (17338.2,37013.8) [0.000]	4011.4 (-4014.3,11850.4) [0.631]
HET ( $\beta_2$ )	2.336 (1.156,3.577) [0.000]	0.272 (-0.306,0.814) [0.637]	2.297 (0.387,4.180) [0.032]	0.136 (-1.855,2.883) [0.888]
Best ML method	Generalized Random forest	Random forest	Generalized Random forest	R-Learner
<b>Panel B: By quartile of predicted impacts on earnings <i>during</i> program (using <i>midline</i>) (GATES)</b>				
Mean in quartile 1 (0 to 25%)	1.430 (0.596,2.233)	-0.695 (-2.047,0.682)	9616.7 (-9485.5,28647.1)	1309.1 (-17605.5,20036.9)
Mean in quartile 2 (25 to 50%)	2.063 (1.260,2.854)	-0.589 (-1.953,0.810)	31539.2 (12393.6,51035.5)	3483.3 (-15126.9,22326.7)
Mean in quartile 3 (50 to 75%)	2.602 (1.763,3.397)	-0.126 (-1.515,1.306)	32238.6 (12793.6,51515.6)	6594.5 (-11856.0,24930.0)
Mean in quartile 4 (75 to 100%)	3.312 (2.521,4.094)	-0.0495 (-1.417,1.326)	33266.5 (13826.0,52380.5)	8086.5 (-11723.2,27267.8)
P-value all coefficients are equal	0.007	1.000	0.292	1.000
Best ML method	Generalized Random forest	Generalized Random forest	Generalized Random forest	Generalized Random forest
<b>Panel C: By quartile of predicted impacts on earnings <i>post</i> program (using <i>endline</i>) (GATES)</b>				
Mean in quartile 1 (0 to 25%)	1.935 (1.102,2.761)	-0.338 (-1.512,0.819)	21497.8 (3457.5,39527.0)	2309.3 (-12952.7,18286.3)
Mean in quartile 2 (25 to 50%)	1.989 (1.164,2.868)	-0.359 (-1.520,0.828)	27897.3 (10231.8,45734.2)	5194.6 (-10461.8,20933.5)
Mean in quartile 3 (50 to 75%)	2.246 (1.456,3.038)	-0.432 (-1.562,0.736)	28716.5 (10596.6,46321.6)	3064.9 (-12565.0,18074.0)
Mean in quartile 4 (75 to 100%)	2.555 (1.768,3.370)	0.368 (-0.800,1.488)	29256.8 (11482.7,46980.5)	5016.5 (-10414.7,21248.5)
P-value all coefficients are equal	0.093	0.682	0.685	0.998
Best ML method	Random forest	Random forest	R-Learner	R-Learner

Heterogeneity analysis based on the approach in Chernozhukov et al. (2020) (see discussion in Section 1.4.2 and Appendix H.2). All baseline variables in the balance check table (Table 1.1) are used as covariates. Columns (1) and (3) (respectively columns (2) and (4)) focus on outcomes at midline (respectively endline). Estimates in Panel A are based on equation 1.9. P-value (in brackets) for  $\beta_1$  tests the hypothesis of no effect. P-value (in brackets) for  $\beta_2$  tests the hypothesis of no heterogeneity. GATES estimates are based on the specification in equation 1.10. Panel B (respectively Panel C) shows impacts per quartile of the predicted treatment effects at midline (respectively endline). The best predictions are reported. The chosen algorithm is the one with the highest  $\Lambda$  (lambda): the larger  $\Lambda$  gets, the stronger the correlation between  $\hat{s}_0(Z)$  noted  $S(Z)$  and  $s_0(Z)$  (see appendix table A16 for comparisons across algorithms). Predictions are estimated for each sample split, reported values are the medians across 100 sample splits. Adjusted confidence intervals at 90% in parentheses and adjusted p-values for partition uncertainty. Earnings variable is in CFA, winsorized at the 97th percentile. For variables ( $y$ ) in logarithms we take  $\ln(y+1)$ .

Table A18 – Estimated impacts on (ln) earnings *post* program, by treatment arms

	(1) Public Works only (PW)	(2) PW and Self-Employment Training (SET)	(3) PW and Wage-Employment Training (WET)
	<i>Endline</i>	<i>Endline</i>	<i>Endline</i>
<b>Panel A: Predicted average treatment effect and heterogeneity loading parameter</b>			
ATE ( $\beta_1$ )	-0.0753 (-0.601,0.447) [1.000]	-0.0116 (-0.545,0.529) [1.000]	0.120 (-0.406,0.648) [0.930]
HET ( $\beta_2$ )	0.0866 (-0.211,0.382) [0.818]	-0.572 (-1.431,0.299) [0.425]	0.450 (-0.595,1.935) [0.722]
Best ML method	Gradient boosting	Elastic net	R-Learner
<b>Panel B: By quartile of predicted impacts on earnings <i>post</i> program (<i>using endline</i>) (GATES)</b>			
Mean in quartile 1 (0 to 25%)	-0.220 (-1.270,0.826)	0.408 (-0.663,1.487)	-0.0540 (-1.112,1.004)
Mean in quartile 2 (25 to 50%)	-0.141 (-1.188,0.923)	0.132 (-0.958,1.206)	0.138 (-0.913,1.187)
Mean in quartile 3 (50 to 75%)	-0.172 (-1.217,0.873)	-0.116 (-1.193,0.962)	0.238 (-0.805,1.279)
Mean in quartile 4 (75 to 100%)	0.148 (-0.909,1.214)	-0.522 (-1.587,0.543)	0.168 (-0.912,1.223)
P-value all coefficients are equal	0.902	0.478	0.950
Best ML method	Gradient boosting	Elastic net	R-Learner

Heterogeneity analysis based on the approach in Chernozhukov et al. (2020) (see discussion in Section 1.4.2 and Appendix H.2). Columns (1-3) show estimated impacts for each treatment arm compared to the control group. Estimates in Panel A are based on equation 1.9. P-value (in brackets) for  $\beta_1$  tests the hypothesis of no effect. P-value (in brackets) for  $\beta_2$  tests the hypothesis of no heterogeneity. Estimates in panel B are based on the specification in equation 1.10. They show impacts per quartile of the predicted treatment effects at endline. The best predictions are reported. The chosen algorithm is the one with the highest  $\Lambda$  (lambda): the larger  $\Lambda$  gets, the stronger the correlation between  $\hat{s}_0(Z)$  noted  $S(Z)$  and  $s_0(Z)$ . Predictions are estimated for each sample split, reported values are the medians across 100 sample splits. Adjusted confidence intervals at 90% in parentheses and adjusted p-values for partition uncertainty. Earnings variable is in CFA, winsorized at the 97th percentile. For variables ( $y$ ) in logarithms we take  $\ln(y + 1)$ .

Table A19 – Estimated impacts on earnings (in levels) *post* program, by treatment arms

	(1)	(2)	(3)
	Public Works only (PW)	PW and Self-Employment Training (SET)	PW and Wage-Employment Training (WET)
	<i>Endline</i>	<i>Endline</i>	<i>Endline</i>
<b>Panel A: Predicted average treatment effect and heterogeneity loading parameter</b>			
ATE ( $\beta_1$ )	2155.1 (-3648.0,8097.0) [0.799]	4166.5 (-2085.9,10463.6) [0.397]	3970.3 (-2233.2,10007.5) [0.412]
HET ( $\beta_2$ )	0.711 (-0.793,2.217) [0.645]	0.167 (-2.294,3.175) [0.822]	0.859 (-0.383,2.666) [0.359]
Best ML method	Generalized Random forest	R-Learner	R-Learner
<b>Panel B: By quartile of predicted impacts on earnings <i>post</i> program (<i>using endline</i>) (GATES)</b>			
Mean in quartile 1 (0 to 25%)	-804.5 (-12141.7,10585.9)	4252.3 (-8217.4,16697.1)	954.7 (-11358.2,13202.6)
Mean in quartile 2 (25 to 50%)	314.7 (-11327.4,12027.1)	5075.0 (-7721.7,17162.5)	3213.2 (-8929.3,15509.9)
Mean in quartile 3 (50 to 75%)	2580.6 (-9034.2,14329.5)	4686.6 (-8013.1,17365.6)	3843.2 (-8184.3,15983.8)
Mean in quartile 4 (75 to 100%)	6306.2 (-5642.9,17819.6)	3651.1 (-8919.7,15948.0)	5448.1 (-6760.2,17345.3)
P-value all coefficients are equal	0.715	0.772	0.828
Best ML method	Generalized Random forest	R-Learner	R-Learner

Heterogeneity analysis based on the approach in Chernozhukov et al. (2020) (see discussion in Section 1.4.2 and Appendix H.2). Columns (1-3) show estimated impacts for each treatment arm compared to the control group. Estimates in Panel A are based on equation 1.9. P-value (in brackets) for  $\beta_1$  tests the hypothesis of no effect. P-value (in brackets) for  $\beta_2$  tests the hypothesis of no heterogeneity. Estimates in panel B are based on the specification in equation 1.10. They show impacts per quartile of the predicted treatment effects at endline. The best predictions are reported. The chosen algorithm is the one with the highest  $\Lambda$  (lambda): the larger  $\Lambda$  gets, the stronger the correlation between  $\hat{s}_0(Z)$  noted  $S(Z)$  and  $s_0(Z)$ . Predictions are estimated for each sample split, reported values are the medians across 100 sample splits. Adjusted confidence intervals at 90% in parentheses and adjusted p-values for partition uncertainty. Earnings variable is in CFA, winsorized at the 97th percentile.

Table A20 – Baseline characteristics of the bottom and top quartiles of predicted impacts on earnings (in levels) *during* program

	(1) Mean in 1st quartile	(2) Mean in 4th quartile	(3) Test (1)-(2) (p-value)
<b>Individual characteristics</b>			
Female	0.18	0.43	0
Live in urban area	0.91	0.97	0
Age	24.91	24.41	0.012
Nb of children	0.84	0.80	0.065
<b>Education</b>			
Years of education	10.16	10.16	0.065
Primary education not completed	0.45	0.46	0.095
Has participated in vocational training	0.53	0.27	0
<b>Household characteristics and assets</b>			
Household size	6.75	5.52	0
Is head of household	0.28	0.20	0.005
Total Nb of assets	0.59	0.51	0
Nb of transportation assets	1.22	0.42	0
Nb of agricultural assets	7.74	2.42	0
Nb of household durables	2.31	1.26	0
Nb of communication assets	8.42	5.70	0
<b>Employment</b>			
Has an activity	0.91	0.66	0
Is Wage-Employed	0.42	0.28	0
Is Self-Employed	0.52	0.22	0
Nb of activities	1.23	0.72	0
Total Earnings (monthly, CFA)	34385.3	8298.2	0
Searched for a job (last month)	0.62	0.52	0.519
<b>Savings, constraints and expenditures</b>			
Has saved (last 3 months)	0.52	0.45	0.033
Of which: share of formal savings (cond. on savings)	0.28	0.24	0.467
Has a savings account	0.15	0.10	0.006
Savings Stock (CFA)	51724.9	14685.6	0
Self-reported constraints to repay loans	0.29	0.11	0
Self-reported constraints to access credit	0.41	0.59	0
Self-reported constraints for basic needs expenditures	0.69	0.72	0.510
Transportation expenditures (last 7 days, CFA)	3066.3	1065.9	0
Communication expenditures (last 7 days, CFA)	2810.9	895.8	0
<b>Cognitive skills and risk preference</b>			
Cognitive (deduction, Raven Test)	0.23	0.23	0.798
Dexterity (Nuts Test)	0.37	0.38	0.104
Dexterity (Bolts Test)	0.34	0.33	0.900
Positive affect (CES-D scale, Nb positive days)	6.31	6.22	0.856
Positive attitude towards the future (ZTPI scale)	29.32	29.20	0.772
Is Risk averse (based on hypothetical lotteries)	0.71	0.72	1
Patience (scale 0 to 10, 10=very patient)	3.30	3.34	0.945
Preference for present (actualization rate for 1 month)	0.57	0.58	0.917

Heterogeneity analysis based on the approach in [Chernozhukov et al. \(2020\)](#) (see discussion in Section 1.4.2 and Appendix H.2). Column (1) (respectively (2)) displays average characteristics of the bottom (respectively top) quartile of the distribution of predicted impacts on earnings during the program. Column (3) reports p-values for a test of equality between the top and bottom quartile. Reported results are based on the algorithm with best predictions for midline : Generalized Random forest. The chosen algorithm is the one with the highest  $\Lambda$  (lambda): the larger  $\Lambda$  gets, the stronger the correlation between  $\widehat{s}_0(Z)$  noted  $S(Z)$  and  $s_0(Z)$  (see appendix table A16 for comparisons across algorithms). Means by quartile are estimated for each sample split, reported values are the medians across 100 sample splits. Adjusted p-values for partition uncertainty. *Household assets* and *savings stock* variables winsorized at the 99th percentile.

Table A21 – Estimated impacts *during* and *post* program on savings

	(1)	(2)	(3)	(4)
	Ln Savings (Monthly)	Ln Savings (Monthly)	Savings in CFA (Monthly)	Savings in CFA (Monthly)
	<i>Midline</i>	<i>Endline</i>	<i>Midline</i>	<i>Endline</i>
<b>Panel A: Predicted average treatment effect and heterogeneity loading parameter</b>				
ATE ( $\beta_1$ )	3.543 (2.981,4.100) [0.000]	0.472 (-0.0260,0.977) [0.127]	36768.4 (30901.6,42641.1) [0.000]	9492.9 (1192.6,17761.4) [0.050]
HET ( $\beta_2$ )	1.303 (0.454,2.169) [0.005]	0.247 (-1.230,2.565) [0.809]	0.440 (-0.0100,0.889) [0.110]	0.193 (-0.101,0.505) [0.388]
Best ML method	Generalized Random forest	R-Learner	Random forest	Gradient boosting
<b>Panel B: By quartile of predicted impacts on savings <i>during</i> program (GATES)</b>				
Mean in quartile 1 (0 to 25%)	2.481 (1.355,3.611)	0.748 (-0.440,1.920)	29852.0 (18249.3,41593.9)	10275.5 (-9226.9,29636.8)
Mean in quartile 2 (25 to 50%)	3.174 (2.052,4.286)	0.302 (-0.870,1.471)	33873.9 (21920.6,45466.9)	7074.7 (-12459.8,27015.8)
Mean in quartile 3 (50 to 75%)	3.785 (2.671,4.889)	0.563 (-0.619,1.746)	38335.9 (26568.8,49862.4)	10085.7 (-9870.0,29882.8)
Mean in quartile 4 (75 to 100%)	4.778 (3.659,5.896)	0.316 (-0.885,1.497)	45983.1 (34465.3,57655.4)	9897.8 (-9570.1,28961.8)
P-value all coefficients are equal	0.031	1.000	0.269	1.000
Best ML method	Generalized Random forest	Generalized Random forest	Random forest	Random forest
<b>Panel C: By quartile of predicted impacts on savings <i>post</i> program (GATES)</b>				
Mean in quartile 1 (0 to 25%)	3.615 (2.557,4.647)	0.316 (-0.703,1.346)	36489.5 (26070.4,47031.0)	3831.7 (-12411.8,19889.7)
Mean in quartile 2 (25 to 50%)	3.551 (2.488,4.582)	0.336 (-0.705,1.367)	37213.5 (26671.1,47659.7)	7044.9 (-9401.7,23694.9)
Mean in quartile 3 (50 to 75%)	3.574 (2.503,4.652)	0.483 (-0.527,1.506)	37366.7 (26941.2,47912.0)	9436.6 (-7461.4,25802.8)
Mean in quartile 4 (75 to 100%)	3.585 (2.532,4.646)	0.731 (-0.286,1.740)	39460.8 (28965.9,50086.8)	16964.8 (816.3,33311.6)
P-value all coefficients are equal	0.507	0.909	1.000	0.377
Best ML method	R-Learner	R-Learner	Gradient boosting	Gradient boosting

Heterogeneity analysis based on the approach in Chernozhukov et al. (2020) (see discussion in Section 1.4.2 and Appendix H.2). Columns (1) and (3) (respectively columns (2) and (4)) focus on outcomes at midline (respectively endline). Estimates in Panel A are based on equation 1.9. P-value (in brackets) for  $\beta_1$  tests the hypothesis of no effect. P-value (in brackets) for  $\beta_2$  tests the hypothesis of no heterogeneity. GATES estimates are based on the specification in equation 1.10. Panel B (respectively Panel C) shows impacts per quartile of the predicted treatment effects at midline (respectively endline). The best predictions are reported. The chosen algorithm is the one with the highest  $\Lambda$  (lambda): the larger  $\Lambda$  gets, the stronger the correlation between  $\hat{s}_0(Z)$  noted  $S(Z)$  and  $s_0(Z)$  (see appendix table A16 for comparisons across algorithms). Predictions are estimated for each sample split, reported values are the medians across 100 sample splits. Adjusted confidence intervals at 90% in parentheses and adjusted p-values for partition uncertainty. *Savings* variable is in CFA, winsorized at the 97th percentile. For variables ( $y$ ) in logarithms we take  $\ln(y + 1)$ .

Table A22 – Baseline characteristics of the bottom and top quartiles of predicted impacts on (ln) savings *during* program

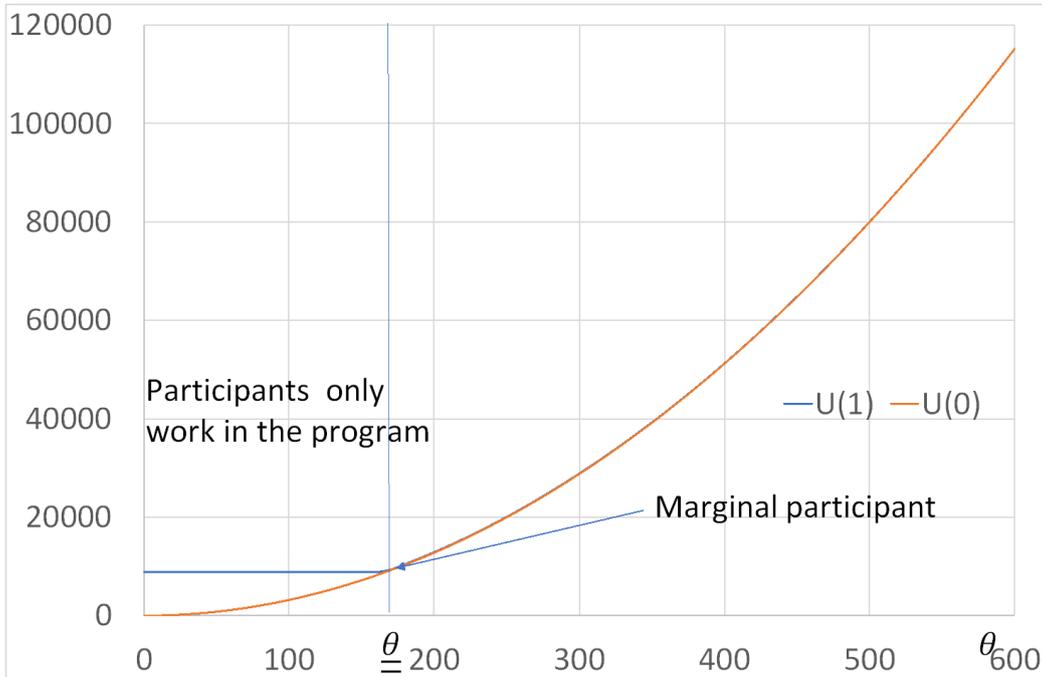
	(1) Mean in 1st quartile	(2) Mean in 4th quartile	(3) Test (1)-(2) (p-value)
<b>Individual characteristics</b>			
Female	0.14	0.46	0
Live in urban area	0.95	0.95	0.598
Age	24.65	24.29	0.080
Nb of children	0.66	0.81	0.070
<b>Education</b>			
Years of education	11.76	9.71	0
Primary education not completed	0.36	0.49	0
Has participated in vocational training	0.47	0.34	0.001
<b>Household characteristics and assets</b>			
Household size	5.35	6.46	0
Is head of household	0.37	0.11	0
Total Nb of assets	0.65	0.44	0
Nb of transportation assets	0.68	0.70	0.331
Nb of agricultural assets	3.88	4.44	0.402
Nb of household durables	1.75	1.70	0.125
Nb of communication assets	6.95	6.72	0.014
<b>Employment</b>			
Has an activity	1	0.45	0
Is Wage-Employed	0.55	0.12	0
Is Self-Employed	0.48	0.14	0
Nb of activities	1.30	0.49	0
Total Earnings (monthly, CFA)	38482.1	658.7	0
Searched for a job (last month)	0.63	0.53	0.756
<b>Savings, constraints and expenditures</b>			
Has saved (last 3 months)	0.74	0.24	0
Of which: share of formal savings (cond. on savings)	0.34	0.18	0.002
Has a savings account	0.19	0.06	0
Savings Stock (CFA)	63881.6	5518.0	0
Self-reported constraints to repay loans	0.22	0.20	0.709
Self-reported constraints to access credit	0.39	0.57	0
Self-reported constraints for basic needs expenditures	0.66	0.72	0.252
Transportation expenditures (last 7 days, CFA)	2754.9	967.5	0
Communication expenditures (last 7 days, CFA)	2760.5	820.6	0
<b>Cognitive skills and risk preference</b>			
Cognitive (deduction, Raven Test)	0.24	0.23	0.781
Dexterity (Nuts Test)	0.37	0.38	0.300
Dexterity (Bolts Test)	0.34	0.33	1
Positive affect (CES-D scale, Nb positive days)	6.42	6.16	0.212
Positive attitude towards the future (ZTPI scale)	29.34	29.10	0.586
Is Risk averse (based on hypothetical lotteries)	0.73	0.71	1
Patience (scale 0 to 10, 10=very patient)	3.39	3.31	1
Preference for present (actualization rate for 1 month)	0.57	0.59	0.507

Heterogeneity analysis based on the approach in [Chernozhukov et al. \(2020\)](#) (see discussion in Section 1.4.2 and Appendix H.2). Column (1) (respectively (2)) displays average characteristics of the bottom (respectively top) quartile of the distribution of predicted impacts on (ln) savings during the program. Column (3) reports p-values for a test of equality between the top and bottom quartile. Reported results are based on the algorithm with best predictions for midline : Generalized Random forest. The chosen algorithm is the one with the highest  $\Lambda$  (lambda): the larger  $\Lambda$  gets, the stronger the correlation between  $\hat{s}_0(Z)$  noted  $S(Z)$  and  $s_0(Z)$ . Means by quartile are estimated for each sample split, reported values are the medians across 100 sample splits. Adjusted p-values for partition uncertainty. *Household assets* and *savings stock* variables winsorized at the 99th percentile.

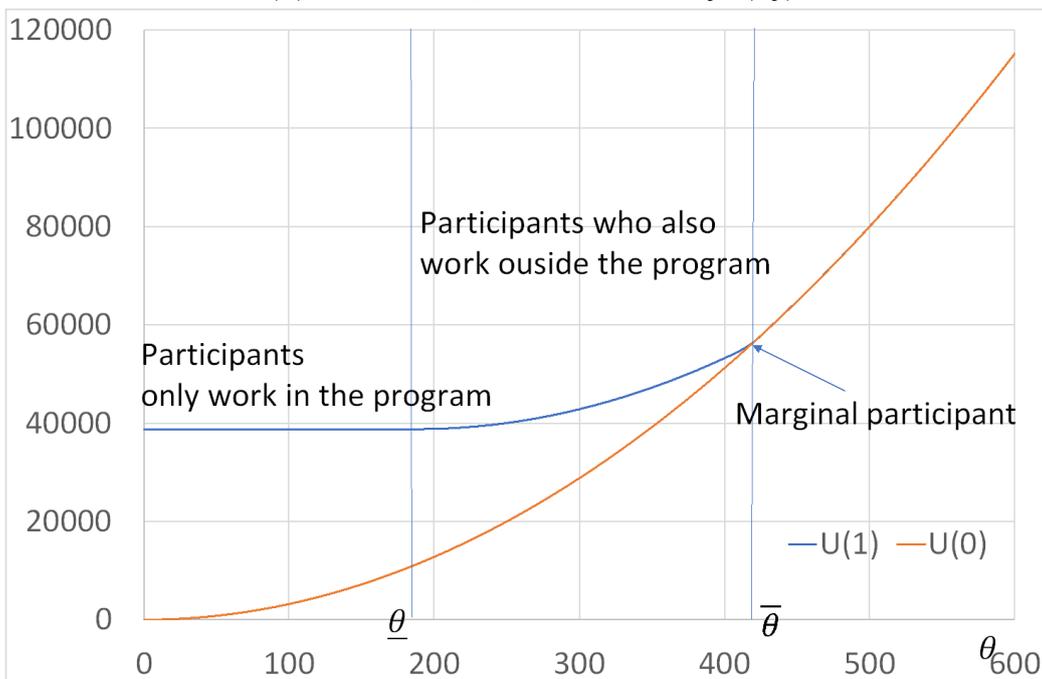
## D Additional Figures

Figure B5 – Participation in the program under different transfer amounts

(a) Case 1: small transfer  $T < h_p c'(h_p)$

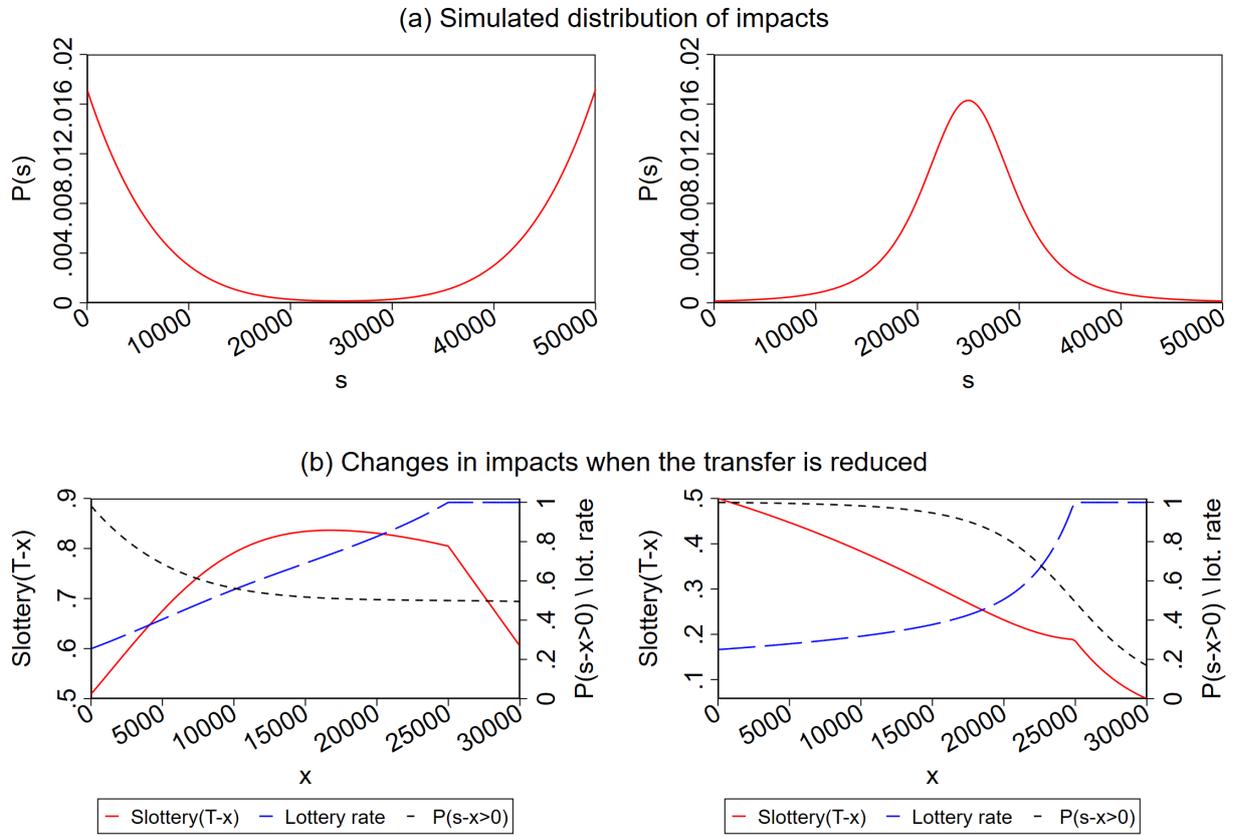


(b) Case 2: large transfer  $T > h_p c'(h_p)$



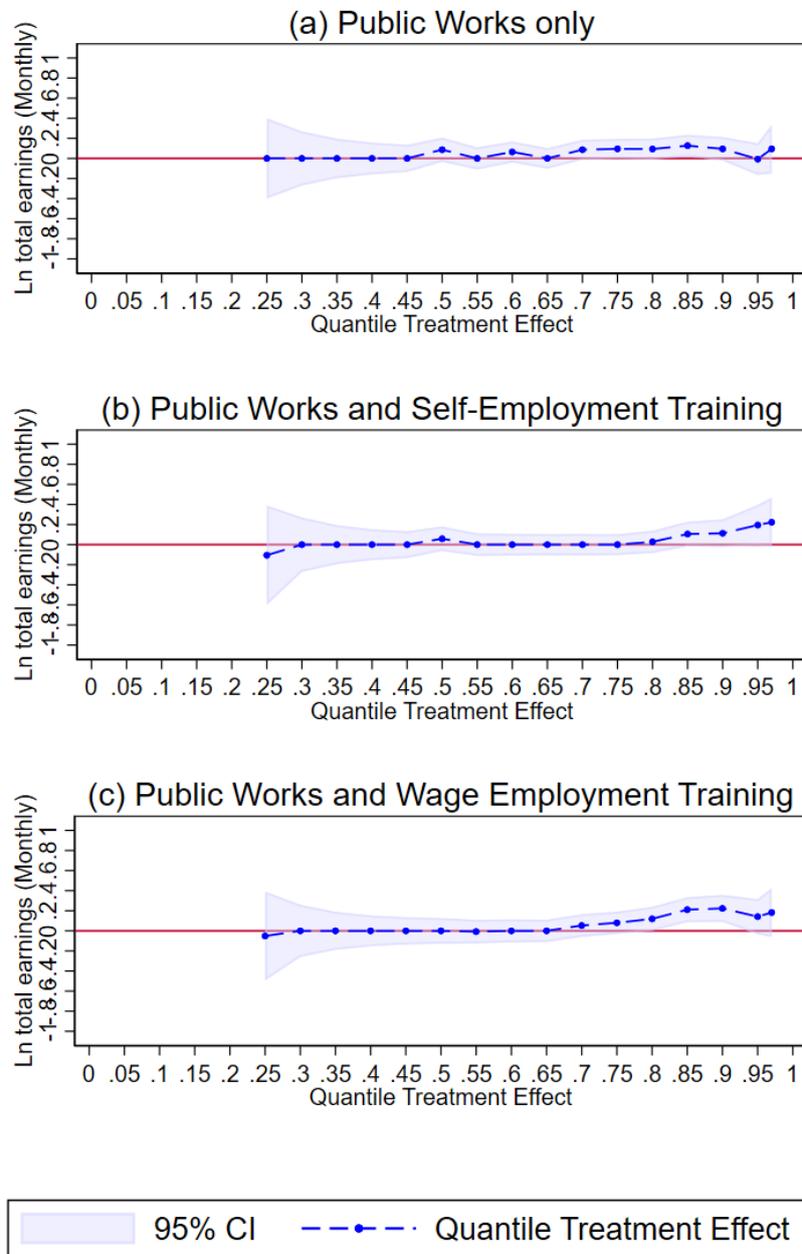
This figure presents two different hypothetical distributions of individual treatment effects. The two panels present the utility level reached by individuals when they are offered, respectively not offered the program. Panel (a) considers the case of a *small* transfer, in such a case, those who decide to participate only work in the program. Panel (b) considers the case of a *large* transfer and shows that the most productive participant also work outside the program. For details, see conceptual framework in section 1.2.

Figure B6 – Distribution of individual treatment effects and impact of a reduction of the transfer amount



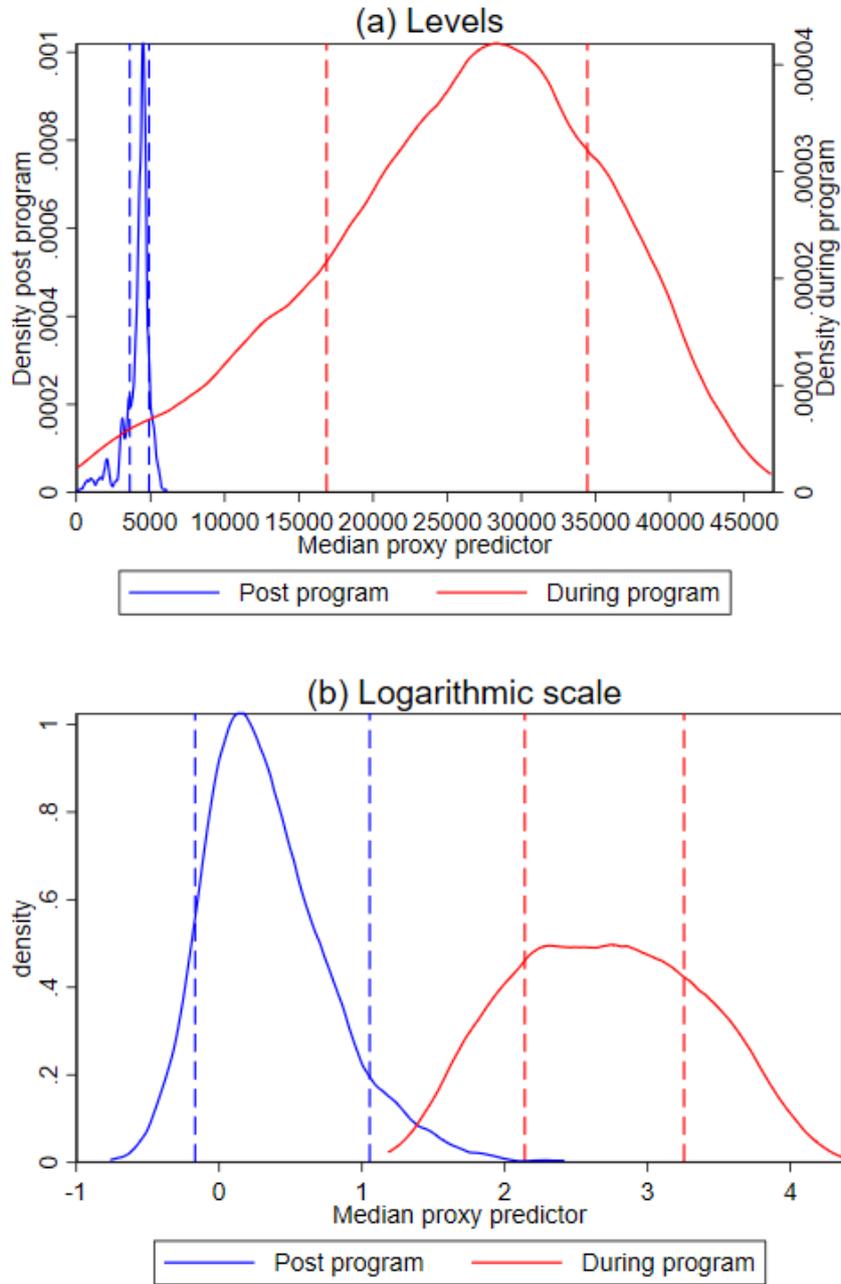
Panel (a) presents two different hypothetical distributions of individual treatment effects. Panel (b) presents simulations using the conceptual framework in section 1.2. It illustrates the changes induced by a reduction in the transfer by  $x$ , assuming the distribution of individual impact on earnings in Panel (a). The dotted black line shows the share of initial applicants who still apply when the transfer is reduced by  $x$ . The dashed blue line shows the lottery success rate when the transfer is reduced by  $x$  (Lottery rate =  $0.25T/((T-x)P(s_T-x > 0))$ ). The solid red line shows the average impact on earnings  $E((s_T-x)1(s_T-x > 0))/(P(s_T-x > 0)(T-x))$  when the transfer amount is reduced by  $x$ ,  $S_{lottery}(T-x)$  from equation 1.5, normalized by  $0.25 \times 50,000$ , the amount available for each initial applicant.

Figure B7 – Quantile treatment effects for (ln) earnings *post* program, by treatment arm



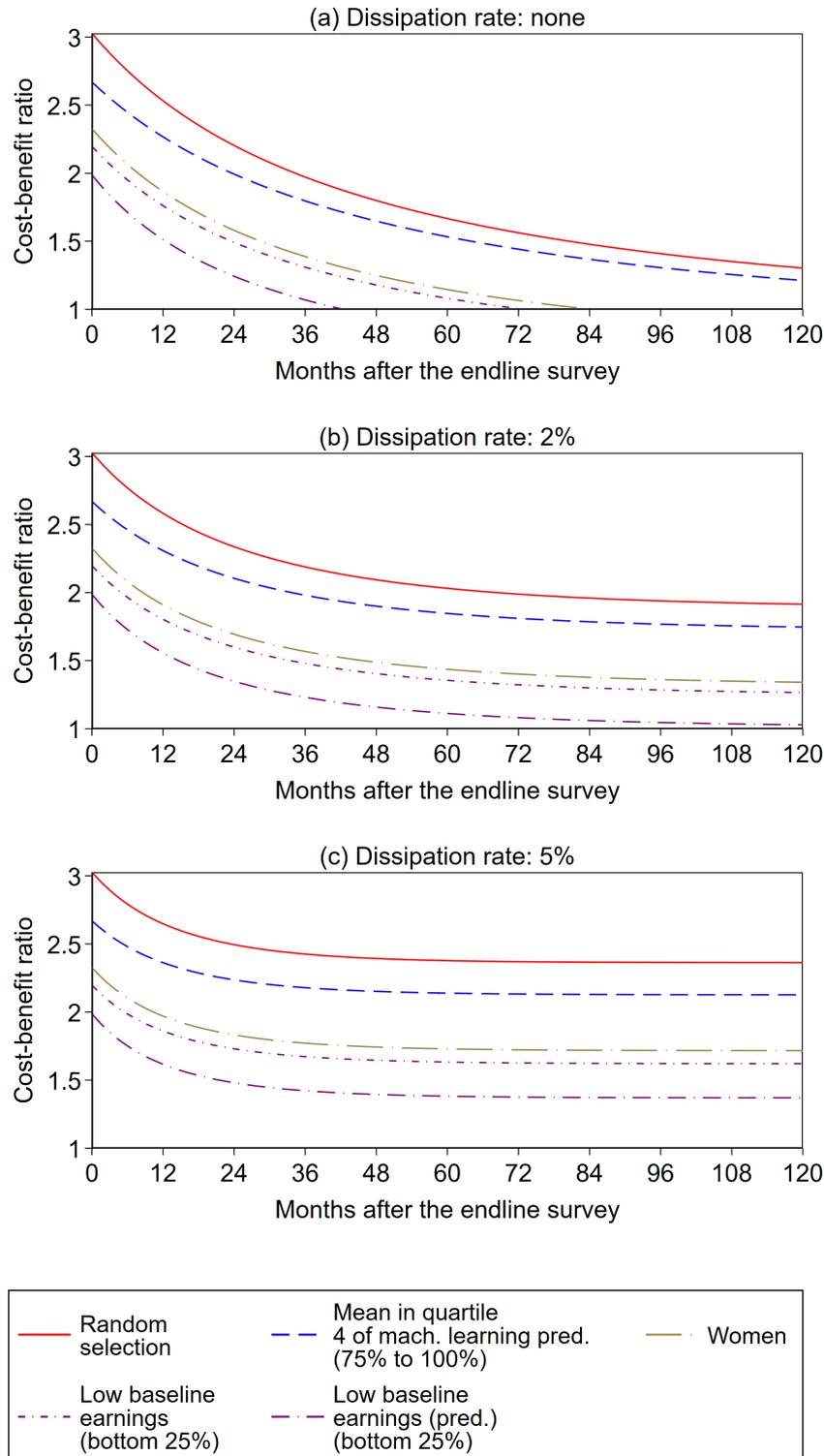
Note: Quantile treatment effects (for positive earnings) up to the 97th percentile. Permutation tests use 10000 permutations for each hypothesis. Wilcoxon rank-sum test permutation p-value is 0.471 for panel (a) 0.464 for panel (b) and 0.459 for panel (c). Kolmogorov-Smirnov permutation p-value is 0.751 for panel (a) 0.776 for panel (b) and 0.750 for panel (c). Total monthly earnings variable is in CFA. For variables ( $y$ ) in logarithms we take  $\ln(y + 1)$ .

Figure B8 – Distribution of predicted impacts on earnings



Note: Heterogeneity analysis based on the approach in Chernozhukov et al. (2020) (see discussion in Section 1.4.2 and Appendix H.2). Estimated distribution of individual treatment effects (across 100 simulations). Dashed lines delimit bottom 25% and top 25% of the distribution. Predictions use Random Forest for impacts during program in levels, and Generalized Random Forest for impacts during program in logarithm. Predictions use R-Learner for post-program impact in levels, and Gradient Boosting for post-program impact in logarithm. Total monthly earnings variable is in CFA, winsorized at the 97th percentile. For variables ( $y$ ) in logarithms we take  $\ln(y + 1)$ .

Figure B9 – Cost-effectiveness ratios over time under alternative targeting rules, depending on the sustainability of post-program impacts



Note: The discounted sum of post-program impacts is assumed to continue beyond what we measure at the endline survey, 15 months after the end of the program. The top panel assumes no dissipation of impacts. The middle and bottom panels assume a dissipation rate on top of the discount rate: a 2% monthly dissipation rate (respectively 5%) is equivalent to a 22% decrease in impact in one year (respectively 49%).

## E Additional details on the framework

### E.1 Individuals impacts

Let  $\theta$  denote potential participants' hourly earnings in non-program activities,  $h$  hours worked, and  $c(h)$  the additive disutility of working  $h$  hours. Absent the workfare program, individuals have an optimal number of hours worked  $h_0(\theta)$  given by  $\theta = c'(h_0)$ , leading to earnings  $W_0(\theta) = \theta h_0(\theta)$  and utility  $U_0(\theta) = \theta h_0(\theta) - c(h_0(\theta))$ . Notice we have  $U'_0(\theta) = h_0(\theta)$ .

