ESSAYS IN DEVELOPMENT ECONOMICS

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it). The copyright of this thesis rests with the author. Quotation from it is permitted, provided that full acknowledgment is made. This thesis may not be reproduced without my prior written consent. I warrant that this authorization does not, to the best of my belief, infringe the rights of any third party.

Statement of Conjoint Work

I confirm that Chapter 2 was jointly co-authored with Oriana Bandiera (Professor at the London School of Economics), Robin Burgess (Professor at the London School of Economics), Imran Rasul (Professor at the University College London), Munshi Sulaiman (Researcher at BRAC Africa), Ricardo Morel (Researcher at BRAC Africa). I contributed 50% of the work.

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Abstract

This thesis contains three chapters that fall under the broad banner of development economics, with a particular focus on the study of mechanisms and strategies that improve public goods delivery.

The first chapter studies the role of financial incentives as signals of job attributes when these are unknown to potential applicants. I create experimental variation in expected earnings and use it to estimate the effect of financial incentives on candidates' perception of a newly created health worker position in Uganda and, through this, on the size and composition of the applicant pool. I find that more lucrative positions are perceived as entailing a lower positive externality for the community, and discourage agents with strong prosocial preferences from applying. While higher financial incentives attract more applicants and increase the probability of filling a vacancy, they hamper retention and performance. This is because the signal they convey reduces the ability to recruit the most socially motivated agents, who are found to stay longer on the job and to perform better.

The second chapter analyzes the role of social connections on the targeting choices of delivery agents. During the expansion of an agriculture extension program in Uganda, we randomly selected one delivery agent out of two eligible candidates per community. We find that social connections matter: relative to farmers connected only to the non-selected candidate, those connected only to the selected delivery agent benefit more from the program. They are indeed more likely to receive advice, training and more likely to adopt improved seeds, a new beneficial technology. We show that these results are consistent with delivery agents (a) putting positive weight on the utility of farmers connected to them (altruism) and (b) putting a negative weight on the utility of farmers connected to the rival candidate (spite). This sheds light on the importance of both positive and negative social preferences in shaping program delivery.

The third chapter studies the effect of movement restrictions on education. The evidence is based on the construction of the West Bank Separation Barrier in 2003. The exposure of an individual to the Barrier is determined both by her locality of residence and by whether she was in school or about to start school when the Barrier was built. Using a difference-in-differences approach, I find that movement restrictions increase the probability of dropping out from elementary and preparatory school by 3.7 and 6 percentage points respectively, i.e. a 50% increase relative to localities with no movement restrictions, while the proportion of children who have never attended school increased by 3.6 percentage points. Among all households, the poorest ones are the most affected, indicating that movement restrictions not only deteriorate the average education level but also increase income inequality.

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1 Introduction

Understanding how individuals respond to incentives is a central question in economics. In standard economic theory, financial incentives affect agents' behavior by increasing the monetary payoff from accomplishing a task. Recent theory points out that, in the presence of incomplete information about a job, financial incentives can also affect agents' effort by conveying a signal about the characteristics of the job (Benabou and Tirole 2003; Sliwka 2007). More lucrative positions may, for instance, be perceived as more difficult and less enjoyable, or may signal distrust or exploitative intentions on the part of the principal. This is analogous to the well-known concept that increasing the price of a new product can change the way people perceive this new product—e.g., its quality—and this signal can affect consumers' decisions (Milgrom and Roberts 1986).

In this paper, I test whether offering stronger financial incentives for a prosocial job changes agents' perceptions of the job and thereby their behavior in the labor market. To this end, I create exogenous variation in expected total earnings for a brand-new position that involves a social task, which entails positive social externalities, and a business task, which entails purely private benefits. I find that higher expected total earnings—or, equivalently, stronger financial incentives—signal the business-oriented nature of the job and decrease the expected social output.[1] This crucially affects the size and composition of the applicant pool. On the one hand, financial incentives increase the aggregate number of applicants. On the other hand, they discourage applications from agents with strong prosocial preferences, who are found to stay longer on the job and to perform better.[2]

These results can be rationalized by the presence of incomplete information on the side of the agents in my setting. For well-known positions and recruiters, financial incentives do

[1]Financial incentives are defined here as any type of monetary reward that induces agents to work harder, i.e., higher wages, higher total earnings (see efficiency wage theory), higher performance reward.

[2]A large theoretical and empirical literature, both in economics and in psychology, suggests that prosocial motivation aligns interests of workers with those of mission-driven organizations.

not convey a signal, i.e., they increase the monetary payoffs without distorting expectations about the prosocial nature of the job. In this scenario, incentives attract workers who value remuneration without displacing workers with strong prosocial preferences, leading to an increase in the number of applicants. In contrast, if the position is unfamiliar, raising financial incentives discourages prosocially motivated agents if these agents perceive more lucrative positions as less "social."[3] In this scenario, the effect on the number of applicants becomes ambiguous.

The field experiment was implemented during the expansion of BRAC's health program in rural villages of Uganda. The program consists in recruiting Community Health Promoters (henceforth, CHPs) to provide their own community with basic health services. An important aspect of the job is that BRAC, as many other NGOs, does not pay a stipend; rather, it allows CHPs to sell household products (e.g., oil, salt, soap) at a margin. Unlike government health workers, this implies that BRAC's agents have two tasks: one that has a prosocial component, i.e., the home-based health services, and one that has purely private benefits, i.e., the sale of goods. This nonprofit entrepreneurial model of community delivery, which is increasingly popular and has been evaluated by Bjorkman-Nykvist et al. (2014), is financially sustainable and easy to implement.[4]

Because both BRAC and the CHP position are nonexistent in the villages prior to the experiment, the job's potential candidates are unable to know precisely ex ante the relative importance of the social versus the business task, the social output of the job, the difficulty of the two tasks, the intentions of the recruiter, etc. The setting is thus ideal for illustrating whether financial incentives affect the perception of these job characteristics.

To test this, I create exogenous variation in whether the position is advertised as being high, medium or low paying, while the actual incentives do not change. This design, which follows a similar methodology as the one used in Ashraf et al. (2014b), is made feasible by the significant variation in earnings among workers (they earn a profit margin for each product they sell but receive no base payment). While information on earnings needs to be conveyed to potential candidates before recruitment, BRAC does not know ex ante how much each candidate would earn if hired and has flexibility on how to advertise the position.

Variation in expected total earnings from the CHP position is created by randomly revealing a different point of the true income distribution of existing health promoters: either the

[3]This can happen in a number of different scenarios. For instance, financial incentives may signal that agents need to devote more time to profit-oriented activities. Moreover, a large literature shows that workers tend to supply labor to the social sector at lower-than-market wages in return for the opportunity to provide goods with positive social externalities (e.g., Weisbrod 1983; Preston 1989). As a consequence, weak financial incentives may be perceived as associated to social-sector positions. All this is more formally described in a simple theoretical framework that I develop in Appendix A to guide the empirical analysis.

[4]The products are bought by BRAC at the wholesale price, sold to the CHPs at a price that is higher than the wholesale price but lower than the market price, and subsequently sold to the community at the market price. Each sale yields a profit for the CHP and generates funds for BRAC that are used to cover the costs of training and monitoring the CHPs.

maximum, the average, or the minimum. Although the effect of the treatments on expected earnings is theoretically ambiguous, I find that the higher the point of the revealed earnings distribution, the higher the expected earnings. These treatments thus allow me to create exogenous variation in perceived incentives.

The research design is composed of two separate experiments—the *information* and the *recruitment* experiments—that both use the same exogenous variation as the one described in the previous paragraph. The *information* experiment aims to test the signal channel of financial incentives: 6,844 respondents, located in 231 rural communities in which BRAC was expanding, were randomly assigned to the high-, medium- or low-pay recruitment strategy, and then interviewed on their perception of monetary and non-monetary aspects of the position.

Because asking questions about perceptions makes salient specific job aspects and in and of itself influences expectations and behavior, the effect of financial incentives on the applicant pool is analyzed in a separate experiment—the *recruitment* experiment—in which candidate's decision on whether to apply for the job is recorded without any questions on perceptions having been asked prior to that decision. In 315 similar rural communities in which BRAC was expanding, the position was randomly advertised either as high, medium, or low paying (randomization at the community level) with the aim of recruiting a health promoter in each community. The impact of financial incentives on the pool of applicants is assessed by administering a survey to all potential candidates on their prosocial preferences and other characteristics.[5] The data are used to compare, across treatments, the types of potential candidates who apply and, among the applicants, those who are appointed by BRAC. To understand which worker trait predicts retention and performance, data on retention and on the number of services provided by each recruited worker (i.e., the number of home visits, the number of pre- and postnatal checks, the number of health products sold) are collected monthly over a period of two years. A household survey is additionally administered to measure the quality of these health services, i.e., the type of household targeted, the amount of information provided during the home visits.

The results indicate that increasing (expected) financial incentives signals the business-oriented nature of the job and decreases the expected social output. Agents in the high-pay treatment are 17 percent more likely than those in the low-pay one to perceive the job as a private-goal job, which health promoters do to earn money, rather than as a social-goal job, which they do to improve health in the community. They also perceive the job as one in which a larger proportion of time is spent on the business task rather than providing health services. Other expectations—e.g., the number of hours of work, the difficulty of the tasks, the candidates' own abilities—are unaffected.

[5] BRAC recruits health promoters within BRAC microfinance groups. Within each community, the pool of potential candidates thus consists of all microfinance members.

The negative signal conveyed on social output affects the applicant pool. Stronger financial incentives are indeed found to attract agents who are interested in the business component (either because they like selling products or care about earning money) while discouraging those who care mainly about the social component, i.e. agents who have volunteered in the health sector in the past or who declare that helping the community is the most important job characteristic. Importantly, this latter effect is explained both by a crowding-in of low social motivation and a crowding-out of high social motivation. The probability that a candidate who has never volunteered in the health sector applies is three times as high in the high-pay treatment as in the low-pay one, while the likelihood that a candidate who has volunteered in the health sector applies is 36 percent lower.[6] Finally, the total number of applicants per vacancy increases with financial incentives, indicating that the distribution of preferences in the candidate pool gives more weight to agents crowded in than those crowded out. On the extensive margin, the probability that at least one individual applies and that a position is filled is found to increase with financial incentives.

Among the applicants of each village, BRAC selects one health promoter following guidelines that prioritize candidates who are educated and who value the job's impact on the community. While the results confirm that prosocial motivation is a key determinant of appointment, the selected 301 health promoters differ significantly in their prosocial attitudes across treatments. When asked to donate a voluntary and private amount of money to a public health NGO, the agents recruited in the high-pay treatment donate 55 percent less than in the low-pay treatment. They are significantly more likely to donate no money at all (crowding-in of low motivation) while less likely to donate large amounts (crowding-out of high motivation). The workers are moreover 32 percent less likely to have volunteered in the health sector. In summary, the signal conveyed by incentives affects not only the composition of the applicant pool, but also the composition of the pool of recruited health promoters. This is because the crowding-out of prosocially motivated agents reduces the likelihood of them applying and, consequently, the chances of them being selected.[7]

In the course of two years, 27 percent of the health promoters dropped out. The probability that a worker quits is 14 and 8 percentage points higher in the high- and medium-pay treatments respectively, than in the low-pay one. Although the magnitude of these results is large, the direction is not surprising: for a given level of earnings, an agent is more likely to drop out if recruited with higher expected earnings. This may happen mechanically, i.e., the ex-ante participation constraint holds while the ex-post does not, or through a more complex

[6] Education level and income are balanced across treatments. One possible explanation is the presence of low heterogeneity in both of these dimensions in the population studied. With more heterogeneity in ability, increasing compensation may lead to a higher-ability pool of workers with better outside options (Guiteras and Jack 2012; Dal Bó et al. 2013; Ashraf et al. 2014b).

[7] Different types of workers are appointed in the three treatments because the applicant pool changes, and not because BRAC uses different recruitment criteria (more details on this in Section 5.2).

negative reciprocity or reference-dependence story. Importantly, the observed difference in retention is largely explained by changes in the composition of workers across treatments. Controlling for agents' prosociality, the treatment effects on retention shrink by 40 percent and lose significance. Prosocially motivated agents are indeed less likely to be affected by the negative surprise of earning less than expected: a one standard deviation increase in the amount donated is associated with a 6.3 percentage points decrease in dropping out, while having volunteered in the health sector reduces the dropout rate by 13 percentage points. These results thus suggest that prosocial agents are more likely to be retained than non-prosocial ones and, although this is true within each treatment, prosocial agents are more frequently recruited when incentives are low.

If workers who stay in their post longer are also those who perform worse while on the job, then the organization faces a clear trade-off on whom to recruit. I do not find such a trade-off in my setting: prosociality not only predicts retention, it also predicts aggregate performance in the first year of work as measured with the number of household visited, the number of pre- and postnatal checks provided, sales profits, and BRAC performance evaluations. Agents recruited in the low-pay treatment also more frequently target the most vulnerable households—i.e., households with a pregnant woman or with young children, who are more time-consuming to visit (these individuals need to be identified and the women need to receive ante- and postnatal checks)—and do not convey worse information about health. Taken together, these results indicate that the performance on the job of the prosocial workers is not low enough to create a trade-off. If anything, the data suggest that their performance on the job is higher both in terms of output "quantity" and "quality": when I exclude from the analysis workers who have dropped out at the time of the data collection, prosociality continues positively predicting most of the performance measures.[8]

This paper contributes to three strands of the literature. First, it contributes to the existing research on the role of financial incentives as signals, which has so far been mainly theoretical (Benabou and Tirole 2003; Sliwka 2007; Godes and Mayzlin 2012). There also exists a small laboratory literature that documents the role of incentives as a signal of the egoistic and exploitative intentions of the principal (Carpenter and Dolifka 2013), of distrust (Fehr and List 2004), of social norms (Danilov and Sliwka 2013) and of difficulty (Bremzen et al. 2011).

Second, this paper is closely related to the literature on price signaling. In the absence of complete information about a product, consumers may infer the product's quality from its price (Milgrom and Roberts 1986; Heffetz and Shayo 2009) and this may affect the number and the types of consumers. Consistent with a low price signaling a low quality, Ashraf et al.