Assume that participation in the workfare program provides a transfer  $T$  in exchange of  $h_p$  hours of work. Further assume that the disutility of time spent in program activities is the same as for time spent in non-program activities. Individuals seek to maximize  $T + \theta(h - h_p) - c(h)$ . This leads to the first-order condition  $\theta = c'(h)$ . It corresponds to hours of work  $h_1(\theta) > h_p$  if  $c'(h_1(\theta)) > c'(h_p)$ , i.e.  $\theta > \underline{\theta}(h_p) \equiv c'(h_p)$ . Notice that in this case  $h_1(\theta) = h_0(\theta)$ .

When  $\theta > \underline{\theta}(h_p)$ , the solution of the previous maximization leads to:  $h_1(\theta) = h_0(\theta)$ . The objective reaches the value  $U_1(T, h_p, \theta) = T + \theta(h_0(\theta) - h_p) - c(h_0(\theta)) = U_0(\theta) + T - \theta h_p$ . The corresponding earnings are  $W_1(T, h_p, \theta) = T + \theta(h_0(\theta) - h_p) = W_0(\theta) + T - \theta h_p$ . Individuals will however participate in the program only if  $U_1(\theta) > U_0(\theta)$ , which is equivalent to  $T - \theta h_p > 0$ . Thus individuals participate in the program if  $\theta < \bar{\theta}(T, h_p) \equiv T/h_p$ .

When  $\theta < \underline{\theta}(h_p)$ , the maximization leads to the constrained solution  $h_1(\theta) = h_p$ . The objective reaches the value  $U_1(T, h_p, \theta) = T - c(h_p) = U_0(\theta) + T - \theta h_0(\theta) - c(h_p) + c(h_0(\theta))$ . The corresponding earnings are  $W_1(T, h_p, \theta) = W_0(\theta) + T - \theta h_0(\theta)$ . Individuals will participate in the program if  $T - c(h_p) > \theta h_0(\theta) - c(h_0(\theta)) = U(\theta)$ . Thus individuals participate in the program if  $\theta < \underline{\theta}(T, h_p) \equiv U_0^{-1}(T - c(h_p))$ .

We can define the difference in earnings  $s(T, \theta) = W_1(\theta) - W_0(\theta)$  absent the final decision to participate:

$$s(T, \theta) = T - \theta (h_p 1(\theta > \underline{\theta}(h_p)) + h_0(\theta) 1(\theta < \underline{\theta}(h_p)))$$

We can show that  $\underline{\theta}(h_p) < \bar{\theta}(T, h_p) \Leftrightarrow \underline{\theta}(h_p) < \underline{\theta}(T, h_p) \Leftrightarrow T > h_p c'(h_p)$ .

Notice first that  $\underline{\theta}(h_p) \equiv c'(h_p)$  and  $\bar{\theta}(T, h_p) \equiv T/h_p$ . Hence,  $\underline{\theta}(h_p) < \underline{\theta}(T, h_p)$  is directly equivalent to  $c'(h_p) < T/h_p$ . Notice also that  $c'(h_0(\underline{\theta}(h_p))) = \underline{\theta}(h_p) = c'(h_p)$ , thus  $h_0(\underline{\theta}(h_p)) = h_p$ . Then:

$$\begin{aligned} & \underline{\theta}(h_p) < \bar{\theta}(T, h_p) \\ \Leftrightarrow & \underline{\theta}(h_p) < T/h_p \\ \Leftrightarrow & \underline{\theta}(h_p) h_p - c(h_p) < T - c(h_p) \\ \Leftrightarrow & U(\underline{\theta}(h_p)) < U(\underline{\theta}(T, h_p)) \\ \Leftrightarrow & \underline{\theta}(h_p) < \underline{\theta}(T, h_p) \end{aligned}$$

There are thus two situations (see Appendix Figures B5):

$T > h_p c'(h_p)$ : individuals with  $\theta < \underline{\theta}(h_p)$  participate in the program and they only work in the program. Their hours of work are given by  $h_1(\theta) = h_p > h_0(\theta)$ . Individuals with  $\underline{\theta}(h_p) < \theta < \underline{\theta}(T, h_p)$  participate in the program and also work outside the program. Their hours of work are given by  $h_1(\theta) = h_p = h_0(\theta)$ . Marginal participants are individuals with  $\theta = \bar{\theta}(T, h_p)$ . Because

total hours of work are the same, for marginal applicants  $W_0(\theta) = W_1(T, h_p, \theta)$ .

$T < h_p c'(h_p)$ . Only individuals with  $\theta < \underline{\theta}(T, h_p)$  participate in the program and they only work in the program:  $h_1(\theta) = h_p > h_0(\theta)$ . Marginal participants are individuals with  $\theta = \underline{\theta}(T, h_p)$ . Because  $h_1(\theta) = h_p > h_0(\theta)$ , for marginal applicants  $W_0(\theta) < W_1(T, h_p, \theta)$ .

## E.2 Overall impact and changes in the amount of the transfer $T$

As discussed in Section 1.2.1 the program is oversubscribed and a lottery is used to allocate available slots among applicants. We discuss here some aspects related to the variation of the overall impact when the amount of transfer is changed from  $T$  to  $T - x$ . In Section 1.2.1 we derive the expression of the average contemporaneous impact:

$$S_{lottery}(T - x) = \lambda(T - x)E((s_T - x)1(s_t - x > 0)) = \lambda(T) \frac{T}{T - x} E(s_T - x | s_T > x) \quad (1.11)$$

where  $\lambda(T - x)$  is the rate of success of the lottery and is given by  $B = \lambda(T - x)(T - x)N_A(T)P(s_T > x)$ .

Assume we start from an initial  $T$  larger than  $h_p c'(h_p)$ , and progressively increase  $x$ . Applicants are those for whom  $U_1(T - x, h_p, \theta) > U_0(\theta)$ , and marginal applicants are those with productivity  $\theta_m(x)$  such that  $U_1(T - x, h_p, \theta_m(x)) - U_0(\theta_m(x)) = 0$ . As emphasized before, as long as  $T - x > h_p c'(h_p)$ , we also have  $W_1(T - x, h_p, \theta_m(x)) - W_0(\theta_m(x)) = 0$ : marginal applicants have the same earnings as if they did not participate in the program. This holds as long as  $T - x < h_p c'(h_p)$ . All individuals participating in the program work longer hours than absent the program and thus  $W_1(T - x, h_p, \theta_m(x)) - W_0(\theta_m(x)) = c(h_p) - c(h_0(\theta_m(x))) > 0$ , but marginal applicants are still defined by  $U_1(T - x, h_p, \theta_m(x)) - U_0(\theta_m(x)) = 0$ .

We can also obtain the derivative of this objective with respect to  $x$ . How this function varies with  $x$  depends on the shape of the density function of  $s_T$ , the density of treatment effects under transfer  $T$ . A change  $dx$  of  $x$  leads to a change  $dS_{lottery}(T - x) = \lambda(x)dE((s_T - x)1(s_t - x > 0)) + E((s_T - x)1(s_t - x > 0))d\lambda(x)$ . The first component  $\lambda(x)dE((s_T - x)1(s_t - x > 0))$  simply writes as  $-\lambda(x)P(s_T > x)dx$ : the increase in  $x$  has a direct negative impact because the transfer is smaller, and this applies to all of those who receive the transfer. The second component is driven by a change in the lottery rate. Using the equation defining the lottery rate, we see that  $d\lambda(x) = \lambda(x)(1/(T - x) + f(x)/P(s_T > x))dx$ . The derivative is unambiguously positive. The savings made because some former participants would no longer apply (the term with  $f(x)$ ) and because the transfer per participant is reduced allows to increase the share of applicants that will be served. The derivative is positive, as long as  $\lambda(x) < 1$ . The negative initial impact due to the reduction in the transfer is mitigated by an increase in the lottery rate. The overall change is thus

$$\frac{dS_{lottery}(T - x)}{dx} = \lambda(x) \left( \left( \frac{1}{T - x} + \frac{f(x)}{P(s_T > x)} \right) E((s_T - x)1(s_t - x > 0)) - P(s_T > x) \right) \quad (1.12)$$

## F Description of complementary training

Randomized subsets of beneficiaries received complementary training on basic entrepreneurship or job search skills. Each training lasted approximately 80-100 hours over two two-week periods. Field exercises were undertaken between the training periods, in parallel to the public works jobs (typically in the afternoons). The training was delivered by work brigades, i.e. in groups of 25 youths. Participants did not have to work during the training, but still received their corresponding daily wage.<sup>64</sup> The curricula for the complementary skills training were tailored for low-skill populations that may not be able to read and write, in particular by relying on drawings and visuals.

The basic entrepreneurship training aimed to build skills to help youth set-up and manage a small non-agricultural micro-enterprise. The training lasted 100 hours and focused on providing cross-cutting business skills and practical guidance to develop simple business plans for small-scale activities that can be set-up using savings from the public works program. A first phase (40 hours over two weeks) covered topics related to basic entrepreneurship and business skills. A second phase included field research for youths to collect information, undertake basic market research and outline a business plan. A third phase (40 hours over two weeks) included feedback on youths' basic business plans, and reviewed related topics from the curriculum. The final phase (20 hours) included post-training follow-up.

The training on wage jobs search skills provided information on wage jobs opportunities, skills on jobs search techniques, as well as a more professional environment during the public works programs and skills certification to facilitate signaling upon exit from the program. The training itself lasted 80 hours. The first phase (40 hours over two weeks) discussed how to identify wage jobs opportunities (either locally or through migration), how to search for wage jobs, prepare a CV, apply for a job and participate in a job interview. The second phase included field exercises to collect information on potential opportunities, identify and visit potential employers or professional networks, etc. The third phase (40 hours over two weeks) provided feedback on field exercises, reviewed part of the curriculum and provided additional practical guidance. In addition, supervisors of the brigades who were offered the wage employment training were also trained on how to manage teams and provide feedback to workers, with the objective to mimic the professional experience one would have in a more formal wage job. Youths were periodically rated on a range of skills, and these evaluations were later used to issue a work certificate that signaled between one and five competencies identified as strengths for each participant.<sup>65</sup>

## G Definition of key outcome variables

*Total monthly earnings* are expressed in CFA francs. They are aggregated over up to three activities undertaken by an individual in the 30 days preceding the survey. They include payments received in cash and the monetary equivalent for in-kind payments. The variable is winsorized at 97% (unless stated otherwise). Total monthly earnings are decomposed in total (monthly) earnings from wage employment and self-employment (as well as earnings from other occupations, which are generally small hence not shown separately). When shown in log, the log transformation is applied to earnings plus one.

<sup>64</sup>Some youths were offered the second half of the training after their exit from the public works program. While these youths were not paid during that time, they received a small stipend to cover transportation costs.

<sup>65</sup>The evaluation policy report contains additional details on the trainings (Bertrand et al., 2016).

*Has an Activity* is a dummy taking a value of 1 if the individual has worked at least one hour over the 7 days preceding the survey, consistent with the official employment indicators used in Côte d'Ivoire. We assign a value of 0 for inactive and unemployed individuals. To provide information on the composition of employment, we also analyze having at least one wage job (*Wage employed*), or at least one self-employment activity (*Self-employed*).

*Weekly hours worked* capture the total number of hours worked over the 7 days preceding the survey. It aggregates information from up to three activities undertaken by an individual across all occupations (wage employment, self-employment or other types of activity). The variable is winsorized at 97%. Weekly hours worked are decomposed in (*hours worked in wage employment*) and (*hours worked in self-employment*) (as well as hours worked in other occupations, which are generally small and not displayed separately).

*Savings stock* is the total amount of savings in CFA francs at the time of the survey. It aggregates savings from formal or informal sources. The variable is winsorized at 97%. When shown in log, the log transformation is applied to savings plus one.

*Total expenditures* is expressed in CFA francs and aggregates several types of expenditures, both for the individual and for other household members. It includes basic expenditures (health, clothing, sanitation, and accommodation), communication expenditures (mobile, internet, and medias), investments (education, training, maintenance of assets), transportation expenditures, temptation goods (alcohol, tobacco, gambling, and luxury goods) and social expenditures (celebrations and charity). The variable is winsorized at 97%. When shown in log, the log transformation is applied to expenditures plus one.

The *well-being index* aggregates 6 measures: two measures of *happiness* and *pride* in daily activities from a time-use module,<sup>66</sup> the Rosenberg *self-esteem* scale,<sup>67</sup> the *positive affect sub-scale* from the CESD scale,<sup>68</sup> the sub-scale of (*positive*) *attitude towards the future* and the inverted sub-scale of *present fatalism* from the ZTPI scale.<sup>69</sup> The well-being index is a z-score, with a mean of zero and a standard deviation of one in the control group, so that estimated coefficients can be interpreted in standard deviations. A positive impact on the well-being index is interpreted as an overall improvement in well-being.<sup>70</sup>

The *behavior index* aggregates 4 measures: an inverted measure of *anger* in daily activities taken

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<sup>66</sup>The time use module measured which activities the respondent performed at different times of the last “business day” (at 6am, 10am, 3pm, 7pm and 10pm). Respondents were also asked whether they felt happy, proud or angry while performing those activities. The measure of happiness (respectively pride) is the number of times (out of the 5 times in the last day) respondents reported feeling happy (respectively proud). A z-score of the measure is included in the well-being index.

<sup>67</sup>The Rosenberg self-esteem scale includes 10 items that measure self-esteem or self-worth. We use a validated version of the instrument in French (Vallieres and Vallerand, 1990).

<sup>68</sup>The Center for Epidemiologic Studies Depression (CESD) scale includes an inverted subscale that measures positive feelings (“Positive Affects”). We use a validated version in French (Morin et al., 2011).

<sup>69</sup>The Zimbardo Time Perspective Inventory (ZTPI) captures different dimensions of time perspectives. We use the two subscales of “future” (to have a positive attitude towards future) and “present fatalism” which is very close to the concept of external locus of control, in the sense that one feels *no* control over life events. The inverted “present fatalism” measure is therefore similar to a measure of internal locus of control. We use a validated version of the instrument in French (Apostolidis and Feulaine, 2004).

<sup>70</sup>The index adds up the 6 measures described above, out of which one is inverted (*present fatalism*). Therefore a negative impact on the *present fatalism* measure induces an improvement in the overall well-being index, corresponding to greater well-being.

from the time-use module,<sup>71</sup> an inverted measure of *impulsiveness* from the DERS scale,<sup>72</sup> the *conduct problems sub-scale* (inverted) and the *pro-social behavior sub-scale* from the Strengths and Difficulties Questionnaire (SDQ).<sup>73</sup> As for the well-being index, the behavior index is a z-score with a mean of 0 and standard deviation of 1 in the control group, so that estimated coefficients can be interpreted in standard deviations. An increase in the index corresponds to an overall improvement in behavior and attitude.<sup>74</sup>

The *risky behavior index* is the mean of 9 risky behaviors measured through list experiments. They include stealing, assaulting someone, believing smuggling is necessary (to earn a living), prostitution, threatening someone, taking illicit drugs, smuggling, having ties with a smuggling network, and having firearms at home. Because respondents may not respond truthfully to direct questions about these sensitive behaviors, we used list experiments instead. Rather than asking directly a sensitive question about a risky behavior (e.g. stealing), 5 affirmations are read to respondents, and respondents are asked how many of these affirmations (between 0 and 5) are true for them. To estimate the proportion of individuals for which the sensitive question is true in a sample, the sample is (randomly) assigned to two lists. The first list includes 5 affirmations including the risky behavior, and the second list only includes the other 4 affirmations (without the risky behavior). We implemented a “double” list experiment to avoid losing statistical power: each half of the sample answered both a list with sensitive questions, and a (different) list with control questions corresponding to the other sample. List experiments were piloted extensively to ensure a good understanding by respondents. In the analysis, we use the likelihood ratio test introduced by Blair and Imai (2012) to test for the existence of design effects.

## H Machine Learning Application to Study Heterogeneity in Treatment Effects

To complement Sections 1.4.2 and 1.6.2, this appendix provides additional details on the application of machine learning methods to analyze treatment effect heterogeneity. The application is based on Chernozhukov et al. (2020). Section H.1 describes the sample used to train the models and make predictions. Section H.2 presents the machine learning algorithms and their parameters. Finally, Section H.3 describes how we adapted the procedure in Chernozhukov et al. (2020) to our experimental setting.

We use similar notations as in Chernozhukov et al. (2020). For clarity, the Baseline Conditional Average (BCA) writes:

$$b_0(Z) := \mathbb{E}[Y(0)|Z]$$

<sup>71</sup>This was built as in footnote 66.

<sup>72</sup>The Difficulties in Emotion Regulation Scale (DERS) is used to measure socio-emotional regulation, in particular the difficulties of regulation of emotions in adults. Three of the six questions of the “difficulties to control impulsive behavior” scale were retained, based on a validated French version of the instrument (Côté et al., 2013).

<sup>73</sup>The Strengths and Difficulties Questionnaire (SDQ) measures behavioral difficulties in young people, initially among children and adolescents from 3 to 16 years old (Goodman et al., 1998). The instrument was slightly adapted for an older age group 18 to 30 years old. We use two of five sub-scales from a validated questionnaire in French available at [www.sdqinfo.com](http://www.sdqinfo.com).

<sup>74</sup>The index adds up the 4 measures described above, which are all inverted in the index except *pro-social behavior* measure. A negative impact on inverted measures, for example *conduct problems*, corresponds to a positive behavior and leads to an improvement in the overall behavior index.

And the Conditional Average Treatment Effect (CATE) is defined as :

$$s_0(Z) := \mathbb{E}[Y(1)|Z] - \mathbb{E}[Y(0)|Z]$$

## H.1 Sample for Machine Learning Implementation

Supervised machine learning algorithms require samples for which both covariates (features) and outcomes are observed. In our case, this requires baseline covariates (a set of  $K$  covariates,  $Z^K$ ) and midline or endline outcome of interest ( $Y$ ). As discussed in the text, our study data has two specificities. First, our midline sample is a subsample of the baseline, while the full baseline sample is included at endline. Second, some control individuals entered subsequent waves of the public works program between midline and endline surveys, and are excluded from the endline sample used for analysis. As a result, the algorithms can use three potential samples: a ‘midline’ ( $Z_i^K, Y_i^{During}, W_i$ ) (respectively ‘endline’ ( $Z_i^K, Y_i^{Post}, W_i$ )) sample can be used to build and apply the model to predict ‘during’ (respectively ‘post’) conditional treatment effects, where  $W$  corresponds to the treatment variable. A third (marginally smaller) sample can be used to study how effects vary ‘during’ and ‘post’ program by taking the intersection of non-attriters and non-missing outcomes for both surveys.<sup>75</sup> When applying the algorithm on the endline data, we drop control individuals who applied to a later wave of the public works program (as in the main analysis).<sup>76</sup> The final sample size depends on the number of missing variables for the outcome considered. The total sample we use ranges between 2,884 and 2,958 units for midline and between 3,745 and 3,910 units for endline.

We use a set ( $Z^K$ , with  $K = 21$ ) of features (covariates) measured at baseline (Table A23). They include both individual and household characteristics, as well as main indicators on employment, financial situation and self-reported constraints on basic needs expenditures. We also show the robustness of the main results to the inclusion of all baseline variables in the balance check table (Table 1.1).

## H.2 Machine Learning Algorithms

We consider five alternative machine learning algorithms to estimate the proxy predictors and apply the procedure in Chernozhukov et al. (2020): elastic net, boosted trees, random forest, Rlearner (based on elastic net and proposed by Nie and Wager (2020)) and generalized random forest (proposed by Wager and Athey (2018)). All algorithms are implemented in **R**, and we adapted the codes provided by the authors.<sup>77</sup> These machine learning methods can be divided in two groups based on the way they approach the CATE function (Künzel et al. (2017)).

### 1. Two learners

The first group of machine learning methods includes Elastic Net, Random Forest and

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<sup>75</sup>There is also some attrition between survey rounds, and some missing values in baseline covariates. We exclude from each sample the attriters from follow-up surveys (since outcomes are not observed). Missing values among baseline covariates are replaced by the mean in the same strata. Individuals with missing values for the outcome of interest (among nonattriters) are dropped from the sample.

<sup>76</sup>Recall that 200 individuals were sampled to be added to the control group at endline to compensate for these observations: because these individuals were not part of the baseline survey, the machine learning model cannot be applied to them since predictions rely on observed  $Z^K$ .

<sup>77</sup><https://github.com/demirermert/MLInference>

Boosted Trees. They predict separately  $\mathbb{E}[Y_i(1)]$  in the treatment group and  $\mathbb{E}[Y_i(0)]$  in the control group. In practice, a first model is fitted on the treatment group and a second on the control group, using an auxiliary sample.<sup>78</sup> The two fitted models are then used to predict potential outcomes  $\hat{Y}_i(1)$  and  $\hat{Y}_i(0)$  for each individual in the remaining sample (main sample). In order to obtain  $S(Z)$ , the difference between the two predictions for each individual are computed. All models are implemented using the **caret** package (Kuhn (2008)) (respectively named `glmnet`, `ranger` and `gbm`).

### Tuning parameters

For each split, the tuning parameters are chosen separately for the model on the control and the treatment group. There are no set rule to choose these parameters. In our case, we let **caret** define a default search grid and we set a relatively high tuning length for all models based on our computational capacities. Tuning parameters were all selected based on the mean squared error estimates and 5-folds cross validation. For all methods, we pre-process outcomes and covariates and center-scale them before feeding the model.

For each method we have the following tuning parameters :

- Elastic net : *alpha* (Mixing Percentage), *lambda* (Regularization Parameter)
- Boosted trees : *n.trees* (# Boosting Iterations), *interaction.depth* (Max Tree Depth), *shrinkage* (Shrinkage), *n.minobsinnode* (Min. Terminal Node Size),
- Random Forest : *mtry* (# Randomly Selected Predictors), *splitrule* (Splitting Rule), *n.minobsinnode*

## 2. Single learners

The two alternative models we consider are Rlearner and Generalized Random Forest (with their variations). They are “single learners” and use a different approach to approximate  $s_0(Z)$ . Instead of fitting a model on the treatment and on the control group separately to estimate  $\mathbb{E}[Y_i(1)]$  and  $\mathbb{E}[Y_i(0)]$ , they directly fit a model to estimate  $\mathbb{E}[Y_i(1)] - \mathbb{E}[Y_i(0)]$ . Athey and Imbens (2016) discuss the benefits of this approach compared to the two-learners approach. One remaining quantity, the Baseline Conditional Average  $b_0(Z)$ , is needed. For Rlearner with boosting, we use boosted trees fitted on the control group to estimate  $b_0(Z)$  and symmetrically elastic net for Rlearner based on lasso. For Generalized Random Forest we predict  $b_0(Z)$  using the random forest already fitted on the control group. We rely on the **grf** package to implement Generalized Random Forest and on the **rlearner**<sup>79</sup> package for Rlearner.

### Tuning parameters

For each split of the data, we choose the tuning parameters separately for  $S(Z)$  and  $B(Z)$ . Again, there is no theoretical basis to determine the choice of search grid parameters. We keep the package default grid and put a convenient length of parameters combination ac-

<sup>78</sup>The sample is split between an auxiliary sample where machine learning predictors are trained and constructed and a main sample where they are used for prediction and on which we estimate the different key features of the CATE function.

<sup>79</sup><https://github.com/xnie/rlearner>

ording to our computational capabilities. Parameters were selected based on the mean squared error estimates and 5-folds cross validation.

### H.3 Adaption to the Experimental Setting

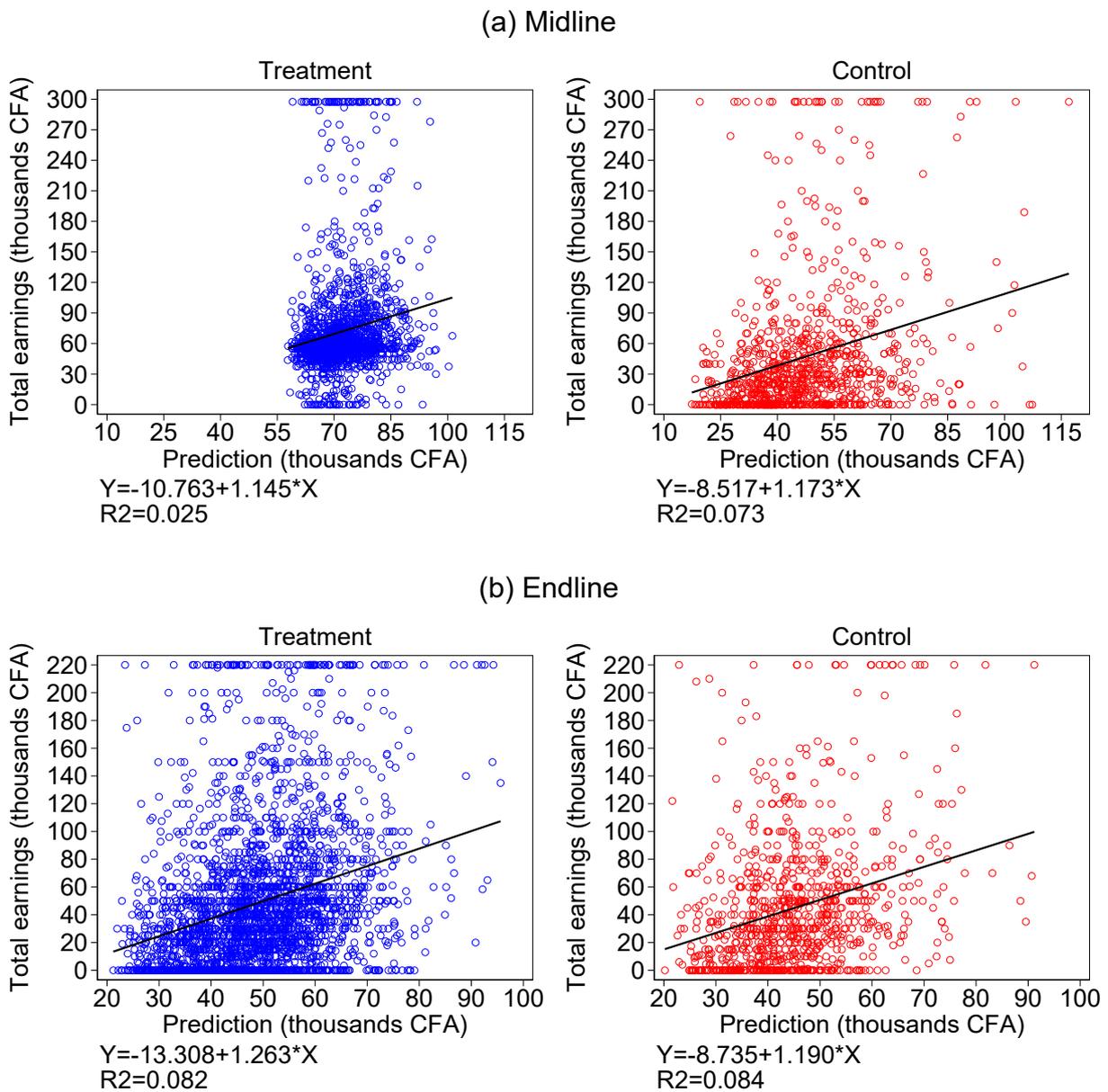
In our application, we repeat  $S = 100$  times the procedure developed by Chernozhukov et al. (2020).<sup>80</sup> The first step of the method requires partitioning our dataset into an auxiliary and a main sample. We adapted the algorithm so that the sample splits are stratified by our randomization blocks (*locality \* gender*), which represents 32 strata. This is important to preserve the identification strategy when estimating directly the CATE for the single learners, since they fit a model on different splits of the data.

Lastly, we introduce two adjustments in the linear projections of Best Linear Predictor (BLP) and Group Average Treatment Effects (GATE) along with predicted baseline effect  $B(Z)$  and predicted treatment effect  $S(Z)$ . We add locality-gender fixed effects, corresponding to the randomization stratification variables. We also adjust the weights used. In the main specification of the paper, we use weights to take into account randomized assignment by lotteries, survey attrition, and sub-sampling at midline. Since our survey weights will be multiplied by inverse propensity score weights as recommended by Chernozhukov et al. (2020), we make sure not to incorporate two times the inverse propensity score.

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<sup>80</sup>Appendix figure B10 shows the scatter plot of earnings  $y_i$  and  $\hat{y}_i$  for midline and endline as well as the regression line of  $y$  on  $\hat{y}$ . The figure shows that the slope coefficients are close to 1 and that the  $R^2$  remain low. Note that a low  $R^2$  on  $y(1)$  and  $y(0)$  does not mean that our algorithm cannot identify heterogeneity in the treatment effects, which is related to difference between  $y(1)$  and  $y(0)$ . This is illustrated in the paper where we find heterogeneity in impacts on midline earnings.

Figure B10 – Relation between predictions and actual earnings (in level) (Random forest)



Note: Predictions estimated using equation  $\hat{Y} = \hat{B}(Z) + T * \hat{\tau}(Z)$ . Each value plotted is the median across 100 sample splits.

Table A23 – Baseline variables used in Machine Learning algorithms

Variable description	Type
<b><i>Individual characteristics</i></b>	
Female	Binary
Age	Continuous
Nb of children	Continuous
Live in urban area	Binary
<b><i>Education</i></b>	
Education (total number of years)	Continuous
Has participated in vocational training	Binary
<b><i>Household characteristics</i></b>	
Household size	Continuous
Is head of household	Binary
<b><i>Household assets</i></b>	
Total nb of assets <sup>1</sup>	Continuous
<b><i>Employment</i></b>	
Total nb of activities	Continuous
Total nb of wage-employment activities	Continuous
Total nb of self-employment activities	Continuous
Is engaged in (at least one) casual activity	Binary
Total Earnings (monthly)	Continuous
<b><i>Savings, Expenditures and Constraints</i></b>	
Has Saved (last 3 months)	Binary
Savings Stock (FCFA)	Continuous
Has a Savings Account	Binary
Self-reported constraints to repay loans	Binary
Self-reported constraints to access credit	Binary
Transportation expenditures (last 7 days, CFA)	Continuous
Communication expenditures (last 7 days, CFA)	Continuous

[1] Assets include livestock, chicken, other animals, plows, field sprayer, carts, wheelbarrows, bicycles, motorcycles, pirogues, refrigerators, freezers, air conditioning units, fans, stoves, computers, radios, television, TV antenna, video players, landline, mobile phones, cars.

# I Weights

This appendix describes the weights used in the analysis. Table A24 summarizes the weights used with midline data, and Table A25 with endline data. In general results are robust if weights are not included.

## Randomization weights

We consider two sets of randomization weights. First, for both midline and endline, we consider weights that account for variations in selection probability by lottery location and gender. There are  $K$  different public lotteries ( $K = 32$ ) with  $N_k$  individuals participating to each lottery. Denote  $N_{k1}$  the individuals from lottery  $k$  selected in the program ('treated') and  $N_{k0}$  those who are not selected, with  $N_k = N_{k1} + N_{k0}$ . Among the  $N_{k0}$ ,  $N_{k0s}$  are randomly drawn to be surveyed and constitute the 'control group'. The size of the population of lottery participants is  $N_P$ , with  $N_P = \sum_k N_k = N_1 + N_0$ . The size of the survey sample is  $N_E = \sum_k N_{k1} + N_{k0s} = N_1 + N_{0s}$ . We use weight  $w_{ki}$  ( $i = 0_s; 1$  according to treatment status) for individuals in the survey sample, with  $w_{k1} = N_k / N_{k1} \times N_1 / N_P$  and  $w_{k0s} = N_k / N_{k0s} \times N_0 / N_P$ . This means that we put a higher weight on lotteries where the demand for the program (total population participating in the lottery) was higher, compared with other lotteries.

Second, when estimating treatment effects by arm using endline survey data, we also consider that the number of brigades assigned to each treatment arm varies by locality. Brigades of treated individuals ( $N_1$ ) are assigned to 3 treatment options  $T_a, T_b$  and  $T_c$ . We use the following notation:  $N_k = N_{a,k} + N_{b,k} + N_{c,k} + N_{0,k}$  with  $N_{1,k} = N_{a,k} + N_{b,k} + N_{c,k}$ , and  $N_P = \sum_k N_k = N_0 + N_a + N_b + N_c$  with  $N_1 = N_a + N_b + N_c$ . We put a weight  $w_{j,k}$  to treated individuals from lottery  $k$  who were assigned to treatment  $T_j$ , and a weight  $w_{k0s}$  for control individuals in the survey sample, with:

- $w_{j,k} = N_{k1} / N_{j,k} \times N_j / N_1$  with  $j = a, b, c$ <sup>81</sup>
- $w_{k0s} = N_{k1} / N_{k0s} \times N_0 / N_1$

## Sub-sampling weights (midline survey only)

The sample for the midline survey includes the control group ( $N_{0s}$ ) and a sub-sample of the treatment group. Consider that we draw a random sub sample of group  $l$  in proportion  $P_l = N_l^S / N_l$ . To take sub-sampling into account, original weights are multiplied by  $S / P_l$ . Therefore, in group  $l = k, 1$  we draw  $N_{k1}^S$  individuals out of  $N_{k1}$ , and the original weight  $w_{k1}$  becomes  $\omega_{k1}^S = w_{k1} \times N_{k1} / N_{k1}^S$ . All control units are included in the midline sample so that their weights  $w_{k0s}$  are unchanged.

## Control group and subsequent enrollment in the program (endline survey only)

When using endline data, we adjust weights for control individuals because some of them were able to apply (and sometimes get selected) in waves 3 and 4 of the program, as discussed in section 1.4.<sup>82</sup> Weights for control individuals depend on their status in wave 3 and wave 4, which is one of the following 7 situations:

<sup>81</sup>Note :  $\sum_j w_{j,k} = w_{k1} = 1$ , which is the weight used for midline data when there is only one treatment group.

<sup>82</sup>Recall that the study focuses on wave 2 (out of 4 waves) of the public works program

1. Group  $C_3T_3\bar{C}_4$ : Applied to wave 3 ( $C_3$ ), was selected as ‘beneficiary’ of wave 3 after public lotteries ( $T_3$ ) and was therefore not allowed to apply to wave 4 ( $\bar{C}_4$ ).
2. Group  $C_3\bar{T}_3C_4T_4$ : Applied to wave 3 ( $C_3$ ), was not selected after public lotteries ( $\bar{T}_3$ ), applied to wave 4 ( $C_4$ ) and was selected as ‘beneficiary’ of wave 4 after lotteries ( $T_4$ ).
3. Group  $C_3\bar{T}_3C_4\bar{T}_4$ : Applied to wave 3 ( $C_3$ ), was not selected after public lotteries ( $\bar{T}_3$ ), applied to wave 4 ( $C_4$ ) and was not selected after lotteries ( $\bar{T}_4$ ).
4. Group  $C_3\bar{T}_3\bar{C}_4$ : Applied to wave 3 ( $C_3$ ), was not selected after public lotteries ( $\bar{T}_3$ ) and did not apply to wave 4 ( $\bar{C}_4$ ).
5. Group  $\bar{C}_3C_4T_4$ : Did not apply to wave 3 ( $\bar{C}_3$ ), applied to wave 4 ( $C_4$ ) and was selected as ‘beneficiary’ of wave 4 after public lotteries ( $T_4$ ).
6. Group  $\bar{C}_3C_4\bar{T}_4$ : Did not apply to wave 3 ( $\bar{C}_3$ ), applied to wave 4 ( $C_4$ ) and was not selected after public lotteries ( $\bar{T}_4$ ).
7. Group  $\bar{C}_3\bar{C}_4$ : Did not apply to wave 3 ( $\bar{C}_3$ ), and did not apply to wave 4 ( $\bar{C}_4$ ).

We introduce a new multiplicative weight for control units ( $\tilde{w}_{k0s,j}$ ). We do not include control units that have benefited from subsequent waves of the program (waves 3 and 4) in the estimation. This means we assign a weight of 0 to groups  $C_3T_3\bar{C}_4$ ,  $C_3\bar{T}_3C_4T_4$  and  $\bar{C}_3C_4T_4$ .<sup>83</sup> To compensate, we put a higher weight on individuals who also applied in subsequent phases (waves 3 and 4) but were not selected during the lotteries. The weights for the remaining four groups are:

- $\tilde{w}_{k0s,C_3\bar{T}_3C_4\bar{T}_4} = \frac{N_{k0s,C_3}}{N_{k0s,C_3\bar{T}_3}} \times \frac{N_{k0s,C_3\bar{T}_3C_4}}{N_{k0s,C_3\bar{T}_3C_4\bar{T}_4}}$
- $\tilde{w}_{k0s,C_3\bar{T}_3\bar{C}_4} = \frac{N_{k0s,C_3}}{N_{k0s,C_3\bar{T}_3}} \times 1 = \frac{N_{k0s,C_3}}{N_{k0s,C_3\bar{T}_3}}$
- $\tilde{w}_{k0s,\bar{C}_3C_4\bar{T}_4} = 1 \times \frac{N_{k0s,\bar{C}_3C_4}}{N_{k0s,\bar{C}_3C_4\bar{T}_4}}$
- $\tilde{w}_{k0s,\bar{C}_3\bar{C}_4} = 1$

### Tracking weights

Lastly, we add a weight taking into account the differential response rate of individuals during each survey (midline and endline). More precisely, each survey consisted in two phases  $a$  and  $b$ :

- A main data collection phase ( $a$ ), during which the response rate is  $R_{a,j}$  for group  $j = 1, 0$ .
- An additional tracking phase ( $b$ ), targeting attriters from the main phase. We note  $R_{b,j}$  the response rate of the tracking phase for group  $j = 1, 0$ .

To determine the tracking sample, we first define a sub-sample of ‘eligible’ attriters.<sup>84</sup>  $E_{b,j}$  from which a random sub-sample is drawn in proportion  $\pi_j = NE_{b,j}^S / NE_{b,j}$  ( $j$  is an index for treatment

<sup>83</sup>Hence  $\tilde{w}_{k0s,C_3T_3\bar{C}_4} = 0$ ;  $\tilde{w}_{k0s,\bar{C}_3C_4T_4} = 1 \times 0 = 0$ ;  $\tilde{w}_{k0s,C_3\bar{T}_3C_4T_4} = \frac{N_{k0s,C_3}}{N_{k0s,C_3\bar{T}_3}} \times 0 = 0$ .

<sup>84</sup>Among the attriters of phase ( $a$ ) some individuals were considered ‘ineligible’ for tracking as they were (quasi) impossible to reach: dead individuals, individuals who migrated to another country, (for endline) individuals who were already impossible to find at baseline.

status x locality). Individuals interviewed during the tracking phase take a different weight than those interviewed during the main survey phase. Tracking respondents are weighted by  $\omega_j^T = (R_{a,j}^S + \lambda_j s_j R_{b,j}^S (1 - R_{a,j}^S)) E_{b,j}^S$ , with  $\lambda_j$ , so that the final weight is  $\omega_j^{S,f} = \omega_j^S \times \omega_j^T$ .

The sum of the weights on population  $j$  is therefore :  $\omega_j \times (N_{a,j}^S + \lambda_j N E R_{s,b,j}^S)$ , with  $N E R_{s,b,j}^S$  the number of individuals from the tracking sample who responded during tracking phase. We make the hypothesis that residual non-response  $R_{b,j}^S$  is random. We seek to be representative of the respondent population of phases  $a$  and  $b$ . This lead us to take  $\lambda_j = N E_{b,j}^S / N E R_{s,b,j}^S$

In group  $j$ , weights will be set such as:<sup>85</sup>

- $\omega_j^S \times 1$  for phase  $a$  respondents
- $\omega_j^S \times N E_{b,j}^S / N E R_{s,b,j}^S$  for phase  $b$  respondents

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<sup>85</sup>In theory,  $\omega_j$  should be adjusted so that it does not use correction  $N_j / N_j^S$  but rather the correction corresponding to the total of eligibles  $N_{a,j} + N E_{b,j}$ . However, this number is only known for selected units  $S_j = 1$ . Therefore we will ignore this aspect, which is fair considering that units where randomly drawn. Finally, it means that we estimate the unknown amount  $N_{a,j} + N E_{b,j}$  by  $N_{a,j}^S + N E_{b,j}^S \times N_j / N_j^S$

Table A24 – Summary of weights used with midline data

Randomization weights $w_k$		Sub Sampling weights $\omega_k^S$		Tracking weights $\omega_j^T$	
Treated	$w_{k1} = N_k/N_{k1} \times N_1/N_P$	Treated	$w_{k,1}^S = N_{k1}/N_{k1}^{S_{k1}}, k=\text{locality}$	Respondents main phase ( $R_a = 1$ )	$\omega^T = 1$
Control	$w_{k0s} = N_k/N_{k0s} \times N_0/N_P, k=\text{locality x gender}$	Control	$w_{k,0}^S = 1$	Non Respondents main phase ( $R_a = 0$ )	$\omega_j^T = NE_{b,j}^S/NER_{s,b,j}^S$ if respondent in tracking phase ( $E_b = 1$ et $R_b = 1$ ), $j=\text{locality x treatment status}$ $\omega^T = 0$ if non respondent (but sampled) in tracking phase ( $E_b = 1$ et $R_b = 0$ ) $\omega^T = 0$ if not sampled for tracking phase ( $E_b = 0$ )
<b>Final weight:</b> $w_{k,i}^F = w_{k,i} \times \omega_{k,i}^S \times \omega_{k,i}^T, i = 0, 1$ (treatment status), $k \in \llbracket 1, 32 \rrbracket$ (locality x gender)					

Table A25 – Summary of weights used with endline data

Randomization weights $w_{j,k}$		Post-enrollment weights $\tilde{\omega}_{k,j}$		Tracking weights $\omega_j^T$	
Treatment arm $T_a, T_b$ or $T_c$	$w_{j,k} = N_k / N_{j,k} \times N_j / N_P, j = a, b, c$	Selected to participate to wave 3 or 4 (groups $C_3 T_3 \bar{C}_4, C_3 \bar{T}_3 C_4 T_4$ et $\bar{C}_3 C_4 T_4$ )	0	Respondents main phase ( $R_a = 1$ )	$\omega^T = 1$
Control	$w_{k0s} = N_k / N_{k0s} \times N_0 / N_P, k = \text{locality x gender}$	Group $C_3 \bar{T}_3 C_4 \bar{T}_4$	$\frac{N_{k0s, C3}}{N_{k0s, C3 \bar{T}_3}} \times \frac{N_{k0s, C3 \bar{T}_3 C4}}{N_{k0s, C3 \bar{T}_3 C4 \bar{T}_4}}$	Non Respondents main phase ( $R_a = 0$ )	$\omega_j^T = NE_{b,j}^S / NER_{s,b,j}^S$ if respondent in tracking phase ( $E_b = 1$ et $R_b = 1$ ), $j = \text{locality x treatment status}$
		Group $C_3 \bar{T}_3 \bar{C}_4$	$\frac{N_{k0s, C3}}{N_{k0s, C3 \bar{T}_3}}$		$\omega^T = 0$ if non respondent (but sampled) in tracking phase ( $E_b = 1$ et $R_b = 0$ )
		Group $\bar{C}_3 C_4 \bar{T}_4$	$\frac{N_{k0s, \bar{C}_3 C4}}{N_{k0s, \bar{C}_3 C4 \bar{T}_4}}$		$\omega^T = 0$ if not sampled for tracking phase ( $E_b = 0$ )
		Group $\bar{C}_3 \bar{C}_4$	1		
<b>Final weight:</b> $w_{k,i}^F = w_{j,k} \times \tilde{\omega}_{i,l} \times \omega_{k,i}^T, j = 0, a, b, c$ (treatment status), $i = 1, 0s, l$ post-enrollment group, $k \in \llbracket 1, 32 \rrbracket$ (locality x gender)					

## Chapter 2

# Economic inclusion, capital constraints and micro-entrepreneurship : experimental evidence from Côte d'Ivoire

### 2.1 Introduction

Promoting access to more productive employment opportunities is at the core of the poverty reduction challenge in Africa. While most individuals rely on agriculture in rural and peri-urban areas, a large share of the population engages in self-employment activities, particularly in low-income and fragile settings (Filmer and Fox (2014)). For many, work means being engaged in multiple activities that provide alternative sources of revenues and diversify risk. Therefore policies to increase the productivity of independent small businesses and enable entrepreneurial efforts are increasingly considered to improve the livelihoods of the poor.