[8]The analysis eliminates 34 workers out of 301. Although this raises the empirical concern that I eliminate from the analysis a selected sample of those initially appointed, the results provide suggestive evidence on performance on the job by separating the effect from retention.

(2013) show that reducing the price of a new (unknown) water-purification product in Zambia reduces the demand for the product.

Finally, my results contribute to the emerging empirical literature on selection in the social sector. Ashraf et al. (2014b) test whether promotion prospects and career advancement change the applicant pool for a government health job in Zambia and how this affects workers' performance. Dal Bó et al. (2013) study the selection effects of a higher fixed wage for a position in the Mexican federal government.[9] In line with my results, these two papers find that stronger incentives (in the form of career advancement or higher wages respectively) increase the number of applicants. In contrast with my results, they find, however, that incentives do not discourage prosocial agents from applying and select agents who perform better in Ashraf et al. (2014b). Financial incentives indeed should not play the role of a signal for the positions analyzed in these two studies, i.e., salaried government positions that consist of one type of task only and with a well-known recruiter. This is one possible explanation for the presence of crowding-out in my context, and the absence in theirs.

By collecting rich data on all eligible candidates for a job, perceptions at the recruitment stage, application decisions, and behaviors on the job, I complement this existing literature by providing the first real-world evidence of the *signaling* effect of incentives on agents' behavior. While the type and the intensity of the signal undoubtedly vary from one context to another (depending on the characteristics of a job, the recruitment strategy and the goal of the recruiter), this study indicates that we need to pay careful attention to the potential signals conveyed by financial incentives, as these may strongly affect the behavior of workers.

The rest of the paper proceeds as follows: Section 2 describes the context and research design; Section 3 provides validation checks, Sections 4 and 5 discuss the role of incentives as signals and their effect on selection. Sections 6 and 7 explore the effect of incentives on retention and performance. Section 8 concludes.

2 Context and Research Design

2.1 The Community Health Promoter Position

In Uganda, as in many developing countries, rural populations are often isolated and lack access to basic services and facilities, e.g., health facilities, schools, agriculture-extension services. In this context, an increasingly popular approach to reach these populations is the community-based approach: workers are recruited within their villages and are trained to

[9] In the private sector, Guiteras and Jack (2012) separately identify the effect of financial incentives on worker selection and worker effort in the context of casual labor markets in Malawi. In a laboratory setting, Banuri and Keefer (2013) and Dohmen and Falk (2011) document sorting and effort effects. Barfort et al. (2015) show that higher public-sector counterfactual wages lead to a more dishonest and less public-sector-motivated pool of candidates for public-service jobs in Denmark.

serve their own community. Examples of these community-based positions abound: community health workers, community agriculture-extension workers, community microfinance group leaders, etc.

An increasing number of these community-based programs follow an entrepreneurial model: in addition to delivering a development program, agents sell products at a margin to the community and earn money from these sales. This model is financially sustainable and easy to implement: the organization buys the products in large quantities at the wholesale price, and distributes them to the delivery agents who subsequently sell them to the community at the market price. While nonprofit organizations tend to choose products that are beneficial—e.g., community extension workers selling improved seeds or fertilizers, community health workers selling health products—the margin on these products is not always enough to generate a decent pay for the workers. Consequently, organizations often allow workers to sell more popular household products—such as commodities and phone airtime, etc.—to increase their pay. The job becomes one with two types of tasks: prosocial tasks, i.e., health services, and tasks with purely private benefits.

In this paper, I focus on the Community Health Promoter (CHP) program implemented by one of the largest existing NGOs, BRAC.[10] BRAC operates in a total of 12 different countries and in Bangladesh alone has more than 70 thousand health promoters. In Uganda, BRAC is composed of 128 branches spread throughout the country and of more than 2500 health promoters. The CHP program is built on top of BRAC's microfinance program: CHPs are recruited from microfinance groups following BRAC guidelines, which prioritize candidates who are educated and who value the job's impact on the community. Given that all BRAC microfinance clients are women, CHPs are all women as well. Once appointed, CHPs receive an initial two weeks of training and a monthly one-day refresher course covering healthcare-related issues.

CHPs have multiple tasks. First, they visit households in their own village, educate household members on health issues and provide ante- and postnatal checks. Second, they sell health products (e.g., pain relievers, soap, oral rehydration salts) and other more popular but less health-oriented products (salt, fortified oil).[11] All these products are bought by BRAC at the wholesale price, sold to the CHPs at a price that is higher than the wholesale price but significantly lower than the market price, and subsequently sold to the community more or less at the market price. Each sale yields a profit for the agent and generates funds

[10] As mentioned in BRAC's Uganda Annual Report 2012: "The main objectives of the health program are to lower morbidity and mortality among pregnant women and children under five from preventable diseases (...). Parallel to this, an economic goal was integrated into the program to make CHPs economically viable."

[11] In this experiment, CHPs do not sell antimalarials and bednets and are not allowed to give any injections. Their role is thus less complete than that of a formal nurse. The list of products they sell includes iodized salt, fortified cooking oil, soap, pain relievers, cold capsules, cough syrup, oral rehydration salts (ORS), deworming Albendazole, eye drops, creams, disposable gloves, cotton.

for BRAC that are used to cover the costs of training and monitoring the CHP.[12]

Monthly profits vary significantly from one CHP to another: the demand for each product and the range of products BRAC sells to CHPs differ indeed from one area to another. A survey carried out by BRAC throughout Uganda in 2012 (the "BRAC CHP survey") indicates that CHP monthly income ranges from as little as 7 to as much as 200 thousand UGX per month, with an average of 30 thousand. As the average monthly income from the main earning activity of the respondents in my sample equals 175 thousand UGX per month, the CHP monthly remuneration varies from being below the average pay for some CHPs to being above the average pay for others.

To limit shirking, CHPs are closely monitored by their supervisors, who visit them weekly. Moreover, BRAC sets minimum monthly targets in terms of volume of products to sell and numbers of households to visit.[13] When a CHP does not reach the targets, her supervisor visits and supervises her more frequently. If the performance does not improve and the CHP is not showing interest, BRAC considers the CHP to be "inactive" and to have "dropped out" and does not invite her to the refresher training anymore. As a result of the existing monitoring system, active CHPs are less likely to shirk on the "quantity" of work provided than in areas that are harder to monitor and have no targets, i.e., output "quality" (information content of the home visits, type of household visited, etc.). The 2012 " BRAC CHP survey" indicates that the average active CHP dedicates fifteen hours of work per week to the position.

In a recent cluster-randomized controlled trial taking place in 10 districts of Uganda, Bjorkman-Nykvist et al. (2014) evaluate the model of community health delivery implemented by the NGOs BRAC and Living Goods and find that the program reduces the under-five mortality rate by 27 percent. The authors observe a 54 percent increase in follow-up visits for under-five children falling sick with malaria, ARI, or diarrhea and a 72 percent increase in home visits in the first seven postnatal days. The main challenge of the program remains low retention, i.e., health promoters quickly drop out after they receive training, and this leads to a loss of critical time and skills (Bhattacharyya et al. 2001; Nkonki et al. 2011).

2.2 Experimental Variation

The empirical design is composed of two experiments. The first experiment, which I will refer to as the *information* experiment, tests the signal channel of financial incentives by collecting data on agents' perception after the job is advertised as being more or less lucrative. Because

[12] The average profit margin across all products equals 20 percent for the health promoters (ranges from 0 to 47 percent depending on the product) and 16 percent for BRAC (ranges from 1 to 82 percent). This is calculated using three prices: (a) the wholesale price, (b) the price at which CHPs buy the products from BRAC, and (c) the price at which BRAC recommends CHPs sell the products to the community.

[13] The number of households CHPs are required to visit and health products they need to sell varies from one branch to another, depending on the number and density of households in the villages. Details on these targets and on the BRAC CHP program can be found in the "BRAC Health System Operations Manual 2012" (available upon request).

asking questions about perceptions makes salient specific job aspects and in and of itself influences expectations and behavior, the effect of financial incentives on the applicant pool is analyzed in a separate experiment—the *recruitment* experiment—in which a candidate's decision on whether to apply for the job is recorded without any questions on perceptions having been asked prior to that decision. While these two experiments are implemented in a different pool of similar villages (more details on the design are listed below), the exogenous variation is identical.

This paper experimentally changes the way the job is advertised. While all respondents were shown a leaflet describing the position and containing the same exact information on the different tasks, on how CHPs acquire their knowledge, and on how they earn money, they were given different information on CHP monthly earnings. The leaflets, displayed in Figure I, revealed different points of the true earnings distribution of existing CHPs. The "high-pay treatment" revealed the maximum of the earnings distribution ("CHPs earn up to 200 thousand UGX per month"), the "medium-pay treatment" revealed the mean ("CHPs earn an average of 30 thousand UGX per month") and the "low-pay treatment" revealed the minimum ("CHPs earn at least 7 thousand UGX per month").[14]

As I show in Section 3, the higher the point revealed in the earnings distribution, the higher the expected compensation with no change in the expected income variability. While different mechanisms may explain the results—e.g., individuals may update their beliefs differently or the amount on the recruitment message may be a focal point (salience theory)—I am not able to precisely disentangle these mechanisms and use the treatments as a tool to vary earnings expectations and solve the potential endogeneity in expectations. Appendix B contains a more detailed discussion of the potential mechanisms.

The experimental design relies on two key features. First, the CHP position is characterized by a complex remuneration scheme that pays CHPs a profit margin for each product they sell, and the profit margin varies from one product to the next. Neither BRAC nor the potential candidates know how much each candidate would earn if recruited. Indeed, this depends on a number of different things, including the demand for the health products in the catchment area in which the individual works, which are hard to predict ex ante. Second, at the recruitment stage, BRAC needs to convey some information on earnings to potential candidates but, due to their limited attention, their limited education, and the complexity of the incentive structure, avoids informing them about the multiple features of the job that determine earnings, i.e., the full list of products CHPs sell, the respective profit margins, and

[14]The data come from the 2012 "BRAC CHP Survey," in which a random sample of BRAC CHPs were interviewed across Uganda and were asked how much profit they make from their CHP position in a typical month. The enumerators in both the information and recruitment experiments were instructed to explain each difficult word of the recruitment leaflets (i.e., the words "at least," "average," or "up to"). Although respondents could have asked questions about points of the earnings distribution that were not revealed to them, enumerators reported this did not happened.

the target BRAC imposes on these sales (which influences the time CHPs spend selling the products). Instead, BRAC gives the candidates an idea of how much they could earn. In this paper, I manipulate this by advertising the position as high paying, medium paying or low paying (by revealing different points of the distribution).[15]

While the CHPs' tasks are described in details to potential candidates at the recruitment stage, along with information on training and expected earnings, a number of other aspects of the job are either too hard or simply impossible to explain, and this leads to incomplete information on the agent side. How difficult each task is, the amount of time an agent needs to spend on each task to reach the target, the social impact the agent will have on the community are all aspects of the job that can vary from one worker to another and are not easy to convey. Moreover, while the recruitment message emphasizes the positive social impact of the program, candidates may not know whether this is the true goal of the CHP program as BRAC is new in the villages and the position inexistent. In other words, they may not know whether BRAC has selfish or altruistic intentions. The existence of incomplete information in this setting is ideal to study whether the perception of the job is affected by expected earnings.

2.3 Information Experiment

The information experiment was carried out in 231 rural villages across four areas of Western Uganda (Kabale, Muhanga, Rukungiri, and Buyanja) where BRAC's health program was nonexistent at the time of the experiment, but where BRAC was opening new branches. It aims at identifying (a) the effect of the treatments on earnings expectations, and (b) the "signal effect," i.e., how changing expected earnings impacts perceptions of other job characteristics, such as the relative importance of the sales versus the public health components.

Stratifying by village, 6,844 women, drawn to be representative at the village level, were randomly assigned to view either the high-, medium- or the low-pay recruitment leaflet and were then asked to answer one of two different sets of questions (randomization at the individual level). Expected earnings distributions for the CHP position were elicited for a random half of the respondents (Sample 1). The other half of the respondents (Sample 2) were given a survey about their perceptions of other monetary and non-monetary aspects of the CHP position, i.e., how CHPs allocate time across tasks, how many hours they work, and how difficult they perceive the position to be.

The causal effect of the treatments is measured by regressing agents' perceptions on treatment dummies under the assumption that treatment assignment is orthogonal to the error

[15] This is not the standard way BRAC advertises the job's earnings. Outside the experiment, BRAC does not give specific guidelines to its staff on whether the job should be advertised as high, medium or low paying, and anecdotical evidence suggests that this greatly varies from one staff member to the next. In this experiment, the new staff recruited by BRAC to conduct the recruitment of CHPs was trained to advertise the position as indicated on the recruitment leaflet without deviating from it.

term.[16] In support of this assumption, Table I presents balance checks on respondent characteristics. For each variable, I report basic summary statistics among all interviewed respondents (in Samples 1 and 2) and then perform two exercises to check balance across treatments. The first exercise tests the joint significance of the full set of treatment dummies in explaining each variable in Samples 1 and 2 separately. Using a multinomial logit model, the second exercise estimates a test of joint significance of all covariates in predicting treatment assignment.

The summary statistics indicate that the average respondent is 44 years old, has completed primary school (6 years of education), and works 35 hours per week. Seven percent of the respondents have volunteered in the health sector (as an unpaid volunteer for the Government or an NGO, or as a counselor), and 30 percent declare that "having a positive impact on the community" is more important as a job feature than earning money or gaining respect. Only 6 percent have borrowed money from a microfinance institution in the year before the survey. This is because BRAC was not present in the villages and access to microfinance loans was extremely limited.

Reassuringly, these characteristics do not differ significantly across treatments in both Samples 1 and 2, indicating that the randomization yields samples that are balanced. In a multinomial logit model, treatment assignment is regressed on all covariates. The null hypothesis that the covariates do not jointly predict treatment assignment is not rejected in either sample (p-values are 0.25 for Sample 1 and 0.41 for Sample 2).

2.4 Recruitment Experiment

The aim of the recruitment experiment is to estimate the treatment effects on the selection of workers and on their retention and performance once recruited. The experiment took place in 2012 when BRAC opened 15 new branches throughout Uganda in areas where it had previously not had a presence and which are different from those of the information experiment.[17] The recruitment process took place in 315 microfinance groups, located in 315 villages across the 15 branches, that were formed as soon as BRAC expanded and which resulted in 301 CHPs being successfully recruited.

Stratifying by branch, the recruitment message used to advertise the CHP position in the microfinance groups was randomly assigned to the high-, the medium- or the low-pay treatment, with randomization at the microfinance-group level. The high-pay treatment was used in 106 microfinance groups, the medium-pay in 104, and the low-pay in 105.More details

[16] To limit spillovers, the enumerators were instructed to minimize the time between interviews of households living in proximity to one another.

[17] Four of the new branches are in West Uganda (Kiganda, Kibito, Bundibugyo, Bwera), four in North Uganda (Adjumani, Moyo, Yumbe and Maracha), four in East Uganda (Musita, Buyala, Mayuge, Idudi), and three in Central Uganda (Nkonkonjeru, Kasawo, Busunju).

on the experimental design are presented in Section 5.[18]

Table II presents balance checks on characteristics of the villages in which the microfinance groups are located and characteristics of the potential applicants, i.e., the microfinance clients. The summary statistics indicate that the microfinance groups are located in villages that are situated 50 minutes away, on average, from the closest hospital (by foot), and 23 minutes away from the closest drugstore. Microfinance groups are composed of an average of 15 literate members present on recruitment day. These characteristics do not differ significantly across treatments, indicating that the randomization yields a sample that is balanced.

Comparing Tables I and II suggests that respondents interviewed in the recruitment experiments are comparable in terms of their education, age, marital status, number of rooms in the house (a proxy for house size), prosocial motivation (i.e., "volunteered in the health sector" and "community driven") to respondents in the information experiment.[19] However, while respondents in the recruitment experiment are necessarily microfinance clients, those sampled in the information experiment are not (BRAC microfinance groups had not been formed when the survey was conducted). Consequently, respondents in the recruitment experiment are more likely to be self-employed in a non-agriculture business rather than in the agriculture sector.

To ensure that responses from the respondents in the information experiment are informative of the expectations of potential candidates in the recruitment experiment, I estimate below the treatment effects in the information experiment using two samples: (a) the full sample of respondents, and (b) a sub sample of these respondents who are matched, based on a number of relevant observed characteristics, to the most similar women in the recruitment experiment. I also make sure that there is nothing specific about the way microfinance clients update their beliefs in the information experiment compared with other respondents.

3 Validation of the Experiment

This section provides evidence that the experimental variation used in this paper overcomes the problem of endogeneity in individuals' expectations. In particular, I show that revealing only a specific point of the earnings distribution can be used as a tool to exogenously vary the expected first moment of the distribution, without changing the expected second moment. This ensures that my treatments affect the types of agents who self-select into the job without changing their perception of the risk involved, and thus without changing the risk attitudes among applicants across treatments (this is tested in Section 5.1).

[18]To limit spillovers, the time elapsed between two recruitment events within a branch was minimized. The distance between villages makes it very unlikely that agents in one microfinance group heard about the recruitment message used in another group before the recruitment in their own group.

[19]The variables "owning a shop", "having ever sold health-related products", "weekly earnings" cannot be compared across tables as these variables were not collected in the information experiment.

After the position was advertised with one of the three treatments, each respondent was asked: "How much do you think CHPs earn in a typical month?" and "How many hours do you think CHPs work in a typical month?". These two questions allow me to estimate expected monthly earnings and expected earnings per hour for each respondent. Table III (Columns 1 and 2) show that both of these variables turn out to be higher in the the high- and medium-pay treatments than in the low-pay one. The expected earnings per hour of work, represented by $\hat{Y}$ in the theoretical framework, increase significantly by 30 and 8 percent in the high-pay and medium-pay treatments, respectively, relative to the low-pay one.

Because it is unclear whether "earnings in a typical month" refer to the mean or the median of the expected distribution, I collected additional data on the expected earnings distribution. Each respondent was asked to assign a probability to the possibility that CHPs earn roughly Y_n (expressed in thousands of UGX) where $Y_n = \{0, 30, 60, 90, 120, 150, 180, 210\}$. This was done by asking respondents to distribute 24 beans across eight cards representing the different levels of earnings Y_n.[20] Assuming a uniform distribution within each bin, the expected average and standard deviation in earnings for respondent i is calculated by weighting each Y_n with its perceived probability $p_i(Y_n)$:

$$E_i(Y) = \sum_n p_i(Y_n) \times Y_n; \quad SD_i(Y) = \sqrt{\sum_n p_i(Y_n) \times (Y_n - E_i(Y))^2}.$$

Table III displays the treatment effects on expected earnings, estimated with

$$E_i(Y) = \alpha + \beta_1 \, Medium\,Pay_i + \beta_2 \, High\,Pay_i + X_i\eta + u_i, \tag{1}$$

where $E_i(Y)$ is the earnings expectation of respondent i, $Medium\,Pay_i$ and $High\,Pay_i$ are dummies indicating which recruitment message the respondent was randomly assigned to observe before answering the questions (randomization is at the individual level), X_i are a set of individual-level controls believed to affect the outcome variables and villages fixed effects, and errors are clustered at the village level. In this section, as well as in the rest of the paper, the omitted treatment is the low-pay one and the list of individual controls includes age, marital status, education level, number of hours of work per week, occupation (agriculture vs. non-agriculture), and size of the house (as a measure of wealth). Note that the results are almost identical both in magnitude and in precision if I drop the controls or if I use robust standard errors rather than clustered ones.

Table III shows that respondents in the high- and medium-pay treatments expect CHPs to earn significantly more money than in the low-pay treatment: both the average and median expected earnings are higher (Columns 3 and 4). There is, however, no difference between

[20] The approach of using visual aids to represent probability units is common practice in the literature (Sequeira et al. 2013; Attanasio 2009; Delavande et al. 2011).

these two higher-pay groups and the low-pay one in terms of perceived income variability $SD_i(Y)$ (Column 5). Finally, the ratio between expected average earnings and expected standard deviation in earnings is higher than one in all the treatments (indicating that the treatments convey more signal than noise), but it is statistically larger in the high- and medium-pay treatments (Column 6).

Figure II estimates the treatment effects on the aggregate distribution of perceived earnings: the probability that respondent i allocates to income Y_n, $p_i(Y_n)$, is regressed on the treatment groups with the same specification as in (1). The right panel suggests that, compared with those who viewed the low-pay leaflet, respondents who were shown the high-pay leaflet shift upward the distribution of expected earnings. In particular, they assign higher probabilities to CHPs ending up at the higher-income levels and lower probabilities to the lower levels. Results for the medium-pay treatment (left panel) follow a similar pattern, although the effects are smaller in magnitude. Overall, this figure indicates that the experimental variation used in my paper shifts upward the expected earnings distribution, without significantly varying the expected variance in earnings.

While respondents seem to understand the meaning of the words "at least" and "up to" in the recruitment message (only 2.3 percent expect less than the minimum in the low-pay group or more than the maximum in the high-pay one), the average expected pay in the medium-pay treatment is higher than what is advertised in the recruitment leaflet. To make sure this is not driven by less-educated respondents not understanding the word "average," I interact treatment effects with the education level of the respondent and find that the interaction term is not significant (see Table A.I, Column 1). This is reassuring, as the contrary would have indicated that literate women understand the recruitment message better and are more likely to update their beliefs.

To ensure the effect of the treatments on expected earnings is not entirely driven by a specific pool of agents, more heterogeneous treatment effects are reported in Table A.I. The results indicate that revealing different points of the distribution affects expected earnings similarly for agents who have borrowed from a microfinance institution versus those who have not. This is reassuring, as the main difference between the recruitment and the information experiment lies in the fact that respondents in the former are all microfinance clients, while those in the latter are often not.

In Table A.II, I use a matching approach to have a more similar group of respondents in the information experiment to those in the recruitment experiment. Based on a list of observable variables (age, marital status, education level, occupation, number of hours of work, size of the house), I use a matching approach with replacement in which each individual interviewed in the recruitment experiment is paired with her five nearest neighbors in the information experiment. Results remain similar to the unmatched sample and are not sensitive to increasing the number of neighbors in the matching procedure.

4 Treatment Effects on Job Perceptions

In this section, I test whether increasing perceived incentives affects job perceptions. It is important to remind here that both BRAC and the CHP position were nonexistent in the villages prior to the experiment. Consequently, agents' perceptions about the job are strongly influenced by what is revealed to them at the recruitment stage. As illustrated in Figure I, all the agents are told what tasks they would do as a CHP, how long and how frequent the training is, how they could have a positive impact on the community and how they would earn money. Given the limited amount of time available to advertise the position, a number of other features of the job are not discussed because too complex, too subjective or simply impossible to convey—e.g., difficulty of each task, amount of time each agent would have to work to reach the targets imposed by BRAC, the proportion of time they would spend on each task, the whole list of products they would sell and their respective profit margins.

As agents do not have a precise idea of a number of aspects of the job, the effect of increasing incentives on perceptions is theoretically ambiguous. A higher pay could signal that CHPs spend more time selling products, thereby changing the perceived nature of the job, or can signal selfish intentions on the part of BRAC. A higher pay may also signal that CHPs work more hours overall, that they sell products with higher profit margin, that the demand for these products is higher, etc. To understand which type of signal was conveyed in my setting, I collected information on agents' perceptions after the job was advertised using one of the three treatments. Data include measures of perceptions about the nature of the job, time allocation across tasks, hours worked, and job difficulty. The perceived allocation of time across tasks is measured by instructing the respondents to distribute 12 beans across different cards representing the different tasks of a CHP (i.e., selling goods, educating the community on health issues, and providing pre- and postnatal exams). The questions on job difficulty ask the respondents to rank on a scale of 1 to 4 the expected difficulty of convincing households to buy goods (which is correlated with the profit margin) and the expected difficulty of improving their health-related behavior.

Following Benabou and Tirole 2003, I test whether financial incentives also affect the agents' perception of their own abilities rather than only their perception about the job. The unknown variable at the recruitment stage could indeed be a characteristic of the applicant herself rather than uncertainty about the task per se. As agents accept the job only if they have sufficient confidence in their own abilities, financial incentives may affect the participation constraint of potential candidates by influencing the perception of their own ability. Perceived ability is calculated by asking agents to rank themselves on a scale of 1 to 10, where 1 means, "If 10 women were recruited, I would be ranked last in terms of performance," and 10 means, "I would be ranked first."

Table IV reports the exhaustive list of survey questions on perceived attributes and sheds light on what aspect of the CHP position is perceived differently when expected financial incentives increase. I find that incentives convey a signal about the nature of the job. Relative to the low-pay treatment, respondents in the high-pay treatment are 17 percent more likely to perceive the position as a "private goal" position that CHPs accept to earn money rather than as a "social goal" position that CHPs accept to improve the health conditions in their villages. Moreover, the perceived proportion of time CHPs spend selling goods versus delivering health services increases by 10 percent in the high-pay framing. Both these results suggest that stronger financial incentives signal the business-oriented nature of the job and decrease the expected social output.

One thing to note is that these results hold when we compare the low- and medium-pay treatments with the high-pay one, but not when the low-pay treatment is compared with the medium-pay one. This indicates that the difference in expected earnings in the low- and medium-pay treatments, which is 6 times smaller than the difference between the high- and low-pay groups, is too small to convey a signal on social output.

While incentives can theoretically also potentially change agent perceptions of a number of other aspects of the job, I do not observe such signals. Incentives are indeed found to have no impact on the agents' expected number of work hours, expected difficulty in convincing households to buy commodities, and expected difficulty in improving their health behavior. Moreover, financial incentives neither boost nor attenuate self-confidence about succeeding in the job: a measure of own perceived ability does not vary across treatments.

Finally, Table A.I shows that the CHP job is perceived equally across different types of workers, and hence that incentives play the same signaling role irrespectively of the worker's type. Importantly, this is true for prosocial versus non-prosocial agents, where prosociality is measured with (1) whether a respondent has ever volunteered in the health sector, and (2) a survey question in which respondents are asked for the most important feature of a job: to have a positive impact on the community, to earn money, or to earn respect.[21] In regressions that have perceptions as outcome variables, the interactions of the treatment dummies with "having volunteered in the health sector" or "being community driven" are indeed small and not significant (Columns 6 and 7). This ensures that the treatment effects on the applicant pool's composition identified in the next section, are explained by different types of workers being less or more likely to apply for the job, rather than different types of workers perceiving the job differently.

[21] Both of these measures of prosociality have their own limitations, i.e., "ever volunteered in the health sector" could be a proxy of prosociality as well as a proxy of experience, "community driven" is a measure of relative weight rather than absolute weight given to "helping the community." For robustness, I will show that all the results of this paper are robust to using both measures. Starting in Section 5.2, I will also complement these survey measures with an incentivized measure of prosociality (i.e., donations to a public-health NGO).

5 Treatment Effects on Selection

This section estimates whether the negative signal on social output affects the type and the number of agents who apply for the CHP position and those who end up being appointed. The theory predicts that, under the observed signal channel, stronger incentives should (a) attract agents who are interested in the "remunerated" component of the job, while displacing agents with strong prosocial preferences, and (b) increase the number of applicants only if the number of agents displaced is smaller than the number of those attracted. I test these predictions next.

The recruitment process took place from August to October 2012 in 315 BRAC microfinance groups, located in 315 villages across 15 new BRAC branches. As soon as a microfinance group was formed, BRAC organized an unexpected visit in the group's weekly meeting with the aim of recruiting a microfinance member as CHP.[22]

In each group, the whole recruitment process lasted half-day and involved four subsequent steps. First, all microfinance members were asked to complete a brief questionnaire, very similar to the one used in the information experiment, about their prosocial preferences, education, main occupation, etc. To avoid biased answers, the survey was administered before the CHP position was advertised so that none of the microfinance clients knew that BRAC would then seek to fill the position.[23] Second, all microfinance clients were asked to view one of the three recruitment leaflets (randomization at the microfinance-group level). Third, each microfinance client was asked, privately and confidentially, whether she wanted to apply for the position. Because this step took place immediately after the position was advertised, there is no monetary cost of applying for the job in my context.[24] Fourth, in each microfinance group, a BRAC officer was in charge of selecting a CHP among the applicants, after a quick individual interview with each of them.[25] While there are no other requirements to become eligible as a CHP other than being literate and being a microfinance client, BRAC's guidelines prioritize candidates who are educated and who value the job's impact on the community.