Micro-entrepreneurs in developing countries face multiple constraints and often lack financial or human capital (Jayachandran (2020)). Large returns to capital have been documented for micro-entrepreneurs in different settings (in Sri Lanka, de Mel et al. (2008) and (2012), in Ghana, Fafchamps et al. (2014) or in Uganda, Blattman et al. (2014)). However, the poor often struggle to access capital or accumulate savings to make the lumpy investments required to start or expand income-generating activities. There is limited or no credit market for first-time entrepreneurs. For other micro-entrepreneurs, credit terms are generally unattractive, with high interest rates and strict collateral conditions. On top of that, the literature has shown that access to micro-loans

have modest impacts on micro-enterprise growth (Banerjee et al. (2015a), ?), their impact on business outcomes being precisely zero along most of the distribution according to Meager (2019). Therefore, assets or cash transfers are increasingly considered to support the capital needs of micro-entrepreneurs. In addition, savings levels are low, which also constrains investments in businesses. Both market failures (high transaction costs) and behavioral failures (costly self-control as well as inter-personal or intra-household barriers) can lead to under-saving and under-investing (Karlan et al. (2014)). Demand for cash from peers is another disincentive for savings accumulation.

Besides capital constraints, human capital shortages may further hinder productivity. This calls for investing in the development of skills, for instance business skills, or behavioral skills such as entrepreneurship spirit. A lack of affordable and relevant training can justify the provision of training in entrepreneurship interventions. Individuals may also underestimate the returns to human capital acquired in training. However, evidence on the impact of stand-alone business training is mixed (McKenzie and Woodruff (2014); Jayachandran (2020)). While some interventions in Uganda and Kenya have shown sustained impacts (Blattman and Annan (2016); McKenzie and Puerto (2017)), many evaluations do not find significant lasting impacts on revenues or profits.<sup>1</sup>

The literature suggests that jointly addressing skill and capital constraints may be needed among the ultra-poor and generate large impacts (Cho and Honorati (2013) ; Blattman and Ralston (2017)). Graduation interventions are one prominent model. These are multi-faceted programs addressing multiple constraints by providing (in-kind) capital and skill training, as well as consumption support, coaching, and facilitated access to savings. They have had solid impacts on the income-generating activities and earnings of poor households in different settings across continents.<sup>2</sup> Other micro-entrepreneurship interventions have successfully combined in-kind or cash grants with technical or business training. However, open questions remain about the optimal mix of support, and how many components are required in an integrated intervention. The provision

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<sup>1</sup>A new approach recently introduced focused on psychological training (Personal Initiative curriculum) has shown promising results in Togo (Campos et al. (2017)) but further implementations in Ethiopia and Jamaica are less convincing (although quality of implementation might be key) (Alibhai et al. (2019) and Ubfal et al. (2019)).

<sup>2</sup>Following the implementation of the Targeting the Ultra Poor (TUP) program in Bangladesh by BRAC, multiple pilots were conducted in other countries to test and adapt the model. Randomized evaluations have documented large impacts of TUP in Bangladesh (Bandiera et al. (2017)) and in Ethiopia, Ghana, Peru, Honduras, India and Pakistan (Banerjee et al. (2015b)), while maintaining a similar approach and implementation. A multi-country experiment is implemented in four Sahel countries (Burkina Faso, Mauritania, Niger, Senegal) (Bossuroy et al. (2020)).

of capital in the form of grants, in-kind transfers, or microcredit is often at the core of these interventions. It is thus particularly important to identify the most cost-effective instrument to alleviate capital constraints.

This paper investigates an economic inclusion and micro-entrepreneurship support program providing a mix of skills training and capital. It tests three alternative instruments to facilitate lumpy investments in small businesses. Specifically, it considers the relative effects of relaxing capital constraints through a cash grant (with or without repayment condition) or relaxing constraints to savings accumulation through Village Savings and Loans Associations (VSLAs), an enhanced savings group. We designed a large-scale randomized controlled trial (RCT) to evaluate the impact of the program across 207 localities in the Western regions of Côte d'Ivoire. Individuals were invited to express interest to participate in the program (enrollment). Localities were then randomly assigned to a control group (60 localities) or one of the three interventions: "VSLAs" (T1, 53 localities), "cash-grants-with-repayment" (T2, 64 localities), or "cash grants" (T3, 30 localities). After that, individuals were selected according to a proxy of their vulnerability level, using data collected during the enrollment of all interested individuals. We measure direct impacts of the interventions by comparing individuals above the program selection cut-offs in control and treatment communities.<sup>3</sup>

The program started with a 35 hours entrepreneurship training designed to support the creation and management of an independent activity. The training also supported the development of simple business plans to assess business opportunities and capital needs. In the cash grant group, individuals received on average 95,000 FCFA per person (USD 384 PPP) conditional on writing a business plan, with no monitoring or supervision after the disbursement.<sup>4</sup> In the cash-grant-with-repayment group, individuals also received a grant (same average amount per person), with a 50% repayment condition and supervision in the months after disbursement. In the VSLA intervention, no capital was injected but savings groups based on the VSLA model were set up. VSLAs are enhanced savings groups designed to facilitate savings accumulation and provide small credit opportunities. After one cycle, share-outs are distributed, which provides individuals with capital for investments, roughly at the same time as the other treatment arms. We collected detailed

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<sup>3</sup>The design of the RCT also allows to study indirect effects, which we do in Chapter 3.

<sup>4</sup>We used the exchange rate USD 1=XOF 580.657 (official average exchange rate 2017 (IMF)) to convert CFA francs to US dollars. We used the PPP conversion factor, GDP (LCU per international \$) of 247.134 for Côte d'Ivoire in 2017 (World Bank).

follow-up data approximately 22 months after the program started, or on average 15 months after capital transfers. The data contains detailed information on income-generating activities, investments and savings.

Our first contribution is to study the overall effectiveness of an integrated intervention addressing both human and financial capital constraints to improve the livelihoods of poor households in a fragile setting. This complements the literature on micro-entrepreneurship and graduation programs, which combine capital injections and training (and possibly other components). While there is strong evidence of lasting impacts for graduation programs on assets and earnings in several countries ([Banerjee et al. \(2015b\)](#) and [Bandiera et al. \(2017\)](#)), evidence is limited in fragile and post-conflict settings. A multi-country experiment implemented in Sahel already shows strong results in Niger for an intervention delivered at large scale through government systems ([Bossuroy et al. \(2020\)](#)). Two graduation programs implemented in Afghanistan and South Sudan confirm the strong results observed in other developing countries ([Bedoya et al. \(2019\)](#) and [Chowdhury et al. \(2017\)](#)). However, the recent evaluation of an intervention in Yemen (where implementation suffered from a political crisis) suggests that graduation programs may be less effective in high-risk environment ([Brune et al. \(2020\)](#)). We find that the program induces new micro-businesses and investments in the assets of those activities, although these effects are not sufficient to significantly increase individuals' earnings. No impacts on food security, household expenditures or household assets are observed 15 months after the end of the intervention.

Our second contribution is to analyze the relative effectiveness of alternative instruments to relax credit or savings constraints. Various instruments have been considered to address capital constraints: asset transfers, cash grants, loans. Graduation programs have extensively relied on transferring assets, most commonly livestock. Delivery is complex but they have the benefit to directly serve as input for income-generating activities. At the opposite end of the spectrum are unconditional cash grants which can be freely invested or consumed. They have showed some encouraging results on economic diversification and earnings in the short and medium run ([Blattman et al. \(2014\)](#), [Macours et al. \(2019\)](#)). However, a large part of the funds may still be used for consumption (see [Haushofer and Shapiro \(2016\)](#) in Kenya with GiveDirectly). Our intervention focuses on cash grants given with no other condition than having developed a sound business plan and attended training. We test two different types of grants to inject capital: cash grants

and grants with repayment conditions. The rationale of the repayment condition is to provide further incentives to maximize the share of the grant invested in an income-generating activity. The comparison of cash grants with or without repayment condition is not yet documented in the literature except [Fiala \(2018\)](#) comparing cash grants and loans, to our knowledge. The additional effect of imposing repayment restrictions is unclear. The literature has highlighted two potential mechanisms. Relaxing the repayment constraint with extended grace periods in micro-credit contracts has led to higher long-term profits given by the opportunity to undertake riskier investments ([Field et al. \(2013\)](#)). This suggests that repayment conditions might hinder riskier but profitable investments. Conversely, [Fiala \(2018\)](#) argues that a repayment condition in loans provides more incentives to invest the cash, similar to the idea that it is tempting to consume cash in-hands compared to in-kind transfers.<sup>5</sup>

With the objective of seeking a cost-effective way to address financial constraints, we also compare cash grant interventions with a VSLA instrument that aims to facilitate savings to finance lump-sum investments without any external capital injection. As such, we test whether savings or capital constraints are more prevalent. Few studies directly compare instruments addressing credit or savings constraints to facilitate investments. [Afzal et al. \(2018\)](#) provide a theoretical framework and show evidence that loans to be reimbursed and savings to be accumulated are substitutes for microcredit clients in Pakistan. However, empirical evidence on the differential effectiveness of saving “up” (using VSLA) or “down” (cash-grant-with-repayment) is scarce. It is unclear if relaxing savings constraint is enough to raise earnings. There is some evidence that providing access to formal savings accounts can lead to productive investments and increase production revenues in Malawi ([Brune et al. \(2016\)](#)) or Kenya ([Dupas and Robinson \(2013\)](#)). In both cases, relaxing the social constraint (demand from peers) is identified as a key channel. While savings groups offer a soft commitment device limiting social claims, evidence on their business impact is mixed. The literature on VSLA suggests positive impacts on food security and housing quality in Mali ([Beaman et al. \(2014\)](#)) and Malawi ([Ksoll et al. \(2016\)](#)), but rarely reports impacts on business outcomes (except [Karlan et al. \(2017\)](#)). Interestingly, VSLAs have been rarely used in graduation programs, which usually include savings support but in other forms (Sahel design as in [Bossuroy et al. \(2020\)](#) is one exception)..

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<sup>5</sup>This is also highlighted as "flypaper" effect in [Fafchamps et al. \(2014\)](#) in which the impacts of an in-kind and cash transfer are compared. In-kind transfers led to larger investments in the business.

We find that the cash grant, cash-grant-with-repayment and savings interventions have different effects on the dynamics of savings and asset accumulation. The VSLA intervention shifts savings towards a more secure savings instrument and increases investments in income-generating activities. The cash grant and cash-grant-with-repayment interventions also induce investments in start-up capital. While these start-up investments are twice as large as in the VSLA group, beneficiaries are found to save a substantial share of the grants, approximately 30%. Despite these differences in mechanisms, impacts on income, number of activities and productive assets are similar across the three interventions. Adding a repayment condition to the grant does not make substantial difference. And injecting capital through cash grants or cash-grant-with-repayment does not achieve larger impacts than facilitating savings and small credits.

The paper is structured as follows. Section 2.2 presents the intervention and data. Section 2.3 presents the experimental design. Section 2.4 presents direct economic impacts of the intervention. Section 2.5 concludes. Result tables are presented in appendix.

## 2.2 Intervention and Data

### 2.2.1 Context: Post-conflict Western Côte d'Ivoire

We study a program designed as a post-conflict intervention aiming to promote economic empowerment and social cohesion. It was implemented in one of the most fragile areas of the country, namely Western Côte d'Ivoire. Although Côte d'Ivoire developed steadily during the 1990s, episodes of conflict induced substantial deterioration in economic conditions between 2002-07 and 2010-11. The Western regions were particularly affected by those two conflicts, in terms of clashes occurring between communities and casualties. A key aspect to understand the fragility of Western regions is the high ethnic fragmentation between three main groups ("native" ethnic groups, other Ivorian ethnic groups and foreign ethnic groups). Most of the tensions across ethnic groups materialize around land ownership. The 2010-11 post-electoral crisis exacerbated them. In Chapter 3 which deals with social cohesion, we provide more elements on this aspect and study the social impacts of the intervention.

### 2.2.2 Intervention : The micro-entrepreneurship and economic inclusion program

The program was coordinated by the “Office Coordinating Employment Programs” (BCP Emploi), which is part of the Ministry of Youth Employment.<sup>6</sup> It was implemented by the International Rescue Committee (IRC), a prominent NGO which has offices in the Western region and a long experience with this type of interventions in Côte d’Ivoire and beyond.

The program was rolled out between 2014 and 2017, in a total of 37 sub-prefectures across four Western regions of Côte d’Ivoire.<sup>7</sup> The study focuses on the largest phase of implementation, which started in July 2015, lasted for 2 years and covered 16 sub-prefectures.<sup>8,9</sup>

The program was conceived as an economic empowerment intervention addressing both human and financial capital constraints. First, a training component to enhance human capital and provide skills. This component is common across the three interventions. Second, a financial component aimed to facilitate access to financial support, with 3 modalities: cash grants, cash-grant-with-repayment and village savings and loan association (VSLA). We provide a summary of the components per intervention arm in Table 2.1.

The training component focuses on building entrepreneurship and business skills. In total, the training lasted 55 hours delivered over 8 days to small groups in each locality. First, a basic training covered entrepreneurship fundamentals (focused on starting your own activity) and a motivational module around peace building and community engagement.<sup>10</sup> Second, individuals

<sup>6</sup>The program is named « PRISE ». It was funded by the Japanese Social Development fund through the World Bank.

<sup>7</sup>The regions are: Tonkpi, Cavally, Guémon and Bafing. The 37 sub-prefectures were identified at project design and agreed between the donor, the coordinating agency and government counterparts. The selection was based on economic needs, vulnerability and displacement levels, as well as the scarcity of other economic interventions.

<sup>8</sup>The program was rolled out in three phases. The first phase was used to test implementation including the targeting instrument, the organization of public lotteries, the relevance of the training curricula, and the use of business plans. The RCT was embedded in the second phase, which was the largest and had the most participants. The third phase used funds recollected from first and second phase to cover some additional localities. Note that each of the 37 sub-prefectures were initially assigned to a given phase, to ensure that there is no overlap of localities across phases. Therefore, there is no geographic overlap between the RCT embedded in the second phase, the first and third phases.

<sup>9</sup>The allocation of localities across interventions required to group sub-prefectures in clusters (so that there would be at least 10 units per cluster). This led to allocate “zero” localities to cash-grant modality in some sub-prefectures. This is due to budget constraints at program design stage, which only planned for 30 localities (out of 147 treated localities) in the cash-grant modality.

<sup>10</sup>The basic entrepreneurship training curriculum covers issues related to starting a business, such as how to choose the right business to start, how to attract potential clients, how to deal with competition, how to manage

Table 2.1 – Intervention content by treatment arm

	T1	T2	T3
	Village Savings and Loan Association (VSLA)	Cash grant with repayment	Cash Grant
<b>Basic training :</b>			
- Peace building and Social Cohesion	X	X	X
- Entrepreneurship 1 (“starting an activity”)			
<b>Development of a business plan</b>	X	X	X
<b>Validation committee of the business plans.</b>		X	X
<b>Village Savings and Loan Association development</b>	X		
<b>Complementary training :</b>			
- Life skills	X	X *	X *
- Entrepreneurship 2 (“managing an activity”)			
<b>Transfer of the grants</b>		X *	X *
<b>Follow-up: individualized counselling</b>		X *	
<b>Repayment (50% of grant, starting 3 months ) after transfer)</b>		X *	

\* To complete this step, the beneficiary’s business plan must have been accepted by the committee.

\*\* Also technical support of business experts for a small number of business plans with technical aspects.

(or groups) worked on a business plan with regular feedback and supervision from the trainers.<sup>11</sup> This was mostly field-based, and participants had to find relevant information on prices, costs and competitors to fill their business plans. A third part of the training covered more advanced topics on entrepreneurship (focused on managing your activity) and included a life skills module.<sup>12</sup> The entrepreneurship training was organized and delivered by the implementing agency (IRC). The training content was tailored to low-skilled target groups, for instance by relying on pictures and hands-on exercises.<sup>13</sup> A “community expert” who was on average more literate than beneficiaries was designated in each locality to participate in the training and help explain or clarify some of the content after training completion.<sup>14</sup>

All beneficiaries prepared business plans for income-generating activities, though the exact process slightly varied across interventions. For ‘cash-grant-with-repayment’ and ‘cash grants’, business plans were evaluated, after which the project was either approved for funding, rejected, or sent for revisions. Three rounds of reviews took place, and ultimately more than 95% of business plans

costs, how to set the right price, etc.

<sup>11</sup>This phase is not included in the 55 hours total.

<sup>12</sup>The advanced entrepreneurship training curriculum covers issues related to the development of existing businesses: managing stocks, monitoring sales, performing basic accounting, monitoring running capital, etc.

<sup>13</sup>The curriculum itself was the output of many tests and adjustments, jointly led by IRC and a consulting firm specialized in training. It was based on a curriculum developed for low-skilled beneficiaries of a Public Works program (Bertrand et al. (2017)) as well as IRC own curriculum “EASE”. The curriculum was revised after the 1st year of the program to be further tailored to the type of beneficiaries and the geographical area.

<sup>14</sup>The “community expert” also received training from the NGO on how to animate a group and efficiently deliver a training. He/she had a key role in ensuring that the content of the training had been understood, and to provide some follow-up based on individual needs.

were approved to be funded.<sup>15</sup> In the VSLA modality, beneficiaries similarly drafted business plans, but these plans were not evaluated.

Village Savings and Loan Associations (VSLAs) were put in place right before the entrepreneurship training based on a well-defined methodology. Facilitators from the locality were trained by the NGO to help beneficiaries set-up VSLAs and keep books. Compared to ROSCAs ("*ton-tines*"), VSLAs are more secure and provide small loans.<sup>16</sup> Beneficiaries are invited to create an association, elect a committee, and regularly meet (weekly or biweekly) to contribute savings to a common pot. After they have reached a certain level of savings, participants can request loans from the VSLA at a rate pre-determined at the start of the cycle. At the end of the cycle (which lasts 9 to 10 months), the pot is distributed among participants proportionally to their saving shares. Participants receive a remuneration on their savings as the final pay-out includes interests paid by borrowers to the pot. In the VSLA intervention, there is no cash injection, but beneficiaries can use the share-out at the end of the cycle for investment, or request a loan from their VSLA to access additional capital. The possibility to take loans at any time during the cycle (after the first four months) is a key advantage compared to ROSCAs: individuals can access capital exactly when they need, while in ROSCAs the timing to access the pot is constrained and usually predetermined.<sup>17</sup>

VSLAs received regular follow-up during the full first cycle, but not after. The NGO came back to the localities to celebrate the end of the first cycle and supervise the distribution of the pot. They invited beneficiaries to start a second cycle in their VSLA. As local facilitators had been trained in each locality by the NGO, they could continue providing support to new cycles, but without further support from the project.

In contrast to the VSLA intervention, the “cash-grant-with-repayment” and “cash grant” modalities provided direct capital injections. The exact amount depended on the business plan, with an average of 95,000 CFA (USD 163 nominal, USD 384 PPP in 2017) per beneficiary (similar

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<sup>15</sup>In cash-grant-with-repayment intervention (respectively cash-grant intervention), 80.9% of selected individuals submitted a business plan for review (respectively 82%). Most of the business plans were approved to be funded (96.7% of business plans and 97.8% of business plans respectively). In the end, 78.9% of selected individuals claimed and received the funds (respectively 81.1%).

<sup>16</sup>A ROSCA is an informal association or group in which members contribute to a common pot of money that is awarded to a different member at each meeting. The VSLA model was launched by CARE in Niger in 1991, and later implemented by many NGOs. IRC developed its VSLA methodology in 2012, based on knowledge and tools shared by practitioners such as VSL Associates.

<sup>17</sup>VSLA interventions sometimes try to align the share-out to the calendar of seasonal activities. This is not the case in this intervention.

across the two modalities).<sup>18</sup> Cash grants were delivered after approval of the business plans, with no further requirements and very limited monitoring. In the “cash-grant-with-repayment” modality, beneficiaries have to repay half of the grant within six months. In both interventions, community experts developed entrepreneur support groups to facilitate discussion and support between beneficiaries. The cash-grant-with-repayment intervention also included some additional follow-up from NGO agents, who monitored participants following the disbursement of funds.<sup>19</sup> The reimbursement was supposed to start three months later, with a grace period of one month. Beneficiaries were expected to go every month to the local branches of the banking partner. The bank sent collectors to the field (especially to the most remote villages). However, money recollection suffered from delays and under-staffing. Recollection started 6 months later and at the end of the recollection period only 39.96% of the targeted recollection amount was achieved.<sup>20</sup> This means that on average, only 20% of the grant (instead of the targeted 50%) was repaid in the cash-grant-with-repayment intervention.<sup>21</sup> Less than one third of beneficiaries successfully reimbursed half of the grant. It is important to mention that field visits by the NGO suggest that most of the participants remained convinced that they would have to reimburse half of the funds at some point. Given their expectations due to the repayment condition, the cash-grant-with-repayment intervention cannot be fully assimilated to the cash grant one.

### 2.2.3 Take-up

Table 2.2 presents the overall take-up rates and the break-down for various components of the program.<sup>22</sup> The take-up of the program is high across interventions, though slightly lower for the

<sup>18</sup>On average, each business plan received 185,000 CFA (USD 319 nominal, USD 749 PPP 2017) because beneficiaries could develop businesses in groups. The transfer depended on the capital needs expressed in the business plan, but in practice 95% of grants were for the maximum amount announced by the program (100,000 CFA). There is no difference in the amount requested and received by beneficiaries across cash grant modalities: the average was 95,624 CFA for cash-grant-with-repayment and 94,946 CFA for cash-grants.

<sup>19</sup>In the absence of monitoring data about this follow-up, we cannot provide an estimate of the follow-up hours, but it is likely to be between 3h-8h per business. The implementing NGO increased follow-up support for businesses facing difficulties, compared to the others. For agriculture projects, technical experts visited participants to provide technical support and advice.

<sup>20</sup>At the end of debt recollection, 13.8% of beneficiaries who received the funds fully reimbursed their due while 28.5% did not reimburse anything. The remaining individuals reimbursed on average 22,109 CFA (23% of the grant instead of 50%) with substantial variation: in terms of repayment share, the 25th percentile is at 11.7% while the 75th percentile is at 33.4%). On average, 1.8 deposits were made per activity (i.e. per business plan funded).

<sup>21</sup>15,078 CFA per beneficiary on average, and 19,054 CFA if we restrict to beneficiaries who received the grant.

<sup>22</sup>Several take-up rates can be considered: the take-up of the first business training, the submission of a business plan, the take-up of the second business training or the reception of funds (or participation in a VSLA).

Table 2.2 – Take up rates

	VSLA (T1)	Cash grant with repayment (T2)	Cash Grant (T3)
<b>Take up for financial support (*)</b>	<b>69.5%</b>	<b>78.9%</b>	<b>81.1%</b>
Training 1 : Entrepreneurship 1 (“starting an activity”) and Peace Building	64.7%	88.4%	91.5%
Writing business plans	<i>not available</i>	82.4%	84.2%
Business plan review and approval	n.a.	80.9%	82.0%
Training 2 : Entrepreneurship 2 (“managing an activity”) and Life Skills	59.8%	61.8%	64.1%

Note : Based on monitoring data. Participation rates are unconditional (i.e. computed over all selected beneficiaries, even if some activities were conditional, e.g. conditional on business plan approval).

(\*) For VSLA intervention, this means joining a VSLA. For other interventions, this means receiving a business grant.

VSLA intervention.<sup>23</sup> The share of selected individuals receiving funds provides a first take-up indicator. In the cash-grant-with-repayment group, 78.9% of selected individuals received the funds and 81.1% in the cash grant group. Participation in entrepreneurship training is above 85% in both groups. The VSLA intervention has a lower take-up rate (69.5%).<sup>24</sup> This is mainly driven by urban areas (districts of cities), where participation is lower. This is not surprising since implementation of savings groups is more difficult in urban context. Conditional on launching a VSLA, participation rate in the entrepreneurship training is 85%.<sup>25</sup> As discussed further below, we focus on intent-to-treat estimates of program impacts among all selected applicants.

## 2.3 Experimental design

### 2.3.1 RCT design

The study was designed as a RCT to assess the effectiveness of the overall program, while also isolating the relative effectiveness of three alternative modalities to address capital constraints: (i) Village Savings and Loan Associations, (ii) cash-grant-with-repayment, and (iii) cash grants.

#### Locality randomization

<sup>23</sup>An individual is considered trained if he/she has participated in at least 60% of the training. More than 75% of individuals submitted a business plan, got a business plan approved, and participated in complementary training.

<sup>24</sup>The training could only be implemented once the group of selected beneficiaries had formed a VSLA. Therefore, participation to a VSLA is a more meaningful measure of take-up.

<sup>25</sup>Although drop-out is small, an analysis of drop-out determinants can be used to assess whether there was systematic selection in program participation. The vulnerability score is positively and significantly associated with program take-up, which means that the few who never participated were less vulnerable. There is no significant difference across individuals who participated in the program between cash-for-repayment and cash-grant interventions. Living in a village (instead of urban areas) is a strong and significant determinant of participation in VSLAs.

The RCT was embedded in the second phase of the program implemented in 16 sub-prefectures across the four regions. Two public lotteries were organized. A first lottery was used to sample 207 localities out of the 354 eligible ones (“sampling lottery”).<sup>26</sup> The second lottery assigned the 207 selected localities to three treatment arms or control group (“assignment lottery”). The organization of the two lotteries was crucial for the design. Between the two lotteries, basic information on the potential program was provided in each locality. Individuals were invited to express interest to participate (enrollment), and baseline data was collected. This process ensured the same level of information across all localities. This allows us to identify eligible individuals who would have been selected in localities ultimately assigned to the control group. The process also ensures that there is no differential selection into the three treatment modalities.

The sampling lottery took place in public in August 2015 in presence of many regional and local officials.<sup>27</sup> It was stratified by clusters of sub-prefectures and urban/rural areas. The three potential intervention modalities were described in broad terms when the sampling lottery took place.<sup>28</sup> It was explained that potential eligible individuals would be pre-identified in each locality, but only some localities would be selected during the second lottery. The assignment lottery was public and took place in March 2016. It was again stratified by cluster of sub-prefectures and type of localities. Following the lottery, 60 localities were assigned to the control group (C), 53 localities to the “VSLA” intervention (T1), 64 localities were assigned to the “cash-grant-with-repayment” intervention (T2), and 30 localities to the “cash grant” intervention (T3). Figure 2.1 summarizes the experimental design. Table 2.3 presents the distribution of localities across interventions with the breakdown of rural/urban localities.

### Individual beneficiary selection

The enrollment phase took place between December 2015 and January 2016. It was led by IRC

<sup>26</sup>In each sub-prefecture, the implementing NGO identified eligible localities. A total of 415 localities were listed, out of which 345 were considered eligible for the intervention. Eligibility criteria included (i) having at least one micro-finance institution within 30km, (ii) a high concentration of vulnerable population and people displaced by the conflict, (iii) a population size reasonable for program implementation (i.e. no micro settlements), (iv) locality not already crowded with another similar program of assistance.

<sup>27</sup>For practical matters, lotteries were organized at the same time in two regions.

<sup>28</sup>Separate lotteries were held for each stratum (cluster of sub-prefectures by urban/rural areas). Sub-prefectures are clustered by county (“*départements*”), which is 11 clusters for 16 sub-prefectures. In each cluster, sites are either in a “urban” or “rural” bin. “Urban” include city neighborhoods as well as settlements (locally known as “*campements*”) closely located to urban areas. “Rural” comprises villages. We use the terms “urban” and “rural” for simplicity, but some settlements are peri-urban. The allocation of localities across interventions required to group sub-prefectures in clusters which led to interventions with “zero” localities allocated to cash-grant modality in some sub-prefectures.

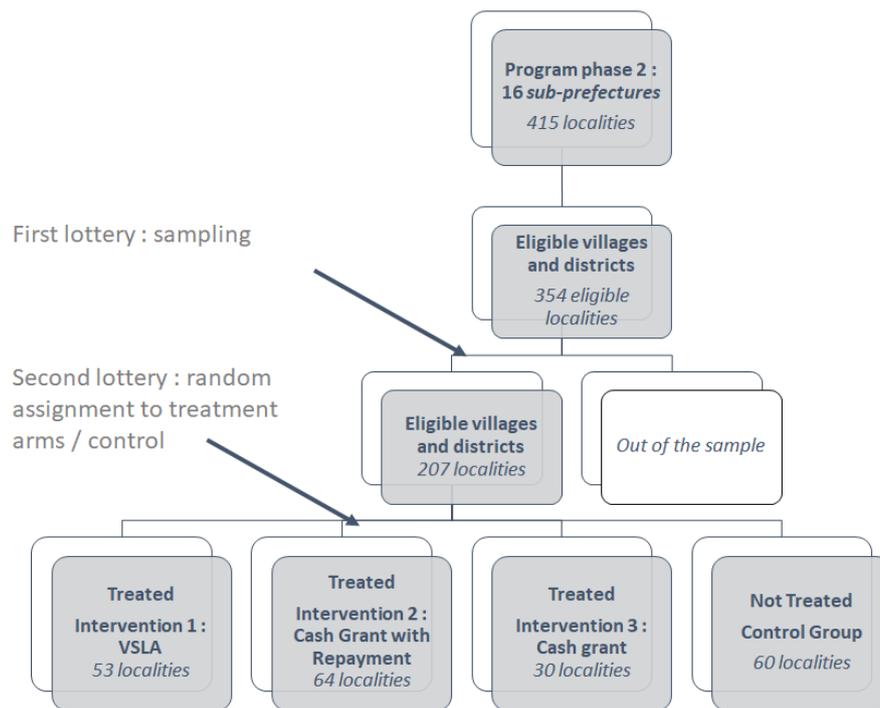


Figure 2.1 – Experimental design

supported by a data collection team. This was done before the final assignment to treatment and control groups. The data collected was used to check compliance with eligibility rules and screen applications by level of vulnerability.

During the enrollment phase, information was collected for all individuals interested in the program across the 207 localities. 14,880 individuals enrolled, among which 12,696 were considered eligible.<sup>29</sup> Eligible individuals had to be 18-40 years-old and not benefit from another assistance program.<sup>30</sup> Eligibility status was first checked using enrollment data.<sup>31</sup> Committees in each community then verified the list of individuals, which led to the identification of 442 (3.3%) ineligible

<sup>29</sup>Eligibility criteria were carefully explained during the enrollment phase in each locality. “Village committees” were formed and included highly respected community members (members of the village council, teachers, etc.). These committees supported the enrollment phase, including to relay information and validate the list of vulnerable households, as further explained below.

<sup>30</sup>Age exceptions are the following: single mothers (above 15 years old) and widows or disabled people (up to 60 years old).

<sup>31</sup>The process was further enhanced by cross-validation using administrative data from development partners. The list was triangulated with similar programs implemented in the region to confirm whether some individuals had already benefitted from assistance. 0.4% of the remaining applicants were identified as ineligible based on this criteria.

Table 2.3 – Summary of survey samples composition

		VSLA (T1)	Cash grant with repayment (T2)	Cash Grant (T3)	Control Group	Total
<b>A</b>	Number of localities (villages and districts)	53	64	30	60	207
	Among which villages	43	52	24	49	168
<b>B</b>	Number of selected individuals (eligible)	1,999	1,870	1,247	-	5,116
<b>C</b>	Among which sampled for endline survey (main sample)	974	919	818	1,218	3,929
<b>Cbis</b>	Among which sampled for endline survey (main sample restricted to common cut-off)	871	919	616	831	3,237
<b>D</b>	Number of non-selected individuals (eligible)	1,558	1,925	443	-	3,926
<b>E</b>	Among which sampled for endline survey (spillover sample)	426	512	209	836	1,983
<b>Ebis</b>	Among which sampled for endline survey (spillover sample restricted to common cut-off)	267	276	209	449	1,201
	Total Baseline sample	3,557	3,795	1,690	3,650	<b>12,692</b>
	Total Endline sample	1,297	1,431	825	1,667	<b>5,220</b>

applicants.<sup>32</sup> Based on the whole process, 2184 (15%) of applicants were deemed ineligible.<sup>33</sup>

The number of beneficiaries for each treatment modality was pre-specified based on available budget.<sup>34</sup> Out of the 12,696 eligible individuals, 5,116 were selected for the program.<sup>35</sup> The final lists of beneficiaries were publicly posted in each site and in the sub-prefecture headquarter.

The final selection of candidates was based on an individual vulnerability score computed using the enrollment data. Table 2.14 details the composition of the score. The score weights criteria including disability, marital status, education, employment, economic status, assets and economic responsibility towards household. Weights were chosen to maximize the dispersion of the score. Selected individuals are those above the vulnerability cut-off established in each location, ordering vulnerability score in descending order from most to least vulnerable. The cut-off used is identical for all localities implementing the same intervention, but it varies across the three interventions

<sup>32</sup>A list of “pre-selected” individuals was shared to village committees. Committees were asked to verify if people might have lied on their personal information or eligibility status during enrollment. Detailed information about applicants were sent back to the implementing NGO, which reviewed each case and decided if it would imply ineligibility.

<sup>33</sup>Note that, although the two first eligibility checks (using baseline data and cross-validation with lists from other programs) were performed uniformly across the 207 sites, the final check (village committee feedbacks) could not be implemented in the 60 control villages because the assignment lottery had already taken place.

<sup>34</sup>The program had been funded with the explicit target of 4,500 individuals trained and receiving financial access (more precisely 1500 for the Semi-credit, 2000 for the VSLA and 1000 for the Cash grant). Given expected drop-outs, the number of people invited in the program was raised from 4,500 to 5,143. However, the study sample is 5,116 taking into account few cases of non-compliance. In particular, VSLAs required a minimum number of 17 people, so a dozen of un-selected participants were invited to create a VSLA together with selected participants.

<sup>35</sup>1,999 in the 53 localities assigned to VSLA, 1,870 in the 64 localities assigned to cash-grant-with-repayment, and 1,247 in the 30 localities assigned to cash grant.

based on the program budget (see Figure 2.2).<sup>36</sup>

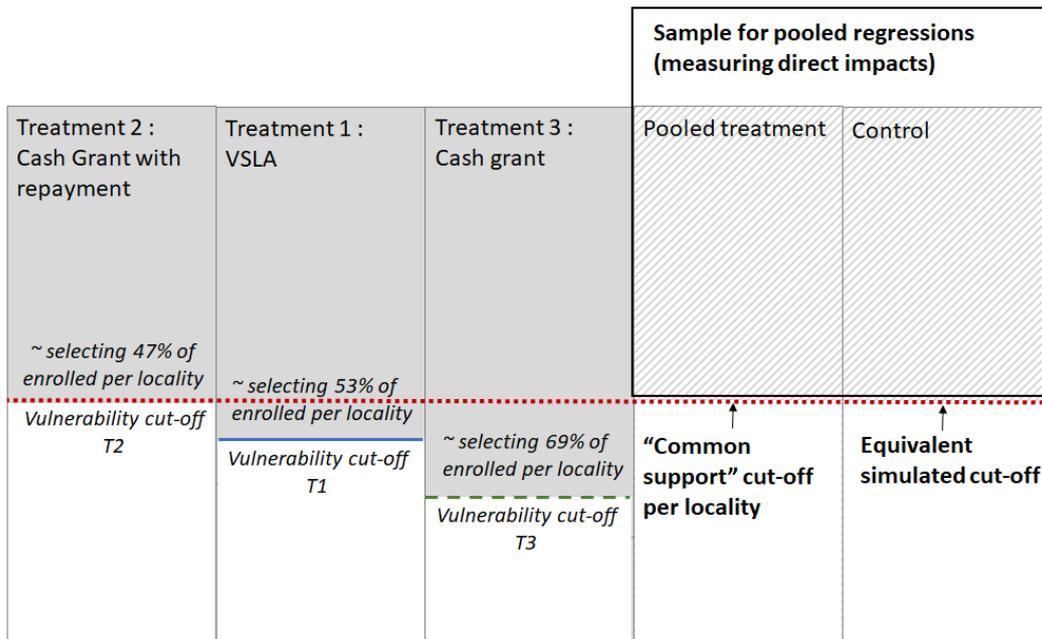


Figure 2.2 – Selection cut-offs by intervention arm and pooled sample to estimate direct impacts

### 2.3.2 Surveys and timeline

**Enrollment timeline.** Figure 2.3 summarizes the program and study timeline.

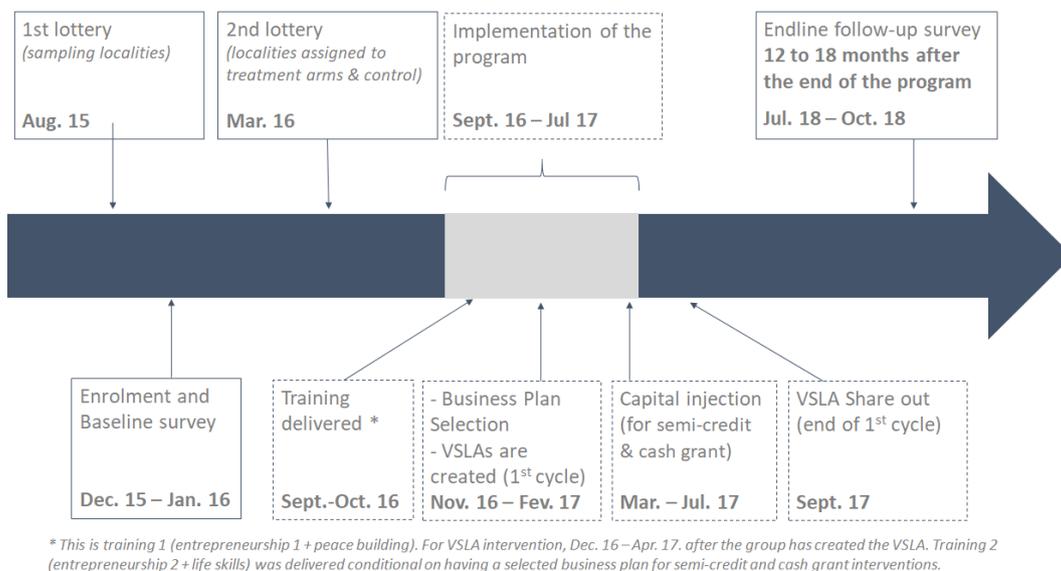


Figure 2.3 – Intervention and experiment timeline

<sup>36</sup>Specifically, it depends on the number of eligible enrolled candidates before the cross-validation and village committee checks. In each location, the capacity of the program is set at  $N_{i,j}$ , which is determined as  $N_{i,j} = \text{Population target}_i * \text{Nb of eligible people}_j / \text{Total nb of eligible people}_i$ , where  $i$  refers to the treatment arm and  $j$  to a locality.

The enrollment was implemented in all 207 localities between December 2015 and January 2016. The process consisted in visiting each site, organizing a public meeting to describe the program content and the targeted population, and collecting (baseline) data on individuals interested in the program. This phase was carried out by the staff of the implementing NGO, based on a survey instrument designed by the research team.<sup>37</sup>

**Baseline survey.** Baseline data included basic measures of employment, assets, education level, household characteristics. A village level questionnaire was also administered to collect information on infrastructure, accessibility and social cohesion. Following the assignment lottery, the final list of beneficiaries was publicly released in each site between July and September 2016.<sup>38</sup>

**Intervention timeline.** Beneficiaries of “cash-grant-with-repayment” and “cash grant” interventions received the first entrepreneurship training between September and October 2016. Business plans were submitted and reviewed between November 2016 and February 2017 and led to the delivery of funds to approved projects between March and July 2017.<sup>39</sup>

In parallel, in VSLA localities, VSLA groups were set up between October and December 2016. They received their first entrepreneurship training between January and April 2017. Although there was no cash injection, beneficiaries had the opportunity to take loans from the group from February 2017 and received an important cash inflow (the total amount of savings plus interests) at the end of the first cycle when the pot was shared across members. The first cycle of the VSLA groups ended between September and December 2017.

**Endline Survey.** The endline survey was conducted on a sample of 5220 individuals between July and September 2018, which is on average 15 months after the end of the program (i.e.

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<sup>37</sup>The standard enrollment procedure was adjusted for the phase of the program being evaluated to ensure impact evaluation data quality. Teams of IRC staff received a reinforced enumerator training on the survey instrument. In addition, an independent team of experienced enumerators was hired to supervise and perform data quality checks for the baseline survey in each site. A double-blind data-entry process was set-up.

<sup>38</sup>The lag between the lottery (March 2016) and the release of beneficiary lists was due to delays in the data entry process, completed only in April 2016, and subsequent cross-validation and “village committee” review.