[22]In 15 of the 315 groups, BRAC officers were instructed to recruit two CHPs, because the formation of the microfinance groups was proceeding too slowly in the branches. The two recruited CHPs were then assigned to work in the village in which the microfinance group is located and in another village nearby. The number of CHPs BRAC aimed to recruit in each microfinance group is balanced across treatments (available upon request).

[23]The survey was kept very short to make sure that all of the microfinance members were still present at the meeting by the time the CHP position was advertised.

[24]To make sure all applicants were seriously interested in the CHP position, all microfinance clients were informed that, conditional on applying and being selected, they would be designed as the CHP in front of the others, without being previously informed privately. They were also told that their decision of whether to apply or not would have no impact on their eligibility to receive future BRAC loans.

[25]Although BRAC officers could have potentially accessed the survey data, anecdotal evidence suggests that most of them did not because "this would have taken too much time," but they certainly could have asked similar questions verbally to each candidate. As the groups had just been formed when the recruitment took place, BRAC officers did not have much more information about the candidates than what I collected from the survey, e.g., they did not know them personally and had no information on their credit history.

5.1 Selection in the Applicant Pool

Composition of the Applicant Pool

Rich data on the characteristics of all potential CHP candidates (microfinance members) allow me to more precisely estimate selection effects using within-group differences between applicants and non-applicants and comparing these differences across treatments:

$$Apply_{ig} = \alpha + \beta_0 \, Y_i + \beta_1 \, Y_i * Medium\,Pay_g + \beta_2 \, Y_i * High\,Pay_g + X_i \eta + \sigma_g + u_{ig}, \qquad (2)$$

where $Apply_{ig}$ equals one if the microfinance client i in microfinance group g applies for the CHP position, and Y_i is a characteristic of microfinance client i, e.g., prosocial motivation or interest in sales. Errors are clustered at the level of the randomization unit, microfinance-group g. The regression includes microfinance group fixed effects σ_g and a list of individual-level controls X_i (age, marital status, education level, number of hours of work per week, occupation, and a measure of wealth).[26] The coefficients of interest, β_1 and β_2, estimate whether the predictive power of characteristic Y_i in determining whether an agent applies differs across treatments.

The first two columns of Table V test whether stronger financial incentives affect the likelihood that prosocial candidates apply. The treatment dummies are interacted with two measures of prosociality: (1) whether a candidate has ever volunteered in the health sector, and (2) whether "having a positive impact on the community" is reported as being the most important feature of a job. Importantly, both these variables were collected before the CHP position was advertised. So, although the respondents might over-report their level of prosociality, this over-reporting is, by construction, uncorrelated with the recruitment strategy. Finally, all regressions control for the respondent's socioeconomic and occupation status to take into account the fact that, for a given level of prosociality, women who are wealthier and more educated, or who work fewer hours, may be more/less likely to say "that they care less about earning money" or may have more/less time available to be a health volunteer.[27]

Column 1 of Table V shows that, in the low-pay treatment, agents who have volunteered in the health sector are 28 percent more likely to apply than those who have not, while in the high-pay treatment this probability drops to 12 percent. Moreover, in comparison with other agents, respondents who are "community driven" (who report that helping the community is

[26] When fixed effects are combined with clustered standard errors, the standard errors obtained from an OLS regression may be biased and may require a degrees-of-freedom correction (see Cameron and Miller 2015). Because the number of clusters is large with respect to the number of observations within each cluster, the cluster-robust standard error estimator converges here to the true standard error.

[27] In the main specification, I do not control for agents' reservation earnings, i.e., profits per hour from their current job, due to missing values in this variable. Results remain consistent to adding this variable, although the number of observations drops. Moreover, note that profits per hour are strongly correlated with being involved in a non-agriculture activity, a measure that I control for in all specifications.

the most important job characteristic) are significantly more likely to apply in the low-pay treatment, while they are not more likely to apply in the other two treatments.

Results go in the opposite direction for respondents who are "money driven" (who declare that earning money is the most important job characteristic), while agents who care about "earning respect" more than anything else are equally likely to apply across treatments (Columns 1 and 2 of Table A.III). The latter result provides evidence that, in my context, higher financial incentives do not crowd out agents for reputational concerns (Bénabou and Tirole 2006). Instead, the findings are consistent with financial incentives crowding-out the most motivated agents because they signal the business-oriented nature of the job (Benabou and Tirole 2003).

Columns 5 and 6 of Table V show that women who enjoy selling products, i.e., who own a shop or who have sold health-related products in the past, are significantly more likely to apply when earnings expectations are higher. This is consistent with the earlier evidence that stronger financial incentives signal the sales-oriented nature of the job (i.e., less time is spent on the sales task).

Applicants and non-applicants do not differ across treatments along other dimensions, such as education level, occupation, reservation earnings (proxied with hourly income at their current non-CHP jobs), wealth (proxied with the house size), number of hours of work or number of small kids to take care of (see Table A.III). One key feature of my setting that may explain the small selection on education and reservation earnings is that CHPs are recruited from the pool of literate microfinance members, which is probably more homogeneous in terms of these variables than the whole population. In other contexts with more heterogeneity in these areas, increasing compensation may lead to a more-productive/higher-skilled pool of workers with better outside options (Guiteras and Jack 2012; Dal Bó et al. 2013; Ashraf et al. 2014b). Finally, the absence of selection on the degree of risk-aversion (Column 9 of Table A.III) supports the findings of the information experiment: revealing different points of the earnings distribution impacts the expected average earnings without changing the expected variance of those earnings. If the contrary were true, we would expect risk-averse agents to be significantly more likely to apply in the treatment with the lower perceived variance.[28]

Table VI compares the likelihood that prosocially and non prosocially motivated agents apply for the position across treatments, holding other agents' traits fixed:

$$\begin{aligned} Apply_{ig} &= \alpha + \beta_0 Prosocial_i + \beta_1 Prosocial_i * Medium\,Pay_g + \beta_2 NotProsocial_i * Medium\,Pay_g \\ &\quad + \beta_3 Prosocial_i * High\,Pay_g + \beta_4 NotProsocial_i * High\,Pay_g + X_i\eta + u_{ig}. \end{aligned} \tag{3}$$

[28] Table A.IV regresses the probability that a potential candidate applies for the job on a set of different characteristics. Although these traits are correlated with each other, the table shows that the main determinants of the likelihood of applying are prosociality, interest in sales, education, and wealth.

In line with the predictions of the simple theoretical framework I develop in Appendix A), increasing monetary compensation for a job not only crowds in low prosocial motivation, but also crowds out high prosocial motivation whenever a negative signal on social output is conveyed. In the high-pay treatment, in which the job is perceived as more lucrative but less "social" (see Tables III and IV), non-prosocial agents are crowded in ($\hat{\beta}_4 > 0$), while prosocial agents are crowded out ($\hat{\beta}_3 < 0$). Candidates who have never volunteered in the health sector are indeed 3 times more likely to apply in the high-pay group than in the low-pay one, while those who have volunteered in the health sector are 36 percent less likely to apply. In the medium-pay treatment, in which the job is perceived as more lucrative but the signal on social output is milder, the results show there is a crowding-in of low prosociality ($\hat{\beta}_2 > 0$) but no significant crowding-out of high prosociality ($\hat{\beta}_1 \sim 0$). Finally, the same results are presented for "interest in sales" on the right-hand side of Table VI. In the low-pay treatment, agents who "have ever sold health-related products" are 8 percentage points more likely to apply, while they are 22 percentage points more likely to apply in the high-pay treatment.

Assessing how costly the displacement of prosocial preferences is for an organization requires knowing the correlation between prosocial preferences and other characteristics in the pool of potential candidates. Data on all potential candidates are usually hard to obtain. Most of the empirical literature that exists on the selection effects of incentives has information on the applicants for a job but does not observe potential candidates who do not apply (e.g., Dal Bó et al. 2013; Ashraf et al. 2014b). In this paper, I take advantage of a key feature of the CHP recruitment procedure—it is conducted within microfinance groups—to collect data on all potential candidates for the job.

In Table A.V (Part A), I explore how individual traits correlate with each other in the pool of potential candidates. As expected, both measures of prosociality (having prior volunteering experience in the health sector and being community driven) are positively correlated with each other. Importantly, prosocial preferences measured with having prior volunteering experience in the health sector and with being "community driven" are positively correlated with the ownership of a shop. Prosocial preferences are also positively correlated with education and wealth, measured with the size of the house. This suggests that, for a given level of prosociality, wealthier and more educated women are more likely to say "that they care less about earning money" or are more likely to have time to be a health volunteer. This positive correlation stresses the importance of controlling for socioeconomic variables whenever prosocial preferences are considered.

Size of the Applicant Pool

Advertising the position as better paying increases the size of the applicant pool only if the number of agents who are crowded in is larger than the number of agents with prosocial preferences who are crowded out. The treatment effects on the size of the applicant pool are estimated with

$$\#\,Applicants_g = \alpha + \beta_1 \, Medium\,Pay_g + \beta_2 \, High\,Pay_g + \gamma \, \#\,Potential\,Candidates_g + u_g, \quad (4)$$

where the outcome variable is the number of candidates applying for the position in microfinance group g, and $Medium\,Pay_g$ and $High\,Pay_g$ are treatment dummies indicating which recruitment message was used in microfinance group g. In all the specifications, the regression additionally controls for the number of potential candidates (microfinance clients) and for branch fixed effects.

Table VII indicates that the average number of applicants per vacancy in the low-pay treatment is 2.2 (15.2 percent of the microfinance clients). This number significantly increases to 3 and 2.6 in the high- and medium-pay groups respectively. While there is no monetary cost in applying for the position in my context, there could be a psychological cost, i.e., agents may prefer not to apply or may feel less obligated to do so if there is a better candidate in the group. Controlling for the number of potentially "outstanding" candidates in the group, i.e., the number of prosocial candidates and those interested in sales, only marginally reduces the coefficients. Finally, the results are robust to adding village-level controls, such as the distance to the closest hospital.

While Table VII indicates a significant mean effect, Figure III shows that financial incentives also impact the upper and lower tails of the distribution. The density of the proportion of microfinance members interested in becoming CHPs in the high- and medium-pay treatments shifts to the right of the density in the low-pay treatment. Taken together, these results provide evidence that the number of agents who become interested in the position when financial incentives increase is larger than the number of agents who become disinterested.

On the extensive margin, Table VII shows that the likelihood of finding at least one person interested in the CHP position is 5 percentage points higher in the high-pay than in the low-pay framing (significant at 10 percent level). Although BRAC officers visited a similar number of microfinance groups across the three treatments, they recruited a total 301 CHPs: 95 CHPs in the low-pay treatment, 102 CHPs in the medium-pay treatment and 105 in the high-pay treatment.[29] Financial incentives thus help the recruiter fill vacancies. In poor rural areas that have an urgent need for a CHP, the recruiter faces a social cost of advertising the job as a low-paying position if the vacancy is unfilled.[30]

As my treatments are allocated randomly across microfinance groups, the experimental

[29] Of the 303 microfinance groups with at least one applicant, BRAC recruited a CHP in 285. It did not recruit any of the applicants in the remaining 18 groups and did not offer the job to anyone in the village.

[30] Table A.VI shows that the high-pay treatment decreases (increases) the number of applicants who are prosocial (non-prosocial) and the probability that at least one prosocial (non-prosocial) person applies. The probability that at least one "community driven" person applies in the high-pay treatment is for instance 14 percent lower than in the low-pay one.

design allows me to estimate the elasticity of labor supply, holding quality constant. To do so, I divide the change in the size of the applicant pool (Table VII, Column 1) by the change in the hourly expected earnings obtained from the information experiment (Table III, Column 2). This yields three different labor-supply arc-elasticities that depend on the pair of treatments considered, i.e., an elasticity of 2.41 from low- to medium-pay, 0.93 from medium- to high-pay and 1.16 from low- to high-pay treatment. Although these results are derived from two separate experiments, they indicate that the relatively small jump in earnings from the low- to the medium-pay treatment raises, for many candidates, the expected utility from taking the job above their outside option. For a larger jump in earnings from low- to high-pay, the signal channel of incentives kicks in and reduces the expected utility from taking the job for prosocial agents and, consequently, reduces labor-supply elasticity.[31] Note that, because the CHP position is not a full-time position and working hours are flexible, the outside option of each candidate is hard to estimate in my context. It is indeed unclear whether the time spent working as a CHP is substituted away from working in another job or from spending time taking care of the children, volunteering, etc.

5.2 Selection in the Pool of Appointed Workers

Differences across Appointed Workers

Two months after being selected, the 301 appointed CHPs attended a two-week training session that took place at the BRAC branch office. At the beginning of the training session, and before starting work, each CHP was asked how much she was expecting to earn as a CHP in a typical month. Figure IV plots the kernel density of the expected-earnings distributions of the 301 CHPs. Consistent with the results of the information experiment, earnings expectations of the CHPs recruited in the high-pay and medium-pay treatments are significantly higher than in the low-pay treatment: both these treatments shift the density to the right of the low-pay density. A Komogorov-Smirnov test rejects the equality of each pairwise distributions at the one-percent level (the highest pairwise p-value is 0.003). The regression counterparts of Figure IV, presented in Table A.VII, indicate that expected earnings, expressed in thousands of UGX, are 28 and 73 percentage points higher in the medium- and high-pay treatments, respectively, than in the low-pay one.[32]

During the training session, CHPs received a payment of 3.5 thousand UGX for attending and were asked to play a contextualized dictator game to measure their prosocial attitudes toward health-related issues. More specifically, each CHP was privately asked for voluntary

[31] The calculated elasticity of 1.16 is smaller than the elasticity of 2.15 estimated in Dal Bó et al. (2013) and is consistent with the absence of crowding-out of prosocial motivation in their setting.

[32] The treatment effects are stronger in changing expected earnings in the recruitment experiment than in the information one. Prosocial agents are not more likely than non prosocial ones to increase their expectations in the higher-pay treatments (Columns 2 to 4 in Table A.VII).

donations to an existing public-health NGO (TAMTAM) that distributes bednets for free in poor communities.[33] The amount donated is taken as a proxy for the agents' motivation for the cause.

Table A.V displays the correlations between the amount donated by a CHP and other traits. Given that donations and volunteering in the health sector both represent one's motivation on health-related issues, these variables are significantly correlated. Being community driven, a measure of relative importance assigns for helping the community, is positively correlated with donation, although less significantly. Finally, donations are positively and significantly correlated with wealth (proxied with the size of the house), while they are not correlated with weekly earnings from non-CHP positions.

Characteristics of the 301 selected CHPs are compared across treatments in Table VIII:

$$y_g = \alpha + \beta_1\, Medium\, Pay_g + \beta_2\, High\, Pay_g + u_g, \qquad (5)$$

where y_g is a trait of the CHP recruited in microfinance group g and branch fixed effects are included in all regressions. Because adding individual-level controls (age, marital status, education, etc.) in the regressions reduces the number of observations below 301 due to missing values in these controls, Table VIII shows the main results without any controls. The first three columns of Table A.VIII report the same specification adding the controls.

The results suggest that CHPs recruited in the medium- and high-pay treatments are significantly more likely not to donate any money at all (crowding-in of low motivation) and significantly less likely to be among to the top 15 percent of donors (crowding-out of high motivation). As a consequence, the overall donation amount is more than 50 percent lower in the high- than in the low-pay treatment.[34] The graphic representation of these results is presented in the left panel of Figure V, which plots donations of recruited agents by treatment group. Table VIII moreover indicates that the CHPs appointed in the high-pay treatment are significantly less likely to have volunteered in the health sector and are less likely to be community driven, though they do not differ significantly in their interest for sales. Finally, Table A.VIII indicates that CHPs do not differ across treatments in the level of education, income, wealth, reference dependence, and occupation.

[33] CHPs were informed that their donation decision would have no consequences on their job or on their participation in BRAC microfinance group. This modified dictator game has been shown to predict performance on prosocial tasks (Lagarde and Blaauw 2013; Ashraf et al. 2014a).

[34] One possible criticism of donations as a measure of intrinsic motivation is that agents in the high pay treatment may have felt "richer" at the training session and may have donated more money. This would lead to an underestimate of the true effect on selection.

BRAC's Appointment Decision

If individual traits such as prosociality and interest in sales are at least partially observable to the recruiter, then the way the recruiter selects CHPs among potential candidates may attenuate or exacerbate the effect of expected earnings on selection in the applicant pool. In particular, candidate selection attenuates the effect of earnings expectations in the applicant pool if (a) each applicant pool includes at least one "outstanding" candidate, with both interest in sales and prosocial motivation, and (b) the definition of the most "outstanding" candidate does not change across treatments, i.e., BRAC officers, who were responsible for the selection of the CHPs and were exposed to the experimental variation of this study, gave the same weights to different individual traits across treatments. Candidate selection instead exacerbates differences in the applicant pool if some individual traits, such as prosociality, are perceived by BRAC to be more appropriate for low earnings expectations than for high earnings expectations. If so, my design would identify the effect of earnings expectations on retention through candidate self-selection and BRAC candidate selection.

Table A.IX compares within-group differences between appointed and non-appointed candidates, across treatments. Two points are of note. First, prosociality and interest in sales are determinants of appointment (see also Column 2 of Table A.IV) and are thus at least partially observable by the recruiter. Second, the way CHPs are selected attenuates differences in sales interest. More importantly, BRAC's candidate selection slightly exacerbates differences in prosociality from an already heterogeneous pool of applicants. This happens to be true mainly in the medium-pay treatment, resulting in recruited CHPs being significantly less prosocial in the medium-pay than in the low-pay, while differences were not significant in the pool of applicants. Overall, because the interaction terms in Table VIII are not significant, I conclude that differences in the "types" of selected CHPs are mainly driven by divergences in the applicant pool rather than differences in candidate selection across treatments.

In summary, the signal conveyed by incentives affects not only the composition of the applicant pool, but also the composition of the pool of recruited health promoters. This is because the crowding-out of prosocially motivated agents reduces the likelihood of them applying and, through this, the chances of them being selected.

6 Treatment Effects on Retention

This section studies whether the selection effect of financial incentives—i.e., discouraging socially motivated agents—affects worker retention. In the health sector, which suffers from a shortage of health workers, attrition leads to a loss of critical workforce (Bhattacharyya et al. 2001; Leaver and Albano 2004; Nkonki et al. 2011). This is particularly true for community-based programs that recruit low-skilled workers to provide health services to their own community: these workers receive intensive and expensive training paid for by the organization, and their learning curve on the job is very steep. Moreover, because their career opportunities in the health sector are often limited, the welfare cost of attrition is high.

Figure VI plots the number of active CHPs over time for the three treatments separately. While the total quantity of recruited workers is larger in the high- and medium-pay treatments than in the low-pay one, two years after CHP recruitment, the number of retained workers is smaller both in the high- and medium-pay treatments. The regression counterpart of this is presented in Figure VII:

$$Drop\,Out_{gt} \quad = \quad \alpha_t + \beta_{1t}\,Medium\,Pay_g + \beta_{2t}\,High\,Pay_g + u_{gt}, \tag{6}$$

where $Drop\,Out_{gt}$ equals one if the CHP recruited in microfinance group g has dropped out (is not active) in month $t = \{0, 1, ..., 24\}$. The left and the right panels, which plot, respectively, $\hat{\beta}_{1t}$ and $\hat{\beta}_{2t}$, show that the probability of a CHP dropping out is higher in the medium-pay and high-pay treatments than in the low-pay one throughout the two years.

Although the observed differences are large, the direction of the results is not surprising: all else being equal, an agent is more likely to drop out if recruited with higher expected earnings. This may happen mechanically (i.e., the ex-ante participation constraint holds while the ex-post does not) or through a more complex negative reciprocity or reference-dependence story. Agents may indeed be less satisfied not just with low pay, but also with pay below a reference (expected) compensation (Koszegi and Rabin 2006; Mas 2006; Esteves-Sorenson et al. 2015).[35] Rather than illustrating this "direct effect" of increasing expected earnings on retention, this section aims to show that the observed treatment effects on retention are largely driven by differences in the composition of workers, i.e., the types of workers who self-selected into the high-pay treatment are more affected by the negative "surprise" of earning less than initially expected compared with workers who self-selected into the low-pay treatment.

To corroborate this, Figure V plots the distribution of donations for the CHPs initially recruited in month 0 (left graph) and for those retained in month 24 (right graph). While the high-pay treatment attracted more agents who did not donate any money, more than 40 percent of them dropped out within the first two years. At the same time, the high-pay treatment discouraged agents who donated larger amounts and who would have stayed on the job with a probability close to one if they had been recruited. As discussed in the theory (see Appendix A), it is the crowding-out of these "good" workers (generated by the signal conveyed by incentives) that leads to a reduction in the number of workers retained. In the absence of such signal and such crowding-out, the number of workers retained would unambiguously

[35] In my context, the CHP position is considered a very "social" job relative to other jobs while earnings may be significantly lower. The main reason for dissatisfaction (and dropping out) is the "negative surprise" regarding the realized level of earnings, not a "negative surprise" having to do with the social impact on the community.

increase.

The same argument is presented more formally in Table IX. Column 1 indicates that CHPs recruited in the high-pay and medium-pay treatments are, respectively, 74 and 43 percent more likely to drop out in the first two years of work than those in the low-pay treatment. Once I control for CHP prosociality, the point estimates of the treatment effects shrink and lose their significance, suggesting that selection matters for understanding retention.[36] Absolute measures of prosociality are found to be strong predictors of retention: an increase of 1,000 UGX in the amount donated (two standard deviations) decreases the dropout rate by 74 percent. Similarly, CHPs who have volunteered in the health sector in the past are 68 percent less likely to drop out (Table IX, Column 3). In contrast, a relative measure of prosociality, i.e., the relative importance given to "helping the community" as a job characteristic relative to "earning money" or "earning respect," does not predict retention. Finally, having sold health products in the past positively correlates with retention as well. All these results are robust to adding individual-level controls and are robust to using a proportional hazard model that corrects for right-censoring (Table A.X).

The differential impact of the treatments with respect to prosociality, as measured with the amount donated, is investigated in Figure VIII with:

$$\begin{aligned} Drop\,Out_{gt} \quad = \quad & \alpha_t + \beta_{1t}\, Medium\,Pay_g + \beta_{2t}\, High\,Pay_g + \beta_{3t}\, Donation_g \\ & + \beta_{4t}\, Medium\,Pay_g * Donation_g + \beta_{5t}\, High\,Pay_g * Donation_g + u_{gt} \end{aligned}$$

The interaction terms between the treatments and the amount donated, i.e., β_{4t} and β_{5t}, suggest that agents who put a larger weight on the social output of the job are less sensitive to discrepancies between expected and realized earnings. These effects are stronger in the first months on the job and attenuate over time as the surprise effect disappears. The regression counterpart of this figure, obtained two years after recruitment, is presented in Table A.XI for the different available measures of prosociality.

Two sets of findings suggest that reference dependence plays a small role in my context. First, as indicated in Table IX, the treatment effects on retention shrink and lose significance once I control for selection. This suggests that reference dependence and negative reciprocity, as residual mechanisms, do not have a relevant role in driving the results. Moreover, the interaction of the treatment groups with a measure of each CHP's reference dependence is negative and not significant (Table A.XI, Column 6).[37] If reference dependence were driving

[36] Given that selection is affected by the treatments, prosociality is not a predetermined control and the estimated treatment effects are not causal. There is, however, no reason to believe that controlling for prosociality leads to overestimating the negative effect of increasing earnings on the dropout rate.

[37] This measure, which was collected during the initial CHP training session, counts the number of lotteries that the CHP is not willing to play from a list of seven hypothetical lotteries of the form "50% chance of winning 5 thousand UGX, 50% chance of losing X," where X varies from 0 to 6 thousand. Commonly used in the behavioral economics literature (Fehr and Goette 2007; Abeler et al. 2011; De Quidt 2013), the idea of

the results on retention, then we would expect agents with a high degree of reference dependence to be significantly more likely to be disappointed and to drop out in the high-pay treatment than in the low-pay one.

Due to space constraints, two last steps of the analysis are reported in the annex but are worth mentioning here. In the first step, I estimate the exact quit elasticity by instrumenting CHPs' expected earnings with the random treatment assignment (more details in Appendix D on the first stage and the exclusion restriction). Table A.XVII shows that a one-standard-deviation increase in expected earnings increases the dropout rate by 15 percentage points. Finally, Appendix E discusses the recruitment campaign as a powerful and more cost-effective tool to improve retention than changing the actual incentives.

7 Treatment Effects on Performance

If the non-prosocial workers who quit the job more quickly are also those who perform better while on the job, then the organization faces a clear trade-off on whom to recruit. This section explores whether this is the case using two sources of data. Output "quantity,"—i.e., the number of home visits, pre- and postnatal checks, and products sold—is measured using data collected by BRAC from each CHP on a monthly basis (more details below). Because BRAC imposes targets on these measures of performance that are easy to observe and easy to monitor, substitution between "quantity" and "quality" is a concern. Data on output "quality"—i.e., information content of the visits and types of households targeted—are collected by interviewing a random sample of CHPs' clients. As the ultimate goal of the BRAC's health program is to improve maternal and child health, the aim of this survey is to collect data on CHPs' targeting: CHPs are asked to prioritize households with women who are pregnant or who have recently given birth ("priority households").

For each data source, the analysis proceeds as follows. First, I test the hypothesis that the types of workers recruited in the high-pay treatment (i.e. non prosocial workers) are "stars," who counterbalance lower retention with higher performance on the job. To do so, I compare the aggregate performance of different types of CHPs, a measure that correlates with the length of time a CHP remains in her post as well as with the performance of the CHP while she is active. Second, I restrict the analysis to the 89 percent of CHPs who are active at the time of the data collection, therefore eliminating differences in retention. While this approach is not without caveat—attrition is not random—it provides suggestive evidence on their performance on the job.

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this measure is that aversion to risk in small-stakes lotteries that involve a loss is better explained by reference dependence than standard concave utility. Note that Table A.VIII shows that the treatments do not affect the average reference dependence among the applicants.

BRAC Monthly Records

Each month, CHPs are required to attend a one-day refresher course, in which they receive further training and are given the opportunity of buying new products. During the refresher courses that ran from May 2013 until April 2014, BRAC recorded information on the number of home visits, the number of pre- and postnatal checks provided, and the sales profits of each CHP.[38]

Data on monthly CHP sales profits indicate that the compensation levels of the CHPs in my experiment are in line with those announced in the recruitment leaflets: CHPs who have been active during the period from May 2013 to April 2014 report an average sales profit of 42 thousand UGX per month, with a maximum of 177 thousand and a minimum of 5 thousand UGX (only one agent earned less than 7 thousand UGX).

The treatment effects on the full sample of 301 recruited CHPs are presented in Table X. The strategy used in this table was to assign a performance of zero for the months of inactivity of each CHP who dropped out before April 2014.[39] The results show that CHPs recruited in the high-pay and medium-pay treatments do worse on the social tasks than those in the low-pay treatment: they visit fewer households and provide fewer pre- and postnatal checks to pregnant women. Performance on the business task, measured by the monthly sales profits, however, does not vary significantly across treatments.[40]

On a scale from 1 to 10, CHPs' supervisors rated the first-year performance of the CHPs 9 and 12 percent lower in the high- and medium-pay treatments than in the low-pay one. This means that, ultimately, BRAC is more satisfied with workers recruited in the low-pay treatment. Importantly, all these measures of aggregate performance are positively and significantly correlated with prosociality, indicating that prosociality not only predicts retention but also aggregate performance.

To estimate treatment effects on performance on the job, Table A.XII (Part 1) displays the same regressions as in Table X but restricts the analysis to the first month of available data (May 2013) and to the 267 health promoters who were active by that time, therefore eliminating differences in retention across treatments. As it eliminates a non-random sample of 34 CHPs, this strategy is not without caveat. However, it provides suggestive evidence that

[38] This information is collected from logbooks in which CHPs record their daily activities. To ensure accuracy in reporting, CHPs' supervisors select a random sample of households each week from the home visits register and follow up with the clients.