<sup>39</sup>The delivery of funds was delayed for two reasons. First, unrest in the area led to the suspension of fund delivery between May and June 2017. Second, the banking partners encountered difficulties in delivering funds in remote locations. To solve this issue, beneficiaries in remote locations were given a lump sum to reimburse their transportation costs to the closest bank branch.

between 12 and 18 months after cash payments for “cash grant” and “cash-grant-with-repayment” interventions, or between 10 and 13 months after the end of the 1st cycle of the VSLA). The endline sample is made of two sub-samples: 3,624 “selected” individuals (above the vulnerability cut-off in treated and control localities) and 1,596 “non-selected” individuals (below the vulnerability cut-off in treated and control localities). The second sample of non-selected individuals is used in Chapter 3 to study local spillovers. Control group individuals were sampled using the simulated vulnerability cut-off. The final attrition rate was respectively 10.6% (using the common support cut-off) and was balanced between treatment and control groups.<sup>40</sup> The detail of baseline and endline samples are provided in Table 2.3.

The endline sample size was determined based on power calculation to detect minimum effects comprised between 0.15 standard deviations (for pooled treatments) and 0.23 standard deviations (for comparisons across arms), for a power level of 80% and significance level of 5% and using baseline data for the intra-cluster correlation (ICC) parameter.<sup>41</sup> Power calculation were recalculated ex-post using the control group to compute the mean, standard deviation and intra-cluster correlations for key outcomes in the control group. The minimum detectable effects remain between 0.15 and 0.22 standard deviations for the main outcomes. This means that the study is powered to detect a 20% impact (+5,300 CFA) on profits in independent activities, a 37% impact (+5,800 CFA) on starting capital or a 23% impact (+5,800 CFA) on the value of assets in the main activity.

In this chapter, we use the modules of the follow-up questionnaire focused on household characteristics, employment, assets, food security, well-being, saving and debt. Other modules, in particular related to social cohesion, are used in the analysis provided in Chapter 3.

### 2.3.3 Descriptive statistics

Table 2.14 presents the characteristics of the people who applied to the program (column 2). 78% of the applicants live in rural areas. Applicants are on average 34 years-old and 62% are

<sup>40</sup>Attrition is respectively 10.56%, 11.30% and 10.44% for T1, T2 and T3 (using the common support cut-off). Over the total sample of selected individuals (not using the common support cut-off), attrition rate is on average 10.8%.

<sup>41</sup>The endline survey sample was established based on power calculations to detect impacts between treatment arms and the control group. Hence, 15 “selected” individuals (respectively 17 and 22) were sampled out of localities in T1 treatment arm (respectively T2 and T3).

women.<sup>42</sup> They have on average 3 children and live in large households comprising 7 members (including themselves). Most of the applicants have limited education. 80% of them have no diploma, meaning they have not reached the end of primary school. 48% report that they have not attended school at all.

Nearly all applicants report having an activity (96%). 83% have been involved in agricultural work over the last 6 months. They are primarily self-employed (80%), while less than 10% report having a wage job. Among the self-employed, the main activity is farming (two thirds of the self-employed). This is very much in line with the composition of employment in Côte d'Ivoire and especially in rural areas (Christiaensen and Premand (2017)).

Access to finance is very limited: only 2% have a bank account and 20% use mobile money. On average half of the applicants reports having saved some money over the last 3 months. Most of the savings are held in ROSCAs, in which 52% of applicants participate.

Selected applicants differ from non-selected applicants in a range of dimensions (Table 2.14, column 3 to 5). This is expected since the selection was based on a vulnerability score. Selected applicants are significantly less educated than non-selected applicants (30pp difference in the share of people who did not complete primary school and have no diploma), and have lower earnings (around 20,000 CFA less). Other characteristics not directly used in the score show that selected applicants tend to be poorer: they are less likely to use mobile money (by 12pp) and they are more likely to face constraints for education and health expenditures (by 4pp). Finally, even though gender was not an explicit selection criterion, the share of women is larger in the selected group (around 71% of selected individuals, which is 15pp more than in the non-selected group).

### 2.3.4 Main outcomes

We measure a first set of key outcomes to document whether the program improved participants' engagement in income-generating activities. *Employment* is a dummy variable indicating if the individual has worked at least 1 hour in the past 7 days. We decompose this measure between wage and self-employment (which includes independent activities in both agriculture and non-agricultural self-employment). The *Number of independent activities* counts all self-employment

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<sup>42</sup>The program was advertised both for youth and vulnerable adults, therefore this average age hides large variation as 61% are between 15 and 35 years old.

activities in which the individual was engaged over the last 30 days. This provides an overall picture of individuals' portfolio of income-generating activities. *Monthly Earnings* (in CFA francs ) are reported separately for wage employment and self-employment. In the case of self-employment, the measure used is self-declared profits (over the last 30 days).<sup>43</sup> *Hours worked* are computed for the 3 main activities (of any type) over the last 7 days.

We also measure intermediary outcomes related to the main elements of the intervention. *Savings Stock* (in CFA francs) is calculated at the time of the survey. It accounts for different types of savings (separately measured) including cash, mobile money, micro finance institutions (MFI), bank, farmers' cooperatives, groups of savings (ROSCA or VSLA). *Start-up capital* (in CFA francs) is the sum of retrospective amount of capital used to launch independent activities operating in the last 12 months before follow-up, regardless of the origin of the funds.<sup>44</sup> *Value of productive assets* is the sum of (self-reported) value of assets in all independent activities operating at the time of the survey (in CFA franc). *Investments* aggregate the value of any (self-declared) investment in the main independent activity over the last 6 months (in CFA franc). *Entrepreneurship knowledge* is the score obtained on a quiz which focuses on the core topics covered during the training including what to include in a business plan and where to get market information. *Business practices* is an index of business best practices for independent activities, such as accounting practices, having done a market assessment and a business plan, separating household and business accounts. Both measures are demeaned and standardized using the control group.

### 2.3.5 Estimation strategy

We estimate intent-to-treat treatment effects by taking differences in outcomes between treatment and control groups at endline. For a given outcome  $Y$  we run the following ordinary least squares (OLS) regression:

$$Y_{i,j} = \alpha_1 + \beta T_j + S_j + \epsilon_{i,j} \quad (2.1)$$

where  $i$  indexes the individual and  $j$  indexes the locality.  $\beta$  is the pooled Intention-To-Treat (ITT) estimate of the program's overall impact.  $T_j$  is assignment to treatment in locality  $j$  and

<sup>43</sup>Self-employment profits are aggregated across activities in the individual's portfolio. It is calculated based on the business cycle of each activity , and then an equivalent measure for 30 days is built.

<sup>44</sup>It includes all activities operating in the last 12 months, regardless of their status (active or not) at the day of the survey. Given it is a retrospective measure, we restricted to activities launched in the past 2 years.

$S_j$  includes stratification variables corresponding to the lotteries, i.e. a dummy for village or city district within each sub-prefecture. Robust standard errors are clustered at the locality level. Monetary outcomes are winsorized at the 99th percentile.

To obtain estimates of the relative impact of each treatment modality, we estimate the following equation:

$$Y_{i,j} = \alpha_1 + \beta_1 * T_{j,1} + \beta_2 * T_{j,2} + \beta_3 * T_{j,3} + S_j + \epsilon_{i,j} \quad (2.2)$$

$\beta_k$  coefficient corresponds to the effect of being in a locality assigned to intervention arm  $k$ .<sup>45</sup>

To study the direct impacts of the program, we compare individuals above the vulnerability cut-off across treated and control localities, as illustrated in Figure 2.2.<sup>46</sup> Because each treatment arm used a different vulnerability cut-off to select beneficiaries (see dot, plain and dashed lines for T1, T2 and T3 respectively, Figure 2.2), it is not possible to simply aggregate all selected individuals across arms. We use a common support cut-off in the analysis to pool observations across arms, which restricts the sample to 3,237 individuals. This approach comes at the cost of losing statistical power in comparison with estimating 2.1 separately for each intervention.<sup>47</sup>

### 2.3.6 Balance and attrition

Table 2.15 tests for balance between the treatment group and the corresponding sample in control localities (separately for the pooled treatment and each treatment modality). A common support cut-off is used, as explained above. Results show that the experiment achieved satisfactory balance between the pooled treatment and control group, as well as between treatment modalities.<sup>48</sup> There are very few statistically significant differences, and they remain of small magnitude. 1 variable out of 33 exhibits a difference at the 5 percent level for the pooled treatment (respectively 1 out

<sup>45</sup>Coefficient  $\beta_k$  can also be estimated separately for each treatment arm T1, T2 and T3. This provides more statistical power because the full sample of selected individuals is used in each regression. However, specification 2.2 is preferred because it allows for an easier comparison of the impacts across treatment arms.

<sup>46</sup>For the control group, the same cut-off is implemented so one can simulate “who would have been selected” if the locality was assigned to treatment rather than control.

<sup>47</sup>As shown in Figure 2.2, the “common support cut-off” used is stricter than the real cut-off implemented in two of the three arms. Pairwise comparisons between each treatment arm and the control group can be made using the whole sample of individuals above the selection cut-off for that specific arm. Since this does not require using a common cut-off, the approach leads to improvements in statistical power. In practice these gains are marginal. These results are not reported here, though when meaningful differences arise, they are noted in the text.

<sup>48</sup>Balance tables are shown for observations effectively interviewed at endline. Balance checks were also conducted on the full sample of eligible applicants. They also showed good balance. Balance checks for the spillover sample are presented in Chapter 3.

of 33 for the test of equality between treatments).<sup>49</sup>

## 2.4 Results: Direct economic impacts

Table 2.4 to Table 2.10 present ITT estimates for the direct impact of the intervention on the main economic outcomes and intermediary outcomes, capturing respectively employment, independent income-generating activities and hours worked (Table 2.4), earnings (Table 2.5), other welfare indicators (Table 2.6), assets and investments in independent activities (Table 2.7), savings (Table 2.8), credit (Table 2.9) and business practices and skills (Table 2.10). This corresponds to the specifications in equation 2.1 and 2.2. Panel A of each table corresponds to the overall impact of the (pooled) treatment. Panel B presents the relative impacts across modalities ( $\beta_1$  = “VSLA”,  $\beta_2$  = “cash-grant-with-repayment” and  $\beta_3$  = “cash grant”).<sup>50</sup> We provide p-values for the pairwise test of equality across modalities, and for the joint test  $\beta_1 = \beta_2 = \beta_3$ . Impacts are measured approximately 15 months after the end of the program, close to 24 months after the start of the interventions.

### 2.4.1 Main economic impacts

#### *Employment and number of independent activities*

The objective of the program was to improve livelihoods by supporting the development or creation of small independent activities. Therefore, we first document impacts on employment and the number of independent activities. We then show how the program affected earnings. We also document impacts on other welfare proxies, such as food security, household durable goods and expenditures on education.

While there are no impacts on the share of individuals employed, there are changes in individuals’ portfolio of activities. At the extensive margin, there is no impact on the overall share of individuals employed (Table 2.4, column 1, Panel A). This is not surprising in a context where almost everyone is engaged in some form of income-generating activity. 95% of individuals in

<sup>49</sup>The reported number of agricultural tools owned differs by 0.04 between treated and control localities (significant at the 5% level), but not across treatment arms. Being wage employed for the main activity, does not differ between pooled treatment and control, but the joint test suggests a small significant difference across treatment arms.

<sup>50</sup>For the pooled treatments specification, we use the “most restrictive” cut-off to pool together the treatments. In the control group, a similar cut-off is used to simulate who would have been selected.

the control group have an activity, and 91.6% have an independent activity. This is consistent with the evidence on the employment situation in sub-Saharan Africa and in Côte d'Ivoire. The program induces a small but significant impact on entry into self-employment (+3pp, Table 2.4, Panel A, column 2). This reflects a slight change in the composition of individuals' portfolio of activities, and offsets a small decrease in participation in wage employment (-3pp, Table 2.4, Panel A, column 3).

In line with these limited impacts on the level of employment, no impact is found on total hours worked (Table 2.4, column 7). This is consistent with the lack of effect on employment at the extensive margin, as well as with the population already working an important number of hours per week (40 hours per week in the control group).

However, we find larger changes at the intensive margin within the portfolio of independent activities. The intervention significantly increased the number of independent activities per individual by 0.32 (Table 2.4, Panel A, column 4). This means that on average, one out of three individuals has added a new activity to his/her portfolio. It represents a 10% increase in the number of activities, since individuals in the control group have an average of 3.13 activities.

Despite this impact on the number of economic activities, the program did not lead to substantial diversification. The increase in the number of activities by 0.32 can be decomposed between an increase in agricultural activities (+0.25) and non-agricultural activities (+0.08) (Table 2.4, column 5 and 6). As such, the net increase in activities is mainly driven by agricultural activities. This is also the type of activities that were most frequent, with 80% of the independent activities in the control group being agricultural. This is surprising since 70% of grant beneficiaries developed business plans for trading activities. However at follow-up, we do not observe this dynamic any longer: 77% of the increase in self-employment activities is driven by agricultural ones. The diversification attempt has failed, or was not pursued.

The modest magnitude of the impacts is intriguing. If one individual out of 3 added an activity, what happened with the other (two thirds) of beneficiaries? It does not necessarily mean that the economic activities of the remaining two-thirds are unchanged. The net impact on activities can hide very different dynamics. First, since many individuals were already engaged in independent activities, some could have invested in existing activities rather than start a new one. Second,

there might be replacement of a former activity with a new one, which means creating and stopping an activity in the same period of time. Both cases could be an efficient reallocation of capital towards more productive activities, even without observing net creation of activities. It is possible that some individuals launched new activities but that these activities stopped before the period captured by the follow-up survey. This would mean that activities were active for less than 3 months, and we do not find strong evidence to support this. Although we do observe a significant increase in the number of activities stopped in the last 12 months, the magnitude is small (+3pp).

Another way to document the dynamics of activity creation is to compare the business plans developed during the program with the activities listed by respondents in the follow-up survey.<sup>51</sup> Consistent with the observed impacts on the net number of activities, only 40% of the business plans developed by cash grant and cash grant-with-repayment beneficiaries can be matched to activities reported by respondents and operating during the 12 months preceding the survey. This suggests that most individuals did not launch their proposed activity, for example using the money in a different activity or for another purpose, as we further explore below.

So far, we have discussed results from the pooled treatment. Importantly, we do not find substantial differences across treatment modalities (Table 2.4, panel B). The change in the composition of employment through an increase in self-employment and decrease in wage employment tends to be driven by both grant interventions. The impact on wage employment is significantly lower in the VSLA modality (at the 10% confidence level). Hours of work are not significantly different across treatment modalities, aside from cash grant beneficiaries spending slightly less time on livestock. There is no significant difference in the impact on independent activities across treatment modalities. Overall, we conclude that the 3 intervention modalities had very similar impacts on beneficiaries employment and independent activities along the extensive margin.

### *Earnings, profits and welfare*

Table 2.5 documents program impacts on earnings, including income from wage employment and profits from independent activities (both agricultural and non-agricultural). The increase in the number of economic activities does not induce a significant impact on overall profits from

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<sup>51</sup>Given that respondents list up to 12 activities, it is quite unlikely that we miss one. The matching was done manually, comparing the title of the business plan project and the names of the activities.

independent activities (Table 2.5, Panel A, column 3). The point estimate on profits is of very small magnitude (+1,727 CFA). This is equivalent to less than 0.05 standard deviation. Since the study is well powered and the point estimate is very close to zero, we conclude that the program did not increase profits from independent activities. No impacts are found when analyzing profits separately from agricultural and non-agricultural activities either. Lastly, impacts on wage earnings are not significant either.

Overall, total earnings are unchanged by the pooled treatment. Similar results are found when analyzing the treatment modalities, with no significant differences between modalities. The only exception is a slight reduction in earnings from livestock in the cash grant arm.

Since a substantial share of independent activities relates to agriculture, we also conducted a more detailed analysis of impacts on agricultural revenues. Consistent with results based on self-reported profits, we do not find impacts on revenues from crop sales or livestock sales over the last 12 months (Table 2.5, Panel A, columns (5) and (6)). We do not find impacts on whether beneficiaries raise livestock, cultivate crops, use fertilizers, or on the surface area or the type of crops cultivated.<sup>52</sup>

To test for potential differences in self-employment earnings (total profits) along the distribution, we display the distribution of profits in the treatment and control group (Figure 2.4) and estimate quantile treatment effects (Figure 2.5). Results show that the profit distributions are not statistically different from each others' at any point (Figure 2.5). Quantile treatment effects suggest small positive but consistently non-significant effects along the distribution (Figure 2.4). We conclude that the lack of impacts on average profits does not hide strong changes along the distribution. We further explore mechanisms to explain the lack of impacts on average profits in the next section, and perform additional heterogeneity analysis later in this section.

Lastly, results on other welfare outcomes are consistent with the lack of impacts on earnings. While we do not have a full measure of household consumption, we do not find impact on food security measured through a food consumption score capturing dietary diversity over the 7 days before the survey. We do not find impacts on the number of durable goods owned by the household, or on household expenditures on education either.

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<sup>52</sup>Results are presented in Appendix, Table 2.16.

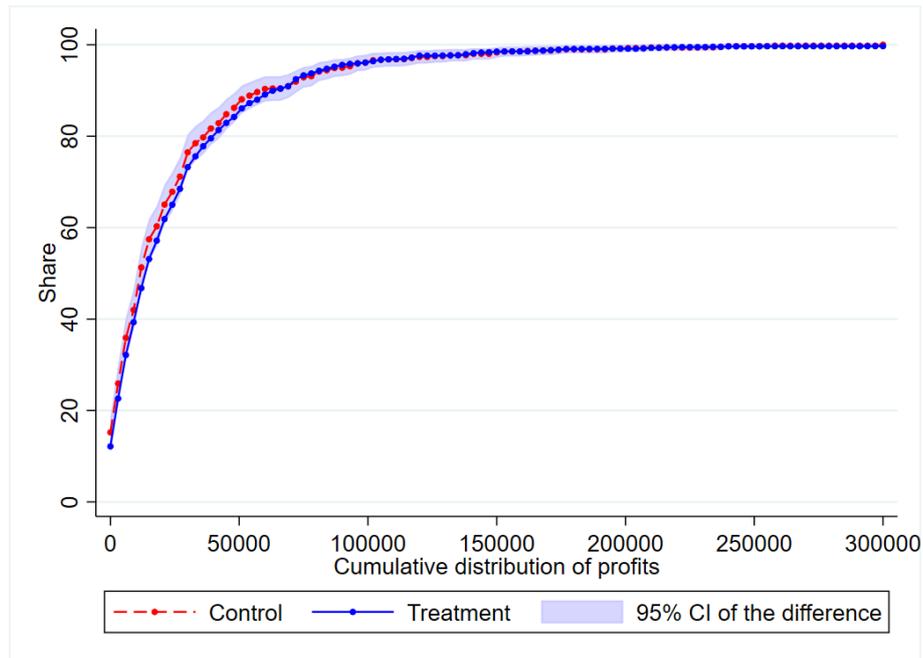


Figure 2.4 – Cumulative distribution of quantile treatment effects for self-employment earnings (total profits)

## 2.4.2 Mechanisms: Capital, Savings and Business Skills

Results so far highlight an allocation of work towards new activities that is not sufficient to increase profits. To better understand mechanisms, we analyze impacts on a range of intermediary outcomes related to core elements of the intervention package. We first test whether capital injection or access to savings instrument led to productive investments. We also test to what extent the business training improved business practices.

### *Investments in independent income-generating activities*

In this section, we document how the interventions affected investments in independent income-generating activities. We start with the cash grant and cash-grant-with-repayment interventions, which provided direct capital injection. Although grants are not conditional, they were explicitly provided to support the realization of the business plans prepared during the program. In the case of cash-grant-with-repayment, the framing is even stronger as there are follow-up visits after disbursement and a requirement to repay half of the grant. We then discuss impacts of the VSLA intervention, which was expected to facilitate investments through savings and access to credit.

Table 2.7 presents impacts on start-up capital and investments for independent activities. Interventions with capital injection induced productive investments in activities, so that beneficiaries

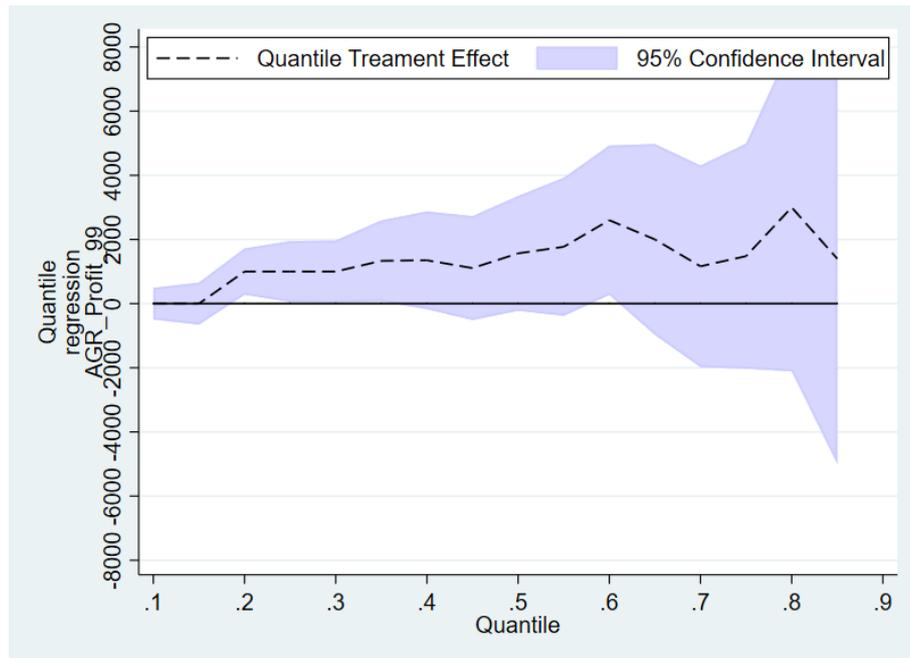


Figure 2.5 – Quantile treatment effects for self-employment earnings (total profits)

have more capital-intensive activities that operate at a larger scale. Past investments in independent activities can be assessed by analyzing the total capital used to start the activities or the value of productive assets in the activity at follow-up (column 1 and 2). The latter would include potential investments or dis-investments occurring between the launch of the activity and the follow-up survey. Results clearly show that the grant interventions led to investments in activities. The total starting capital for all independent activities nearly doubled, increasing by 105% for cash grant recipients and 88% for cash-with-repayment (+15,000 CFA, respectively +17,900 CFA) (Table 2.7, Panel B, column 1). Similar results are found when analyzing the value of productive assets across activities at follow-up, which is significantly higher for cash grant (+13,300 CFA, or 28%) as well as cash-grant-with-repayment recipients (+9,500 CFA, or 17%) (Table 2.7, Panel B, column 2).<sup>53</sup>

Quantile treatment effects suggest that impacts on capital (proxied by starting capital and value of business assets at follow-up) materialize mostly for quantiles above the median (Figure 2.6 and 2.7). This means that for half beneficiaries, these impacts are close to zero, suggesting no investment in assets taking place. This further reinforces the idea that a share of beneficiaries did

<sup>53</sup>While we observe impacts on initial investments in business activities, we do not find impacts on investment in the 6 months before the follow-up survey. Note that this indicator is only measured for the main activity, while the activity supported by the program is not listed as the “main” activity by most of the respondents. Indeed, when we compare business plans and the actual portfolio of activities, only 20% of the business plans matched correspond to the main activity of beneficiaries.

not use the grant for an economic activity.

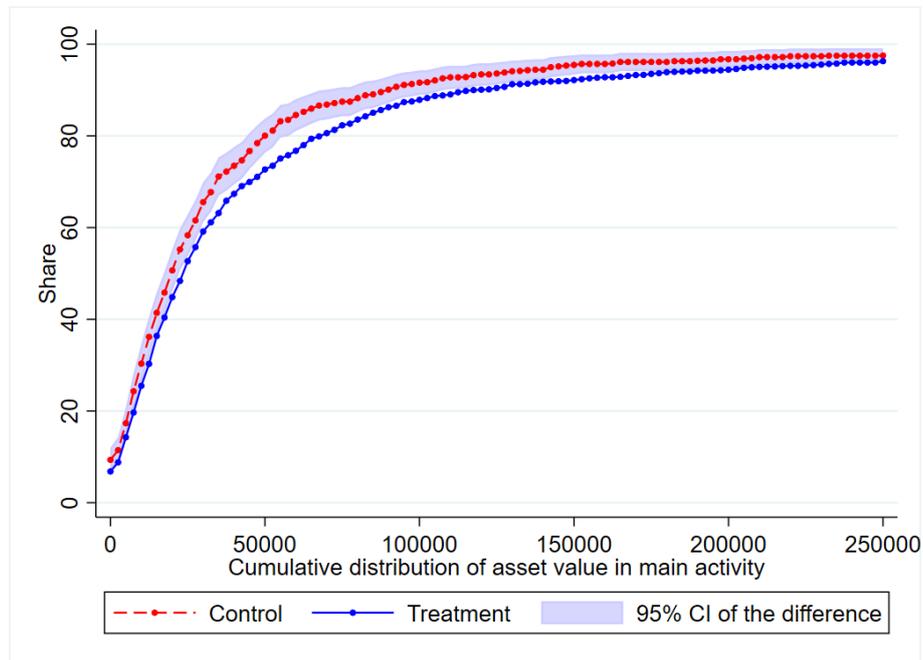


Figure 2.6 – Cumulative distribution of quantile treatment effects for the value of productive assets in independent activities

For the intervention without capital injection (VSLA treatment arm), impacts on the number of economic activities could be driven either by access to small loans through the newly created saving groups, or by access to a lumpy sum following the share-out at end of the saving group cycle. This lumpy sum corresponds to the savings accumulated plus interests (from loans provided by the saving group), and not to an external capital injection. The first VSLA cycle ended around September 2018 at a timing comparable with the capital injection interventions (which took place mostly in July 2018).

We previously mentioned that the impact of the VLSA intervention on the number of economic activities was not statistically different from the impact of the grant interventions. Interestingly, however, there are differences in initial capital investments between VSLA and grant arms, but there are no significant differences in the value of productive assets at follow-up. We find that the VSLA treatment increased starting capital by +7,700 CFA, a 50% increase (Table 2.7, Panel B, column 1). This is a slightly lower impact on investment than the cash-grant-with-repayment and cash grant intervention, which led to increases of 15,000 CFA, respectively +17,900 CFA. In the VSLA arm, the value of productive assets increases by 29% (+11,600 CFA, Table 2.7, Panel

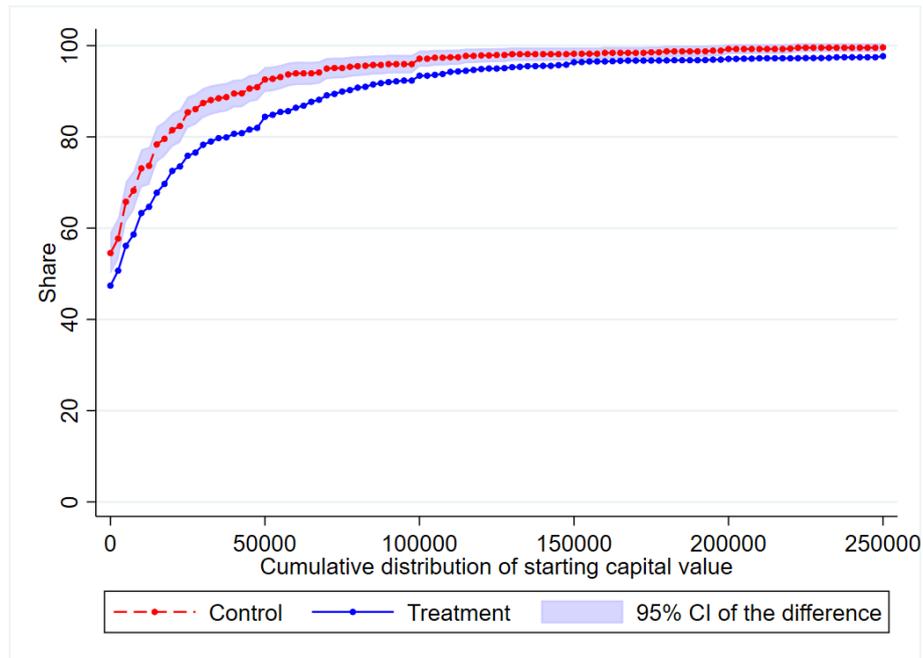


Figure 2.7 – Cumulative distribution of quantile treatment effects for the value of starting capital in independent activities

B, column 2).<sup>54</sup> However, this is not statistically different from the cash-grant-with-repayment (+9,500 CFA) or cash grant (13,300 CFA). If anything, in the VSLA arm there are larger impacts occurring at a later stage of activity development (investments) rather than initial stage (starting capital). The opposite is observed for both grant interventions. Despite similar impacts on economic activities at follow-up, this provides suggestive evidence that individuals in the VSLA treatment may be on a different trajectory than individuals in the grant treatments (cash grant and cash-grant-with-repayment).

In addition to being more capital intensive, beneficiaries' activities also make a greater use of labor input (Table 2.7, Panel A, column 4-6). The impact on labor input is consistent with an increase of activities' operating scale. For independent agricultural activities (which include farming and livestock raising), the number of workers increased by 12% in the cash-with-repayment arm (+6.68 people working over the last 30 days, whether paid or not, significant at 10% level). There is no significant change for non-agricultural activities. This is driven by work performed by other household members. Although positive, note that labor use impact in the cash grant arm is not significant. The VSLA intervention also induces an increase in the use of labor input in independent agricultural activities (+5.87 individuals working over the last 30 days).

<sup>54</sup>Similar to the two other interventions, we do not observe impacts on current investments in the main activity.

Lastly, there is no impact on assets others than those related to the income-generating activities. We do not find significant impact on the number of agricultural equipment assets (Table 2.7, Panel A, column 8) or household transportation assets (Table 2.6, column 5). Livestock holding does not significantly change either, as proxied by an index weighting animals by type (Table 2.7, column 7-8). This is consistent with other indicators showing no impact on income-generating activities related to livestock raising or livestock sales.

### *Savings*

In this section, we document how the grant interventions (cash-grant and cash-grants-with-repayment) affected savings, before discussing the VLSA intervention in more details.

We find strong impacts of the grant interventions on savings. Is it possible that part of the grant has been saved? While part of the capital provided by the grant interventions was invested in small-scale independent activities, the amounts invested do not account for the full capital injection. We can calculate the ratio of investments in starting capital as a share of the grants. Note that this does not account for other investments in the activity. We estimate that around 31.5% (15,000/47,500) of the cash-grants-with-repayment has been invested in business capital, respectively 19% (18,000/95,000) of cash grants.

Table 2.8 shows that the cash grant intervention increased savings by 29,300 CFA (Panel B, column 7). This corresponds to a 50% increase in savings, equivalent to close to 30% of the grant. For the cash-grant-with-repayment intervention, the point estimate also suggests an increase in savings (+13,317 CFA) which is approximately half the magnitude of the estimate for cash grant. After taking into account the amount to be repaid, this is a similar share of the grant. The effect is less strong for this intervention, but we cannot reject equality between the two coefficients of the grant interventions (T2 and T3).

The findings that beneficiaries save a large share of the cash grant likely explains the moderate effects on investments and earnings. Most of those savings are held in savings groups (50% of the saving stock is from ROSCA in the control group), which reflects the unavailability of formal savings instrument. The overall increase in savings is strongly driven by savings in ROSCA (+23,357 CFA) in the cash grant intervention (Table 2.8, Panel B, column 10). While ROSCAs may not be the saving instruments offering the highest returns, an accumulation of precautionary

savings may be rational if households face uncertain earnings. Uncertainty may be particular high in rural areas in a post-conflict setting for households that do not have access to formal credit markets. To further investigate savings behavior, we analyze quantile treatment effects on savings (Figure 2.8). One would expect stronger impacts in the bottom of the distribution if people accumulate precautionary savings. We see that impact arises mostly in the bottom of the distribution, between 25% and 65% quantiles of the distribution (the bottom quantiles being at zero).

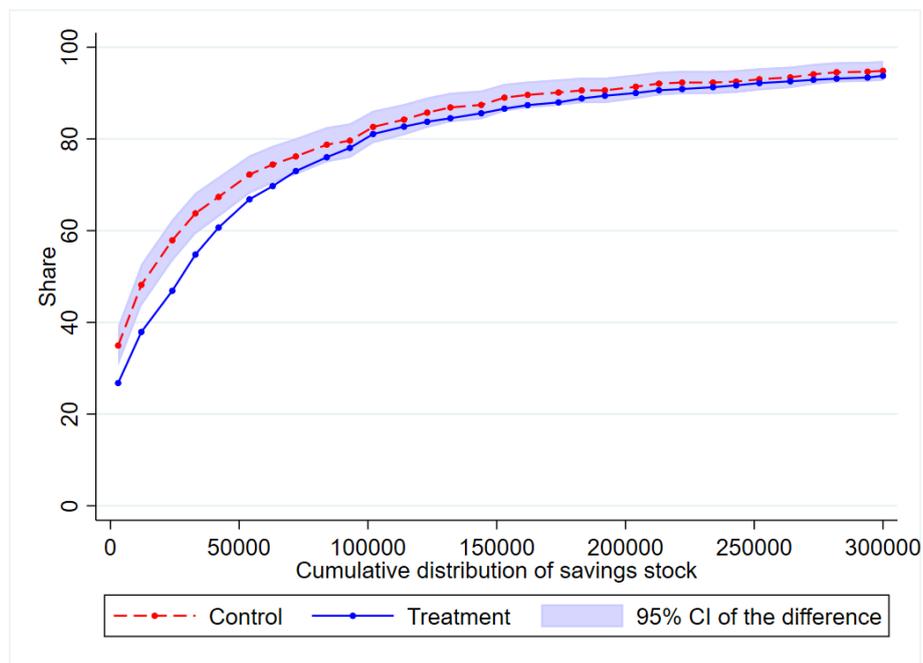


Figure 2.8 – Cumulative distribution of quantile treatment effects for (total) Savings stock

The VSLA intervention (T1) affected savings through different channels than the grant interventions. The VSLA intervention was successful in increasing participation in “improved” savings groups and led to sustained changes in savings behavior at follow-up. The VSLA intervention increased individuals’ propensity to save from 82 to 88 percent (Table 2.8, Panel B, column 1). Most importantly, the intervention shifted propensity to save in informal means to “improved” savings groups that also provide credit (VSLAs). 30% of control individuals save in ROSCAs and 60% in other informal ways (cash, informal collector), but only 17% participate in a VSLA. In contrast, 54% of individuals in the VSLA treatment arm participate in a VSLA at follow-up (+38pp, column 5). We observe an important substitution away from ROSCAs (-13pp impact on ROSCA participation, decreasing from 30% to 18%) and marginally from other informal sources too (-6pp, significant at 10% level) (Table 2.8, Panel B, column 3 and 6). VSLAs are considered

more efficient savings instruments than ROSCAs (“tontine” in West Africa), which are less secure and less flexible. Most importantly, ROSCAs do not provide access to loans while VSLA do as they operate as an informal rural bank. ROSCAs provide rotating payments at different pre-determined times of the year while VSLAs offer full flexibility to take a loan when there is a need. The 38pp impact on VSLA participation is noteworthy because it shows sustained impacts 15 months after the end of the program. Given that VSLA cycles are 9 months, this means that individuals not only joined VLSAs, but remained in their group and launched new savings cycles. These results are also interesting as some VSLA interventions in the literature have suffered from moderate take-up.<sup>55</sup>

However, the VSLA intervention did not increase the total amounts saved. The total savings stock remains similar (respectively 59,000 CFA and 50,000 CFA in the control and VSLA arm, but not significantly different from each other). Rather, as observed for the propensity to save, the intervention allowed beneficiaries to save through enhanced (and likely more efficient) savings instruments. The increase in the amount saved through VSLAs (+9,500 CFA) is offset by a decrease in savings held in ROSCAs (-15,000 CFA) (Table 2.8, Panel B, column 9-10). As such, observed impacts on economic activities are not explained by a larger savings mobilization: VSLA beneficiaries save similar amounts than individuals in the control group.

The VSLA credit facility likely explains part of the impacts on economic activity. The increased participation in VSLAs leads to an increase in credits obtained from these groups. The share of individuals who took a credit over the last two years significantly increased from 57% to 70% in the VSLA arm (Table 2.9, Panel B, column 1). In comparison in the grants interventions, there is no meaningful impact on credit.<sup>56</sup> The total amount of credits taken has significantly increased by one-third, reaching 30,423 CFA for the last two years (+7,687 CFA, Table 2.9, column 6). Again, there is no change in credit amounts for other interventions. This impact is fully driven by credits taken from saving groups, which amounts were initially low (3,700 CFA in the control group) and nearly tripled thanks to the intervention. 41% percent of beneficiaries have taken a credit with a savings group in the last two years, versus 10% percent in the control group (Table 2.9, column 3). The sustained impacts on VSLAs still operating at follow-up contributes to medium term

<sup>55</sup>Take up is only 31.6% across the three countries in Karlan et al. (2017), 37% in Beaman et al. (2014) and 45% in Ksoll et al. (2016).

<sup>56</sup>Only the coefficient for cash-grant-with-repayment is marginally significant at 10%, and the 4pp magnitude is low compared to the 12pp impacts in VSLA arm.

impacts on credit: 35% of beneficiaries have taken a credit in the current cycle of their VSLA (Table 2.9, column 5).

Note that the program also marginally increased VSLA participation in other treatment modalities, although the methodology of savings groups was not directly developed with them (+7pp and +8pp respectively for cash-grant-with-repayment and cash-grants, significant for the first modality only) (Table 2.9, column 5, Panel B). The VSLA model promotes replications and spillover have been consistently documented in the VSLA literature (see Beaman et al. (2014), Ksoll et al. (2016) and Karlan et al. (2017)). In our context, the increase in VSLA participation is substantially smaller than in VSLA treatment localities (one fifth of the coefficient). What we find is similar to other studies.<sup>57</sup> Formally, we identify treatment effects net of spillovers across localities. This means that when comparing VSLA intervention with the other grant interventions, differences might be underestimated. Still, spillovers are limited in magnitude and their magnitude does not seem large enough to explain key results such as the lack of impact on profits.

### ***Business skills and practices***

We can also document whether the entrepreneurship training effectively improved business knowledge (i.e. what you know you should do) and business practices (i.e. what you do in reality). As mentioned above, knowledge is measured through a standardized score to a quiz related to the content of the entrepreneurship training. We build a standardized index of self-reported business practice based on whether respondents declare having done a market assessment, a business plan, using formal bookkeeping or keeping business accounts separate from household accounts in their main independent activity. Table 2.10 presents both the business practices index, and the underlying variables. We observe significant overall impacts on both the business knowledge score (+0.2 standard deviations, Table 2.10, Panel A, column 1) and the business practice index (+0.2 standard deviations, Panel A, column 2). While these impacts are statistically significant, the magnitude is moderate.<sup>58</sup> The percentage increase in business practices is high given the very

<sup>57</sup>In Karlan et al. (2017), whose design allowed to quantify replications, they observed a 19.4% participation rate in VSLA in neighboring villages. Ksoll et al. (2016) finds that 21% of control villages had joined VSLAs in treatment localities. In our case, we estimate participation rates between 17% and 24.5% in other localities.

<sup>58</sup>The order of magnitude is comparable with the impact of a similar training provided to public works beneficiaries in Côte d'Ivoire. Bertrand et al. (2017) find a +0.18 standard deviation when comparing public works beneficiaries receiving an entrepreneurship training to a control group. These results were based on a similar quiz. The public works program entrepreneurship training curriculum was the basis of PRISE training curriculum. The PRISE training was less intensive (55h versus 80h), but some of the visual aids and pedagogical supports were

low levels in the control group. For instance, only 4% of beneficiaries use a formal bookkeeping system and 38% keep business accounts separately from household accounts.<sup>59</sup>

The differences of impact between the treatment modalities reflect differences in implementation and follow-up intensity (Table 2.10, Panel B). Both grant interventions show substantially higher impacts on knowledge compared to VSLA treatment arm (p-values of joint and pairwise tests involving T1 strongly reject equality across arms). All interventions receive the same entrepreneurship training delivered by the same trainers. Training attendance differs across arms (around 25pp less in VSLA intervention, Table 2.2) but this is not enough to explain the difference in point estimates. The difference in impacts likely partly captures local follow-up on the content of the training, which did not take place in the VSLA intervention. For both cash-grant and cash-grant-with-repayment localities, NGO trainers selected and trained a “community expert” (from each locality) responsible for supporting beneficiaries after training delivery (clarifying training content, in particular). Regarding business practices, cash-grant-with-repayment intervention stands out (again, p-values of join and pairwise tests involving T2 strongly reject equality across arms). This suggests that stronger individual follow-up with beneficiaries contributed to the application of training content. Compared to other interventions, beneficiaries received several visits from the NGO to support the start of the activity planned during the training in the first months after disbursement.

### 2.4.3 Discussion and heterogeneity of impacts

#### *Discussion of direct economic impacts*

This section further discusses how a lack of impacts on earnings can be interpreted given the observed impacts on economic activities and investments. Overall, it is likely that the observed impacts on investments were not large enough to generate returns sufficient to increase profits.

On average, the cash grant intervention provided 95,000 CFA. Results show that 18,000 CFA were invested in start-up capital in the cash grant intervention and 29,000 CFA were saved. For the cash-grant-with-repayment intervention, 15,000 CFA were invested in start-up capital, 13,000

further improved.

<sup>59</sup>Note that all beneficiaries had to develop a simple business plan and do market assessment during the training. However, the indicators capture business practices of the main independent activity at the time of the survey. For most beneficiaries, the main activity listed in the follow-up survey is not the one developed during the program. As such, the low share of people reporting having prepared a business plan or a market assessment for their main independent activity is not inconsistent with data on program take-up and training completion rates in Table 2.2.

CFA were saved, and 15,000 CFA were repaid. This leaves 48,000 CFA from the cash grant and 52,000 CFA from the cash-grant-with-repayment for other business investments or expenditures other than capital investments and savings. This remaining amount could have been spent on operating expenses in independent activities, or investments others than starting capital. We do not detect impacts on food security, on expenditures in health and education, or on household assets at follow-up, but we cannot rule out that part of the grants has been used for consumption or other household expenditures.

Given these results, the lack of impacts on earnings is unlikely to be explained by the lump sum grants not being large enough. An important explanation seems to be that a large share of the capital is saved. This may reflect precautionary savings motives, in particular in a post-conflict rural setting where households face uncertainty, do not have access to formal credit markets and do not receive regular transfers for consumption support as in other graduation interventions.

The lack of significant impact on profits may also partly be explained by investments being spread across several activities rather than concentrated in one activity with potential for high returns. Beneficiaries have on average 3.45 activities. As seen before, most of the activities developed during the program are secondary activities (80% of the activities matched to business plans are not the main activity in the portfolio of beneficiaries).