[39] The 41 CHPs who dropped out between May 2013 and April 2014 are assigned a performance of zero in the months following the one in which they dropped out. The 34 CHPs who dropped out before May 2013 are assigned an average monthly performance of zero. While the lack of information on their performance is a caveat, the results are unlikely to be strongly affected, as the proportion of CHPs who dropped out immediately after they were hired is small.

[40] Because information on the number of hours each CHP spent selling goods would have to be self-reported and CHPs have no incentives to tell the truth, I am unfortunately not able to compare "earnings per hour of work," which would be the best proxy of sales performance.

performance on the job in the high-pay treatment is lower than in the low-pay one, although most of the coefficients are not significant. Interestingly, prosociality as measured with the amount donated strongly and significantly positively predicts the number of home visits but does not predict sales profits.

While these results reject the hypothesis that the CHPs hired with higher expected earnings are "stars," previous analysis has shown that higher earnings have the advantage of increasing the number of CHPs recruited. To analyze the trade-off between the number and quality of CHPs recruited, Table A.XII (Part 2) replicates the analysis of Table X using the full sample of 315 villages in which the recruitment took place and assigning a monthly performance of zero to all villages in which no CHP was recruited. The table shows that performance remains lower in the higher-pay treatment: while the coefficients are not significant anymore, they remain economically large. The higher number of workers hired in the high-pay treatment hence does not outweigh the lower quality of these recruited workers.

Household Survey

One year after CHPs' recruitment, a random sample of 10 households per village (10 percent of the experiment's population) was surveyed on the services received from the CHP. Details on the timing and the sampling strategy are presented in Appendix C.

While there may be complementarities between the quantity of products sold and the number of clients visited, these complementarities are less likely to exist between sales and the proportion of priority households targeted. Visiting priority households indeed takes more time than visiting other households: CHPs first need to identify them, and during the visits they need to ensure pre- and postnatal checks. Although agents who care about earning money (who are mainly recruited in the high-pay treatment) may have an incentive to visit a large number of households, they should have less of an incentive to target "priority households."

Table XI confirms this is the case. While priority households are 8 and 11 percentage points (13 and 20 percent) more likely than non-priority households to know the CHP and to have been visited by the CHP in the low-pay treatment, they are not more likely to be targeted in the high-pay treatment.[41] These results hold when the analysis is restricted to the 255 CHPs who were still active at the time of the household survey (Table A.XV), indicating that on-the-job performance as measured by the targeting of priority households is significantly worse in the high-pay treatment.

Finally, the data show that households in the high-pay treatment do not have better health knowledge and are not less likely to suffer from health problems such as malaria, diarrhea, or intestinal worms. If anything, the results indicate that households in the high-pay treatment

[41] Although the "number of times a household was visited by the CHP" does not differ significantly across treatments, the coefficient on the high-pay treatment is negative and, taking into account the number of households in a village, it is in line with the results on household visits of Section 7.

are less likely to know how to treat diarrhea and more likely to have suffered from intestinal worms. Because these data were collected only for the subsample of households who knew the CHP when interviewed, the results suffer from sample selection and need to be taken with caution. A more detailed analysis of the findings and of how I tackle the selection problem are presented in Appendix C (Part 2).

8 Conclusion

We know that individuals respond to financial incentives. There is indeed evidence that financial incentives affect agents' effort on the job and selection into the job (Dal Bó et al. 2013; Ashraf et al. 2014b). While the existing literature has focused on the *motivational* effect of incentives, this paper provides, to the best of my knowledge, the first real-world empirical evidence of the *signaling* effect of incentives: when candidates have incomplete information about a job, incentives can convey a signal about the job characteristics that affects agents' behavior (Benabou and Tirole 2003; Sliwka 2007).

To cleanly identify the *signal channel* of incentives, I conduct a field experiment that creates exogenous variation in expected earnings for a newly created health position in Uganda that consists of both a business and a social component. I find that financial incentives signal the business-oriented nature of the job and decrease the perceived social output. While higher financial incentives attract agents who are interested in the remunerated component of the job and increase the probability of filling a vacancy, the signal discourages agents with strong prosocial preferences from applying and reduces the likelihood they are hired.

The crowding-out of prosocial motivation, generated by the signal, is costly for the organization. Prosocial motivation, measured both with survey questions and with a contextualized dictator game, is found to be the key predictor of workers' retention and performance. This supports the existing theoretical literature that sheds light on the role of prosocial motivation in aligning the interests of the workers with interests of the organizations (Besley and Ghatak 2005).

For wider applicability of the results, it is important to point out that the type and the intensity of the signal conveyed by financial incentives may undoubtedly vary from the one identified in this experiment and will ultimately depend on the context, i.e., how standardized a job is, its characteristics, the way it is advertised, who the recruiter is, and who the potential candidates are. In the private sector, for instance, more lucrative positions may be perceived as more difficult or less enjoyable rather than less social. While the context certainly matters, this paper provides evidence that "money can talk," especially at the recruitment stage, and organizations need to pay careful attention to what it says in order to design optimal contracts and optimal recruitment strategies.

Appendices

A Theoretical Framework

This appendix develops a simple theoretical framework that is tailored to my context, in which the principal (BRAC) needs to hire agents (health promoters) for a position that consists of a "social" task, which entails positive externality for the community (health education, pre- and postnatal checks), and a "private" (business) task, which entails a monetary benefit for the agents and the principal (sales). The model makes it clear how changing expected earnings (or, equivalently, financial incentives) at the recruitment stage affects the agents' perceptions of the relative importance of the two tasks, and, through this, the type and number of applicants. Although this section focuses mainly on the agent's problem, I discuss the principal's problem at the end of this appendix.

The position is one in which agents dedicate a proportion λ of their time to the social task and $1 - \lambda$ to the private task. The two tasks vary both in the monetary and the non-monetary dimensions. One hour of work on the social task generates social output s. One hour of work on the private task yields a profit y_i that varies across workers: $y_i = \mu + \varepsilon_i$, where ε_i is an individual-specific error term that is drawn from a symmetric continuous distribution g (cumulative distribution function G), has a mean of 0 and is larger or equal than $-\mu$. Monetary compensation, $Y_i = y_i(1 - \lambda)$, thus varies across workers and decreases with λ, while the social output, $s\lambda$, is constant across workers and increases with λ.[42]

Agents differ in their prosocial preferences, i.e., how much they care about the social task, and in their interest in selling products, i.e., how much they care about the private task. At the recruitment stage, they decide whether to apply for the position or not. To simplify the model without changing the intuition of its predictions, I assume that workers have no degree of freedom in the allocation of time across tasks: λ is imposed by the principal through a monitoring technology and all workers are required to work a similar number of hours, which I normalize to 1. The setting I am considering is thus one that abstracts from moral hazard. Rather than deciding their level of effort, recruited workers decide whether to stay on the job and exert the amount of effort they are "required" to provide or whether to drop out. Predictions of the model when effort is a choice variable are discussed below.

At the recruitment stage, agents do not know their own realization of y_i, while, without

[42] The model can be generalized to tasks that are both remunerated and that both create social output, as long as one is relatively more lucrative and the other creates relatively more social output. In the health-promoter position, the sales component (i.e., sales of oil, salt soap, etc.) may entail a positive externality, but this is certainly smaller than the one generated by educating the community on how to treat and prevent serious diseases such as malaria, diarrhea, etc.

loss of generality, s is assumed to be common knowledge. As the position is new, both μ and λ are unknown to potential candidates and are not revealed during recruitment.[43] Agents' expected earnings from the position, $\hat{Y}$, are assumed to be entirely determined by the vacancy advertisement and are treated as exogenous in the agent's problem:

$$\hat{Y} = E\left(Y_i\right) = E\left(\mu(1-\lambda)\right).$$

Because the setting is one with multitasking and the agents are not separately informed about λ and μ, agents make inferences on λ and μ after seeing $\hat{Y}$. They update their beliefs about $\hat{\lambda}(\hat{Y})$, the expected proportion of time dedicated to the social task, and about $\hat{\mu}(\hat{Y})$, the expected average profit per hour spent on the private task.

Depending on the agents' inference process, increasing $\hat{Y}$ has three possible effects on $\hat{\lambda}$: (1) a decrease in $\hat{\lambda}$, (2) no change in $\hat{\lambda}$, (3) an increase in $\hat{\lambda}$.[44] Under scenario 1, which I call the *Inference* scenario, increasing $\hat{Y}$ decreases the expected proportion of time allocated to the social task, i.e., $\hat{\lambda}'(\hat{Y}) < 0$. In this case, expected earnings signal the "business-oriented" nature of a job and are substitutes with expected social output. Under scenario 2, which I call the *No Inference* scenario, expected earnings $\hat{Y}$ do not convey a signal on the expected allocation of time across tasks, i.e., $\hat{\lambda}'(\hat{Y}) = 0$, and thus do not affect the expected social output. This case arises if the inference on $\hat{Y}$ is made entirely on μ and not on λ, e.g., $\hat{\lambda}$ is unaffected by $\hat{Y}$ or λ is known/revealed before recruitment.[45] Finally, although scenario 3 is theoretically possible, i.e., $\hat{\lambda}'(\hat{Y}) > 0$, it is less intuitive, it is not supported by my data, and, for exposition reasons, I exclude it from the model. This case would indeed require agents to perceive a more lucrative position as one in which more time is spent on the social task, and hence as being more "social."[46]

Definition 1: Financial Incentives as Signals. *The effect of financial incentives (or equivalently, expected earnings $\hat{Y}$) on the agents' perceived nature of the job depends on whether incentives carry a signal about the social output. (i) In the "No Inference" scenario*

[43] Although the principal does not know the ex-ante realization of y_i for a given worker, she knows μ and λ and has more information than the agents at the recruitment stage. In the specific context of this paper (rural villages in Uganda), due to the limited education and limited attention of agents, the principal does not reveal μ and λ at the recruitment stage (this would require agents to understand what proportions and probabilities are and to have calculation skills in order to figure out their expected earnings). Instead, the principal decides which expected earnings to give to the candidates.

[44] The inference process depends on the prior joint distribution of (μ, λ) and on whether or not agents know the distribution G. If people do not know the underlying distribution G but know the bounds of the distribution, Laplace's principal of insufficient reasons (PIR) says that people perceive the probability as being uniform and thus treat events as equally likely.

[45] We have: $\hat{Y} = \hat{\mu}(1-\lambda)$, $\hat{\mu}'(\hat{Y}) = (1-\lambda)^{-1} > 0$ and $\hat{\lambda}'(\hat{Y}) = 0$.

[46] This could happen if agents perceive λ and μ to be sufficiently positively correlated. As better paid positions usually have lower social output (prosocial motivation works as a compensating differential), it is more intuitive to assume that λ and μ are negatively correlated in agents' priors, and that $\hat{\lambda}'(\hat{Y}) \leq 0$ and $\hat{\mu}'(\hat{Y}) \geq 0$.

($\hat{\lambda}'(\hat{Y}) = 0$), higher financial incentives do no affect the job's expected social output, i.e., a more lucrative position is perceived as one in which the same amount of time is devoted to the social task. (ii) In the "Inference" scenario ($\hat{\lambda}'(\hat{Y}) < 0$), higher financial incentives decrease the job's expected social output, i.e., a more lucrative position is perceived as one in which less time is devoted to the social task.

Section 4 of the paper explores whether the data of my experiment support the *Inference* or the *No Inference* scenario.

It is important to note that, although the "signal channel" is modeled here as the signal that earnings provide on the allocation of time across tasks, other theoretical frameworks would have similar predictions. For instance, under equilibrium conditions in which positions with large social output are paid less (prosocial motivation works as a compensating differential), increasing the pay of a new job may adversely affect an agent's perception of the social output.[47]

Agents make two decisions: (a) whether to apply for the position, and (b) once on the job, whether to stay. The rest of the model explores both agent decisions and makes clear how these decisions depend on their preferences. The goal of the principal, which is discussed in more detail below, consists here in recruiting and retaining as many workers as possible. Indeed, the position is self-sustainable: the cost of recruiting, training, and compensating workers to perform the social task is covered by requiring workers to spend a proportion of time on the private task, which yields a profit both to the worker and to the principal. As a consequence, the model assumes that any applicant is recruited and no worker is fired by the principal.[48]

Agent's Choice: Application

Agents apply for the position if the expected utility from working, which depends on how much they like the social and the private tasks, is greater than the outside option:

$$\hat{U}_i(\hat{Y}) = \sigma_i \hat{Y} + \gamma_i s \hat{\lambda}(\hat{Y}) \geq \bar{u}, \tag{7}$$

where σ_i is the "joy of selling" (or the taste for money), and γ_i is the "joy of giving" (or the prosocial motivation).[49] Each individual's preferences $(\sigma_i, \gamma_i) \,\epsilon\, \mathbb{R}^2$ are drawn independently

[47] A large theoretical and empirical literature shows that positions with large social output are often paid less than others (prosocial motivation works as a compensating differential - e.g., Weisbrod 1983, Preston 1989).

[48] An alternative model is one in which the principal recruits a single worker among all the applicants. If the probability of filling a position is strictly smaller than one and is affected by the way the position is advertised, then the predictions remain similar. However, the fact that the principal selects one worker among different applicants may attenuate or exacerbate differences in the applicant pool, depending on whether or not worker preferences are observable to the principal.

[49] I assume here that people who like selling care about the expected value of products sold, $E(\mu(1-\lambda))$. I get similar predictions if, alternatively, I assume that agents care about the expected time spent selling,

from a continuous density f, where $\sigma_i \geq 0$ and $\gamma_i \geq 0$. Finally, I denote with N the number of applicants.

The effect of stronger financial incentives, or equivalently higher expected earnings $\hat{Y}$, on the applicant pool depends on whether increasing $\hat{Y}$ affects $\hat{\lambda}$. This is represented in Figure A.I(a). In the *No Inference* scenario ($\hat{\lambda}'(\hat{Y}) = 0$), a higher $\hat{Y}$ encourages an extra number of agents to apply, while discouraging no one. Indeed, the expected utility function of potential applicants with $\sigma_i > 0$ increases with $\hat{Y}$, i.e., $\hat{U}_i'(\hat{Y}) > 0$, while the expected utility of agents who do not care about selling products ($\sigma_i = 0$) is unaffected by $\hat{Y}$ and $\hat{U_i}'(\hat{Y}) = 0$. As a consequence, raising $\hat{Y}$ unambiguously increases the number of applicants: $N'(\hat{Y}) > 0$.