Some may think that it is too early to observe changes in profits because investments or changes on business practices take time to materialize in the profits of the activities.<sup>60</sup> Some papers on entrepreneurship training have highlighted the fact that changing business practices takes time and are likely to pay back in the medium to long run rather than in the short run. [McKenzie and Puerto \(2017\)](#) is one example. However, and most importantly, the timeline at which the study assesses impacts is aligned with the timeline from graduation study findings very large effects on earnings. Since most activities for which beneficiaries developed business plans are not active at follow-up, it seems unlikely that large returns are to be expected in the future.

Finally, we can compare the economic impacts of the interventions with or without capital injection. Given the lack of impact on profits, we look at other relevant indicators: investment in start-up capital and the value of current assets, which were both significantly impacted by

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<sup>60</sup>[Bloom et al. \(2013\)](#) provide one example of that, in the case of medium to large firms. However other studies suggest that business practices themselves revert to previous habit in the long run.

the program. VSLA enabled individuals to achieve comparable investments in activities without external funds. The impact on starting capital for the VSLA intervention is half of the impact for interventions with capital injection (cash-grant-with-repayment and cash grant), respectively +7,700 CFA compared to +15,000 CFA and +17,900 CFA (Table 2.7, column 6, Panel B). Again, when looking at the value of current assets in activities, beneficiaries of saving groups are not worse than beneficiaries from other interventions: the magnitude of the coefficient is comparable across interventions, and the joint test across arm suggests no statistical differences (p-value of the joint test is 0.80). However, to achieve such investments, there was no capital injection. VSLAs provided a new instrument widely used among beneficiaries: easier access to credit, at a reasonable rate (defined by the group). This is noteworthy since the capital transferred is the costliest part of the package, for cash and cash-with-repayment interventions. In addition, while the VSLA intervention requires regular supervision and support during the first cycle (around 9 months), it is designed to be sustainable after the end of the program. Endline data suggest that many groups have started a second cycle on their own, as mentioned above. Overall, the VSLA intervention associated with lower costs seems to perform at least as well as grants.

### ***Heterogeneity of economic impacts***

Tables 2.11 to 2.12 (column 1-6) and Appendix 2.17 to 2.18 present the results of heterogeneity analysis along key dimensions. We first look at women and youth (defined as less than 35 years-old in Côte d'Ivoire). These constitute two target groups of interest that represent respectively 69% and 56.5% of beneficiaries sampled at follow-up. Youth benefit significantly more from the program and its various modalities (Table 2.11). Indicative of the constraints they face, youth are less likely to be active in self-employment (-0.08pp difference between youth and older individuals in control group, column 1) and have fewer independent activities (-0.43 activities, column 2). The program partly offsets these differences by increasing youth participation in self-employment. Access to capital increases for youth: start-up investment amounts double (a 111% increase compared to youth in the control group). The impact is 2.5 times larger for youth than for older participants (+17,666 CFA for youth, compared to +7,085 for older participants). The youth group is also the one driving the overall program impact on savings stock. Youth savings increase by 43% while there is no significant change in savings for the older group.

Conversely, there is no significant heterogeneity in program impacts by gender (Table 2.17).

Businesses run by women in the control group rank lower than those run by male on a set of economic indicators (column 3 to 5). For example, profits in female-run activities are 22% less than for male, and their productive assets 20% lower too. The total impacts on the number of activities or on capital are significant for both men and women, but there is no significant difference between the two (Appendix Table 2.17, Panel A, column 3 to 5). This means that the program impacted men and women in the same way. While higher impacts could have been expected on women in the VSLA arm (women being more likely to participate in saving groups in Sub Saharan Africa), the interaction coefficient is again not statistically different from zero (Panel B, column 4-5).

## 2.5 Conclusion

This chapter reports results from the randomized controlled trial of an economic inclusion program providing training and access to capital or savings to vulnerable individuals in Côte d'Ivoire. The study makes three contributions. First, in the spirit of graduation programs, it measures the direct impacts of an integrated intervention on economic outcomes. Second, it tests the relative effectiveness of 3 alternative instruments to relax capital constraints: cash grants, cash-grant-with-repayment and enhanced saving groups known as village savings and loan associations (VSLA).

The intervention induced a slight shift from wage employment to independent work, in particular agricultural activities. It was successful at increasing the number of income-generating activities as well as the level of productive assets (start-up capital as well as current productive assets) in these activities. However, the increase in the number of independent economic activities or their capitalization is not sufficient to increase business profits or household welfare. These results hold across the 3 intervention arms.

The intervention modalities induce investments in income-generating activities through different mechanisms, however. The two grant interventions led to increases in productive assets (start up capital and current assets in independent activities), but also had strong impacts on savings. The fact that beneficiaries save a large share of the grant likely contribute to explain why impacts on investments are not sufficient to translate into higher earnings. Overall the repayment condition on the grant does not seem to be effective at further encouraging investments : the two arms lead

to results of the same magnitude.<sup>61</sup> The VSLA intervention led to similar impacts on productive assets than the grant interventions, and smaller but positive and significant impacts on start-up capital. Although the level of savings is unchanged overall in VSLA arm, it redirected savings to VSLAs, a more secure instrument. In rural areas with low reach of formal financial institutions, the VSLA intervention also increased credit. Access to a more efficient (although informal) credit and saving technology facilitated investments in income-generating activities, even in the absence of capital injection. Our results are consistent with the literature, in particular [Karlan et al. \(2017\)](#) who found improved business outcomes (in terms of number of activities or use of non-household labor) and an increased use of credit for investments, but no further impacts on income or consumption.

Results from the experiment contrast with the large positive impacts on total income and welfare documented by graduation programs with asset transfers ([Banerjee et al. \(2015b\)](#) in 6 countries, [Bandiera et al. \(2017\)](#) in Bangladesh or [Bedoya et al. \(2019\)](#) in Afghanistan), or by some integrated micro-entrepreneurship programs such as [Blattman et al. \(2014\)](#) in Uganda or [Blattman et al. \(2016\)](#) in Liberia .

The absence of consumption support in the intervention (usually part of graduation packages) might explain the modest impacts. Consumption support, in the form of regular food or cash transfers, have been shown to help households smooth consumption and manage risk. In graduation programs, beneficiaries usually receive consumption support until they can obtain revenues from the asset transfer, and avoid dis-saving or selling the asset right away. In the absence of consumption support, beneficiaries seem to have consumed and saved a large share of the grants. Interestingly, a disruption in consumption support for a multi-faceted intervention in Yemen led to similar muted impacts ([Brune et al. \(2020\)](#)).

Compared to most multi-faceted economic inclusion programs, the intervention we study did not induce a diversification of income-generating activities. Many graduation programs transfer livestock, and these programs have large increases in livestock revenues. We only find muted occupational changes in our study. Interestingly, many beneficiaries intended by themselves to diversify their activities towards trading.<sup>62</sup> However at follow-up, we do not observe much of an

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<sup>61</sup>However, note that impacts are similar but conditional on a smaller value of available grant (half of it) in the case of the grant-with-repayment arm.

<sup>62</sup>While 60% of their work time is devoted to agricultural activities (as measured in the control group), 70% of

impact on non-agricultural activities. Impacts are driven by (non-livestock) agricultural activities, which was the main occupation at baseline. Returns on investments made in those activities were not high a year later, with no impact on profits. It is unclear why beneficiaries' diversification attempts failed. Does it mean that grants should be further conditioned or framed toward specific occupations? It is not clear this is necessary, since there is empirical evidence on several interventions with cash grants that led to occupational changes ( [Blattman et al. \(2016\)](#), [Sedlmayr et al. \(2020\)](#), [Macours et al. \(2019\)](#), or [Bossuroy et al. \(2020\)](#)).

Of course, differences between country settings may matter too. Our intervention is implemented in a post-conflict context, where needs for consumption smoothing and precautionary motives for savings may be particularly prevalent. This may explain why individuals save a large share of the cash grants, in the absence of consumption support. In an experiment in South Sudan comparing an asset-based graduation program with an unconditional cash transfer, a large share of the cash has gone to short-term consumption and savings which increased by 45% (compared with a 49% increase in our cash grant arm) ([Chowdhury et al. \(2017\)](#)). There are two examples of graduation programs in fragile context (Afghanistan and Niger) for which large shares of savings out of the transfer were not observed ([Bedoya et al. \(2019\)](#) and [Bossuroy et al. \(2020\)](#)), and both included regular consumption support. Conversely in South Sudan (TUP arm) and Yemen, impacts are muted and similar to what we observe : an increase in assets and savings, but no impact on household's total income and medium-term consumption.<sup>63</sup> These results suggest that consumption support plays a role in the effectiveness of graduation programs, even though we cannot infer what the marginal impact of consumption support would be on top of the rest of the package in our setting. The complementarities between cash transfers for consumption support and other elements of multi-dimensional graduation programs would deserve additional research in post-conflict settings.

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grant beneficiaries developed business plans for trading activities. However at follow-up, 77% of the increase in self-employment activities is driven by agricultural ones.

<sup>63</sup>Comparing the net impact on assets and savings to the value of the transfer across studies is difficult, both in terms of reported results and timing of measurement. Most studies observe that the impact of the program on asset values is lower than the cost of the assets, except in Ethiopia (only country in [Banerjee et al. \(2015b\)](#)) and Bangladesh ([Bandiera et al. \(2017\)](#)). Yemen would be a good comparison since the consumption support was disrupted and there is no evidence of increase in income. The increase in wealth (which includes assets and savings) is around 49% of the initial transfer ([Brune et al. \(2020\)](#)). We find a similar rate, but we measure it 15 month after cash grants, instead of 3 years after. [Banerjee et al. \(2015b\)](#) provide a cost-benefit analysis showing that in 5 out of the 6 countries, the share of the value of assets over transfer is below 50% (even below 20% for 3 of them), after two years. In Afghanistan, this share is between 56% and 73% after one year (authors' own calculation). However for the latter, part of the savings could be explained by increased income rather than saving from the transfer received.

Our study offers a slightly more optimistic view than the current evidence on the potential of saving components within multi-faceted programs. Savings facilitation has not been documented as a key component driving the effectiveness of integrated programs so far. [Sedlmayr et al. \(2020\)](#) test the additional impact of saving groups on top of a micro-entrepreneurship intervention in Uganda. They find no impact on asset accumulation. Our intervention includes similar saving groups but without cash grants and find substantial impacts on the capital of independent activities. [Banerjee et al. \(2018\)](#) decomposes the standard TUP package in Ghana, isolating its saving component (formal savings account with field collector). Note that graduation programs rarely include the organization of enhanced saving groups similar to VSLAs, but rather other forms of savings support.<sup>64</sup> They find that the savings only intervention is much less efficient than the graduation package with savings, in particular to promote spending savings in investments. One of the proposed interpretations is that coaching and handholding provided in the integrated program constitute a nudge towards investment. Our paper is consistent with this interpretation. We bundle a VSLA intervention with business training : the training (in particular business plan development) plays the role of the nudging, and the group support within the VSLA is close to coaching. Our results on asset accumulation without capital injection but VSLA organization only highlight the potential of enhanced saving groups. More empirical evidence on its effectiveness when coupled with micro-enterprise support or graduation programs would be welcomed.

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<sup>64</sup>Access to formal saving accounts is much more frequent across implementation examples, sometimes coupled with field collectors helping to bring savings to the account (Afghanistan, Ethiopia, Ghana, Honduras, India) ([Banerjee et al. \(2015b\)](#), [Bedoya et al. \(2019\)](#)). Other forms of savings support include microfinance groups (Bangladesh), ROSCA (Pakistan) or traditional saving groups (Peru, Yemen) but without credit opportunity ([Banerjee et al. \(2015b\)](#), [Brune et al. \(2020\)](#)).

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## Appendix

## A Result tables

Table 2.4 – Impacts on main employment outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Employment (Has an activity of any type)	Self employed (at least 1 activity)	Wage employed (at least 1 activity)	# Independent Activities per indiv.	# Non-Ag. Independent Activities per indiv.	# Agricultural Independent Activities per indiv.	Total hours worked (last 7 days)	Hours worked in wage employment	Hours worked in self employment (non ag.)	Hours worked in independent ag. activities	Hours in livestock raising	Total # days worked in agriculture (last 12 mths)
<b>Panel A. Pooled Estimates</b>												
Program Treatment (ITT)	0.01 (0.01)	0.03** (0.01)	-0.03** (0.01)	0.32*** (0.10)	0.08* (0.04)	0.25** (0.11)	0.78 (1.50)	-1.14** (0.52)	1.32 (1.25)	1.02 (1.47)	-0.39 (0.31)	5.80 (15.08)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control Observations	95.0% 2,620	91.6% 2,620	10.4% 2,620	3.13 2,620	0.67 2,620	2.45 2,620	40.08 2,620	3.08 2,620	10.59 2,620	24.02 2,620	2.25 2,620	230.36 2,620
<b>Panel B. Treatment Arm Estimates</b>												
VSLA (T1) (ITT)	0.01 (0.01)	0.02 (0.01)	-0.01 (0.02)	0.30** (0.12)	0.05 (0.06)	0.25* (0.13)	2.35 (1.88)	-1.59*** (0.57)	1.00 (1.47)	0.90 (1.65)	-0.45 (0.33)	14.77 (15.98)
Cash Grant with repayment (T2) (ITT)	0.02 (0.01)	0.03** (0.01)	-0.04** (0.02)	0.37*** (0.12)	0.09* (0.05)	0.28** (0.13)	0.09 (1.68)	-0.43 (0.61)	1.80 (1.52)	1.59 (1.90)	-0.08 (0.38)	-3.18 (17.39)
Cash Grant (T3) (ITT)	0.01 (0.01)	0.04*** (0.01)	-0.05** (0.02)	0.27 (0.18)	0.10 (0.07)	0.17 (0.21)	-0.89 (1.82)	-1.60** (0.65)	1.06 (1.73)	0.13 (2.17)	-0.99*** (0.34)	7.77 (21.99)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	95.0%	91.6%	10.4%	3.13	0.67	2.45	40.08	3.08	10.59	24.02	2.25	230.36
p-value T1=T2	0.65	0.64	0.06	0.58	0.58	0.78	0.20	0.02	0.59	0.71	0.20	0.19
p-value T2=T3	0.82	0.38	0.80	0.57	0.86	0.58	0.56	0.99	0.97	0.71	0.00	0.59
p-value T1=T3	0.81	0.17	0.05	0.85	0.53	0.70	0.09	0.04	0.67	0.52	0.05	0.72
p-value T1=T2=T3	0.90	0.35	0.09	0.79	0.80	0.85	0.22	0.04	0.84	0.81	0.01	0.43
Observations	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620

Robust standard errors clustered at locality level. Hours and days worked are winsorized at the 99th percentile. Hours (columns 7 to 11) are measured on the last 7 days. Column 12 refers to farming only (livestock rearing excluded). The number of independent activities per individual is based on the list of up to 12 operating activities.

\* p <.1, \*\* p <.05, \*\*\* p <.01

Table 2.5 – Impacts on earnings and profits

	(1)	(2)	(3)	(4)	(5)	(6)
	Earnings in Wage Employment (monthly)	Earnings in Self Employment (Profits, monthly)	Earnings in Self Employment : Non Ag. Activities (Profits, monthly)	Earnings in Self Employment : Ag. Activities (Profits, monthly)	Sales from farming activities (last 12 mths)	Sales from livestock activities (last 12 mths)
<b>Panel A. Pooled Estimates</b>						
Program Treatment (ITT)	-68.05 (718.09)	1,726.78 (1,777.12)	351.97 (1,383.47)	1,386.09 (1,278.23)	19,909.81 (17,808.79)	-2,091.02 (1,544.53)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control Observations	3,057.36 2,615	24,050.42 2,620	13,155.21 2,620	10,793.20 2,620	124,989.51 2,620	8,013.67 2,620
<b>Panel B. Treatment Arm Estimates</b>						
VSLA (T1) (ITT)	458.48 (928.10)	859.10 (2,372.50)	-14.67 (2,070.74)	819.54 (1,345.97)	26,657.37 (24,159.71)	-685.07 (1,990.21)
Cash Grant with repayment (T2) (ITT)	-200.52 (837.45)	2,393.22 (2,217.70)	182.68 (1,468.91)	2,242.47 (1,766.89)	12,659.68 (19,719.88)	-2,162.26 (1,609.86)
Cash Grant (T3) (ITT)	-852.09 (817.22)	2,000.59 (3,098.32)	1,497.98 (2,075.77)	602.27 (2,540.08)	22,505.26 (28,995.56)	-4,839.52*** (1,546.04)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	3,057.36	24,050.42	13,155.21	10,793.20	124,989.51	8,013.67
p-value T1=T2	0.48	0.57	0.92	0.42	0.55	0.36
p-value T2=T3	0.40	0.91	0.53	0.55	0.74	0.02
p-value T1=T3	0.14	0.74	0.55	0.93	0.89	0.01
p-value T1=T2=T3	0.32	0.85	0.79	0.70	0.83	0.01
Observations	2,615	2,620	2,620	2,620	2,620	2,620
Robust standard errors clustered at locality level. Earnings and sales are in CFA franc and winsorized at 99%. "Ag." stands for Agricultural. * p <.1, ** p <.05, *** p <.01						

Table 2.6 – Impacts on welfare indicators

	(1)	(2)	(3)	(4)	(5)
	Food Consumption Score	Expenditures in Education	Well Being index (z-score)	# assets owned : household durable goods	# assets owned : transportation equipment
<b>Panel A. Pooled Estimates</b>					
Program Treatment (ITT)	0.79 (0.99)	-4,375.47 (3,550.97)	0.08 (0.06)	0.04 (0.17)	-0.02 (0.04)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes
Mean in Control	52.57	48,798.88	-0.00	3.69	0.31
Observations	2,618	2,617	2,618	2,618	2,618
<b>Panel B. Treatment Arm Estimates</b>					
VSLA (T1) (ITT)	0.22 (1.16)	-2,018.40 (3,983.48)	0.05 (0.07)	0.06 (0.21)	-0.01 (0.05)
Cash Grant with repayment (T2) (ITT)	1.01 (1.19)	-4,898.63 (4,218.46)	0.08 (0.07)	-0.02 (0.20)	-0.04 (0.05)
Cash Grant (T3) (ITT)	1.48 (1.70)	-8,050.23* (4,487.61)	0.14 (0.09)	0.15 (0.25)	0.00 (0.07)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes
Mean in Control	52.57	48,798.88	-0.00	3.69	0.31
p-value T1=T2	0.50	0.44	0.60	0.70	0.51
p-value T2=T3	0.47	0.14	0.50	0.72	0.92
p-value T1=T3	0.79	0.45	0.32	0.50	0.53
p-value T1=T2=T3	0.69	0.34	0.60	0.79	0.73
Observations	2,618	2,617	2,618	2,618	2,618

Robust standard errors clustered at locality level. Expenditures are in CFA franc.

Expenditures and number of assets are winsorized at 99%.

The *Food Consumption Score* (FCS) is based on World Food Programme definition.

The *Well Being index* includes the CESD Positive Affect scale, the Rosenberg Self esteem scale and a self-assessed measure of life satisfaction.

\* p <.1, \*\* p <.05, \*\*\* p <.01

Table 2.7 – Impacts on business outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total start-up capital (all activities)	Value of productive assets (all activities)	Investments in <i>main</i> activity (last 6 mths)	# individuals working in independent activities (all types) (last 30 days)	# individuals working in independent non ag. activities (last 30 days)	# individuals working in independent ag. activities (last 30 days)	Livestock ownership index	# assets owned : agricultural equipment
<b>Panel A. Pooled Estimates</b>								
Program Treatment (ITT)	12,693.22*** (2,432.35)	10,990.68*** (3,240.73)	1,296.55 (1,042.82)	6.49** (3.08)	0.67 (0.74)	5.75* (3.05)	-0.08 (0.06)	0.41* (0.21)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	15,260.21	39,538.50	5,094.23	60.93	6.79	54.13	0.39	5.26
Observations	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,618
<b>Panel B. Treatment Arm Estimates</b>								
VSLA (T1) (ITT)	7,666.52** (2,998.70)	11,581.19*** (3,947.51)	2,410.21 (1,614.32)	7.01** (3.52)	1.04 (0.99)	5.87* (3.45)	-0.11* (0.06)	0.37 (0.26)
Cash Grant with repayment (T2) (ITT)	14,970.75*** (3,162.20)	9,452.43** (3,873.84)	37.43 (911.81)	7.32* (3.89)	0.59 (0.83)	6.68* (3.82)	-0.03 (0.08)	0.40 (0.26)
Cash Grant (T3) (ITT)	17,897.50*** (5,215.74)	13,283.04** (6,282.30)	1,867.79 (1,929.52)	3.49 (5.62)	0.12 (1.06)	3.37 (5.57)	-0.15** (0.06)	0.52 (0.34)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	15,260.21	39,538.50	5,094.23	60.93	6.79	54.13	0.39	5.26
p-value T1=T2	0.05	0.61	0.11	0.94	0.64	0.83	0.28	0.90
p-value T2=T3	0.60	0.56	0.33	0.52	0.65	0.57	0.09	0.73
p-value T1=T3	0.07	0.79	0.80	0.54	0.43	0.66	0.40	0.67
p-value T1=T2=T3	0.07	0.80	0.21	0.79	0.73	0.85	0.23	0.91
Observations	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,618

Robust standard errors clustered at locality level.

Start-up capital, value of assets and investments are in CFA franc and winsorized at 99%. The main activity is directly identified by the respondent among the list of independent activities undertaken. The number of assets owned and the livestock headcounts are measured at the time of survey. The *livestock index* aggregates the number of heads across species (cows, pigs, goats, sheep, poultry and rabbits) taking into account relative importance.

\* p < .1, \*\* p < .05, \*\*\* p < .01

Table 2.8 – Impacts on savings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Has Saved (last 2 yrs)	Has Saved : formal savings	Has Saved : oth. informal savings	Has Saved: any saving group (last 12 mths)	Participate in a VSLA (currently)	Participate in a ROSCA (currently)	Savings stock (Total)	Savings stock : formal savings	Savings stock : VSLA	Savings stock : ROSCA	Savings stock : oth. informal savings
<b>Panel A. Pooled Estimates</b>											
Program Treatment (ITT)	0.05** (0.02)	0.02 (0.02)	0.02 (0.03)	0.12*** (0.03)	0.19*** (0.03)	-0.04 (0.03)	7,580.02 (10,554.80)	-1,659.04 (2,198.10)	7,615.26** (3,180.46)	760.39 (9,261.98)	884.78 (2,577.53)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	81.8%	17.2%	59.7%	48.3%	16.8%	30.3%	59,411	7,244	4,983	29,913	17,240
Observations	2,620	2,620	2,620	2,620	2,620	2,620	2,618	2,620	2,620	2,618	2,620
<b>Panel B. Treatment Arm Estimates</b>											
VSLA (T1) (ITT)	0.06** (0.03)	0.02 (0.02)	-0.06* (0.04)	0.21*** (0.04)	0.38*** (0.04)	-0.13*** (0.03)	-9,245.95 (10,139.02)	-2,314.13 (2,778.35)	9,504.65*** (2,601.07)	-15,107.56* (8,664.97)	-1,306.51 (2,759.05)
Cash Grant with repayment (T2) (ITT)	0.03 (0.03)	0.01 (0.02)	0.07** (0.03)	0.06* (0.04)	0.07** (0.03)	0.01 (0.03)	13,317.48 (13,883.07)	-1,923.95 (2,130.14)	8,143.30 (5,257.53)	5,246.59 (11,673.10)	1,873.02 (2,998.70)
Cash Grant (T3) (ITT)	0.07** (0.03)	0.03 (0.03)	0.09* (0.05)	0.07 (0.05)	0.08 (0.05)	0.03 (0.04)	29,300.76** (13,803.35)	302.83 (2,928.52)	2,496.31 (2,434.49)	23,356.57* (12,666.02)	3,163.99 (3,448.83)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	81.8%	17.2%	59.7%	48.3%	16.8%	30.3%	59,411	7,244	4,983	29,913	17,240
p-value T1=T2	0.19	0.70	0.00	0.00	0.00	0.00	0.05	0.84	0.76	0.02	0.19
p-value T2=T3	0.20	0.60	0.70	0.88	0.91	0.63	0.30	0.34	0.22	0.18	0.68
p-value T1=T3	0.85	0.77	0.00	0.00	0.00	0.00	0.00	0.37	0.00	0.00	0.13
p-value T1=T2=T3	0.30	0.85	0.00	0.00	0.00	0.00	0.00	0.61	0.01	0.00	0.22
Observations	2,620	2,620	2,620	2,620	2,620	2,620	2,618	2,620	2,620	2,618	2,620
Robust standard errors clustered at locality level.											
Savings stock is in CFA franc and winsorized at 99%. Formal saving sources are microcredit institutions, banks, agricultural cooperatives, mobile money.											
Other informal saving sources are cash, informal saving collector, designated goods.											
* p <.1, ** p <.05, *** p <.01											

Table 2.9 – Impacts on credit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Has taken a credit (last 2 yrs)	Has taken a credit from formal sources (last 2 yrs)	Has taken a credit from a savings group (last 2 yrs)	Has taken a credit from oth. informal sources (last 2 yrs)	Has taken a credit from a VSLA	Credits taken (all) (cumulative, last 2 yrs)	Credits taken from formal sources	Credits taken from VSLA	Credits taken from oth. informal sources
<b>Panel A. Pooled Estimates</b>									
Program Treatment (ITT)	0.07*** (0.02)	-0.01 (0.01)	0.15*** (0.03)	-0.02 (0.03)	0.12*** (0.03)	4,476.93 (3,096.88)	-81.09 (1,228.21)	3,863.41** (1,536.26)	694.61 (2,388.54)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	57.2%	2.6%	10.1%	48.9%	8.9%	22,736.24	2,954.45	3,730.34	16,051.44
Observations	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620
<b>Panel B. Treatment Arm Estimates</b>									
VSLA (T1) (ITT)	0.12*** (0.03)	-0.01 (0.01)	0.31*** (0.03)	-0.05 (0.03)	0.26*** (0.04)	7,687.51* (3,965.06)	-461.29 (1,745.11)	9,780.38*** (2,114.22)	-1,631.58 (2,645.91)
Cash Grant with repayment (T2) (ITT)	0.04* (0.02)	-0.00 (0.01)	0.04* (0.03)	0.02 (0.03)	0.02 (0.03)	2,413.97 (3,249.14)	-367.88 (1,241.19)	3.84 (1,649.10)	2,778.01 (2,635.87)
Cash Grant (T3) (ITT)	-0.01 (0.04)	0.00 (0.01)	0.07* (0.04)	-0.04 (0.05)	0.04 (0.04)	2,542.97 (5,238.28)	1,361.56 (2,249.03)	430.83 (1,983.03)	750.58 (3,558.64)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	57.2%	2.6%	10.1%	48.9%	8.9%	22,736.24	2,954.45	3,730.34	16,051.44
p-value T1=T2	0.01	0.22	0.00	0.05	0.00	0.14	0.95	0.00	0.04
p-value T2=T3	0.23	0.52	0.55	0.27	0.59	0.98	0.43	0.84	0.52
p-value T1=T3	0.00	0.14	0.00	0.80	0.00	0.35	0.48	0.00	0.45
p-value T1=T2=T3	0.01	0.24	0.00	0.12	0.00	0.32	0.73	0.00	0.13
Observations	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620

Robust standard errors clustered at locality level.

Credits are in CFA franc and winsorized at 99%. Formal credit sources are microcredit institutions, banks, agricultural cooperatives.

Other informal credit sources are family / friends, informal lender / pawnbroker, credit from another business.

\* p < .1, \*\* p < .05, \*\*\* p < .01

Table 2.10 – Impacts on business practices and knowledge

	(1) Entrepreneurship knowledge (quiz) (z-score)	(2) Index of business practices (z-score)	(3) Has done a market assessment	(4) Has developed a business plan	(5) Uses formal bookkeeping	(6) Separate regular payments to self
<b>Panel A. Pooled Estimates</b>						
Program Treatment (ITT)	0.19*** (0.06)	0.21*** (0.06)	0.03** (0.01)	0.02*** (0.01)	0.03*** (0.01)	0.04 (0.03)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	0.00	-0.00	5.9%	0.9%	4.1%	34.4%
Observations	2,618	2,620	2,620	2,620	2,620	2,620
<b>Panel B. Treatment Arm Estimates</b>						
VSLA (T1) (ITT)	0.04 (0.08)	0.12* (0.07)	0.00 (0.02)	0.01* (0.01)	0.03 (0.01)	0.03 (0.03)
Cash Grant with repayment (T2) (ITT)	0.28*** (0.07)	0.36*** (0.07)	0.07*** (0.02)	0.02*** (0.01)	0.04*** (0.01)	0.07** (0.03)
Cash Grant (T3) (ITT)	0.31*** (0.08)	0.07 (0.09)	0.02 (0.02)	0.02** (0.01)	0.03 (0.02)	-0.03 (0.04)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	0.00	-0.00	5.9%	0.9%	4.1%	34.4%
p-value T1=T2	0.00	0.00	0.00	0.11	0.21	0.15
p-value T2=T3	0.68	0.00	0.08	0.82	0.57	0.11
p-value T1=T3	0.00	0.52	0.57	0.21	0.74	0.01
p-value T1=T2=T3	0.00	0.00	0.01	0.20	0.45	0.03
Observations	2,618	2,620	2,620	2,620	2,620	2,620
Robust standard errors clustered at locality level.						
Z-scores are centered on the control group. The index of business practices is based on four variables : market assessment, business plan development, use of formal bookkeeping and way of paying oneself.						
* p <.1, ** p <.05, *** p <.01						

Table 2.11 – Heterogeneous impacts on main outcomes : by age (youth)

	(1)	(2)	(3)	(4)	(5)	(6)
	Self employed (at least 1 activity)	# Independent activities per indiv.	Earnings in Self Employment (Profits) (monthly)	Total start-up capital (all activities)	Value of productive assets (all activities)	Total Savings stock
<b>Panel A. Pooled Estimates</b>						
Treatment (ITT)	-0.01	0.22	1,795.77	7,085.09**	7,604.53	-9,387.04
	0.02	0.16	2,626.00	3,198.09	4,649.13	15,824.49
Treatment x (Youth=1)	0.07***	0.20	-136.43	10,580.87**	6,396.08	31,881.84*
	0.03	0.18	3,682.69	4,299.88	6,723.13	19,249.12
(Youth=1)	-0.08***	-0.43***	-794.42	64.97	975.44	-15,862.39
	0.02	0.15	3,061.67	2,561.90	4,880.80	16,547.59
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control (Youth=0)	95.3%	3.30	24,730	15,800	39,255	68,649
Total Treatment Effect Youth=1	0.06	0.42	1,659	17,666	14,001	22,495
p-value Total Treatment Youth=1	0.00	0.00	0.51	0.00	0.00	0.08
Observations	2,620	2,620	2,620	2,620	2,620	2,618
<b>Panel B. Treatment Arm Estimates</b>						
VSLA (T1) (ITT)	-0.02	0.16	1,004.41	3,309.79	5,455.10	-25,695.49
	(0.02)	(0.19)	(3,595.78)	(4,414.71)	(6,755.47)	(16,433.40)
Cash Grant with repayment (T2) (ITT)	0.00	0.34*	1,947.47	7,739.38**	6,084.19	-8,261.62
	(0.02)	(0.18)	(2,809.17)	(3,831.84)	(4,809.05)	(16,173.45)
Cash Grant (T3) (ITT)	-0.00	0.04	3,043.76	13,222.60**	15,362.39*	20,897.48
	(0.02)	(0.23)	(3,875.28)	(6,532.69)	(8,062.99)	(21,256.50)
VSLA (T1) x (Youth=1)	0.08***	0.26	-240.79	8,070.83	11,250.88	30,712.27*
	(0.03)	(0.23)	(5,374.07)	(5,790.09)	(10,637.66)	(18,504.61)
Cash Grant with repayment (T2) x (Youth=1)	0.05*	0.04	830.11	13,791.33**	6,499.88	40,848.28
	(0.03)	(0.22)	(3,901.18)	(5,863.66)	(6,701.33)	(25,216.60)
Cash Grant (T3) x (Youth=1)	0.08**	0.43*	-2,030.03	9,020.99	-3,965.00	15,704.61
	(0.03)	(0.25)	(4,393.31)	(8,173.55)	(11,681.15)	(24,894.31)
(Youth=1)	-0.08***	-0.43***	-786.34	140.25	954.75	-15,634.73
	(0.02)	(0.15)	(3,065.56)	(2,577.91)	(4,882.45)	(16,537.85)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes
Mean in T1 Control (Youth=0)	95.8%	3.50	24,563	20,117	42,119	62,488
Mean in T2 Control (Youth=0)	94.5%	3.36	25,121	18,900	43,112	57,764
Mean in T3 Control (Youth=0)	94.9%	3.49	24,593	18,545	41,000	51,691
Total T1 Effect Youth=1	0.06	0.43	764	11,381	16,706	5,017
p-value Total T1 Effect Youth=1	0.01	0.00	0.83	0.00	0.01	0.65
Total T2 Effect Youth=1	0.05	0.39	2,778	21,531	12,584	32,587
p-value Total T2 Effect Youth=1	0.02	0.01	0.37	0.00	0.02	0.12
Total T3 Effect Youth=1	0.07	0.47	1,014	22,244	11,397	36,602
p-value Total T3 Effect Youth=1	0.00	0.02	0.79	0.00	0.21	0.02
p-value T1=T2	0.66	0.80	0.61	0.06	0.54	0.14
p-value T2=T3	0.25	0.70	0.67	0.93	0.90	0.86
p-value T1=T3	0.44	0.84	0.96	0.15	0.60	0.05
p-value T1=T2=T3	0.50	0.92	0.85	0.11	0.79	0.06
Observations	2,620	2,620	2,620	2,620	2,620	2,618

Robust standard errors clustered at locality level. Youth defined as up to 35 years old.

Earnings, sales, capital and value of assets are in CFA franc and winsorized at 99%.

\* p <.1, \*\* p <.05, \*\*\* p <.01

Table 2.12 – Heterogeneous impacts on main outcomes : by ethnic group

	(1)	(2)	(3)	(4)	(5)	(6)
	Self employed (at least 1 activity)	# Independent activities per indiv.	Earnings in Self Employment (Profits) (monthly)	Total start-up capital (all activities)	Value of productive assets (all activities)	Total Savings stock
<b>Panel A. Pooled Estimates</b>						
Treatment (ITT)	0.04	0.55**	497.50	23,271.05***	15,509.27	-16,249.76
	0.03	0.28	5,323.94	6,633.26	10,666.32	19,777.68
Treatment x (Native Group=1)	-0.02	-0.27	1,086.23	-12,758.53*	-5,979.94	28,010.54
	0.03	0.32	5,981.05	7,018.34	12,106.25	22,240.30
(Native Group=1)	-0.01	-0.11	-12,125.71**	4,503.79	-13,580.12	-31,688.86*
	0.03	0.30	4,748.45	4,710.04	10,238.68	18,979.86
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control (Native Gp.=0)	91.3%	3.08	34,441	14,165	54,828	89,720
Total Treatment Effect Native Gp.=1	0.02	0.27	1,584	10,513	9,529	11,761
p-value Total Treatment Native Gp.=1	0.05	0.03	0.42	0.00	0.01	0.31
Observations	2,620	2,620	2,620	2,620	2,620	2,618
<b>Panel B. Treatment Arm Estimates</b>						
VSLA (T1) (ITT)	0.04	0.48	2,151.82	14,299.44*	16,575.61	-22,770.84
	(0.03)	(0.30)	(7,111.07)	(7,779.25)	(12,236.35)	(25,624.10)
Cash Grant with repayment (T2) (ITT)	0.04	0.63*	-4,828.68	35,442.11***	9,044.26	-21,850.72
	(0.03)	(0.37)	(5,159.37)	(12,762.48)	(10,811.99)	(22,080.31)
Cash Grant (T3) (ITT)	0.04	0.57*	6,215.71	21,300.50**	24,524.97	4,331.11
	(0.03)	(0.32)	(8,732.89)	(10,066.70)	(21,064.23)	(22,619.75)
VSLA (T1) x (Native Gp.=1)	-0.02	-0.23	-2,348.93	-8,023.18	-7,050.18	14,937.15
	(0.04)	(0.35)	(7,605.15)	(8,451.84)	(14,046.16)	(26,646.69)
Cash Grant with repayment (T2) x (Native Group=1)	-0.02	-0.30	8,418.13	-23,972.11*	422.58	41,216.21
	(0.04)	(0.41)	(5,936.00)	(12,968.41)	(12,560.68)	(26,899.35)
Cash Grant (T3) x (Native Gp.=1)	0.00	-0.38	-6,077.98	-4,027.20	-14,981.04	29,204.38
	(0.03)	(0.39)	(9,425.37)	(8,870.08)	(23,389.52)	(27,315.59)
(Native Group=1)	-0.01	-0.11	-12,044.11**	4,360.69	-13,536.35	-31,583.67*
	(0.03)	(0.30)	(4,759.31)	(4,729.05)	(10,246.71)	(18,951.72)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes
Mean in T1 Control (Native Gp.=0)	93.2%	3.30	35,423	32,917	64,858	82,111
Mean in T2 Control (Native Gp.=0)	92.6%	3.13	40,179	26,706	69,084	81,219
Mean in T3 Control (Native Gp.=0)	92.9%	3.16	36,750	31,425	65,480	78,087
Total T1 Effect Native Gp.=1	0.02	0.25	-197	6,276	9,525	-7,834
p-value Total T1 Effect Native Gp.=1	0.33	0.08	0.93	0.05	0.03	0.44
Total T2 Effect Native Gp.=1	0.02	0.33	3,589	11,470	9,467	19,365
p-value Total T2 Effect Native Gp.=1	0.09	0.02	0.15	0.00	0.03	0.22
Total T3 Effect Native Gp.=1	0.04	0.19	138	17,273	9,544	33,535
p-value Total T3 Effect Native Gp.=1	0.01	0.38	0.96	0.00	0.12	0.04
p-value T1=T2	0.55	0.55	0.15	0.14	0.99	0.03
p-value T2=T3	0.33	0.47	0.24	0.26	0.99	0.43
p-value T1=T3	0.13	0.74	0.91	0.04	1.00	0.00
p-value T1=T2=T3	0.29	0.71	0.30	0.09	1.00	0.00
Observations	2,620	2,620	2,620	2,620	2,620	2,618

Robust standard errors clustered at locality level. Earnings, sales, capital and value of assets are in CFA franc and winsorized at 99%.

\* p &lt;.1, \*\* p &lt;.05, \*\*\* p &lt;.01

## **B Additional tables**

Table 2.13 – Vulnerability score

Criteria	Weights	Score
<b>Vulnerability categories</b> - Never been to school or primary school dropout - Single mother - Jobless - Disabled	20%	=1 if belongs to any category listed
<b>Level and source of income</b> A) Employment instability Based on the following employment status : - Family worker or Domestic or Intern/Apprentice ; - Employed with piecework pay or daily pay ; - Not working B) Level of resources (sum of labor incomes and money transfers last 30 days) $B$	5%       20% (*)	=1 if belongs to any category listed       $= (B)$
<b>Financial dependence of the household</b> - Ratio of people contributing to household' expenses to total household' members ( $C$ ) - Share of financially dependant people in the household ( $D$ )	10%	$= C + (1 - D)$
<b>Living Standards of the household</b> - Economic and agriculture assets - Other economic goods - Transportation related goods - Equipment and comforts goods - Nb of rooms in the house/Nb of people in the household ( $E$ )	10% (*) 10% (*) 10% (*) 10% (*) 5% (*)	$=$ Livestock heads $=$ Nb of plows + field sprayer + carts + wheelbarrows $=$ Nb of bikes + motorcycles + cars $=$ Nb of fridge + fans + TV $= (E)$
<i>(*) Indicates that this item enters negatively in the score (so a high score on this item decreases the total vulnerability score)</i>		

Table 2.14 – Comparison of selected and non-selected beneficiaries at baseline in treated localities

	(1)	(2)	(3)	(4)	(5)
	Nb obs	All applicants	Mean among selected individuals	Mean among non-selected individuals	pvalue of test (3)-(4)
<b>Personal and Household characteristics</b>					
Type of locality (1 = village)	9,042	80.3%	79.9%	80.0%	0.76
Rural area	9,042	78.0%	76.4%	77.1%	0.24
Gender	9,042	50.7%	70.8%	62.1%	0.00
Age	9,042	31.74	35.28	33.74	0.00
Youth (<35 years-old)	9,042	70.5%	55.3%	61.9%	0.00
Single mother	9,042	0.5%	1.2%	0.9%	0.01
Out of school or out of school in primary school	9,042	62.8%	92.8%	79.8%	0.00
Disabled	9,042	2.9%	5.3%	4.3%	0.00
Native ethnic group	9,042	78.7%	80.3%	79.6%	0.18
Married	9,041	37.9%	28.5%	32.6%	0.00
Lives with a partner (but not married)	9,041	25.2%	19.3%	21.9%	0.00
Widowed	9,041	10.6%	29.6%	21.4%	0.00
Nb of children	9,042	2.99	3.62	3.35	0.00
Has been to school	9,042	62.0%	42.8%	51.1%	0.00
No Diploma	9,037	62.9%	92.8%	79.8%	0.00
Head of the Household	9,041	48.8%	56.7%	53.3%	0.00
Spouse of the Head of the Household	9,041	28.8%	25.0%	26.6%	0.01
Nb of household members (excl. respondent)	9,042	5.87	5.51	5.66	0.00
<b>Household and productive assets</b>					
Nb of livestock heads	9,042	1.87	0.63	1.17	0.00
Nb of poultry heads	9,042	4.98	1.82	3.19	0.00
Nb of agricultural tools owned	9,042	0.42	0.09	0.23	0.00
<b>Employment</b>					
Has an activity (last 7 days)	9,042	98.6%	94.5%	96.3%	0.00
Has worked in an agricultural activity (last 6 months)	9,042	85.0%	81.5%	83.0%	0.00
Has a non-ag business	9,042	78.8%	71.9%	74.9%	0.00
Main Activity is non-agricultural self-employment (last 7 days)	9,042	25.9%	25.0%	25.4%	0.44
Main Activity is agricultural self-employment (last 7 days)	8,695	59.1%	51.2%	54.7%	0.00
Main Activity is wage work (last 7 days)	8,695	7.7%	8.8%	8.3%	0.22
Total income from main activity over the last month	9,042	55,339	22,878	36,972	0.00
<b>Business practices</b>					
Does not do accounting	6,754	12.7%	14.2%	13.5%	0.15
Does accounting by memory	6,754	70.0%	77.9%	74.3%	0.00
<b>Savings and Credits</b>					
Has Saved over the last 3 months	9,042	60.9%	48.8%	54.1%	0.00
Amount saved over the last 3 months	9,040	37,782	15,297	25,060	0.01
Has a mobile money account	9,039	27.5%	15.0%	20.5%	0.00
Has a bank account	9,039	2.5%	1.2%	1.8%	0.00
Has participated in a Tontine (ROSCA)	9,039	50.0%	50.7%	50.4%	0.64
Has debt	9,042	22.0%	23.0%	22.6%	0.35
<b>Financial constraints</b>					
Report strong binding constraints for education expenditures	9,042	59.0%	63.1%	61.3%	0.00
Report strong binding constraints for health expenditures	9,041	65.9%	70.4%	68.4%	0.00

Table 2.15 – Descriptive statistics and Balance checks (Main sample)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Nb obs	Mean in Control Group	Mean in Treatment Group (pooled)	pvalue of test (2)-(3)	Mean in VSLA (T1)	Mean in Cash Grant with Repayment (T2)	Mean in Cash Grant (T3)	pvalue of joint test T1=T2=T3
<b>Personal and Household characteristics</b>								
Type of locality (1 = village)	2,936	80.6%	80.2%	0.94	80.3%	81.2%	77.8%	0.94
Rural area	2,936	80.1%	78.5%	0.78	75.9%	81.9%	75.6%	0.64
Gender	2,936	71.4%	70.3%	0.65	70.7%	70.2%	69.6%	0.96
Age	2,936	35.21	34.91	0.63	34.48	35.23	34.98	0.58
Youth (<35 years-old)	2,936	56.2%	56.5%	0.89	56.5%	56.9%	55.7%	0.95
Native ethnic group	2,936	83.4%	81.2%	0.56	78.4%	83.9%	80.6%	0.59
Married	2,935	26.6%	30.8%	0.28	33.1%	28.2%	32.4%	0.54
Lives with a partner (but not married)	2,935	18.2%	19.0%	0.79	20.7%	19.0%	15.6%	0.50
Widowed	2,935	30.8%	28.5%	0.43	25.7%	30.3%	30.0%	0.34
Nb of children	2,936	3.58	3.60	0.83	3.57	3.62	3.63	0.95
Has been to school	2,936	40.3%	39.4%	0.80	39.2%	41.1%	35.9%	0.50
No Diploma	2,935	93.9%	94.2%	0.81	93.9%	93.9%	95.5%	0.58
Head of the Household	2,935	58.9%	56.2%	0.36	53.9%	58.1%	56.0%	0.56
Spouse of the Head of the Household	2,935	24.1%	26.7%	0.34	30.5%	24.7%	24.2%	0.17
Nb of household members (excl. respondent)	2,936	5.30	5.45	0.43	5.27	5.50	5.69	0.35
<b>Household and productive assets</b>								
Nb of livestock heads	2,936	0.54	0.59	0.62	0.60	0.58	0.61	0.97
Nb of poultry heads	2,936	1.65	1.72	0.73	1.98	1.52	1.71	0.31
Nb of agricultural tools owned	2,936	0.05	0.09	0.02	0.09	0.09	0.09	1.00
<b>Employment</b>								
Has an activity (last 7 days)	2,936	91.3%	94.6%	0.07	94.3%	93.8%	96.9%	0.11
Has worked in an agricultural activity (last 6 months)	2,936	80.7%	83.2%	0.52	82.3%	82.6%	86.4%	0.61
Has a non-ag business	2,936	71.7%	73.4%	0.61	72.5%	72.3%	77.5%	0.38
Main Activity is wage work (last 7 days)	2,745	7.7%	8.3%	0.74	5.5%	9.8%	9.9%	0.04
Main Activity is non-agricultural self-employment (last 7 days)	2,936	23.6%	22.3%	0.71	22.4%	21.4%	24.2%	0.86
Main Activity is agricultural self-employment (last 7 days)	2,745	53.2%	53.9%	0.88	53.5%	55.0%	52.3%	0.92
Total income from main activity over the last month	2,936	19,124	21,207	0.32	20,741	21,659	21,066	0.94
<b>Business practices</b>								
Does not do accounting	2,133	15.7%	15.9%	0.92	16.1%	17.2%	13.0%	0.49
Does accounting by memory	2,133	76.8%	77.0%	0.95	75.2%	76.1%	81.8%	0.27
<b>Savings and Credits</b>								
Has Saved over the last 3 months	2,936	48.9%	48.4%	0.89	47.9%	47.4%	51.4%	0.74
Amount saved over the last 3 months	2,935	12,041	17,956	0.28	13,215	23,278	14,965	0.66
Has a mobile money account	2,936	13.2%	13.0%	0.93	13.9%	13.5%	10.3%	0.44
Has a bank account	2,935	1.3%	0.9%	0.35	1.0%	0.9%	0.7%	0.84
Has participated in a Tontine (ROSCA)	2,934	48.9%	48.2%	0.84	44.0%	50.2%	51.6%	0.21
Has debt	2,936	24.4%	22.7%	0.54	22.8%	24.2%	19.4%	0.48
<b>Financial constraints</b>								
Report strong binding constraints for education expenditures	2,936	60.5%	60.0%	0.86	59.5%	60.7%	59.3%	0.93
Report strong binding constraints for health expenditures	2,936	69.1%	70.6%	0.59	68.8%	70.9%	73.2%	0.55

Table 2.16 – Direct impacts on agricultural outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Is engaged in farming activities	Is engaged in livestock raising activities	Total # plots managed	Total surface cultivated (ha)	Used fertilizer (all types)	Has cultivated cash crops	Has cultivated food crops	Production (cash crops) (kg)	Sales (cash crops) (kg)
<b>Panel A. Pooled Estimates</b>									
Program Treatment (ITT)	0.02 (0.02)	0.00 (0.03)	0.12 (0.14)	0.17 (0.25)	0.05 (0.03)	0.01 (0.03)	0.04 (0.02)	4,122.33 (4,314.23)	4,126.05 (4,314.09)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	86.0%	52.5%	3.04	4.09	60.8%	61.7%	78.2%	238.45	230.48
Observations	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620
<b>Panel B. Treatment Arm Estimates</b>									
VSLA (T1) (ITT)	0.01 (0.03)	-0.00 (0.03)	0.09 (0.16)	0.35 (0.32)	0.03 (0.04)	0.01 (0.03)	0.03 (0.03)	-519.85 (2,565.06)	-517.17 (2,564.68)
Cash Grant with repayment (T2) (ITT)	0.02 (0.02)	0.01 (0.03)	0.24 (0.18)	0.20 (0.29)	0.06 (0.04)	-0.01 (0.04)	0.05* (0.03)	-982.10 (2,530.11)	-978.20 (2,529.85)
Cash Grant (T3) (ITT)	0.05** (0.02)	-0.02 (0.04)	-0.10 (0.23)	-0.26 (0.33)	0.09* (0.05)	0.05 (0.04)	0.05 (0.03)	25,399.19 (23,497.67)	25,404.65 (23,497.81)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	86.0%	52.5%	3.04	4.09	60.8%	61.7%	78.2%	238.45	230.48
p-value T1=T2	0.78	0.68	0.38	0.66	0.45	0.71	0.48	0.85	0.85
p-value T2=T3	0.18	0.42	0.17	0.18	0.54	0.17	0.91	0.28	0.28
p-value T1=T3	0.14	0.66	0.40	0.10	0.22	0.23	0.44	0.28	0.28
p-value T1=T2=T3	0.27	0.72	0.36	0.22	0.46	0.34	0.69	0.56	0.56
Observations	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620	2,620

Robust standard errors clustered at locality level. All variables are measured over the last 12 months.

Areas (in ha) and volumes of production and sales (in kg) are winsorized at 99%. Cash crops include hevea, coffee, cocoa, cashew and cotton.

\* p < .1, \*\* p < .05, \*\*\* p < .01

Table 2.17 – Heterogeneous impacts on main economic outcomes : by gender

	(1)	(2)	(3)	(4)	(5)	(6)
	Self employed (at least 1 activity)	# Independent activities per indiv.	Earnings in Self Employment (Profits) (monthly)	Total start-up capital (all activities)	Value of productive assets (all activities)	Total Savings stock
<b>Panel A. Pooled Estimates</b>						
Treatment (ITT)	0.02	0.47***	201.66	17,006.89***	17,862.83***	13,373.56
	0.02	0.17	3,552.94	4,657.58	6,419.71	12,966.17
Treatment x Gender	0.01	-0.20	1,964.88	-6,255.98	-9,920.94	-8,048.19
	0.02	0.19	3,868.65	5,903.57	6,725.01	14,682.91
Gender (Female==1)	-0.01	-0.07	-6,271.47**	-6,770.24	-9,188.36*	3,370.52
	0.02	0.16	3,091.17	4,326.59	4,664.17	10,761.35
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control (Female==0)	92.8%	3.25	28,089	18,228	44,854	52,684
Total Treatment Effect Female==1	0.03	0.26	2,167	10,751	7,942	5,325
p-value Total Treatment Female==1	0.01	0.03	0.26	0.00	0.02	0.66
Observations	2,620	2,620	2,620	2,620	2,620	2,618
<b>Panel B. Treatment Arm Estimates</b>						
VSLA (T1) (ITT)	0.02	0.47**	2,011.10	11,914.47*	17,851.11**	-2,956.06
	(0.02)	(0.21)	(4,038.57)	(6,060.80)	(8,672.13)	(11,825.09)
Cash Grant with repayment (T2) (ITT)	-0.00	0.47**	-1,490.84	23,829.12***	15,696.41**	14,016.00
	(0.02)	(0.19)	(3,886.72)	(6,312.61)	(6,583.79)	(19,533.29)
Cash Grant (T3) (ITT)	0.04*	0.45	12.02	12,122.07	22,812.70	46,268.77*
	(0.02)	(0.30)	(8,673.48)	(9,137.51)	(16,506.20)	(25,401.64)
VSLA (T1) x Gender	-0.01	-0.24	-1,956.04	-6,417.39	-9,432.83	-8,855.34
	(0.03)	(0.22)	(4,715.14)	(7,021.01)	(9,095.86)	(13,834.92)
Cash Grant with repayment (T2) x Gender	0.04	-0.15	5,296.26	-12,456.63*	-8,845.41	-913.42
	(0.03)	(0.22)	(3,818.11)	(7,382.44)	(6,723.18)	(22,657.56)
Cash Grant (T3) x Gender	-0.01	-0.26	2,699.23	7,946.03	-13,319.84	-23,521.11
	(0.03)	(0.30)	(9,002.87)	(12,421.16)	(17,591.52)	(25,979.84)
Gender (Female==1)	-0.01	-0.07	-6,282.75**	-6,823.83	-9,221.33**	2,798.68
	(0.02)	(0.16)	(3,093.90)	(4,326.04)	(4,667.80)	(10,720.97)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes
Mean in T1 Control (Female==0)	93.7%	3.50	27,591	30,415	55,430	64,649
Mean in T2 Control (Female==0)	94.4%	3.45	29,170	25,982	56,666	57,217
Mean in T3 Control (Female==0)	93.4%	3.50	28,717	31,146	56,299	53,804
Total T1 Effect Female==1	0.02	0.23	55	5,497	8,418	-11,811
p-value Total T1 Effect Female==1	0.27	0.08	0.98	0.10	0.03	0.32
Total T2 Effect Female==1	0.04	0.33	3,805	11,372	6,851	13,103
p-value Total T2 Effect Female==1	0.01	0.02	0.09	0.00	0.09	0.42
Total T3 Effect Female==1	0.04	0.20	2,711	20,068	9,493	22,748
p-value Total T3 Effect Female==1	0.01	0.29	0.30	0.00	0.11	0.11
p-value T1=T2	0.25	0.45	0.21	0.12	0.70	0.06
p-value T2=T3	0.94	0.48	0.69	0.22	0.66	0.55
p-value T1=T3	0.28	0.86	0.40	0.04	0.85	0.00
p-value T1=T2=T3	0.46	0.68	0.45	0.07	0.88	0.01
Observations	2,620	2,620	2,620	2,620	2,620	2,618

Robust standard errors clustered at locality level. Earnings, sales, capital and value of assets are in CFA franc and winsorized at 99%.  
\* p <.1, \*\* p <.05, \*\*\* p <.01

Table 2.18 – Heterogeneous impacts on main economic outcomes : by type of area (urban/rural)

	(1)	(2)	(3)	(4)	(5)	(6)
	Self employed (at least 1 activity)	# Independent activities per indiv.	Earnings in Self Employment (Profits) (monthly)	Total start-up capital (all activities)	Value of productive assets (all activities)	Total Savings stock
<b>Panel A. Pooled Estimates</b>						
Treatment (ITT)	0.09***	0.61**	3,131.25	19,824.40**	8,253.20	-55,971.18
	0.03	0.24	3,630.93	7,669.21	8,478.90	35,609.80
Treatment x (Village==1)	-0.08**	-0.35	-1,710.98	-8,687.48	3,334.91	77,436.49**
	0.03	0.27	4,188.16	8,094.96	9,111.62	36,586.41
(Village==1)	0.09***	0.92***	-2,062.42	3,699.96	-11,428.61	-93,774.20***
	0.03	0.23	3,092.59	7,566.32	7,658.41	31,704.75
Department	No	No	No	No	No	No
Mean in Control (Village==0)	82.2%	2.14	24,557	18,277	50,731	140,761
Total Treatment Effect Village==1	0.01	0.26	1,420	11,137	11,588	21,465
p-value Total Treatment Village==1	0.22	0.03	0.48	0.00	0.00	0.02
Observations	2,620	2,620	2,620	2,620	2,620	2,618
<b>Panel B. Treatment Arm Estimates</b>						
VSLA (T1) (ITT)	0.07*	0.63**	5,099.03	18,951.45**	21,800.76**	-76,330.77**
	(0.04)	(0.28)	(6,350.68)	(8,717.83)	(9,947.25)	(36,950.66)
Cash Grant with repayment (T2) (ITT)	0.10**	0.81**	582.53	17,654.02*	2,716.63	-53,805.27
	(0.04)	(0.35)	(3,728.38)	(9,555.01)	(10,568.45)	(43,379.18)
Cash Grant (T3) (ITT)	0.11***	0.22	3,353.40	25,991.06**	-11,139.40	-13,591.81
	(0.04)	(0.38)	(5,213.62)	(12,950.73)	(7,952.22)	(44,779.41)
VSLA (T1) x (Village==1)	-0.07	-0.40	-5,360.01	-14,086.27	-13,017.42	82,584.25**
	(0.04)	(0.32)	(6,847.18)	(9,207.17)	(10,437.43)	(37,794.14)
Cash Grant with repayment (T2) x (Village==1)	-0.09**	-0.53	2,119.46	-3,345.01	8,106.50	80,975.98*
	(0.04)	(0.38)	(4,538.06)	(10,142.48)	(11,320.61)	(45,050.16)
Cash Grant (T3) x (Village==1)	-0.09**	0.06	-1,675.32	-9,917.52	30,283.24***	51,686.37
	(0.04)	(0.44)	(6,401.49)	(13,986.41)	(10,699.91)	(46,702.40)
(Village==1)	0.09***	0.92***	-2,057.48	4,071.90	-11,567.81	-92,151.44***
	(0.03)	(0.23)	(3,077.53)	(7,658.62)	(7,327.03)	(31,724.12)
Department X (Urban/Rural)	No	No	No	No	No	No
Mean in T1 Control (Village==0)	88.7%	2.49	25,474	28,414	48,269	110,247
Mean in T2 Control (Village==0)	87.4%	2.31	29,122	29,728	57,406	101,491
Mean in T3 Control (Village==0)	87.8%	2.43	27,715	28,900	57,957	90,603
Total T1 Effect Village==1	0.01	0.23	-261	4,865	8,783	6,253
p-value Total T1 Effect Village==1	0.53	0.09	0.92	0.10	0.02	0.43
Total T2 Effect Village==1	0.01	0.28	2,702	14,309	10,823	27,171
p-value Total T2 Effect Village==1	0.36	0.03	0.29	0.00	0.01	0.05
Total T3 Effect Village==1	0.02	0.29	1,678	16,074	19,144	38,095
p-value Total T3 Effect Village==1	0.06	0.15	0.64	0.00	0.01	0.00
p-value T1=T2	0.78	0.66	0.30	0.02	0.64	0.07
p-value T2=T3	0.46	0.97	0.79	0.77	0.26	0.50
p-value T1=T3	0.27	0.76	0.62	0.07	0.15	0.01
p-value T1=T2=T3	0.50	0.90	0.57	0.03	0.35	0.01
Observations	2,620	2,620	2,620	2,620	2,620	2,618

Robust standard errors clustered at locality level. Village defined by administrative status used for lotteries.

Earnings, sales, capital and value of assets are in CFA franc and winsorized at 99%.

\* p < .1, \*\* p < .05, \*\*\* p < .01

## Chapter 3

# Economic and social spillovers of a micro-entrepreneurship program : evidence from post-conflict Côte d'Ivoire.

### 3.1 Introduction

In conflict or post-conflict countries, tensions and instability can threaten efforts to reduce poverty. By 2030, the World Bank estimates that up to two thirds of the world's extreme poor could live in fragile settings. Targeting fragile areas to promote economic development is crucial, but likely to be more challenging.

Support to micro-entrepreneurship in post-conflict environments with thin formal sectors generate policy interest. The underlying idea is a standard one : that more stability and social cohesion would be derived from more prosperity. However, the social climate can also be disrupted by the implementation of new programs. They may benefit some, but not all, thereby possibly creating tensions across groups. They may have effects on the local economy by supporting new businesses at the expense of the existing ones. Adverse economic consequences can affect social cohesion, especially if tensions already exist. Obviously, this risk is exacerbated in fragile areas. At the same time, improving social ties and peace can be at the core of some economic interventions. Some forms of community support or savings groups have potential for that. Despite being secondary, those social objectives sometimes justify the choice of a policy instrument over another one. Unfortunately, we lack evidence on the social impacts of economic interventions.

This chapter investigates the social impacts of an economic inclusion and micro-entrepreneurship program, presented in Chapter 2 in which we evaluate its economic impact. The study takes place in the Western regions of post-conflict Côte d'Ivoire. The setting is characterized by important ethnic fragmentation with tensions between local ethnic groups, internal migrants and foreign migrants. Improving social cohesion was an objective of the micro-entrepreneurship support program. We analyze direct impacts on social outcomes among beneficiaries. Another fundamental consideration is whether there are any negative side effects on non-beneficiaries. Adverse economic effect can further deteriorate the social climate. Therefore we also test for indirect economic and social impacts within localities.

The intervention provides a mix of skills training and capital, testing three alternative instruments to facilitate investments in small businesses : Village Savings and Loans Associations (VSLAs), cash grants and cash grants with repayment. We designed a large-scale randomized controlled trial (RCT) to evaluate not only the direct impact of the program (see Chapter 2), but also its indirect impacts on individuals from the same locality who were not selected ("non-selected"). The program was implemented in 147 localities in the Western regions of Côte d'Ivoire. Individuals who expressed interest for the program enrolled and participated in a short baseline survey. Localities were randomly assigned to a control group or one of the three interventions: "VSLAs", "cash-grants-with-repayment" or "cash grants". Within treatment localities, individual participants were selected according to a proxy of their vulnerability level, using baseline data. We measure local indirect impacts among individuals just below the program selection cut-offs in control and treatment communities.

The program provided both training and capital support, either in the form of cash injection (cash grants) or by developing enhanced savings groups (VSLA). VSLAs are enhanced savings groups designed to facilitate savings accumulation and provide small credit opportunities. The training supported the development of simple business plans to assess business opportunities and capital needs. We collected follow-up data on beneficiaries and non-beneficiaries approximately 22 months after the program started, or on average 15 months after capital transfers. The data

contains detailed information on employment, income-generating activities, investments, savings and social outcomes.

Fostering social cohesion is often an important aspect of the policy agenda in fragile areas. Employment programs are increasingly used as policy instruments based on the idea that improved livelihoods would increase social stability. However, there is limited evidence on interventions showing linkages between improved economic opportunities and social climate. Direct economic interventions with capital transfers have been implemented to improve livelihoods and decrease tensions. However, even in highly fragile settings such as Afghanistan ([Bedoya et al. \(2019\)](#)), Uganda ([Blattman et al. \(2014\)](#)) or Liberia ([Blattman et al. \(2016\)](#)), they only had small or no impact on social behavior. Interventions targeting ex-soldiers or high-risk individuals have proved to be more effective at affecting social behavior, but psychological components seem to play a greater role than the economic one ([Blattman and Ralston \(2017\)](#)). There is no clear evidence either on whether interventions increasing economic interactions across communities successfully improve social outcomes.<sup>1</sup> We document program impacts on social outcomes by collecting detailed data on the existence and composition of social and economic groups in the village, as well as self-reported information on solidarity, trust, perception of other ethnic groups and tensions.

Economic interventions and employment programs can have indirect (spillover) effects at the local level. Economic spillovers within a village can go in both directions, either sustaining or negatively affecting local economic activities among non-beneficiaries. They may also have cascading effects on social cohesion among non-beneficiaries, again possibly in both directions. Our randomization took place after the collection of vulnerability scores that determine individuals' prioritization for the program in treatment and control villages. As such, our design generates comparable groups of non-beneficiaries across treatment and control villages (similar to [Angelucci and De Giorgi \(2009\)](#)). By collecting follow-up data on non-beneficiaries, we document the local spillover effects of the micro-entrepreneurship intervention on both economic and social outcomes.

Our main contribution is to document direct and indirect (spillover) impacts on social outcomes

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<sup>1</sup>Community-driven development (CDD) programs have been a popular tool to strengthen fragile communities, but even these interventions have rarely been shown to improve collective action or social cohesion ([Fearon et al. \(2015\)](#); [Humphreys et al. \(2019\)](#); [Avdeenko and Gilligan \(2015\)](#)).

with communities, in a post-conflict environment. As such, this is one of the first papers to provide a comprehensive assessment of impacts in both economic and social dimensions of an economic inclusion intervention in a post-conflict area.

There are various potential pathways for economic interventions to affect social cohesion. Some economic programs in post-conflict settings directly target specific individuals who could otherwise negatively affect social cohesion, for instance ex-combatants. The rationale is to increase the opportunity cost of time spent on illicit (crime-related) activities (*incapacitating*); Blattman et al. (2016) is an example of evaluation of such program. This is not the focus of our intervention. In our study localities, there are few or no active mercenaries. Rather, the conflict affected trust and perceptions between ethnic groups. Different ethnic groups tend to live in separate areas and not interact much. Migration created tensions related to land ownership.

Reinforcing social ties through an economic intervention would be another way, but this is not automatic. During entrepreneurship training or delivery of capital, beneficiaries only interact sporadically with each other's. The VSLA intervention triggered regular social interactions through weekly meetings taking place over 9 months. This is substantive enough to suggest that the program increased social interactions. In addition to increasing the intensity of social contacts, it also affected the type of contacts, as participants from various ethnic groups were mixed. As such, social interactions were more frequent and differed between VSLA and grant localities (the latter only received training in group over a dozen days).

Some papers suggest that increased personal contact can positively change the perception of others (e.g. Rao (2019) on poor and richer students in India, or Lowe (2019) across caste groups). The microcredit literature suggests that social ties can improve following frequent repayment group meetings and have long-term economic benefits through increased risk-sharing (Feigenberg et al. (2013) and Feigenberg et al. (2014)). This suggests that repeated contacts can reinforce social ties, which could later induce greater social cohesion. However, and surprisingly, papers in the literature on savings groups do not find significant impacts on social cohesion indicators.

Our results show that the micro-entrepreneurship program impacts social outcomes among beneficiaries. Group participation increases (including in mixed groups) as well as solidarity between members. However, we do not find broader impacts on community events or trust in other groups.

Furthermore, the social impacts are not different between the VSLA, cash-grant-with-repayment and cash grants, which suggests that enhanced social ties may not be the main channel.

Lastly, the paper relates to the broader literature on indirect effects of economic inclusion programs. Cash transfer programs may indirectly affect households' labor supply or consumption. [Angelucci and De Giorgi \(2009\)](#) have been among the first to study them, identifying positive impacts on consumption of ineligible families thanks to the informal risk-sharing networks in villages. However cash transfer can also generate negative spillovers, for example on the nutrition of children of non-beneficiaries due to increases in the price of certain food items ([Filmer et al. \(2018\)](#)).<sup>2</sup> Several studies have expressed concerns about negative externalities of entrepreneurship programs, in particular the crowding-out of other businesses following training and capital transfers ([Calderon et al. \(2019\)](#)) and [McKenzie and Puerto \(2017\)](#)), large cash transfer to individuals ([Egger et al. \(2019\)](#)) and subsidies to small firms ([Rotemberg \(2019\)](#)). With the exception of [Rotemberg \(2019\)](#), no negative spillovers are observed. [McKenzie and Puerto \(2017\)](#) find no significant impact on business outcomes for untreated firms or individuals even though there was clear market growth, and the experiment was sufficiently powered.<sup>3</sup> [Egger et al. \(2019\)](#) find positive impacts on household expenditures and income, and no effect on psychological well-being for the untreated. In the context of graduation programs, both [Banerjee et al. \(2015\)](#) and [Sedlmayr et al. \(2020\)](#) find no evidence of spillovers in treated villages.

The savings literature also raises the risk of disrupting informal risk-sharing networks when introducing new saving instruments. Even if they cannot formerly measure it, [Dupas and Robinson \(2013\)](#) and [Brune et al. \(2016\)](#) warn that commitment devices protect from social pressure on savings, and as such may cut a social sharing channel with negative consequences. Studying spillovers within localities is not only useful to ensure there is no unintended adverse impact, but also to provide a comprehensive measure of the impact which might be under or overestimated if restricted to direct beneficiaries. Lastly, even though economic spillovers may also affect social

<sup>2</sup>[Cunha et al. \(2019\)](#) study the effects of in-kind versus cash transfers and find that cash transfers cause a positive but negligible increase in food prices. In comparison, [Filmer et al. \(2018\)](#) distinguish between perishable and less tradable food items (which are most affected by price increases) and others, and study the effects in villages with different treatment saturation levels (the price effect increasing with saturation).

<sup>3</sup>In [Calderon et al. \(2019\)](#) the point estimate for profits is negative, but estimated with large standard errors and not significant. The experiment is not powered enough to precisely measure spillovers.

cohesion, we are not aware of studies analyzing local spillovers on social cohesion.

We do not find evidence of negative economic impacts on non-beneficiaries within treated localities. The program increases the number of economic activities among beneficiaries, but does not reduce economic activities for non-beneficiaries. On the contrary, the intervention generates positive spillovers by increasing savings, which led to marginally higher investments in non-beneficiaries' activities. In particular within VSLA localities, savings groups self-replicate or spread to include more people among the non-beneficiaries.

The paper is structured as follows. Section 3.2 presents the intervention and data. Section 3.3 presents the experimental design. Section 3.4 discusses results on local indirect impacts and Section 3.5 concludes. Tables are presented in appendix.

## 3.2 Intervention and Data

### 3.2.1 Post-conflict Western Côte d'Ivoire

We study a program designed as a post-conflict intervention aiming to promote economic empowerment and social cohesion. It was implemented between 2014 and 2017 in Western Côte d'Ivoire in four regions : across the Montagne district (regions of Tonkpi, Cavally and Guémon) as well as neighboring Bafing region.

The Western region is the most unstable area of the country. The region is marked by chronic instability, and repeated violent events. Notably, the four regions together have registered more than half of the victims of the 2010-2011 post-electoral crisis outside Abidjan.<sup>4</sup> Land, security and identity issues have fuelled conflicts across ethnic groups. Three interrelated factors have contributed to the fragility of this highly diverse area : the land, the politics, and the border with Liberia (Group (2014)).

The land of the Western regions is fertile and has attracted many migrants for decades. The region is an important producer of cocoa, coffee, and other cash crops. However, in the absence

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<sup>4</sup>774 deaths occurred in Guemon and Cavally out of 3,248 (which includes 1,497 in Abidjan). "Rapport d'enquête sur les violations des droits de l'homme et du droit international humanitaire survenues dans la période du 31 octobre 2010 au 15 mai 2011", CNE, July 2012.

of a formal land tenure system, land ownership has been a source of conflicts.<sup>5</sup> To simplify, three groups dispute ownership and use of the land in the region : (i) "native" groups ("*autochtones*"), (ii) migrants from other regions ("*allogenes*"), (iii) migrants from abroad ("*allochtones*"). The native groups of the area are the Krou/Wê group (which include Guéré and Wobé), and are the traditionnal landowners. The Mandés (Yacouba and Dan) are also native but live further north in the mountains. Ivorian migrants come mainly from the center of Côte d'Ivoire (Baoulés) and the north (Malinkés, Dioulas, Sénoufos). Migrants from abroad are usually identified as coming from Burkina Faso (Mossis).<sup>6</sup> The Wê group, living in the fertile forest areas, rented land to other groups. Informal agreements to sell or lease the land to newcomers have been active for decades (Chauveau and Dozon (1987)). The economic crisis in the 90s led many natives to come back in the region and claim back land, in the name of customary law.<sup>7</sup>

The competition for land has been further instrumentalised by the political parties. In parallel to the economic crisis, a controversial political debate started around the question of nationality, "l'Ivoirité".<sup>8</sup> The polarisation increased as political parties used the different groups to mobilise votes for the presidential elections, claiming they would defend their rights against the others. Violent incidents opposing the different communities occurred regularly, and the 2002 war further increased violence. The Western region itself was divided by the buffer zone established in 2004. The 2011 post-electoral crisis was another opportunity for confrontations. Two remarkable episodes of violence have taken place during the 2011 post-electoral crisis. An attack at Duékoué in 2011 (quartier Carrefour) in which 300 people, mainly Guérés, were killed. In July 2012, the destruction of the a camp for 5,000 internally displaced people (mainly Guérés) in Nahibly, close to Duékoué, was destroyed. Both Duékoué and Nahibly are in the sample.

The border with Liberia has aggravated the situation since Liberia's civil war. During the post-election crisis, mercenaries crossed the border and participated in looting and massacres. There are refugee camps for citizens of both countries on both sides of the border. In 2013, after the

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<sup>5</sup>Land ownership was previously governed by traditional arrangements (tutorat). Several landowners (natives) took benefit from a system of arrangements in which others (migrants) use the land and pay a fraction of what they produce, or are given shares of the land.

<sup>6</sup>However note that some are second or third generation and actually citizens of Côte d'Ivoire. To further complicate, Mossis and Northern ethnic groups are culturally close to each other.

<sup>7</sup>When the economic crisis started and land conflicts increased, a new rural land code (1998) was established. This law in particular stipulated that non-Ivoriens could not own land. However by putting all Ivoriens equal in front of the land issue, the new code did not solve the issue of land ownership from the point of view of native groups. It remained largely unenforced.

<sup>8</sup>The debate around nationality is explained by political stakes. Granting citizenship to foreign ethnic groups (mostly from Burkina) who have settled in Côte d'Ivoire would favor a specific party / candidate.

end of the political crisis, several militia from Liberia made incursions from Liberia.

At the time of implementation, the region has come back to a relative peaceful status, but tensions across communities are not solved and remain palpable. While the program started in 2014, disarmament campaigns continued until June 2015 and the region was the last to be considered safe and stable.

### 3.2.2 Intervention

We provide a short description of the intervention here for the chapter to be a standalone paper, however this is the same intervention presented in Chapter 2.

It is a multi-faceted program, combining a training and capital component to address the multiple constraints faced by the poor.<sup>9</sup> All beneficiaries are invited to participate in an entrepreneurship training, lasting 55 hours in total. The training is designed to provide support and incentives in starting an independent activity (self-employment). A simple business plan is developed with the help of trainers as part of the training, in which participants identify their financial needs. Some modules have been developed to transfer relevant skills for the management of the activities, including how to fix the right price, basic book-keeping and stocks management.

To facilitate access to capital, the training is complemented by a financial component with 3 alternative modalities detailed below : cash grants, cash-grant-with-repayment or set up of village savings and loan association (VSLA). Two modalities are capital injections : depending on their locality, beneficiaries receive either a "cash grant" with no condition attached, or a "cash-grant-with-repayment" of similar amount but with the requirement to repay half of the grant.<sup>10</sup> On average, the grant was worth 95,000 CFA (USD 163 nominal, USD 384 PPP in 2017) per beneficiary (similar across the two modalities).<sup>11</sup> In both interventions, community experts developed

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<sup>9</sup>The program is named « PRISE ». It was funded by the Japanese Social Development fund through the World Bank. The program was coordinated by the "Office Coordinating Employment Programs" (BCP Emploi), which is part of the Ministry of Youth Employment. The program was implemented by the International Rescue Committee (IRC), a prominent NGO which has offices in the Western region and a long experience with these type of interventions in Côte d'Ivoire and beyond.

<sup>10</sup>Recollection was supposed to start 3 month after disbursement, but it started 6 months after. At the end of the recollection period only 39.96% of the targeted recollection amount was achieved. on average, only 20% of the grant (instead of the 50%) was repaid in this intervention arm. Less than one third of beneficiaries successfully reimbursed half of the grant. Given that the expectations to repay were maintained, the cash-grant-with-repayment intervention cannot be fully assimilated to the "cash grant".

<sup>11</sup>We used the exchange rate USD 1=XOF 580.657 (official average exchange rate 2017 (IMF)) to convert CFA francs to US dollars. We used the PPP conversion factor, GDP (LCU per international \$) of 247.134 for Côte d'Ivoire in 2017 (World Bank).

“entrepreneur support groups” to facilitate discussion and support between beneficiaries. The cash-grant-with-repayment intervention also included some additional follow-up from NGO field agents, who monitored participants following the disbursement of funds.

For the third modality, there was no cash injection. Instead, Village Saving and Loan Associations (VSLAs) were put in place in the remaining localities by the implementing partner, the NGO IRC. VSLAs are “enhanced” rotating savings and credit associations (ROSCAs).<sup>12</sup> In the “VSLA” intervention, there is no cash injection, but beneficiaries can use the share-outs at the end of the cycle or request a loan from their VSLA to access additional capital to invest in a business. VSLAs received regular follow-up from the NGO during the full first cycle, which lasted 9 months. Beneficiaries were invited to start a second cycle, after the end of the program. At the end of the program, facilitators were also trained and encouraged to further diffuse the VSLA model.

Figure 3.1 provides a summary of the content of each intervention arm (see also Table 2.1, Chapter 2).

We evaluate the largest phase of implementation, which started in July 2015, lasted for 2 years and covered 16 sub-prefectures. The take-up of the program is high across interventions.<sup>13</sup>

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<sup>12</sup>A ROSCA is an informal association or group in which members contribute to a common pot of money that is awarded to a different member at each meeting. Compared to ROSCAs, VSLAs are more secure and provide loans at low interest rates. The VSLA model was launched by CARE in Niger in 1991. Beneficiaries are invited to create an association, elect a committee, and regularly meet (weekly or biweekly) to contribute savings to a common pot. After they have reached a certain level of savings, participants can request loans from the VSLA at a rate pre-determined at the start of the cycle. At the end of the cycle (which lasts 9 to 10 months), the pot is distributed among participants proportionally to their saving shares. The savings include some remuneration for participants as the final pay-out includes interests paid by borrowers to the pot. The possibility to take loans at any time during the cycle (after the first four months) is a key advantage compared to ROSCA : it enables individuals to access capital exactly when they need, while in ROSCA the timing to access the pot is constrained and usually predetermined.

<sup>13</sup>In the cash-grant-with-repayment group, 78.9% of selected individuals received the funds and 81.1% in the cash grant group. The take-up rate is lower for the VSLA intervention, with 69.5% of selected beneficiaries joining a VSLA, explained by a lower participation rate in urban areas. Table 2.2 in Chapter 2 presents in more details the break-down for various components of the program, including the entrepreneurship training.

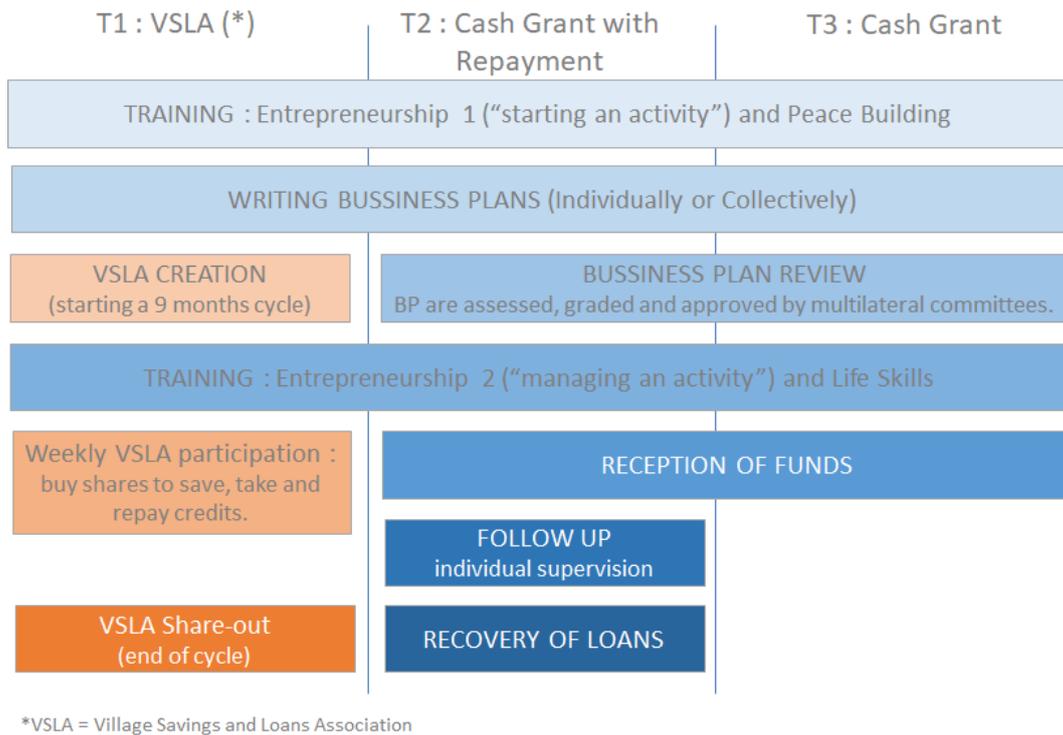


Figure 3.1 – Components of the integrated interventions

### 3.3 Experimental design

#### 3.3.1 RCT design

A randomized controlled trial (RCT) was embedded in the program from the beginning. The objective was to measure both the direct impacts of the program, as well as indirect impacts (spillovers) for non-beneficiaries in treated villages. Chapter 2 presents the results of the evaluation regarding the direct economic impacts on beneficiaries, to assess the effectiveness of the overall program as well as the relative effectiveness of the three alternative modalities to address capital constraints: (i) Village Savings and Loan Associations, (ii) cash-grant-with-repayment, and (iii) cash grants. In this chapter, relying on the same RCT design we are able to assess how the economic program indirectly affected other dimensions : the economic situation of other villagers, as well as local social impacts.

The design of the RCT is presented in detail in chapter 2. We present here only the main aspects. The identification of indirect causal impacts relies on a rigorous design combining the randomization of localities, and a transparent rule to select individuals in the program, presented below.

## Locality randomization

The RCT was embedded in the second phase of the program implemented in 16 sub-prefectures across the four regions. A first (public) lottery was organized in August 2015 to sample 207 localities for the study. It was followed by a second (public) lottery in March 2016, assigning each locality to one of the three intervention arms or the control group.<sup>14</sup> 60 localities were assigned to the control group (C), 53 localities to the “VSLA” intervention (T1), 64 localities were assigned to the “cash-grant-with-repayment” intervention (T2), and 30 localities to the “cash grant” intervention (T3). Figure 2.1 in Chapter 2 summarizes the experimental design. Table 3.1 presents the distribution of localities across interventions with the breakdown of rural/urban localities.

## Individual beneficiary selection

The enrollment phase took place between December 2015 and January 2016, between the two lotteries. The timing is key since baseline data was collected at the same time as enrollment, and required all individuals to have the same level of information regarding the program. This was the case, since it happened before the assignment of localities to treatment arms and control group. 12,696 individuals enrolled across the 207 localities and were considered eligible to the program.<sup>15</sup> Village-committees were formed as another layer of verification, in particular regarding eligibility.<sup>16</sup>

The data collected was further used to compute a vulnerability score, which would determine selection in the program in each locality. Indeed, as expected the program was over-subscribed : out of the 12,696 eligible individuals, only 5,116 could be selected for the program (due to budget

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<sup>14</sup>The two lotteries were stratified by clusters of sub-prefectures and urban/rural areas. “Urban” areas include city neighborhoods (“*quartiers*”) as well as peri-urban settlements (locally known as “*campements*”) . “Rural” comprises villages.

<sup>15</sup>Eligible individuals had to be 18-40 years-old and not benefit from another assistance program. Few exceptions existed regarding the age criteria : single mothers (above 15 years old) and widows or disabled people (up to 60 years old) were eligible too. Regarding other assistance programs, the lists were cross-validated with other development partners.

<sup>16</sup>A list of “pre-selected” individuals was shared to village committees. Committees were asked to verify if people might have lied on key personal information or eligibility status. The NGO took decisions based on that. Unfortunately, the village committee feedbacks could not be implemented in the 60 control villages because the assignment lottery had already taken place. However, only 3.3% of the applicants in treated villages were disqualified as a result of this. We consider this is reasonably low not to affect the validity of our methodology.

limit). We are interested in the remaining individuals, eligible but not selected. The selection was based on an individual vulnerability score computed using the enrollment data.<sup>17</sup> Therefore we can identify similar non-selected individuals as those below the vulnerability cut-off. The cut-off used is identical for all localities implementing the same intervention arm, but it varies across the three interventions based on the program budget (see Figure 3.2).<sup>18</sup>

### 3.3.2 Surveys

**Baseline survey.** The baseline survey was implemented during enrollment, therefore using a short instrument. Baseline data include basic measures of employment, assets, education level, household characteristics. It also captures some indicators related to social cohesion.

**Timeline.** Figure 2.3 in Chapter 2 summarizes the program and study timeline. Beneficiaries of the two grant interventions received the entrepreneurship training late 2016. The business plans they developed were reviewed by committee, and those approved received the grants between March and July 2017.<sup>19</sup> In parallel, in VSLA localities, VSLA groups were set up late 2016 and the entrepreneurship training received later, early 2017. The first cycle of the VSLA groups ended between September and December 2017.<sup>20</sup> More details on the implementation timeline can be found in Chapter 2.