In the *Inference* scenario ($\hat{\lambda}'(\hat{Y}) < 0$), increasing $\hat{Y}$ is "good news" for agents with little prosocial motivation and a large interest in sales, but "bad news" for those with large prosocial motivation and little interest in sales. As a consequence, increasing $\hat{Y}$ both crowds in and crowds out agents. Individuals who are crowded out have a higher γ_i and lower σ_i than those who are crowded in. A higher $\hat{Y}$ increases the number of applicants ($N'(\hat{Y}) > 0$) only if the weight given to B is larger than the weight given to C by the distribution f - see bottom graph of Figure A.I(a). For a given distribution f, the stronger is the inference on $\hat{\lambda}$, i.e., the larger is $|\hat{\lambda}'(\hat{Y})|$, the larger is the area C relative to B, and the less likely it is that $N'(\hat{Y}) > 0$.[50]

Result 1: Number of Applicants. *The effect of financial incentives on the applicant pool depends on whether incentives carry a signal about the social output. (i) In the "No Inference" scenario ($\hat{\lambda}'(\hat{Y}) = 0$), higher financial incentives increase the number of applicants ($N'(\hat{Y}) > 0$) without displacing applicants with high prosocial motivation. (ii) In the "Inference" scenario ($\hat{\lambda}'(\hat{Y}) < 0$), higher financial incentives displace applicants with high prosocial motivation and have an ambiguous effect on the number of applicants N.*

Result 1 is tested in Section 5 of the paper.

Agent's Choice: Retention

Once on the job, workers learn λ and their own realization of y_i and decide whether to stay on the job or whether to drop out. An agent is retained only if her participation constraint remains satisfied:

$$U_i = \sigma_i y_i(1 - \lambda) + \gamma_i s\lambda \geq \bar{u}. \tag{8}$$

Conditional on being recruited, any worker who is "prosocial" enough, i.e., $\gamma_i \geq \frac{\bar{u}}{s\lambda}$, is always retained. Because there is a positive probability that $y_i = 0$, no matter the level of σ_i, agents with $\gamma_i \leq \frac{\bar{u}}{s\lambda}$ drop out with a positive probability $G(\frac{\bar{u}-\gamma_i s\lambda}{\sigma_i(1-\lambda)} - \mu)$.

$E(1 - \lambda)$.

[50] Whether increasing $\hat{Y}$ decreases or increases the average prosociality and interest in sales in the applicant pool depends on f and in particular on the correlation between prosociality and interest in sales in the population.

Let $\hat{Y}_1$ be the level of expected earnings, which, in expectation, equals the average realized earnings: $\hat{Y}_1 = \mu(1-\lambda)$. As represented in Figure A.I(b), any agent lying on the solid black locus is indifferent between applying or not when the position is advertised with $\hat{Y}_1$ and, once recruited, drops out with probability $\frac{1}{2}$. Agents lying below this black solid locus drop out with a probability larger than $\frac{1}{2}$, while all agents lying above the locus drop out with a probability smaller than $\frac{1}{2}$.

All else being equal, the probability of dropping out decreases with stronger prosocial motivation γ_i and higher interest in sales σ_i.[51] Which preference parameter is the strongest predictor of retention (all else being equal) depends on the relative importance of the social benefits $s\lambda$ with respect to the average monetary benefits $\mu(1-\lambda)$. In positions that generate relatively high social output but low average earnings (such as the health position analyzed in this paper), all else being equal, the marginal increase in retention with respect to γ_i is larger than the increase with respect to σ_i.

The information that $\hat{Y}$ conveys on $\hat{\lambda}$ at the recruitment stage impacts the effect of increasing $\hat{Y}$ on the total number of workers retained R, i.e., the number of agents initially recruited who decide to stay. If all recruited workers stay, then $R = N$. As presented in the top graph of Figure A.I(b), in the *No Inference* scenario, raising $\hat{Y}$ increases the number of retained workers: $R'(\hat{Y}) > 0$. Indeed, a higher $\hat{Y}$ increases the number of workers recruited N, and a proportion of these subsequently remain active.

In the *Inference* scenario (bottom graph), the effect of increasing $\hat{Y}$ on R is instead ambiguous. The sign of $R'(\hat{Y})$ depends on (a) the sizes of the areas B, C_1, and C_2, i.e., the intensity of the signal, (b) the weights given to B, C_1, and C_2 by the distribution f, and (c) the distribution of earnings g.

Result 2: Number of Retained Workers. *The effect of expected earnings $\hat{Y}$ on the number of workers retained depends on whether they carry a signal about the social output. (i) In the "No Inference" scenario ($\hat{\lambda}'(\hat{Y}) = 0$), higher expected earnings increase the total number of retained workers ($R'(\hat{Y}) > 0$). (ii) In the "Inference" scenario ($\hat{\lambda}'(\hat{Y}) < 0$), higher expected earnings have an ambiguous effect on the number of retained workers R.*

Result 2 is tested in Section 6 of the paper. In the *No Inference* scenario, increasing $\hat{Y}$ above $\hat{Y}_1$ reduces conditional retention, i.e., the proportion of the recruited agents who stay ($\frac{R}{N}$). Conditional on being recruited, agents who are crowded in (area B) are indeed more likely to drop out than other recruited workers (areas A_1 and A_2).[52]

[51] If σ_i is not held constant, then the effect of prosocial motivation on dropping out is negative only if the correlation between γ_i and σ_i is not too negative.

[52] Note that the predictions change if $\hat{Y}$ increases from level $\hat{Y}_2$ to $\hat{Y}_1$, where $\hat{Y}_2 < \hat{Y}_1$. Whenever agents are recruited with expectations $\hat{Y}_2$, they perceive the job as being more social than it actually is. As a consequence, agents with strong prosocial preferences may end up dropping out from the position if the discrepancy between $\hat{\lambda} - \lambda$ is negative enough. In this case, for levels of $\hat{Y}$ below $\hat{Y}_1$, increasing $\hat{Y}$ reduces retention only if the signal is weak enough ($|\hat{\lambda}'(\hat{Y})|$ is small enough).

In the *Inference* scenario, the effect of increasing $\hat{Y}$ above $\hat{Y}_1$ on conditional retention is negative if retention among the agents who are crowded out (areas C_1, C_2) is large enough. In this case, we can end up in a situation in which increasing $\hat{Y}$ above $\hat{Y}_1$ leads to more agents being recruited but fewer being retained ($N'(\hat{Y}) > 0,\ R'(\hat{Y}) < 0$). This situation is more likely to arise for positions that generate relatively high social output and low average earnings (such as the health position analyzed in this paper).

Result 3: *In the "Inference" scenario ($\hat{\lambda}'(\hat{Y}) < 0$), higher expected earnings $\hat{Y}$ may increase the total number of recruited workers ($N'(\hat{Y}) > 0$) while reducing the number of retained workers ($R'(\hat{Y}) < 0$).*

Although the results for retention are driven, in my specific framework, by the discrepancy between expectations and realizations ($\hat{Y} \neq Y$), the same predictions would hold in a model in which the principal changes the real incentives Y rather than the earnings expectations $\hat{Y}$, and in which recruited workers have a probability of receiving an offer for another, better-paying position and then decide whether to quit.

Extension 1: Adding Agents' Effort

The model abstracts from the possibility that agents shirk. The intuition of the model holds if I introduce shirking on any of the two tasks as long as a minimum level of λ is imposed by the principal. In particular, the predictions for selection remain similar as long as agents' ex-ante perception of the minimum level of effort they will be "required" to exert on the private task increases with expected earnings. Once on the job, the choice of how much effort to exert on the social and the private tasks differs according to agents' preferences. All else being equal, the optimal level of effort on the private task increases with interest in sales σ_i, while effort on the social task increases with prosocial motivation γ_i.

The effort predictions change if we consider the more realistic case in which performance in the social task has two dimensions: social-output "quantity" (e.g., number of households visited) and social-output "quality" (e.g., information content of the visits, types of households visited). If "quality" is harder to monitor by the principal than "quantity," the model predicts little shirking on sales and social-output quantities but more shirking on social-output quality. In particular, the effort exerted in providing high-quality health services increases with prosocial motivation γ_i. This would predict that signaling high financial returns may actually decrease effort and performance on the job. This possibility is explored in Section 7.

Extension 2: Modeling the Principal's Problem

In order to make the theoretical framework as similar as possible to the empirical setting, I set up the problem of the principal as one in which, rather than choosing the financial incentives, the principal chooses the optimal earnings expectations $\hat{Y}$ to give to the agents, by manipulating the $\hat{Y}$ advertised in the recruitment message of the position.

I assume a three-period model. In period 0, the principal chooses $\hat{Y}$. After seeing $\hat{Y}$ and forming expectations ($\hat{\lambda}$ and $\hat{\mu}$), agents decide whether to apply, and the principal recruits workers from the pool of applicants. In period 1, recruited agents work. By the end of the period, they have learned λ and their own realization of y_i and decide whether to stay on the job. In period 2, agents who decide to stay continue working while those who drop out stop working. I assume that the principal recruits workers in period 1 only.

As already mentioned, the principal asks agents to spend a proportion λ of their time on the social task and $1-\lambda$ on the private task. While the principal gets no monetary benefits from letting the agent work on the social task, she earns $\tilde{y}$ from each unit of time the agent spends on the private task.[53] The larger is λ, the larger is the relative weight the principal puts on the social output s relative to money $\tilde{y}$. Given the absence of moral hazard (every worker puts the same effort once hired), the utility of the principal from hiring an agent in a given period is the same across workers and equals $s\lambda + \tilde{y}(1-\lambda)$.

The position is self-sustainable and the costs of the social task are covered by the private task. The private task pays the workers a salary and covers the fixed per-period cost T of training and monitoring agents. If the principal cares exclusively about the social task, she chooses $\bar{\lambda} = 1 - \frac{T}{\tilde{y}} < 1$. The more the principal cares about the private task, the larger is λ, where $\lambda \,\epsilon\, [\bar{\lambda}, 1)$.

Given the absence of moral hazard and the self-sustainability of the position, the goal of the principal is to recruit and retain as many agents as possible:

$$max_{\hat{Y}} \quad \left(N(\hat{Y}) + \delta R(\hat{Y})\right),$$

where δ is the discount rate, which is lower if the need for hiring workers is urgent, N is the number of workers recruited in period 1, and R is the number of workers retained in period 2 ($R < N$).[54]

When $\hat{Y}$ provides no signal on $\hat{\lambda}$ ($\hat{\lambda}'(\hat{Y}) = 0$), both $N'(\hat{Y})$ and $R'(\hat{Y})$ are positive, and it is thus optimal for the principal to increase $\hat{Y}$ as much as she can, no matter what the discount rate is. On the other hand, if $\hat{Y}$ provides a signal about $\hat{\lambda}$ ($\hat{\lambda}'(\hat{Y}) < 0$), increasing $\hat{Y}$ may backfire. Three cases, which depend on the intensity of the signal $|\hat{\lambda}'(\hat{Y})|$, need to be

[53] This is equivalent to saying that an hour of work on the private task yields a total profit $M_i = \tilde{y} + y_i$. The principal and the worker split the profits: a fixed amount $\tilde{y}$ goes to the principal, while $y_i = M_i - \tilde{y}$ goes to the agent. An alternative way of modeling the split is to assume that the principal takes a proportion α of the total profit M_i, while $1-\alpha$ goes to the agent. Predictions remain similar.

[54] An alternative model is one in which the principal recruits a single worker among all the applicants. If the probability of filling a position is strictly smaller than one and is affected by the way the position is advertised, then the predictions on the number of hired workers remain similar. However, the fact that the principal selects one worker among different applicants may attenuate or exacerbate differences in the applicant pool, depending on whether workers' preferences are observable to the principal.

considered. If the signal is strong enough such that $N'(\hat{Y}) < 0$, it is optimal for the recruiter to choose the lowest possible $\hat{Y}$, as $R'(\hat{Y})$ is also negative. In the opposite case in which the signal is weak enough such that both $N'(\hat{Y}) > 0$ and $R'(\hat{Y}) > 0$, the recruiter chooses $\hat{Y}$ the highest possible. Finally, in intermediary cases with $N'(\hat{Y}) > 0$ but $R'(\hat{Y}) < 0$, a trade-off arises and the principal sets a high $\hat{Y}$ only if δ is low enough, i.e., the health situation is urgent such that the principal gives more weight to the short-term workforce relative to the long-term workforce.

As a conclusion, it is optimal for the principal to advertise the position with high expected earnings $\hat{Y}$ only if (a) $\hat{Y}$ conveys no signal on the social output, or (b) $\hat{Y}$ conveys a weak negative signal on the social output such that the number of applicants increases with $\hat{Y}$ ($N'(\hat{Y}) > 0$) and the discount rate is low enough.

It is important to note that the objective function of the principal would be different if the maximization problem were subject to a budget constraint, e.g., if the fixed cost of training workers in the first period are too large to be covered by the profits that the principal makes in one single period. In this case, the principal can recruit only a specific number of agents rather than all the applicants. As soon as the number of applicants exceeds the number of workers to be recruited, the value of N no longer matters for the principal. Retention becomes crucial: the principal cares about recruiting workers who are retained in the second period, while recruiting a worker who is not retained leads to a loss.

B The Treatment Effects' Underlying Mechanisms

Section 3 provides evidence that the higher the point revealed in the earnings distribution, the higher the expected earnings. This appendix discusses the potential underlying mechanisms.