**Endline Survey.** The endline survey was conducted on a sample of 5220 individuals between July and September 2018, which is on average 15 months after the end of the program.<sup>21</sup> The endline sample is made of two sub-samples. The main sample comprises 3,624 “selected” individ-

<sup>17</sup>The composition of the score is detailed in the appendix of Chapter 2, table 2.13. The score weights criteria include disability, marital status, education, employment, economic status, assets and economic responsibility towards household. Weights were chosen to maximize the dispersion of the score.

<sup>18</sup>Specifically, it depends on the number of eligible enrolled candidates before the cross-validation and village committee checks. In each location, the capacity of the program is set at  $N_{ij}$ , which is determined as  $N_{ij} = \text{Population target}_i * \text{Nb of eligible people}_j / \text{Total nb of eligible people}_i$ , where  $i$  refers to the treatment arm and  $j$  to a locality.

<sup>19</sup>The delivery of funds was delayed for two reasons. First, unrest in the area led to the suspension of fund delivery between May and June 2017. The unrest was led by mutinous soldiers who claimed unpaid salaries for their help to the president during the 2011 post electoral crisis. Second, the banking partners encountered difficulties in delivering funds in remote locations. To solve this issue, beneficiaries in remote locations were given a lump sum to reimburse the transportation costs to the closest bank branch.

<sup>20</sup>Although there was no cash injection, beneficiaries had the opportunity to take loans from the group from February 2017 and received an important cash inflow (the total amount of savings plus interests) at the end of the first cycle when the pot was shared across members.

<sup>21</sup>Between 12 and 18 months after payments for “cash grant” and “cash-grant-with-repayment” interventions, or between 10 and 13 months after the end of the 1st cycle of the VSLA).

uals (above the vulnerability cut-off in treated and control localities) and is used to estimate the direct impacts on social outcomes (as well as direct economic impacts in Chapter 2). The spillover sample comprises 1,596 “non-selected” individuals (below the vulnerability cut-off in treated and control localities), used to study indirect impacts. Control group individuals were sampled using the simulated vulnerability cut-off. Sample size was determined by power calculations (counter-balanced by budget considerations), presented in Chapter 2. 10 “non-selected” individuals were sampled out of all villages to study spillovers. Note that the spillover sample has been restricted to villages (therefore excluding districts of cities) in which we expected the magnitude of the spillovers is likely to be higher.

The final attrition rate was 7.8% (using the common support cut-off) and was balanced between treatment and control groups. Attrition rate for the main sample is similar and reported in Chapter 2.<sup>22</sup>

In this chapter, we use the social cohesion modules administered to the main sample which covers social relationships, community activities and interpersonal trust. A shorter version of the full questionnaire was administered to the spillover sample, covering employment, assets, income-generating activities, savings as well as similar social cohesion modules.

### 3.3.3 Key outcomes

For the analysis of indirect economic effects, since data were collected using the same (although shortened) questionnaire, our main set of outcomes corresponds to the one described in Chapter 2.

It comprises *Employment* status (decomposed between wage employment, self-employment in agriculture and self-employment in non-agricultural activities), the total *Number of independent activities*, *Monthly Earnings* (in CFA francs ) reported separately for wage employment and self-employment (profits self-reported), as well as *Hours worked*.

Intermediary economic outcomes are defined as in Chapter 2 and include *Savings Stock*, *Start-up capital* and *Value of productive assets* in all operating independent activities, *Investments* made in the main independent activity, a *Business practices* index related to practices used in managing

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<sup>22</sup>Attrition is respectively 7.92%, 8.22% and 8.17% across T1, T2 and T3 (using the common support cut-off). Over the total spillover sample (not using the common support cut-off), attrition rate is on average 8.4%.

one's activity.

For the analysis of (direct and indirect) social impacts, we measure a second set of key social outcomes. We capture the *total number of groups or associations* in which the individual participated over the last 12 months. It aggregates economic groups and non-economic groups (political, religious, women and youth groups). *Solidarity received* (respectively *Solidarity given*) is the sum of the number of times the participant received help from other (and respectively the number of times she/he helped others). "Help" is defined as giving cash for food or health care, school fee, or business inputs. *Participation to community works* (cleaning, rebuilding public infrastructure) and *Participation to social activities* (celebrations, funerals, festivals) proxy levels of involvement in the community and is computed as the number of times one participated to the related activities over the last 12 months. *Trust* is a z-score index that captures the level of trust in various type of socio-economic relationships and with different groups and institutions.<sup>23</sup> The index is demeaned and standardized using the control group (separately for selected and non-selected groups). *Conflicts* is a z-score index that accounts for the reported frequency of robbery, racket, physical assault and armed conflicts.<sup>24</sup> We also capture individuals' *Perception of insecurity*, by questions about whether the participants ever feared to be victim of physical violence in the past 12 months and about how she/he sees the general level of insecurity due to the presence of other ethnic groups in the locality.

### 3.3.4 Descriptive statistics

We provided in Chapter 2 a description of the people who applied to the program.

The non-selected individuals differ from the selected, as a result of the selection based on the vulnerability score (see Table 2.14 in Chapter 2). We consider them as "near poor", as they are less vulnerable than individuals in the program. Non-selected individuals have a higher level of assets (both livestock and durable productive assets) than beneficiaries and face less constraints for education and health expenditures (by 4pp). The use of their labor is relatively similar in

<sup>23</sup>It includes measures of trust in general and trust in economic relations (such as credit, sharing agreements). It also includes different measures of trust towards neighbors, people of same and different ethnic groups, local leaders, and foreigners. Finally, it includes opinion on the way the presence of other ethnic groups in the locality affect the economy and security in the locality.

<sup>24</sup>For robbery and racket, the variable takes the value of 1 if those events happen "often". For physical assault and armed conflicts, it takes the value of 1 when it happens "sometimes" and "often" (and 0 when it never happens).

terms of type of activities, with a large share involved in agricultural self-employment while being also involved in non-agricultural activities. Financial inclusion indicators are also slightly better in terms of propensity to save (+5pp) and savings flux. However, participation to savings groups is similar and averages 50%.

### 3.3.5 Estimation strategy

Similar to Chapter 2, we estimate intent-to-treat treatment effects by taking differences in outcomes between treatment and control groups at endline. The key difference here is in the sample we use. For direct social impacts, we use the main sample as in Chapter 2 while for indirect impacts, we use the spillover sample of non-selected individuals. For a given outcome  $Y$  we run the following ordinary least squares (OLS) regression:

$$Y_{i,j} = \alpha_1 + \beta T_j + S_j + \epsilon_{i,j} \quad (3.1)$$

where  $i$  indexes the individual and  $j$  indexes the locality.  $\beta$  is the pooled Intention-To-Treat (ITT) estimate of the program's overall impact.  $T_j$  is assignment to treatment in locality  $j$  and  $S_j$  includes stratification variables corresponding to the lotteries, i.e. a dummy for villages or city districts within each a sub-prefecture.<sup>25</sup> Robust standard errors are clustered at the locality level. Monetary outcomes are winsorized at the 99th percentile.

To obtain estimates of the relative impact of each treatment modality, we estimate the following equation:

$$Y_{i,j} = \alpha_1 + \beta_1 * T_{j,1} + \beta_2 * T_{j,2} + \beta_3 * T_{j,3} + S_j + \epsilon_{i,j} \quad (3.2)$$

$\beta_k$  coefficient corresponds to the effect of being in a locality assigned to intervention arm  $k$ .<sup>26</sup>

Note that by design, the spillover sample was restricted to villages (which means excluding urban districts) since spillovers were expected in areas where the share of the treated population is higher.<sup>27</sup> To study indirect impacts a common support cut-off is used to focus on comparable

<sup>25</sup>When studying indirect impacts, the sample does not include city districts. Therefore this stratification variable is not used

<sup>26</sup>Coefficient  $\beta_k$  can also be estimated separately for each treatment arm T1, T2 and T3. This provides more statistical power because the full sample of selected individuals is used in each regression. However, specification 3.2 is preferred because it allows for an easier comparison of the impacts across treatment arms.

<sup>27</sup>We expected the treatment intensity to be higher in villages than in cities, however we do not have a proper

groups (below the cut-off) across treatment and control villages. Because each treatment arm used a different vulnerability cut-off to select beneficiaries, it is not possible to simply pool observations across arms. This is illustrated in Figure 3.2). Using the common cut-off, the spillover sample is restricted to 1201 individuals.<sup>28</sup>

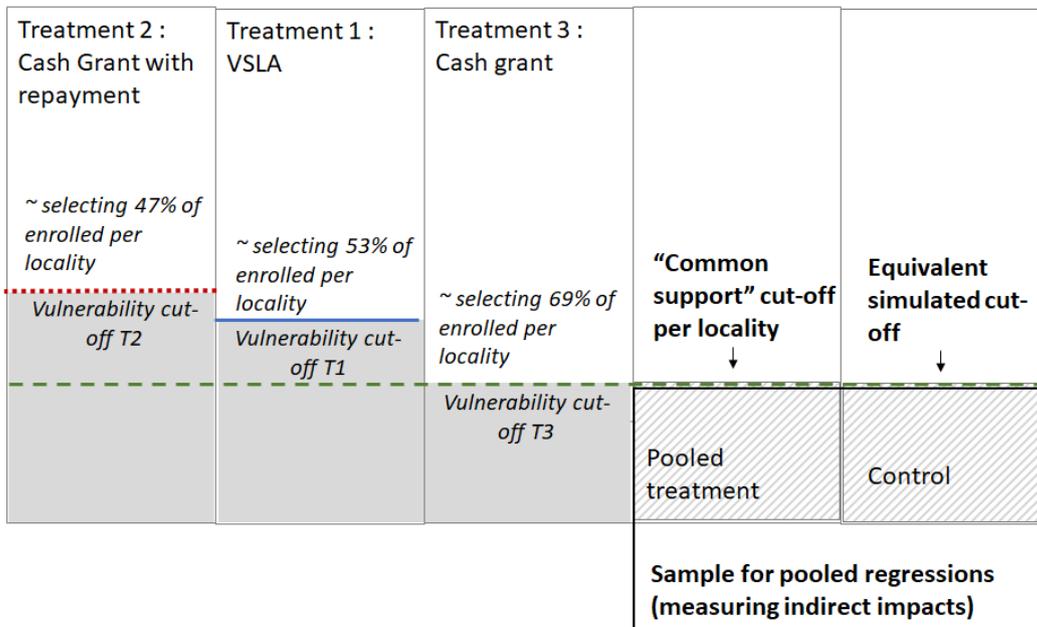


Figure 3.2 – Selection cut-offs by intervention arm and pooled sample to estimate indirect impacts

### 3.3.6 Balance and attrition

Table 3.11 shows baseline sample means for the non-selected group in treated and control localities, and tests for balance between each treatment modality (using a common support cut-off). In Chapter 2, we present similar results on the main sample, for the selected group (2.15).

Results shows that the experiment achieved satisfactory balance both between the pooled treatment and control group, as well as between treatment modalities. This is true for both main and spillover samples, although we present only the balance for the latter in this chapter.<sup>29</sup> There are

measure of intensity since we lack data on the total population. Treatment share is proportional to the number of interested people eligible. Contrary to ? recommendation, we could not vary treatment intensity in an exogenous way.

<sup>28</sup>Using a common decreases our statistical power. We also look at pairwise comparisons between each treatment arm and the control group using the whole sample of individuals below the selection cut-off for that specific arm. Since this does not require using a common cut-off, the approach leads to slightly improvements in statistical power. In practice these gains are marginal. These results are not reported here, though when meaningful differences arise, they are noted.

<sup>29</sup>Balance tables are shown for observations effectively interviewed at endline. Balance checks were also conducted on the full sample of eligible applicants. They also showed good balance.

very few statistically significant differences, and they remain of small magnitude. 1 variable out of 33 exhibits a difference at the 5 percent level for the pooled treatment (respectively 2 out of 33 variables for the test of equality between treatments).<sup>30</sup>

## 3.4 Indirect economic impacts and overall impact on social outcomes

In this section we present estimates of the indirect impacts of the intervention on employment and business outcomes (Table 3.2 to 3.6), as well as the total impact on social cohesion for both beneficiaries and non-beneficiaries (Tables 3.7 and 3.8). In each table, we present ITT estimates for the overall (indirect) impact of the pooled treatment in Panel A, and we document differences across intervention arms in Panel B.

### 3.4.1 Local spillovers on economic outcomes

The design of the RCT combined with a threshold for selection allows us to identify indirect impacts on non-beneficiaries who were eligible to the program but not selected (referred as “non-selected” individuals) in treated villages. Non-selected individuals were ranked below the cut-off selecting the most vulnerable in the program. Consequently, our measure of indirect impacts is valid for “near” vulnerable people, but might not be representative of wealthier groups. The spillovers we identify are within locality. We study economic spillovers on non-selected individuals for two reasons.

First, there might be positive economic spillovers at the locality level in terms of employment, income-generating activities and savings of non-beneficiaries. The program sought to generate new economic dynamics in the village by supporting business creation and savings accumulation. As shown in Chapter 2, the three interventions led to the creation of new activities operating at a larger scale. Since new income-generating activities were trying to connect to local value chains and rely on local labor supply, it is possible that the program generates positive spillovers on

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<sup>30</sup>The significant differences are the following. There are slightly less individuals head of the household in treated villages (6.5pp difference). Having worked in an agricultural activity is similar between pooled treatment and control but differs across treatment arms (magnitude of no more than 7pp). The same holds for not using accounting practices (of any sort) (magnitude of the difference up to 8pp).

non-selected individuals within the same village.

Second, and going in the opposite direction, there are concerns that new income-generating activities among beneficiaries crowd-out opportunities for others. The same applies to savings : the creation of VSLA may disrupt informal sharing networks with other villagers, affecting their net impacts. In this section, we consider if one of those two effects is at play, and what is the net impact of the intervention on the local economy.

On average, and this is an important policy finding, non-selected individuals are not economically worse-off. Table 3.2 shows that there is no significant local spillover on earnings among non-selected people.<sup>31</sup> The coefficients of the estimated indirect effects on earnings from wage employment and self-employment have opposite signs, but are far from statistical significance.

In fact, there are positive indirect effects on employment within localities (Table 3.3). Similar to beneficiaries, non-selected individuals have high employment rates (close to 98 percentage in the control group) and therefore spillovers on entry in employment are unlikely. There is no change in the share of people working, or on participation in wage or self-employment. The same holds for work intensity, measured by hours worked over the last week. However, the number of activities performed by individuals significantly increased. This is equivalent to one out of three individual adding a new activity to her or his portfolio (+0.35 activities, a 11% increase, Table 3.3). This highlights important spillovers. The magnitude of the effect (and standard error) is similar to the one found for direct beneficiaries. The spillover effects are clearly concentrated on agricultural independent activities (+0.32 activities, Table 3.3), while the direct effects had significantly increased both agricultural and non-agricultural independent activities (Table 2.4).<sup>32</sup>

Does it mean that there was no crowding-out? The positive net effect on the number of independent activities could hide business closures due to new competition driven by the program. We find no impact on the number of activities stopped by non-selected people in the last 12 months. In addition, a self-reported indicator on the level of local competition regarding the main independent activity of the respondent is also unchanged. Overall, we find no evidence that some businesses were negatively affected by the micro-entrepreneurship program taking place in the

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<sup>31</sup>We do not have indicators for consumption or psychological well-being among non-beneficiaries sample.

<sup>32</sup>We further investigate below whether some of these activities were collectively created between beneficiaries and non-beneficiaries from the same village, or if this corresponds purely to new activities as a “value-chain” reaction.

same locality.

Apart from the increase in income-generating activities, did the program lead to the creation of wage jobs in localities? We documented in Chapter 2 an increase in the number of individuals working in the independent agricultural activities of direct beneficiaries (+5.75 individuals, Table 2.7). This suggests positive effects on employment at the local level. However, we do not detect impacts on entry or hours spent in wage-employment, nor on earnings from wage-employment among non-selected individuals from the same locality. Therefore it is difficult to conclude whether activities from beneficiaries further employed other villagers or not (for near vulnerable individuals).<sup>33</sup>

There are also positive and significant indirect impacts on the value of productive assets held by non-selected individuals, driven by assets in agricultural activities (+10,211 CFA, Table 3.4). An indirect impact on the value of productive assets of 10,211 CFA is large, and surprisingly of similar magnitude to the one observed among direct beneficiaries (+10,991 CFA). In addition, investments in the main independent activity doubled (+6,610 CFA on average, column 5), although this is measured with large standard errors and significant only at 10%.

Together, the set of indicators presented in Table 3.4 suggests that the program generated additional economic activities in the village, and did not lead to negative crowding out effects. The program indirectly supported productive investments in new or existing activities held by non-beneficiaries. The net impact on the number of activities is positive and driven by agricultural activities.

### 3.4.2 Potential mechanisms

#### *How do we explain those economic spillovers ?*

We now discuss mechanisms behind the observed spillovers among non-selected individuals at the

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<sup>33</sup>Surprisingly, we also observe a significant increase in the number of employed individuals in agricultural independent activities in the spillover sample (+6.97 individuals, Table 3.3), an effect of similar magnitude than for the sample of beneficiaries.

local level. Recall that for direct beneficiaries the program results in a higher number of micro-businesses, a higher level of available capital (increased savings) and to a lesser extent, slightly improved business practices.

Improvements in business practices are unlikely to explain spillovers. In theory, non-beneficiaries in treatment localities could benefit from diffusion of learning from the entrepreneurship training or imitation of new practices. Table 3.6 provides no evidence to support this mechanism. The index of business practices is unchanged among the non-selected individuals in treated localities, even if the use of formal book-keeping slightly increases.

We can also rule out the possibility that the new activities are jointly-owned between beneficiaries and non-selected people in the same village. In both grant treatment arms, program participants could pool resources for activities requiring more expensive equipment. Non-beneficiaries might have partnered with beneficiaries as well. We look at indicators on business ownership. Around one third of direct beneficiaries participate in an activity with joint ownership. The proportion is similar in the corresponding control group. We do not observe a change in the number of jointly-owned businesses in either samples (beneficiaries and non-selected individuals) (Table 3.3, column 7).<sup>34</sup>

The increase in activities for non-selected individuals is likely to be a combination of the three following channels.<sup>35</sup>

First, complementarities between activities created by beneficiaries and the activities of non-selected individuals can increase economic activity. In the control group, respondents declare that 54% of sales and 31% of suppliers happen or are in the locality. We have indicators of businesses' local integration for both beneficiaries and non-selected individuals, however it is hard to measure it precisely. Overall, we see that businesses mainly serve the local market but are not connected much to other local activities. Upstream, most of the supply comes from outside of the locality and there is no significant impact on the use of local suppliers among beneficiaries or non-selected individuals. Downstream, half of the market is local, but mostly business-to-consumer.

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<sup>34</sup>The table only shows the impact on jointly-owned businesses among the non-selected individuals, but a similar result holds among selected individuals.

<sup>35</sup>Note that we do not consider demand effects driven by increased income (or consumption) from beneficiaries, since those two variables are not significantly impacted by the program.

Less than 15% of individuals report selling to other businesses (in both samples, in the control group). We see no significant impact on serving local market either.<sup>36</sup> More integration could happen with other activities though, and would not be captured by our instrument (restricted to main activity) or our sample (near vulnerable).

A second possible mechanism is that although people do not start activities together, part of the grants is somehow "shared" with others, through informal or household networks. This is consistent with observing that close to half of the grant is not spent on business capital nor saved (although we do not account for consumables). Such "sharing" from the beneficiaries could take several forms, from debt reimbursement to social pressure, but note that we do not observe impacts on debts for beneficiaries. If so, this means that part of the impacts on assets in others' income-generating activities are driven by transfers made by beneficiaries (out of the grant received).

A last explanation is the increased availability (and possibly circulation) of capital in the locality. For cash interventions, the program led to large increases in savings among beneficiaries, especially in ROSCAs. We interpret this as a substantial share of the grant being saved which could benefit others. ROSCAs are rotating savings group that regularly provide a lump-sum to one of its participants, as such creating an opportunity to invest in income-generating activities. Similarly for the VSLA started by the program. Access to VSLA for other villagers as well as increased savings and credit among beneficiaries are likely to facilitate the circulation of capital in the locality, which could contribute to investments in non-beneficiaries' micro-businesses.

At the activity level, we cannot measure where the capital comes from (in particular if it was provided by other individuals from the locality). However, we do see impacts on investments made in the last 6 months (as reported earlier), which indicates circulation of capital. At the individual level, if more capital circulates in the locality, we can expect the non-selected individuals to increase their participation in savings groups (ROSCA or VSLA) to benefit from increased liquidity, and possibly "borrow" more from participants. Overall participation to any type of savings groups increased for the non-selected individuals living in villages allocated to the VSLA treatment (+7pp, Table 3.5, column 4), but not for the other intervention arms. Point estimates suggest again a shift from ROSCA to VSLA, although the magnitude is smaller and standard errors larger than the point estimate for direct beneficiaries (the negative coefficient for ROSCA

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<sup>36</sup>Tables not shown. One exception is cash grant villages for which there is a 23% increase in reporting that the local market is their main market.

participation is not significant at 10%, Table 3.5, column 6). More broadly, propensity to save increases slightly for non-beneficiaries (+6pp, Panel A, column 1) and similarly across arms.

### ***Access to enhanced savings groups and VSLA replication***

Positive spillovers on financial dynamics are observed among non-selected households in VSLA villages. The propensity to save increases (+6pp, Panel B, column 1) as well as participation to savings groups (column 4). This is the result of VSLA expansion. Participation to VSLA increases by 70% (+10pp, Panel B, column 5) and the savings stock in VSLA doubles (+4,553 CFA, column 6). There are two ways for VSLAs to expand. First, a natural expansion can occur when new members are added at the start of a new cycle of the savings group. In the content of the intervention, the first cycle was restricted to program beneficiaries. Non-beneficiaries could join in subsequent cycles, and most VSLA started a second cycle.<sup>37</sup> A second source of expansion is the replication of the model by other groups, who can learn how to run their own VSLA. Both mechanisms are likely to have occurred, since a given VSLA cannot expand beyond 30 to 35 members.

A key point is the quality of replicated VSLAs. Previous research (Beaman et al., 2014) has shown that depending on the type of replication (whether it is organic and led by villagers, or actively led by specialized VSLA trainers), the quality of savings groups can vary. The quality of a VSLA is related, among other things, to the possibility to take credits or to the existence of a solidarity fund. These VSLA features can further influence outcomes. In our context, we cannot identify whether the replication of VSLA was organized or led by VSLA trainers. Results show that access to credit through VSLA doubles (+8pp, significant at 1%) for non-selected individuals in VSLA villages (Panel B, column 12). However, the impact on credit amounts is positive, but not significant (Panel B, column 13). Note that we also observe an increase in VSLA participation in cash grant villages, although standard errors are large and a joint test does not reject equality of coefficients between the treatment arms. VSLA may have developed in some other localities, which most likely happened organically since we do not see impacts on credit

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<sup>37</sup>We have anecdotal evidence that non-beneficiaries wanted to enter savings groups during program implementation. There was a strict enforcement of the rule about VSLA participation during the first cycle, confirmed by monitoring data. We also know that most VSLA started a second cycle, both from monitoring data and survey data, see Chapter 2

through VSLA. However, note that the magnitude of impacts on non-selected households remain relatively limited in magnitude.

One concern expressed in the literature is that introducing VSLA could disrupt informal sharing arrangements among non-beneficiaries. We do see a reduction in the use and amounts saved in ROSCA, but standard errors are large and the coefficient is not significant. The same holds for the total level of savings, so that we do not observe significant disruptions.

### 3.4.3 Direct and indirect impacts on social outcomes

One of the main objectives of the experiment was to measure impacts on both economic and social outcomes. Indeed, improving social relationships was envisioned as a positive consequence of the economic inclusion program. This was considered as particularly relevant in a multi-ethnic fragile environment. How would the program affect social outcomes?

First, the program can induce a higher level of interactions across individuals from various ethnic groups. Second, it can lead to positive economic dynamics for beneficiaries cascading into others. Improved livelihoods are expected to increase individuals' support to the community, and solidarity between villagers. We test this mechanism by analyzing the level of interactions after the end of the program, through participation to groups or associations (including economic and social groups) and community activities (community works, sports, festivals). We want to see if the program helped different groups to interact more in the longer run. We then look at solidarity levels (financial support provided to others, and received from others). Solidarity could either increase because people are "wealthier", or because people have created stronger social ties thanks to regular interactions.

We expect the VSLA intervention to be more successful at increasing social interactions since it implies bi-monthly meetings over 9 months, while in grant interventions beneficiaries only got together for training and punctual activities. Conversely, we expect grant interventions to generate stronger economic dynamics driven by cash injections. The three interventions could lead to greater solidarity, through different channels.

Positive effects on economic and social interactions could translate into impacts on higher-level outcomes related to trust, sense of inclusion, and insecurity feeling. Those three outcomes relate to more personal perceptions. We measure changes in those outcomes to assess whether the in-

tervention led to deeper attitude changes.

Table 3.7 presents direct impacts on a range of social indicators while Table 3.8 presents similar results on non-selected individuals (within treated villages).

The program succeeded in generating positive impacts on social outcomes on its beneficiaries. Results show a moderate increase in the number of groups in which individuals participate (+0.18 groups, from a mean of 1.2 in the control group) (Table 3.7, column 1, Panel A). This is particularly driven by an increase in participation in economic groups, but not by an increase in participation in social groups. Beneficiaries participate in more group meetings and are also more likely to hold leadership positions. Notably, nearly all additional groups in which individuals participate are mixed ethnic groups. Those effects are observed consistently and with the same magnitude across program modalities, therefore they cannot be attributed only to the mechanical effect of creating savings groups in VSLA localities during the program.

Results also show a significant increase in solidarity among direct beneficiaries within localities (Table 3.7, column 2-3). There are significant increases both in the number of times individuals have been helped by someone (+0.16pp) or have helped someone else themselves (+0.28pp) (Table 3.7, column 2-3, Panel A). We also measure the amounts of solidarity transfers and see a significant increase in the amount individuals gave to others (Table not shown). Increases in solidarity are overall consistent across treatment arms, suggesting again no strong trade-offs in the social effects of the various interventions (p-values of equality tests in Table 3.7, column 2-3, Panel B).

Besides increases in group participation and in solidarity, no effects on broader indicators of community participation, inclusion, trust, or insecurity are observed (Table 3.7, column 4-8, Panel A). A composite indicator capturing the level of trust and attitudes towards other groups is unaffected by the intervention (Table 3.7, column 6). No increases in the participation in locality-wide events or community works are observed, consistent with very limited exclusion from these events being observed in the first place (Table 3.7, column 4-5). Finally, there are no changes in indices of insecurity perception nor on the conflict index capturing exposure to crime or other violent activities in the locality (Table 3.7, column 7-8).

Beyond direct program beneficiaries, do these social impacts extend to non-selected individuals?

Initially, there were high expectations that economic impacts at the locality level would benefit non-beneficiaries as well, and therefore raise the level of trust, inclusion and social cohesion in the locality as a whole. As described earlier, non-selected individuals in treated villages indeed benefit from increases in economic dynamics. However, this does not translate into improved social outcomes. As shown in Table 3.8, impacts observed on group participation and solidarity among beneficiaries do not translate to the non-selected. Participation in community works or community social activities are unaffected either. Trust, inclusion and insecurity remain unchanged as well.

Overall, we interpret these results as pointing to localized effects on social cohesion through group participation or solidarity, but which did not translate to more widespread gains in social cohesion in the localities. The similarity of results on social outcomes across interventions is important to note. While VSLAs are successful at bringing together people for an economic goal, the social interactions induced by frequent group meeting do not seem to be the main drivers of higher social cohesion. The business dynamics observed across the three interventions are more likely to explain the similar levels of increased solidarity for participants.

### *Heterogeneity of impacts on social outcomes*

In Tables 3.9 and 3.10 we present results on the heterogeneity of social impacts on beneficiaries. Heterogeneity analysis on non-selected people is limited by the sample size for the spillover sample. We analyze whether impacts on social outcomes vary depending on whether individuals are young or not (3.9), from native ethnic groups or not (Table 3.10), or from localities with higher level of ethnic diversity (more than 2 different ethnic groups) or not (Appendix, Table 3.13).<sup>38</sup> Table 3.14 in Appendix shows differences in impacts between rural and urban areas.

Overall, we do not detect strong heterogeneity on social outcomes between our socio-economic groups of interest. One important exception is for non-native ethnic groups. Impacts on social outcomes are lower for native ethnic groups than for non-native groups. The interaction coefficient is negative for all social outcomes, although only significant for solidarity received. Both

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<sup>38</sup>Belonging to a native ethnic group is a self-declared variable measured at baseline.

native and non-native ethnic groups participate more in (mixed) groups due to the program, but the magnitude of the impact is substantially higher among non-natives: participation to groups increases by additional 0.13pp for the non-native, leading to a 28% increase in total (compared to a 13% increase for native groups). Interestingly, receiving solidarity from others is significantly more frequent among non-native ethnic groups, with no impacts on native groups. This can be interpreted as a sign of improved inclusiveness in treatment localities.

For other groups, the heterogeneity is less clear. While one could have expected stronger impacts in villages, we do not find larger impacts on social impacts. On the contrary, being youth, male or living in an urban area seem to increase program impacts on solidarity provided to others (Table 3.9 and in Appendix Table 3.12 to 3.14, column 9). There are larger impacts on group participation for male compare to female who had already higher levels without the program.

Overall, we conclude that the average social impacts observed earlier are not specifically driven by one of the specified sub-groups.

## 3.5 Conclusion

Using a RCT design combined with a selection cut-off, we are able to measure the local (within-village) spillovers of an economic inclusion program providing skills training and access to capital to vulnerable households in post-conflict Côte d'Ivoire. We contribute to the existing research on potential adverse effects of micro-enterprise support interventions, to test whether the program had negative unintended effects. Finally, we measure the social impact of an economic program implemented in a fragile region, both on beneficiaries and others. Taken together with the results presented in Chapter 2, this shows a comprehensive picture of both economic and social impacts of an integrated economic program.

We do not find evidence of negative economic spillovers within villages. Independent activities operating on the same markets do not show decreased profits. The labor allocation of non-beneficiaries has not significantly changed, as well as their level of earnings. This is in line with the empirical evidence on integrated programs so far. [Banerjee et al. \(2015\)](#) find no externalities in the 3 countries (Ghana, Honduras, Peru) for which the design of the evaluation allowed to measure spillovers (within localities). [Bandiera et al. \(2017\)](#) do not observe crowding out of other

livestock rearing businesses. [Sedlmayr et al. \(2020\)](#) do not measure significant spillovers in the aggregate either. Overall our results show no major disruption caused by the combination of training and capital injection in existing markets.

Rather, we find signs of positive indirect effects on employment and income-generating activities among non-beneficiaries. [Bandiera et al. \(2017\)](#) had highlighted positive indirect impacts on the labor allocation of casual paid work of non-beneficiaries. We do not see changes in labor supply across occupations, but we observe an increase in the number of independent activities run by non-beneficiaries, and the level of productive assets. This is driven by agricultural activities. Beneficiaries might have partnered with other villagers to set up new activities, but this is not enough to explain what we observe given the low share of jointly owned businesses. An increased share of individuals report saving money. Financial flows and informal sharing arrangements in the locality are likely to have benefited non-beneficiaries, by fostering more investment in economic activities. We find evidence of increased paid labor in those activities, but we cannot confirm those results on the labor supply side.

VSLA have created positive dynamics benefiting non-participants who have later joined or created other savings group. More importantly, there is an increased access to credit through VSLA, in areas which are not served by micro-finance. Other studies on VSLA reported positive impacts of the replication of VSLAs ([Ksoll et al. \(2016\)](#), [Karlan et al. \(2017\)](#), [Beaman et al. \(2014\)](#)), but not how this further impacts the economic activities of non-beneficiaries. [Angelucci and De Giorgi \(2009\)](#) had found that informal risk-sharing networks can extend the positive benefits of a conditional cash transfer program in Mexico to the ineligible. We present another piece of evidence that increased financial relationships, for example through savings groups, can benefit others.

Two caveats need to be considered to correctly interpret those results. First, our design allows us to look at indirect effects on near vulnerable individuals, but not the rest of the population. Given that we use the selection cut-off implemented in the program among eligible individuals, the sample we use to study spillovers comprise less vulnerable individuals than the sample used to measure direct effects in Chapter 2. [Bandiera et al. \(2017\)](#) were able to study a much wealthier segment of the population, on top of studying the "near poor" as we do. Second, we have no clear measure of the saturation in treated villages, but it is overall modest to low. Our results do not tell what the level of externalities would be if the program was implemented with a much higher

saturation level. Current evidence goes both ways. [Egger et al. \(2019\)](#) find no economic spillovers in the evaluation of a more sizable one-time cash transfer (GiveDirectly, in Kenya), even in more saturated locations. However, [Rotemberg \(2019\)](#) find that around two-thirds of the growth of newly subsidized Indian firms are purely crowding out, hurting firms involved in less tradable goods.

The intervention took place in a post-conflict setting, which raised questions on whether it affected social outcomes in the locality, directly or indirectly. Evidence on social outcomes is scarce, rarely mentioned in the literature on micro entrepreneurship. While we start to accumulate evidence on graduation programs implemented in post-conflict settings (recent evaluations in Yemen, Afghanistan and South Sudan to which we referred) none of them report social impacts on the community. Reinforcing social cohesion was part of the rationale for this economic inclusion program, and it is also a rationale for other multi-faceted programs in fragile countries. This paper provides novel empirical evidence on the social impacts of an economic inclusion program, both on beneficiaries and non-beneficiaries.

Results show a moderate increase in the number of (mixed) groups in which beneficiaries participate, as well as in solidarity among beneficiaries. However, we do not find effects on broader indicators of community participation, inclusion, trust, or insecurity. We interpret these results as pointing to localized effects on social outcomes among beneficiaries, which did not translate to more widespread gains in social cohesion in the localities. This is consistent with finding limited social spillovers from the intervention. Despite a high frequency of interactions in savings groups meetings, the VSLA arm does not exhibit higher social impacts. Note that, interestingly, none of the VSLA studies in the literature find significant impacts on social cohesion (to our knowledge).<sup>39</sup> We are adding another piece of evidence to that.

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<sup>39</sup>See [Ksoll et al. \(2016\)](#), [Karlan et al. \(2017\)](#) and [Beaman et al. \(2014\)](#)

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## Appendix

## A Result tables

Table 3.1 – Summary of survey samples composition

		VSLA (T1)	Cash grant with repayment (T2)	Cash Grant (T3)	Control Group	Total
<b>A</b>	Number of localities (villages and districts)	53	64	30	60	207
	Among which villages	43	52	24	49	168
<b>B</b>	Number of selected individuals (eligible)	1,999	1,870	1,247	-	5,116
<b>C</b>	Among which sampled for endline survey (main sample)	974	919	818	1,218	3,929
<b>Cbis</b>	Among which sampled for endline survey (main sample restricted to common cut-off)	871	919	616	831	3,237
<b>D</b>	Number of non-selected individuals (eligible)	1,558	1,925	443	-	3,926
<b>E</b>	Among which sampled for endline survey (spillover sample)	426	512	209	836	1,983
<b>Ebis</b>	Among which sampled for endline survey (spillover sample restricted to common cut-off)	267	276	209	449	1,201
	Total Baseline sample	3,557	3,795	1,690	3,650	<b>12,692</b>
	Total Endline sample	1,297	1,431	825	1,667	<b>5,220</b>

Table 3.2 – Indirect impacts on earnings and profits

	(1) Earnings in Wage Employment (monthly)	(2) Earnings in Self Employment (Profits, monthly)	(3) Earnings in Self Employment : Non Ag. Activities (Profits, monthly)	(4) Earnings in Self Employment : Ag. Activities (Profits, monthly)
<b>Panel A. Pooled Estimates</b>				
Program Treatment (ITT)	-1,912.28 (1,724.72)	1,141.09 (3,964.52)	-1,662.02 (3,378.86)	2,712.78 (2,466.43)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes
Mean in Control	6,382.68	32,513.35	16,957.01	15,514.05
Observations	1,099	1,102	1,102	1,102
<b>Panel B. Treatment Arm Estimates</b>				
VSLA (T1) (ITT)	-1,372.32 (2,016.95)	1,347.10 (5,791.10)	-391.93 (4,970.00)	1,674.04 (3,281.00)
Cash Grant with repayment (T2) (ITT)	-2,803.23 (1,743.58)	969.78 (4,537.98)	-2,900.62 (3,266.45)	3,683.24 (3,569.55)
Cash Grant (T3) (ITT)	-777.81 (2,383.68)	1,148.59 (6,034.99)	-1,157.27 (4,355.10)	2,406.81 (3,524.55)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes
Mean in Control	6,382.68	32,513.35	16,957.01	15,514.05
p-value T1=T2	0.35	0.95	0.56	0.65
p-value T2=T3	0.28	0.98	0.64	0.77
p-value T1=T3	0.76	0.98	0.88	0.86
p-value T1=T2=T3	0.47	1.00	0.79	0.90
Observations	1,099	1,102	1,102	1,102

Robust standard errors clustered at locality level. Earnings and sales are in CFA franc and winsorized at 99%.

"Ag." stands for Agricultural.

\* p < .1, \*\* p < .05, \*\*\* p < .01

Table 3.3 – Indirect impacts on employment outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Employment (Has an activity of any type)	Self employed (at least 1 activity)	Wage employed (at least 1 activity)	# Independent Activities per indiv.	# Non-Ag. Independent Activities per indiv.	# Agricultural Independent Activities per indiv.	# Independent activities jointly owned	Total hours worked (last 7 days)	Hours worked in wage employment	Hours worked in self employment (non ag.)	Hours worked in independent ag. activities
<b>Panel A. Pooled Estimates</b>											
Program Treatment (ITT)	0.01 (0.01)	0.01 (0.02)	-0.01 (0.02)	0.35** (0.15)	0.04 (0.05)	0.32** (0.14)	0.13 (0.11)	-2.10 (1.88)	0.24 (0.73)	-0.63 (1.37)	-0.48 (1.70)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	97.9%	93.2%	12.4%	3.23	0.50	2.72	0.79	44.11	2.83	9.32	28.86
Observations	1,102	1,102	1,102	1,102	1,102	1,102	1,102	1,102	1,102	1,102	1,102
<b>Panel B. Treatment Arm Estimates</b>											
VSLA (T1) (ITT)	-0.00 (0.01)	0.00 (0.03)	0.00 (0.03)	0.41** (0.18)	-0.04 (0.07)	0.45*** (0.17)	0.07 (0.13)	-2.54 (2.29)	0.41 (0.95)	-2.72 (1.66)	0.94 (1.97)
Cash Grant with repayment (T2) (ITT)	0.01 (0.01)	0.02 (0.02)	-0.03 (0.03)	0.26 (0.19)	0.10 (0.07)	0.16 (0.18)	0.13 (0.14)	-2.60 (2.33)	-0.13 (0.91)	0.96 (1.83)	-2.55 (2.06)
Cash Grant (T3) (ITT)	0.02* (0.01)	0.01 (0.02)	-0.01 (0.03)	0.46** (0.22)	0.04 (0.08)	0.42* (0.21)	0.27 (0.19)	0.09 (2.44)	0.83 (1.23)	-0.33 (1.56)	1.77 (2.69)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	97.9%	93.2%	12.4%	3.23	0.50	2.72	0.79	44.11	2.83	9.32	28.86
p-value T1=T2	0.33	0.56	0.26	0.47	0.10	0.15	0.71	0.98	0.62	0.06	0.10
p-value T2=T3	0.53	0.77	0.63	0.40	0.54	0.30	0.50	0.29	0.47	0.49	0.11
p-value T1=T3	0.17	0.77	0.68	0.81	0.35	0.88	0.32	0.30	0.75	0.16	0.76
p-value T1=T2=T3	0.39	0.84	0.53	0.65	0.24	0.32	0.60	0.48	0.75	0.15	0.15
Observations	1,102	1,102	1,102	1,102	1,102	1,102	1,102	1,102	1,102	1,102	1,102
Robust standard errors clustered at locality level. Hours and days worked are winsorized at the 99th percentile. The number of independent activities per individual is based on the list of up to 12 operating activities. * p <.1, ** p <.05, *** p <.01											

Table 3.4 – Indirect impacts on business outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total start-up capital (all activities)	Value of productive assets (all activities)	Value of productive assets (non ag. activities)	Value of productive assets (ag. activities)	Investments in <i>main</i> activity (last 6 mths)	# individuals working in independent activities (all types) (last 30 days)	# individuals working in independent non ag. activities (last 30 days)	# individuals working in independent ag. activities (last 30 days)
<b>Panel A. Pooled Estimates</b>								
Program Treatment (ITT)	1,091.27 (5,120.99)	9,631.05 (7,824.18)	-507.06 (6,182.54)	10,210.73** (3,927.38)	6,609.88* (3,751.85)	6.23* (3.75)	-0.73 (0.86)	6.97* (3.79)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	25,358.29	59,658.83	30,252.69	29,296.44	7,126.27	66.22	5.27	60.94
Observations	1,102	1,102	1,102	1,102	1,102	1,102	1,102	1,102
<b>Panel B. Treatment Arm Estimates</b>								
VSLA (T1) (ITT)	-3,209.52 (5,080.75)	5,252.25 (9,122.65)	-1,350.40 (7,139.71)	6,715.91 (5,098.14)	14,146.48* (7,231.47)	8.48* (5.00)	-1.55 (0.94)	10.04** (4.93)
Cash Grant with repayment (T2) (ITT)	3,823.40 (6,901.26)	10,767.99 (9,806.66)	-659.39 (7,808.94)	11,509.25** (5,560.75)	2,201.99 (2,936.90)	2.47 (4.94)	-0.65 (0.98)	3.13 (5.02)
Cash Grant (T3) (ITT)	3,055.09 (7,695.99)	15,762.51* (9,165.47)	1,606.67 (6,203.42)	14,121.84** (5,467.70)	2,214.27 (5,969.27)	11.07** (5.06)	0.75 (1.45)	10.33** (5.03)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	25,358.29	59,658.83	30,252.69	29,296.44	7,126.27	66.22	5.27	60.94
p-value T1=T2	0.27	0.57	0.92	0.47	0.07	0.30	0.30	0.23
p-value T2=T3	0.93	0.61	0.74	0.70	1.00	0.16	0.32	0.23
p-value T1=T3	0.38	0.24	0.63	0.24	0.20	0.66	0.10	0.96
p-value T1=T2=T3	0.44	0.50	0.88	0.49	0.19	0.34	0.21	0.39
Observations	1,102	1,102	1,102	1,102	1,102	1,102	1,102	1,102
Robust standard errors clustered at locality level. Start-up capital, value of assets and investments are in CFA franc and winsorized at 99%. The main activity is directly identified by the respondent among the list of independent activities undertaken. * p < .1, ** p < .05, *** p < .01								

Table 3.5 – Indirect impacts on financial outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Has Saved (last 2 yrs)	Has Saved : formal savings	Has Saved : oth. informal savings	Has Saved: any saving group (last 12 mths)	Participate in a VSLA (currently)	Participate in a ROSCA (currently)	Savings stock (Total)	Savings stock : formal savings	Savings stock : VSLA	Savings stock : ROSCA	Savings stock : oth. informal savings	Has taken a credit from a VSLA	Credits taken from VSLA
<b>Panel A. Pooled Estimates</b>													
Program Treatment (ITT)	0.06** (0.02)	0.04 (0.03)	0.03 (0.04)	0.02 (0.03)	0.08** (0.03)	-0.04 (0.03)	-10,986.98 (14,747.08)	-7,292.48 (7,824.70)	4,770.73*** (1,502.82)	-7,858.09 (10,043.35)	35.53 (5,011.78)	0.04* (0.02)	1,347.00 (1,249.89)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	81.8%	24.1%	63.4%	43.7%	14.4%	28.1%	99,044	19,884	3,539	42,099	33,521	7.2%	2,615
Observations	1,102	1,102	1,102	1,102	1,102	1,102	1,101	1,102	1,101	1,102	1,102	1,102	1,102
<b>Panel B. Treatment Arm Estimates</b>													
VSLA (T1) (ITT)	0.06* (0.03)	0.00 (0.03)	-0.02 (0.05)	0.07* (0.04)	0.10** (0.05)	-0.02 (0.04)	-14,206.90 (16,351.49)	-7,363.58 (8,738.70)	4,553.32** (2,134.31)	-11,517.95 (11,145.30)	2,199.62 (5,715.15)	0.08*** (0.03)	2,021.27 (1,346.07)
Cash Grant with repayment (T2) (ITT)	0.05 (0.03)	0.04 (0.04)	0.07 (0.04)	-0.02 (0.04)	0.04 (0.04)	-0.04 (0.04)	-22,848.34 (15,239.54)	-7,656.44 (7,907.93)	4,737.46** (1,914.75)	-18,850.16** (9,355.21)	-1,152.79 (6,441.64)	0.01 (0.03)	688.47 (1,641.27)
Cash Grant (T3) (ITT)	0.08*** (0.03)	0.09** (0.05)	0.00 (0.06)	-0.00 (0.06)	0.12** (0.05)	-0.10*** (0.04)	25,366.19 (28,454.78)	-6,232.20 (8,652.75)	5,298.25* (2,823.74)	27,268.92 (25,458.08)	-1,421.04 (6,963.37)	0.05 (0.03)	1,617.42 (1,568.76)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	81.8%	24.1%	63.4%	43.7%	14.4%	28.1%	99,044	19,884	3,539	42,099	33,521	7.2%	2,615
p-value T1=T2	0.71	0.32	0.04	0.06	0.19	0.63	0.51	0.95	0.94	0.44	0.59	0.03	0.38
p-value T2=T3	0.28	0.30	0.23	0.77	0.14	0.15	0.08	0.81	0.85	0.06	0.97	0.17	0.59
p-value T1=T3	0.45	0.06	0.72	0.26	0.80	0.07	0.16	0.86	0.82	0.14	0.61	0.30	0.79
p-value T1=T2=T3	0.53	0.16	0.11	0.15	0.23	0.15	0.20	0.97	0.97	0.14	0.82	0.08	0.68
Observations	1,102	1,102	1,102	1,102	1,102	1,102	1,101	1,102	1,101	1,102	1,102	1,102	1,102
Robust standard errors clustered at locality level. Savings stock and credit amounts are in CFA franc and winsorized at 99%.													
Formal saving sources are microcredit institutions, banks, agricultural cooperatives, mobile money. Other informal saving sources are cash, informal saving collector, designated goods.													
* p <.1, ** p <.05, *** p <.01													

Table 3.6 – Indirect impacts on business practices and knowledge

	(1)	(2)	(3)	(4)	(5)
	Index of business practices (z-score)	Has done a market assessment	Has developed a business plan	Uses formal bookkeeping	Separate regular payments to self
<b>Panel A. Pooled Estimates</b>					
Program Treatment (ITT)	0.08 (0.07)	-0.01 (0.02)	0.00 (0.01)	0.04** (0.02)	0.02 (0.04)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes
Mean in Control	-0.00	9.7%	2.1%	9.4%	37.5%
Observations	1,102	1,102	1,102	1,102	1,102
<b>Panel B. Treatment Arm Estimates</b>					
VSLA (T1) (ITT)	0.07 (0.08)	-0.03 (0.03)	0.01 (0.01)	0.04* (0.02)	0.03 (0.05)
Cash Grant with repayment (T2) (ITT)	0.17* (0.09)	0.02 (0.03)	0.00 (0.01)	0.05* (0.03)	0.05 (0.04)
Cash Grant (T3) (ITT)	-0.09 (0.08)	-0.02 (0.03)	-0.01 (0.01)	0.02 (0.02)	-0.06 (0.04)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes
Mean in Control	-0.00	9.7%	2.1%	9.4%	37.5%
p-value T1=T2	0.28	0.15	0.93	0.83	0.67
p-value T2=T3	0.00	0.22	0.38	0.29	0.02
p-value T1=T3	0.06	0.84	0.37	0.35	0.08
p-value T1=T2=T3	0.01	0.32	0.57	0.47	0.05
Observations	1,102	1,102	1,102	1,102	1,102

Robust standard errors clustered at locality level.

Z-scores are centered on the control group. The index of business practices is based on four variables : market assessment, business plan development, use of formal bookkeeping and way of paying oneself.

\* p <.1, \*\* p <.05, \*\*\* p <.01

Table 3.7 – Impacts on social outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Participation to groups or associations (# groups)	Solidarity received : # times received financial support (last 12 mths)	Solidarity given : # times was financially supported (last 12 mths)	Take parts in community works (# times in last 12 mths)	Take parts in social activities (# times in last 12 mths)	Trust Index (z-score)	Conflict Index (being victim of) (z-score)	Insecurity Index (perception) (z-score)
<b>Panel A. Pooled Estimates</b>								
Program Treatment (ITT)	0.18*** (0.05)	0.21** (0.10)	0.29** (0.12)	0.04 (0.03)	0.18 (0.36)	0.03 (0.06)	-0.04 (0.04)	0.04 (0.06)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	1.19	0.88	1.31	0.61	6.59	0.00	0.00	0.00
Observations	2,620	2,620	2,620	2,620	2,620	2,374	2,614	2,617
<b>Panel B. Treatment Arm Estimates</b>								
VSLA (T1) (ITT)	0.18*** (0.07)	0.15 (0.13)	0.27* (0.14)	0.05 (0.04)	0.33 (0.45)	0.05 (0.08)	-0.01 (0.06)	0.03 (0.07)
Cash Grant with repayment (T2) (ITT)	0.16** (0.07)	0.12 (0.11)	0.31** (0.14)	0.03 (0.03)	-0.25 (0.40)	0.00 (0.07)	-0.05 (0.05)	0.03 (0.07)
Cash Grant (T3) (ITT)	0.20*** (0.08)	0.53** (0.23)	0.28 (0.19)	0.02 (0.04)	0.85 (0.52)	0.04 (0.11)	-0.10* (0.05)	0.07 (0.11)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	1.19	0.88	1.31	0.61	6.59	0.00	0.00	0.00
p-value T1=T2	0.75	0.84	0.78	0.49	0.18	0.53	0.58	0.99
p-value T2=T3	0.61	0.09	0.90	0.69	0.03	0.72	0.35	0.70
p-value T1=T3	0.82	0.13	0.94	0.35	0.33	0.93	0.17	0.71
p-value T1=T2=T3	0.87	0.23	0.96	0.62	0.08	0.81	0.37	0.92
Observations	2,620	2,620	2,620	2,620	2,620	2,374	2,614	2,617

Robust standard errors clustered at locality level. Z-scores are centered on the control group.

The *trust index* is based on 16 variables measuring different form of trust (economic, financial, security) and trust towards different types of groups or institutions (neighbors, same and other ethnic groups, leaders, foreigners). The *victim index* is based on 2 variables, measuring if the respondent has been victim of an attack or has been involved in a conflict over the past 12 months. The *insecurity index* is based on 6 variables, measuring the insecurity feeling of the respondent (perceived likelihood of a robbery, attack or shootings, subjective assessment of the peace status in the locality, fear of conflicts, knowledge of past conflicts in the locality).

\* p < .1, \*\* p < .05, \*\*\* p < .01

Table 3.8 – Indirect impacts on social outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Participation to groups or associations (# groups)	Solidarity received : # times received financial support (last 12 mths)	Solidarity given : # times was financially supported (last 12 mths)	Take parts in community works (# times in last 12 mths)	Take parts in social activities (# times in last 12 mths)	Trust Index (z-score)
<b>Panel A. Pooled Estimates</b>						
Program Treatment (ITT)	0.00 (0.06)	0.04 (0.19)	0.17 (0.20)	-0.00 (0.02)	-0.71 (0.44)	-0.06 (0.10)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	1.29	1.24	2.06	0.77	8.80	0.00
Observations	1,102	1,102	1,102	1,102	1,102	995
<b>Panel B. Treatment Arm Estimates</b>						
VSLA (T1) (ITT)	-0.05 (0.08)	-0.04 (0.30)	0.33 (0.30)	-0.01 (0.03)	-1.10** (0.53)	-0.11 (0.11)
Cash Grant with repayment (T2) (ITT)	0.02 (0.08)	0.18 (0.22)	-0.07 (0.22)	-0.01 (0.03)	-0.63 (0.58)	-0.00 (0.11)
Cash Grant (T3) (ITT)	0.05 (0.10)	-0.13 (0.24)	0.45 (0.36)	0.03 (0.03)	-0.11 (0.66)	-0.12 (0.14)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes	Yes	Yes
Mean in Control	1.29	1.24	2.06	0.77	8.80	0.00
p-value T1=T2	0.46	0.52	0.21	0.93	0.46	0.23
p-value T2=T3	0.78	0.26	0.16	0.17	0.48	0.32
p-value T1=T3	0.35	0.79	0.77	0.16	0.16	0.91
p-value T1=T2=T3	0.59	0.53	0.25	0.24	0.37	0.41
Observations	1,102	1,102	1,102	1,102	1,102	995

Robust standard errors clustered at locality level. Z-scores are centered on the control group.

The trust index is based on 16 variables measuring different form of trust (economic, financial, security) and trust towards different types of groups or institutions (neighbors, same and other ethnic groups, leaders, foreigners).

\* p <.1, \*\* p <.05, \*\*\* p <.01

Table 3.9 – Heterogeneous indirect impacts on social outcomes : by age (youth)

	(1)	(2)	(3)	(4)
	Participation to groups or associations (# groups)	Solidarity received : # times received financial support (last 12 mths)	Solidarity given : # times was financially supported (last 12 mths)	Trust Index (z-score)
<b>Panel A. Pooled Estimates</b>				
Treatment (ITT)	0.16**	0.17	0.09	0.09
Treatment x (Youth==1)	0.02	0.07	0.38*	-0.12
(Youth==1)	0.10	0.21	0.20	0.09
Department X (Urban/Rural)	-0.10	-0.08	-0.22	0.09
	0.08	0.16	0.16	0.08
Mean in Control (Youth==0)	Yes	Yes	Yes	Yes
Total Treatment Effect Youth==1	1.24	0.91	1.44	-0.05
p-value Total Treatment Youth==1	0.19	0.24	0.47	-0.03
Observations	0.01	0.11	0.00	0.74
	2,620	2,620	2,620	2,374
<b>Panel B. Treatment Arm Estimates</b>				
VSLA (T1) (ITT)	0.14*	0.02	-0.03	0.11
	(0.09)	(0.15)	(0.20)	(0.10)
Cash Grant with repayment (T2)	0.16*	0.10	0.23	0.06
	(0.08)	(0.15)	(0.19)	(0.09)
Cash Grant (T3) (ITT)	0.21**	0.64*	0.01	0.14
	(0.11)	(0.36)	(0.26)	(0.14)
VSLA (T1) x (Youth==1)	0.07	0.24	0.56**	-0.11
	(0.12)	(0.27)	(0.24)	(0.11)
Cash Grant with repayment (T2) x (Youth=1)	0.00	0.03	0.15	-0.10
	(0.10)	(0.23)	(0.25)	(0.11)
Cash Grant (T3) x (Youth==1)	-0.03	-0.21	0.52	-0.18
	(0.12)	(0.37)	(0.36)	(0.13)
(Youth==1)	-0.10	-0.08	-0.22	0.09
	(0.08)	(0.16)	(0.16)	(0.08)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes
Mean in T1 Control (Youth==0)	1.34	1.07	1.48	0.02
Mean in T2 Control (Youth==0)	1.33	1.04	1.38	0.02
Mean in T3 Control (Youth==0)	1.34	0.95	1.45	0.01
Total T1 Effect Youth==1	0.21	0.26	0.53	0.00
p-value Total T1 Effect Youth==1	0.02	0.22	0.00	0.98
Total T2 Effect Youth==1	0.16	0.13	0.38	-0.04
p-value Total T2 Effect Youth==1	0.06	0.43	0.04	0.63
Total T3 Effect Youth==1	0.18	0.43	0.53	-0.04
p-value Total T3 Effect Youth==1	0.04	0.06	0.05	0.71
p-value T1=T2	0.56	0.58	0.45	0.62
p-value T2=T3	0.79	0.21	0.60	0.99
p-value T1=T3	0.72	0.52	0.99	0.69
p-value T1=T2=T3	0.84	0.44	0.73	0.87
Observations	2,620	2,620	2,620	2,374

Robust standard errors clustered at locality level. Youth defined as up to 35 years old.

Earnings, sales, capital and value of assets are in CFA franc and winsorized at 99%.

\* p <.1, \*\* p <.05, \*\*\* p <.01

Table 3.10 – Heterogeneous impacts on main social outcomes : by ethnic group

	(1) Participation to groups or associations (# groups)	(2) Solidarity received : # times received financial support (last 12 mths)	(3) Solidarity given : # times was financially supported (last 12 mths)	(4) Trust Index (z-score)
<b>Panel A. Pooled Estimates</b>				
Treatment (ITT)	0.29**	0.72***	0.38	0.18
	0.12	0.21	0.28	0.14
Treatment x (Native Group==1)	-0.13	-0.61***	-0.11	-0.19
	0.14	0.23	0.31	0.14
(Native Group==1)	0.21*	0.38**	-0.06	-0.10
	0.12	0.15	0.24	0.12
Department X (Urban/Rural)	Yes	Yes	Yes	Yes
Mean in Control (Native Group==0)	1.02	0.47	1.37	0.08
Total Treatment Effect Native Group==1	0.16	0.11	0.27	-0.01
p-value Total Treatment Native Group==1	0.01	0.32	0.04	0.92
Observations	2,620	2,620	2,620	2,374
<b>Panel B. Treatment Arm Estimates</b>				
VSLA (T1) (ITT)	0.38***	0.89***	0.39	0.11
	(0.14)	(0.30)	(0.32)	(0.16)
Cash Grant with repayment (T2)	0.20	0.45**	0.73*	0.27*
	(0.15)	(0.19)	(0.38)	(0.15)
Cash Grant (T3) (ITT)	0.22	0.73	-0.30	0.19
	(0.16)	(0.55)	(0.37)	(0.19)
VSLA (T1) x (Native Group==1)	-0.23	-0.91***	-0.16	-0.08
	(0.17)	(0.32)	(0.35)	(0.17)
Cash Grant with repayment (T2) x (Native Group==1)	-0.06	-0.40*	-0.49	-0.31*
	(0.17)	(0.22)	(0.41)	(0.16)
Cash Grant (T3) x (Native Group==1)	-0.01	-0.24	0.73	-0.18
	(0.19)	(0.61)	(0.46)	(0.21)
(Native Group==1)	0.21*	0.38**	-0.06	-0.10
	(0.12)	(0.15)	(0.24)	(0.12)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes
Mean in T1 Control (Native Group==0)	1.14	0.85	1.59	0.16
Mean in T2 Control (Native Group==0)	1.20	1.04	1.48	0.08
Mean in T3 Control (Native Group==0)	1.20	0.98	1.73	0.09
Total T1 Effect Native Group==1	0.14	-0.02	0.23	0.03
p-value Total T1 Effect Native Group==1	0.08	0.88	0.13	0.70
Total T2 Effect Native Group==1	0.15	0.06	0.24	-0.04
p-value Total T2 Effect Native Group==1	0.05	0.66	0.11	0.56
Total T3 Effect Native Group==1	0.21	0.49	0.42	0.00
p-value Total T3 Effect Native Group==1	0.02	0.06	0.06	0.97
p-value T1=T2	0.95	0.59	0.97	0.38
p-value T2=T3	0.53	0.10	0.42	0.70
p-value T1=T3	0.49	0.06	0.42	0.83
p-value T1=T2=T3	0.76	0.18	0.69	0.67
Observations	2,620	2,620	2,620	2,374

Robust standard errors clustered at locality level.

Earnings, sales, capital and value of assets are in CFA franc and winsorized at 99%.

\* p <.1, \*\* p <.05, \*\*\* p <.01

## **B Additional tables**

Table 3.11 – Descriptive statistics and Balance checks (Spillover sample)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Nb obs	Mean in Control Group	Mean in Treatment Group (pooled)	pvalue of test (2)-(3)	Mean in VSLA (T1)	Mean in Cash Grant with Repayment (T2)	Mean in Cash Grant (T3)	pvalue of joint test T1=T2=T3
<b>Personal and Household characteristics</b>								
Type of locality (1 = village)	1,200	100.0%	100.0%	.	100.0%	100.0%	100.0%	.
Rural area	1,200	95.8%	93.9%	0.56	93.3%	96.7%	90.9%	0.48
Gender	1,200	40.6%	40.3%	0.91	40.8%	41.7%	37.8%	0.74
Age	1,200	31.23	30.67	0.28	30.98	30.63	30.33	0.70
Youth (<35 years-old)	1,200	71.7%	73.3%	0.59	71.5%	73.6%	75.1%	0.74
Native ethnic group	1,200	81.7%	82.4%	0.88	83.1%	84.8%	78.5%	0.72
Married	1,200	42.2%	42.7%	0.91	42.3%	43.8%	41.6%	0.92
Lives with a partner (but not married)	1,200	26.3%	25.9%	0.92	25.5%	27.9%	23.9%	0.79
Widowed	1,200	8.3%	6.1%	0.21	8.2%	4.3%	5.7%	0.30
Nb of children	1,200	2.95	2.80	0.31	2.85	2.85	2.68	0.64
Has been to school	1,200	62.9%	63.7%	0.88	59.6%	63.8%	68.9%	0.45
No Diploma	1,198	61.1%	55.7%	0.21	60.3%	54.5%	51.2%	0.38
Head of the Household	1,200	57.4%	50.9%	0.05	50.2%	47.5%	56.5%	0.24
Spouse of the Head of the Household	1,200	25.0%	26.1%	0.74	27.0%	29.3%	20.6%	0.17
Nb of household members (excl. respondent)	1,200	5.35	5.83	0.09	5.71	5.85	5.96	0.88
<b>Household and productive assets</b>								
Nb of livestock heads	1,200	2.26	2.49	0.58	2.67	2.33	2.49	0.82
Nb of poultry heads	1,200	5.11	5.09	0.99	4.79	5.29	5.22	0.80
Nb of agricultural tools owned	1,200	0.44	0.58	0.07	0.60	0.55	0.60	0.87
<b>Employment</b>								
Has an activity (last 7 days)	1,200	99.3%	99.6%	0.62	99.6%	99.3%	100.0%	0.22
Has worked in an agricultural activity (last 6 months)	1,200	93.1%	93.6%	0.79	93.6%	90.6%	97.6%	0.01
Has a non-ag business	1,200	81.7%	83.6%	0.51	82.8%	83.3%	85.2%	0.82
Main Activity is wage work (last 7 days)	1,193	4.0%	4.4%	0.78	3.0%	6.9%	2.9%	0.21
Main Activity is non-agricultural self-employment (last 7 days)	1,200	18.5%	19.1%	0.86	16.1%	21.0%	20.6%	0.56
Main Activity is agricultural self-employment (last 7 days)	1,193	70.6%	70.3%	0.96	74.3%	67.5%	68.9%	0.52
Total income from main activity over the last month	1,200	60,978	73,048	0.12	72,373	67,858	80,766	0.56
<b>Business practices</b>								
Does not do accounting	994	15.6%	12.7%	0.36	18.6%	10.4%	8.4%	0.04
Does accounting by memory	994	65.0%	68.2%	0.49	66.8%	70.4%	66.9%	0.80
<b>Savings and Credits</b>								
Has Saved over the last 3 months	1,200	63.4%	64.9%	0.72	64.8%	62.7%	67.9%	0.71
Amount saved over the last 3 months	1,198	27,752	65,450	0.29	33,860	119,012	35,075	0.67
Has a mobile money account	1,200	19.6%	26.2%	0.08	22.1%	30.4%	25.8%	0.36
Has a bank account	1,200	2.5%	2.5%	0.94	1.9%	2.9%	2.9%	0.65
Has participated in a Tontine (ROSCA)	1,200	46.0%	51.3%	0.22	50.6%	52.9%	50.2%	0.88
Has debt	1,200	21.9%	22.2%	0.92	20.6%	24.3%	21.5%	0.75
<b>Financial constraints</b>								
Report strong binding constraints for education expenditures	1,200	50.9%	53.7%	0.49	56.6%	52.5%	51.7%	0.68
Report strong binding constraints for health expenditures	1,200	62.7%	66.8%	0.36	67.4%	68.8%	63.2%	0.67

Table 3.12 – Heterogeneous impacts on main social outcomes : by gender

	(1) Participation to groups or associations (# groups)	(2) Solidarity received : # times received financial support (last 12 mths)	(3) Solidarity given : # times was financially supported (last 12 mths)	(4) Trust Index (z-score)
<b>Panel A. Pooled Estimates</b>				
Treatment (ITT)	0.26*** 0.08	0.20 0.17	0.48** 0.23	-0.02 0.10
Treatment x Gender	-0.10 0.10	0.00 0.19	-0.28 0.23	0.06 0.11
Gender (Female==1)	0.29*** 0.08	-0.20 0.14	-0.26 0.18	-0.23** 0.10
Department X (Urban/Rural)	Yes	Yes	Yes	Yes
Mean in Control (Female==0)	0.99	1.07	1.48	0.17
Total Treatment Effect Female==1	0.15	0.20	0.20	0.04
p-value Total Treatment Female==1	0.02	0.07	0.08	0.54
Observations	2,620	2,620	2,620	2,374
<b>Panel B. Treatment Arm Estimates</b>				
VSLA (T1) (ITT)	0.31*** (0.10)	0.23 (0.23)	0.33 (0.28)	-0.02 (0.13)
Cash Grant with repayment (T2) (ITT)	0.21** (0.10)	0.01 (0.18)	0.69** (0.28)	-0.03 (0.12)
Cash Grant (T3) (ITT)	0.23** (0.11)	0.56 (0.40)	0.35 (0.44)	0.00 (0.17)
VSLA (T1) x Gender	-0.17 (0.12)	-0.12 (0.24)	-0.10 (0.28)	0.09 (0.14)
Cash Grant with repayment (T2) x Gender	-0.07 (0.12)	0.14 (0.21)	-0.53* (0.29)	0.04 (0.12)
Cash Grant (T3) x Gender	-0.04 (0.15)	-0.04 (0.52)	-0.10 (0.42)	0.05 (0.16)
Gender (Female==1)	0.29*** (0.08)	-0.20 (0.14)	-0.25 (0.18)	-0.23** (0.10)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes
Mean in T1 Control (Female==0)	1.12	1.16	1.81	0.16
Mean in T2 Control (Female==0)	1.14	1.26	1.65	0.14
Mean in T3 Control (Female==0)	1.16	1.12	1.79	0.15
Total T1 Effect Female==1	0.14	0.10	0.23	0.07
p-value Total T1 Effect Female==1	0.06	0.45	0.09	0.42
Total T2 Effect Female==1	0.14	0.16	0.16	0.01
p-value Total T2 Effect Female==1	0.07	0.24	0.24	0.87
Total T3 Effect Female==1	0.19	0.52	0.26	0.05
p-value Total T3 Effect Female==1	0.06	0.08	0.13	0.63
p-value T1=T2	0.99	0.73	0.57	0.49
p-value T2=T3	0.64	0.25	0.55	0.70
p-value T1=T3	0.62	0.20	0.89	0.90
p-value T1=T2=T3	0.87	0.43	0.78	0.77
Observations	2,620	2,620	2,620	2,374

Robust standard errors clustered at locality level.

Earnings, sales, capital and value of assets are in CFA franc and winsorized at 99%.

\* p <.1, \*\* p <.05, \*\*\* p <.01

Table 3.13 – Heterogeneous impacts on main social outcomes : by level of ethnic diversity

	(1) Participation to groups or associations (# groups)	(2) Solidarity received : # times received financial support (last 12 mths)	(3) Solidarity given : # times was financially supported (last 12 mths)	(4) Trust Index (z-score)
<b>Panel A. Pooled Estimates</b>				
Treatment (ITT)	0.26*** 0.08	0.20 0.17	0.48** 0.23	-0.02 0.10
Treatment x Gender	-0.10 0.10	0.00 0.19	-0.28 0.23	0.06 0.11
Gender (Female==1)	0.29*** 0.08	-0.20 0.14	-0.26 0.18	-0.23** 0.10
Department X (Urban/Rural)	Yes	Yes	Yes	Yes
Mean in Control (Female==0)	0.99	1.07	1.48	0.17
Total Treatment Effect Female==1	0.15	0.20	0.20	0.04
p-value Total Treatment Female==1	0.02	0.07	0.08	0.54
Observations	2,620	2,620	2,620	2,374
<b>Panel B. Treatment Arm Estimates</b>				
Treatment (ITT)	0.13* 0.08	0.32** 0.13	0.26 0.17	0.02 0.08
Treatment x (Ethnic diversity==1)	0.09 0.11	-0.22 0.17	0.06 0.23	0.02 0.12
(Ethnic diversity==1)	-0.17** 0.09	0.10 0.13	0.06 0.19	0.04 0.10
Department X (Urban/Rural)	Yes	Yes	Yes	Yes
Mean in Control (Ethnic diversity==0)	1.30	0.84	1.29	0.00
Total Treatment Effect Ethnic diversity==1	0.21	0.10	0.32	0.04
p-value Total Treatment Ethnic diversity==1	0.00	0.48	0.04	0.63
Observations	2,620	2,620	2,620	2,374
<b>Panel B. Treatment Arm Estimates</b>				
VSLA (T1) (ITT)	0.15* (0.09)	0.24 (0.15)	0.11 (0.19)	-0.03 (0.11)
Cash Grant with repayment (T2) (ITT)	0.11 (0.09)	0.24 (0.16)	0.34* (0.20)	0.01 (0.09)
Cash Grant (T3) (ITT)	0.14 (0.10)	0.88* (0.46)	0.44 (0.36)	0.21 (0.16)
VSLA (T1) x (Ethnic diversity==1)	0.05 (0.14)	-0.18 (0.28)	0.39 (0.28)	0.20 (0.15)
Cash Grant with repayment (T2) x (Ethnic diversity==1)	0.09 (0.13)	-0.25 (0.20)	-0.08 (0.28)	-0.01 (0.14)
Cash Grant (T3) x (Ethnic diversity==1)	0.13 (0.15)	-0.59 (0.51)	-0.29 (0.42)	-0.30 (0.21)
(Ethnic diversity==1)	-0.18** (0.09)	0.09 (0.13)	0.07 (0.19)	0.04 (0.10)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes
Mean in T1 Control (Ethnic diversity==0)	1.36	1.08	1.45	0.04
Mean in T2 Control (Ethnic diversity==0)	1.36	1.10	1.36	0.02
Mean in T3 Control (Ethnic diversity==0)	1.37	1.00	1.40	0.00
Total T1 Effect Ethnic diversity==1	0.19	0.06	0.50	0.17
p-value Total T1 Effect Ethnic diversity==1	0.06	0.80	0.01	0.11
Total T2 Effect Ethnic diversity==1	0.20	-0.01	0.27	0.00
p-value Total T2 Effect Ethnic diversity==1	0.03	0.95	0.16	0.98
Total T3 Effect Ethnic diversity==1	0.27	0.29	0.15	-0.09
p-value Total T3 Effect Ethnic diversity==1	0.02	0.19	0.49	0.51
p-value T1=T2	0.96	0.77	0.29	0.13
p-value T2=T3	0.54	0.17	0.60	0.50
p-value T1=T3	0.53	0.42	0.15	0.07
p-value T1=T2=T3	0.78	0.39	0.33	0.13
Observations	2,620	2,620	2,620	2,374

Robust standard errors clustered at locality level.

Earnings, sales, capital and value of assets are in CFA franc and winsorized at 99%.

\* p < .1, \*\* p < .05, \*\*\* p < .01

Table 3.14 – Heterogeneous indirect impacts on social outcomes : by type of area (urban/rural)

	(1)	(2)	(3)	(4)
	Participation to groups or associations (# groups)	Solidarity received : # times received financial support (last 12 mths)	Solidarity given : # times was financially supported (last 12 mths)	Trust Index (z-score)
<b>Panel A. Pooled Estimates</b>				
Treatment (ITT)	0.13	0.25	0.59**	0.10
Treatment x (Village==1)	0.08	0.20	0.26	0.17
(Village==1)	0.05	-0.05	-0.36	-0.09
	0.11	0.23	0.29	0.18
	-0.03	0.41**	0.27	0.18
	0.09	0.20	0.27	0.15
Department X (Urban/Rural)	Yes	Yes	Yes	Yes
Mean in Control (Village==0)	1.20	0.62	1.33	-0.21
Total Treatment Effect Village==1	0.19	0.20	0.23	0.01
p-value Total Treatment Village==1	0.00	0.08	0.08	0.83
Observations	2,620	2,620	2,620	2,374
<b>Panel B. Treatment Arm Estimates</b>				
VSLA (T1) (ITT)	0.31***	0.41	0.58**	0.19
	(0.11)	(0.30)	(0.28)	(0.19)
Cash Grant with repayment (T2) (ITT)	-0.03	0.10	0.65*	0.13
	(0.10)	(0.19)	(0.35)	(0.19)
Cash Grant (T3) (ITT)	0.03	0.18	0.49	-0.12
	(0.12)	(0.24)	(0.40)	(0.23)
VSLA (T1) x (Village==1)	-0.16	-0.33	-0.39	-0.17
	(0.14)	(0.34)	(0.32)	(0.21)
Cash Grant with repayment (T2) x (Village==1)	0.23*	0.02	-0.41	-0.14
	(0.12)	(0.23)	(0.39)	(0.20)
Cash Grant (T3) x (Village==1)	0.21	0.44	-0.25	0.21
	(0.15)	(0.36)	(0.45)	(0.26)
(Village==1)	-0.04	0.42**	0.27	0.18
	(0.09)	(0.20)	(0.27)	(0.15)
Department X (Urban/Rural)	Yes	Yes	Yes	Yes
Mean in T1 Control (Village==0)	1.18	0.66	1.59	-0.17
Mean in T2 Control (Village==0)	1.31	0.82	1.58	-0.19
Mean in T3 Control (Village==0)	1.27	0.80	1.59	-0.17
Total T1 Effect Village==1	0.14	0.08	0.20	0.02
p-value Total T1 Effect Village==1	0.06	0.57	0.22	0.81
Total T2 Effect Village==1	0.20	0.12	0.24	-0.02
p-value Total T2 Effect Village==1	0.01	0.35	0.11	0.80
Total T3 Effect Village==1	0.24	0.62	0.24	0.09
p-value Total T3 Effect Village==1	0.01	0.03	0.27	0.48
p-value T1=T2	0.51	0.78	0.80	0.64
p-value T2=T3	0.64	0.09	0.99	0.39
p-value T1=T3	0.29	0.07	0.85	0.61
p-value T1=T2=T3	0.56	0.18	0.96	0.67
Observations	2,620	2,620	2,620	2,374

Village defined by administrative status used for lotteries. Robust standard errors clustered at locality level.

Earnings, sales, capital and value of assets are in CFA franc and winsorized at 99%.

\* p < .1, \*\* p < .05, \*\*\* p < .01

## Conclusion: Policy implications and future research

Support for more productive employment is needed. However, the evaluation of two interventions implemented in Côte d'Ivoire show that there is no silver bullet. While the expected impacts on earnings did not materialize after the end of the programs, the interventions affected people's lives. Promising changes in the number or size of independent activities, as well as savings behavior or savings level could both pave the way for further improvements in economic livelihoods, in theory. We do not capture them within the survey's time frame. Should we expect increased impacts in the coming years? If there is a minimal threshold of capital needed to start an activity, and if there is a savings technology that is available to all, then there is no reason to assume that non-beneficiaries outcomes will not catch up with beneficiaries by setting their own micro-firms. This is what [Blattman et al. \(2020\)](#) formalize and observe in Uganda, nine years after the end of the promising cash-transfer program they evaluated. <sup>40</sup>

The two programs evaluated are not cost-effective in the sense that the (monetary) benefits do not outweigh the costs. However, in both cases, we tried to examine what is beyond the monetary impacts and include other dimensions such as well-being, or social outcomes. The THIMO public works improved well-being including in the long run, while public works are sometimes criticized for their scarring effect or for the dis-utility in the tasks performed. We document positive social impacts on PRISE beneficiaries, whose solidarity level increase as well as a result of the intensity of interactions during the program. The design of the PRISE allowed us to measure positive economic externalities at the village level, which draws a different picture of the overall effectiveness of the program.

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<sup>40</sup>[Blattman et al. \(2020\)](#) show with a simple theoretical model that if there is access to a savings technology and a fixed cost to start a business, even with credit constraints people will reach at some point the minimal capital required to start their activity (by saving) and launch their business. Therefore, cash injections act as a kick-starter for beneficiaries, but can only lead to short term impacts since others will eventually catch up with their own savings.

In general, and true for these two types of programs, understanding non-monetary effects and externalities require further research. In the case of public works, there are two dimensions which we do not currently assess correctly.

First, and most important to me, we need further research to understand the psychological benefits of having a job (even if temporary) and a structured workday when participating in a public works. This is often a key dimension of employment programs, that distinguish them with pure social assistance. My field experience showed me compelling (although anecdotal) evidence of that : pride in going to work in the morning, pride in earning a monthly salary, joy at meeting new people through work and possibly becoming friends. Working hours are strict and discipline enforced. For young people, this is a work experience building personality too. Those intangible effects are at the core of what distinguish a public works program transferring a monthly income and a monthly cash transfer of the same amount. But how large are they ? And is it worth the implementation cost ? Cash transfers are usually cheaper to implement than employment programs, especially if you think of a public works. At least the first question could be answered by a field experiment with strict comparison of a public works versus cash transfer, with identical payment patterns.

Second, we need more evidence and rigorous measurement of the value of services provided by public works (e.g. regarding road maintenance, urban sanitation) or the economic benefits of the infrastructure created (e.g. building new road). Those positive externalities for the communities should be taken into account when measuring public works impact in a comprehensive way. So far we lack quantitative measures of that. The reason is that it is both challenging to design an experiment able to identify those effects (randomizing across territories or markets similar enough) and to measure those effects in a survey.

Third, and still with the objective of providing comprehensive assessments of programs' impact, we need more evidence on externalities. Even at a small scale, programs can have unexpected consequences on non beneficiaries, positive or negative. Learning about it is important to protect people (in the case of adverse consequences, for example [Filmer et al. \(2018\)](#) regarding impacts of a cash transfer on child nutrition through price increases), and before scaling-up an intervention which could have larger general equilibrium effects (e.g. [Crépon et al. \(2013\)](#) regarding job placement assistance). The positive externalities of a program should also be assessed so we have a true picture of the potential of a program, and not only on economic outcomes. We

have already some examples of interventions evaluating both direct and indirect impacts (e.g. [Angelucci and De Giorgi \(2009\)](#) to complement what we know on Progreso in Mexico or [Egger et al. \(2019\)](#) on GiveDirectly unconditional cash transfer in Kenya). Similar to the literature on technology adoption, one can think about how to correctly implement an "initial" intervention which effects trickle down to reach many more individuals. The Village and Savings Associations (VSLA), in that matter, should deserve more attention since it is a model known for its easy natural replication. From a methodological point of view, progress have been made in designing interventions such that we can learn on the magnitude of spillovers depending on the intensity of the intervention (share of population treated) (see [Baird et al. \(2018\)](#)).<sup>41</sup> This is not something we could implement for the PRISE evaluation. More experiments using such designs are required.

Regarding integrated approaches for economic inclusion, we still have not pinned down what was the right mix of components. There is ongoing research addressing the question of the effectiveness of each part of the package (e.g. [Banerjee et al. \(2018\)](#) to isolate the effect of pure asset transfer and access to savings). From a policy perspective, this is much needed to understand how we can decrease the cost and implementation complexity of those programs. The VSLA component is intriguing in our case. In the PRISE intervention, we compare different ways of transferring cash with VSLAs in which there is no cash injection, and the effects are similar to our surprise.

While many interventions include some training, we still do not understand well the features of "good" training : what is the optimal intensity? Duration? What mix of theory and practice? How much in class training versus individualized coaching? Evaluations help us learn about the impact of training programs, but we tend to forget the diversity of training interventions behind, whether it is the modality or content of the training. Those are the first questions faced by the implementer, and except on the additional coaching feature, we have learned little so far.

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Finally, on the social consequences of economic interventions, more research is needed but also more innovative programs. Most likely the solution will not lie in employment only. Some more

<sup>41</sup>Randomizing the intensity of the treatment in treatment clusters is the ideal to study spillovers at different levels of treatment saturation. This way, it is possible to learn about the full function generating spillover effects and not only on one point.

<sup>42</sup>An exception is the contribution by [Drexler et al. \(2014\)](#). By proposing two slightly different training approaches, one following the standard curriculum and a second with a simplified and more pragmatic approach, they determine that the latter is a more effective "methodological" approach to transfer knowledge.

behavioral interventions such as cognitive therapy for high-risk individuals in Liberia have shown promising results (Blattman et al. (2017)). Other programs trying to shape aspirations first, through inspiring videos (Bernard et al. (2014)) or psychological training (Personal Initiative Training, Campos et al. (2017)). Increasing collaborations with sociologists and psychologist would be fruitful in this area.

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**Titre :** Trois essais sur l'impact économique et social de programmes d'emploi en Afrique subsaharienne : enseignements tirés de la Côte d'Ivoire.

**Mots clés :** Emploi ; Marché du travail ; Entrepreneuriat ; Politiques publiques ; Etudes d'impact ; Afrique Sub-Saharienne.

**Résumé :** En Afrique subsaharienne, les programmes d'emploi font partie intégrante des stratégies de réduction de pauvreté. Dans les pays fragiles, ils sont d'autant plus stratégiques qu'ils pourraient contribuer à la stabilisation sociale. Outre la création d'emplois, l'un des principaux défis en matière d'emploi est l'amélioration de la productivité des personnes travaillant dans des activités indépendantes. C'est le cas pour 80% des travailleurs subsahariens. Toutefois, il existe peu d'études documentant leurs effets économiques et sociaux et étudiant les caractéristiques optimales de ces interventions. En s'appuyant sur des évaluations d'impact (essais contrôlés randomisés), cette thèse apporte de nouveaux éclairages sur l'efficacité de deux programmes d'emploi soutenant la productivité de l'emploi en Côte d'Ivoire. Le premier chapitre étudie l'impact d'un programme de travaux publics en zones urbaines. Nous constatons que si ce programme fonctionne à court terme en tant que filet social,

les impacts productifs à long terme sont plus ténus. Nous documentons comment des cibages alternatifs pourraient améliorer le coût-efficacité du programme. Dans les deuxième et troisième chapitres, nous étudions un programme de micro-entrepreneuriat offrant une formation et un accès au capital en zone rurale, dans des régions caractérisées par des tensions importantes entre groupes ethniques. Injecter des capitaux et améliorer l'accès à l'épargne et au crédit par le biais de groupes d'épargne renforcés ont des effets similaires. Cependant aucune des interventions n'augmente les revenus de manière significative. Nous observons des retombées économiques positives sur les activités des non bénéficiaires. Les relations sociales s'en trouvent légèrement améliorées localement, mais cela ne s'étend pas plus largement au reste de la communauté. Les deux évaluations soulignent que davantage de recherche est nécessaire pour mieux comprendre et concevoir les interventions en matière d'emploi.

**Title :** Three essays on the economic and social impact of employment programs in Sub-Saharan Africa : evidence from Côte d'Ivoire.

**Keywords :** Employment ; Labor markets ; Entrepreneurship ; Public Policy ; Impact Evaluation ; Sub Saharan Africa.

**Abstract :** Employment programs are increasingly being used as policy instruments for poverty reduction in Sub Saharan Africa. In fragile countries especially, this is a strategic instrument to restore social stability. In addition to job creation, a major employment challenge is to support the productivity of people working in independent activities. In Sub Saharan countries, 80% of the workforce is engaged in small independent activities. However, there is limited evidence on both the employment and social impact of employment programs, and their design features. Using rigorous impact evaluation methods and a randomized controlled trial design, this thesis provides empirical evidence on the effectiveness of two employment programs supporting a shift towards more productive activities in Côte d'Ivoire. The first chapter investigates the impact of a public works program in urban areas. We find that the public works functions as a safety net in the short term. However, the evidence regarding its longer-term

productive impacts is weak. We document how alternative targeting methods could improve the cost-effectiveness of the program. In the second and third chapter, we study a micro-entrepreneurship program providing training and access to capital targeted towards rural areas, in regions characterized by significant tensions between ethnic groups. We find that injecting capital or improving access to savings and credit through enhanced saving groups have similar positive effects on activities. However, in both interventions it did not significantly increase earnings. We find economic spillovers for both, positively affecting non beneficiaries' income-generating activities. Social relations are locally improved, but this does not extend more broadly to the rest of the community. The two evaluations emphasize that more research is needed to better understand and design employment interventions.