Firstly, as the recruitment was carried out in areas where the health program was previously nonexistent, the experiment context is subject to incomplete information. Individuals may thus update their beliefs about the earnings distribution differently when different points of the distribution are revealed, therefore changing the expected average income.[55] Note, however, that this updating story is consistent with the findings only under specific priors, i.e., the maximum is lower than 200 thousand UGX and the minimum is higher than 7 thousand UGX. Given that 7 thousand (200 thousand) UGX translates into earnings per hour that are well below (above) the average per hour earnings in Uganda, these priors are realistic. To corroborate this, I collected information on agents' priors for 102 microfinance clients

[55] The updating of beliefs as a consequence of an information campaign has been examined empirically in many settings (e.g., Jensen 2010). In the labor market, Osman (2014) shows that providing information about the whole distribution of incomes in different occupations to high school students in Egypt leads to changes in beliefs about the average expected income in the different occupations. Beam (2013) finds that informing Filipinos on the average earnings of overseas Filipino workers affects their expected wage abroad if their baseline beliefs are wrong.

across 10 BRAC microfinance groups of Kabale district, where the health program was not yet present. The respondents were first asked to observe the same CHP recruitment leaflet as the one in Figure I, but without the sentence "CHPs earn at least 7 (an average of 30) (up to 200) thousand UGX per month." They were then asked to answer the following question: "Imagine a very bad (good) month in which a CHP sells very few (many) health products. How much would a CHP earn in this very bad (good) month?" The results indicate that, across respondents, the perceived minimum earnings takes a median value of 20 thousand UGX, while the median perceived maximum equals 155 thousand UGX. This suggests that the updating story is indeed a potential mechanism.

A second potential explanation is that salience matters: the amount on the recruitment message may be a focal point (Bordalo et al. 2013; Chetty et al. 2009), and the task could be perceived as more lucrative in some vague sense, unrelated to a more complex updating story. Other theories, such as the overweighting of small probabilities, could also explain the results.

While I am not able to disentangle these mechanisms, I use the treatments as an exogenous source of variation of earnings expectations and use this to test the effect of financial incentives on perceptions of other job attributes (Section 4), selection (Section 5), retention (Section 6), and performance (Section 7).

C The Household Survey

C.1 Sampling and Data

A random sample of ten households in each CHP's area (ten percent of the CHP clients) were interviewed from July-September 2013, roughly a year after the CHPs were recruited. Without a full census of residents from which to draw a random sample within each village, it was necessary to select the households to be interviewed through a "random walk" method. Enumerators started walking from the CHP house, moving in the direction of the sun, and attempted to visit every fifth house on the road. If no one was home, they interviewed the house on the right. If there was still no one home, they moved to the house on the left. After each interview, they repeated the same procedure, skipping another four households before interviewing the fifth. At the end of a road, they were instructed to alternate directions (right, left, right, etc.).[56]

Table A.XIII provides evidence that the sample is balanced across treatment groups on a number of observed characteristics, e.g., distance to CHP house, number of children below age 5, number of female household members of reproductive age. Twenty-one percent of

[56] The survey covers 299 out of the 301 villages. Two villages (one for a CHP who dropped out and the other for an active CHP), were kept out of the survey in the Adjumani branch due to problems accessing the villages.

the households interviewed are "priority households"—i.e., at the time of the interview, the household included at least one pregnant woman or one woman who had given birth in the 6 months before the interview. Reassuringly, the percentage of priority households is balanced across treatments. Because the data are not balanced on whether the respondent is a female or a member of a BRAC microfinance group, I control for these variables in all the household-level regressions

C.2 Health Knowledge and Health Outcomes

Table A.XVI compares household health knowledge and health outcomes across treatments, focusing on the three main health problems in Uganda: malaria, diarrhea, and intestinal worms. Due to limited resources, the data were collected only for the subsample of households who knew who the CHP was when interviewed. To alleviate the concern that this is a selected sample of households, I control for the household's relationship with the CHP (relatives, friends, or acquaintances), distance from the CHP house in walking minutes, gender of the respondent, household composition (number of children under 5, number of women of reproductive age), and a dummy for priority households. Moreover, I estimate a weighted OLS regression that gives each CHP a weight equal to the number of households who know the CHP. This ensures that areas in which only a few respondents know the CHP are given less weight in the regression (these respondents are more likely to be a family member or a friend of the CHP and are less representative of the whole village).

The first thing to note from Table A.XVI is the high incidence of malaria, diarrhea, and intestinal worms in the study population: in the six months preceding the survey, 70 percent of the households had a member who suffered from malaria, 23 percent from diarrhea and 15 percent from intestinal worms. Moreover, households lack knowledge on how to treat and prevent these diseases. Most worrisome is the fact that only 33 percent of the respondents know that diarrhea should be treated with zinc supplementation plus oral dehydration salts.

The results indicate that households located in the village of a CHP recruited in the high-pay treatment are 24 percent less likely to know how to treat diarrhea (significant at the 5 percent level) than households located in the low-pay treatment. Moreover, the probability of a household member suffering from malaria, diarrhea, or intestinal worms is higher in the high-pay treatment, although the only coefficient that is significant is the one for intestinal worms: households are 37 percent more likely to have suffered from worms in the high-pay treatment than in the low-pay treatment. Overall, this table provides suggestive evidence that CHPs recruited in the low-pay treatment were more efficient in informing their community about health along a number of dimensions, although not all of them.

D Drop-out and Performance Elasticities

This appendix estimates the exact drop-out and performance elasticities with respect to expected earnings by instrumenting CHP earnings expectations with the random treatment assignment. Because the data on expectations were collected before the CHPs began working, the estimated elasticities incorporate the selection effect, i.e., agents who apply for a job that is perceived as being more lucrative have different traits, and have a different propensity to drop out, than agents who self-select into a less lucrative job.

The instrumental variable strategy is valid only if the exogeneity condition is satisfied, i.e., the treatments impact retention and performance only through a change in expected earnings. This condition is violated if the treatments impact selection through a change in the perceived income variability, which itself impacts retention. Since no selection is found on risk aversion (see Section 5.2), I can rule out this possibility. The exogeneity assumption is also violated if the treatments have a direct impact on the behavior of BRAC after the recruitment stage, which subsequently impacts retention and performance. This could happen, for instance, if BRAC decided to set different targets of performance across treatments or monitored more intensively in some treatments than in others. This hypothesis can be ruled out, as BRAC policies are the same all over Uganda and are determined by the Bangladesh head office. Finally, the exogeneity condition is violated if the differences between earnings expectations of the CHPs and those of the other microfinance clients varies significantly across treatments. If this difference-in-difference is significant, my treatments could impact retention through the selection of specific types of agents, e.g., the most optimistic women (with very high expected earnings) are recruited in the high-pay treatment and the most pessimistic ones (low expected earnings) in the low-pay treatment. Because the data on expectations in the recruitment experiment are available for the 301 selected CHPs and not for the whole microfinance group, I am not able to rule out this possible violation. Nonetheless, there is no specific reason to believe this would happen.

Table A.XVII estimates the same results as in Tables VII to X using the instrumental variable strategy rather than a reduced form approach. The results remain consistent with the OLS ones, e.g., a one-standard-deviation increase in expected earnings increases the dropout rate by 15 percentage points, while it decreases the overall number of households visited per month by 9 percent (see Part A of Table A.XVII). The F-statistic from the first stage equals 27 and validates the relevant condition. Part B and C of Table A.XVII provides further evidence that increasing expected earnings reduces the probability that CHP health services target priority households, that a household knows how to treat diarrhea (one of the most deadly diseases in Uganda) and increases the probability that a family member suffers from intestinal worms.

E The Powerful Role of the Advertisement Strategy

Organizations are concerned about two key challenges: how to motivate workers and how to retain the best ones. Empirical evidence highlights the role of financial incentives as a solution to both of these challenges, through a selection or an incentive effect (see Bandiera et al. 2011 for a review). However, financial incentives are costly and may not be the most cost-effective solution. This paper sheds light on the advertisement strategy of a position as an alternative cheaper way of improving the retention and performance of workers. In particular, I show that manipulating agent expectations at the recruitment stage has a strong effect on the selection of workers and, thus, on retention and aggregate performance. I indeed find that BRAC can increase the retention of workers by 72 percent simply by low-balling the position, i.e., lowering earnings expectations, and this can be done at zero cost.

Choosing an appropriate advertisement strategy is particularly relevant for jobs with ex-ante ambiguous earnings, i.e., jobs in which both the applicants and the recruiter do not know, at the recruitment stage, how much each applicant will earn if recruited. In this type of job—e.g., jobs that pay a piece-rate, a performance bonus, or in zero-hours contracts—workers do not receive a fixed wage each month. Earnings vary across workers depending on the characteristics of the applicant (e.g., ability and motivation) and/or the characteristics of the applicant's environment (e.g., in a sales position, earnings depend on the demand; in position with team bonuses, earnings depend on the performance of others, etc.). During the recruitment stage, the organization needs to convey information on the compensation level of the position to potential applicants and has the choice of advertising the position as being better or worse paying. Although this paper provides evidence that this can be done by making salient different points of the earnings distributions (revealing the top of the distribution versus the bottom), the recruiter can certainly vary expectations in other ways. In the case of a piece-rate position, the organization can for instance vary the information conveyed on the "difficulty" of a task; in zero-hour contracts, the expected number of days of work can also be manipulated, etc.

Figure I(a): Low-Pay Treatment

Do you want to become

COMMUNITY HEALTH PROMOTER?

Only literate women are eligible

What will you do?

Educate your community on disease prevention and treatment	Provide your community with access to medicines and health products	Identify and assist pregnant women
		

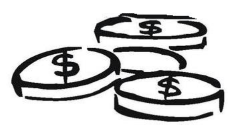
Why should you become Community Health Promoter?

- Gain the skills to prevent diseases and promote health for your family and neighbours
- Serve your community
- Become a respected leader in your community

How much will you earn?

- Buy medicines and health products from BRAC at a low price (panadol, fortified oil, soap, etc.)
- Sell them at a price higher than purchase price
- The more you sell, the more you earn

CHPs earn at least 7,000 Ushs per month



How will you acquire health knowledge?

2 weeks of initial training + 1 day refresher each month

⇩

Learn about the most important health issues
\+ Become highly trained on how to treat and prevent them

Figure I(b): Medium-Pay Treatment

brac

Do you want to become COMMUNITY HEALTH PROMOTER?

Only literate women are eligible

What will you do?

Educate your community on disease prevention and treatment	Provide your community with access to medicines and health products	Identify and assist pregnant women
		

Why should you become Community Health Promoter?

- Gain the skills to prevent diseases and promote health for your family and neighbours
- Serve your community
- Become a respected leader in your community

How much will you earn?

- Buy medicines and health products from BRAC at a low price (panadol, fortified oil, soap, etc.)
- Sell them at a price higher than purchase price
- The more you sell, the more you earn

CHPs earn an average of 30,000 Ushs per month

How will you acquire health knowledge?

2 weeks of initial training + 1 day refresher each month

⇩

Learn about the most important health issues

\+ Become highly trained on how to treat and prevent them

Figure I(c): High-Pay Treatment

brac

Do you want to become

COMMUNITY HEALTH PROMOTER?

Only literate women are eligible

What will you do?

Educate your community on disease prevention and treatment	Provide your community with access to medicines and health products	Identify and assist pregnant women
		

Why should you become Community Health Promoter?

- Gain the skills to prevent diseases and promote health for your family and neighbours
- Serve your community
- Become a respected leader in your community

How much will you earn?

- Buy medicines and health products from BRAC at a low price (panadol, fortified oil, soap, etc.)
- Sell them at a price higher than purchase price
- The more you sell, the more you earn

CHPs earn up to 200,000 Ushs per month

How will you acquire health knowledge?

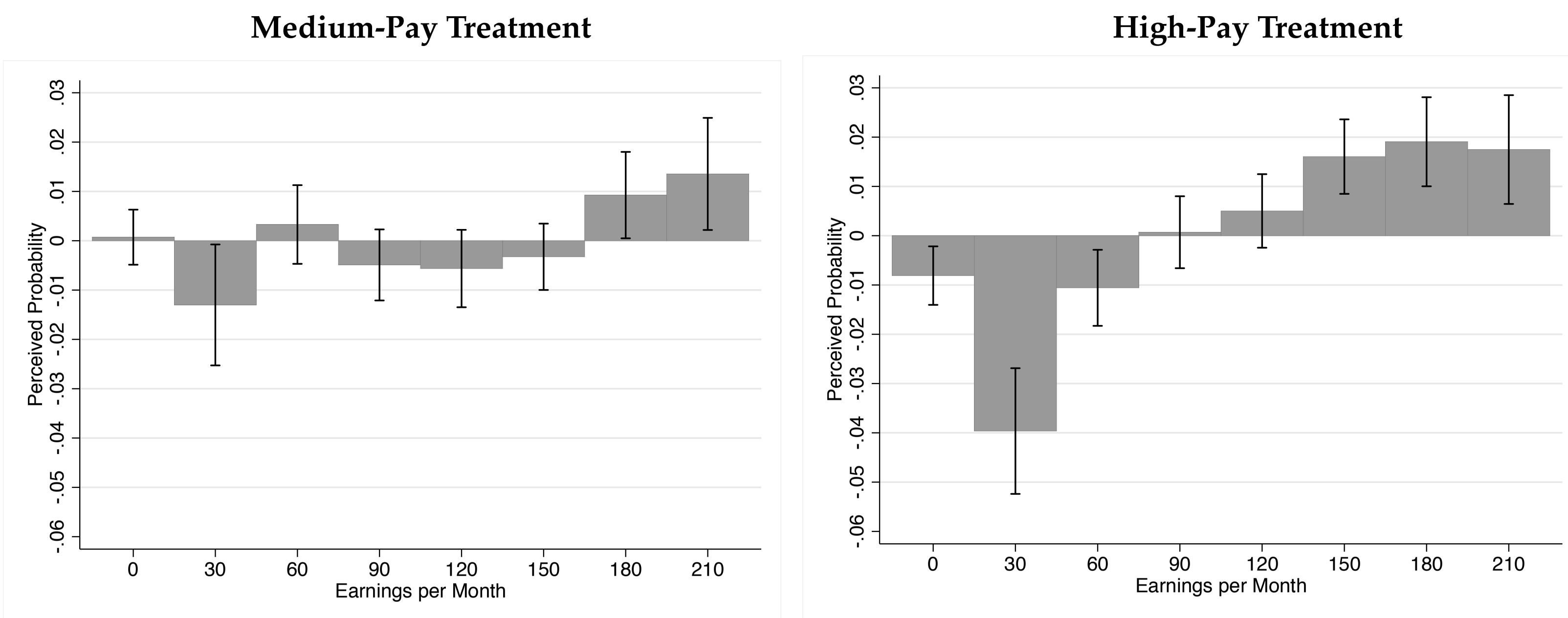
2 weeks of initial training + 1 day refresher each month

⇩

Learn about the most important health issues

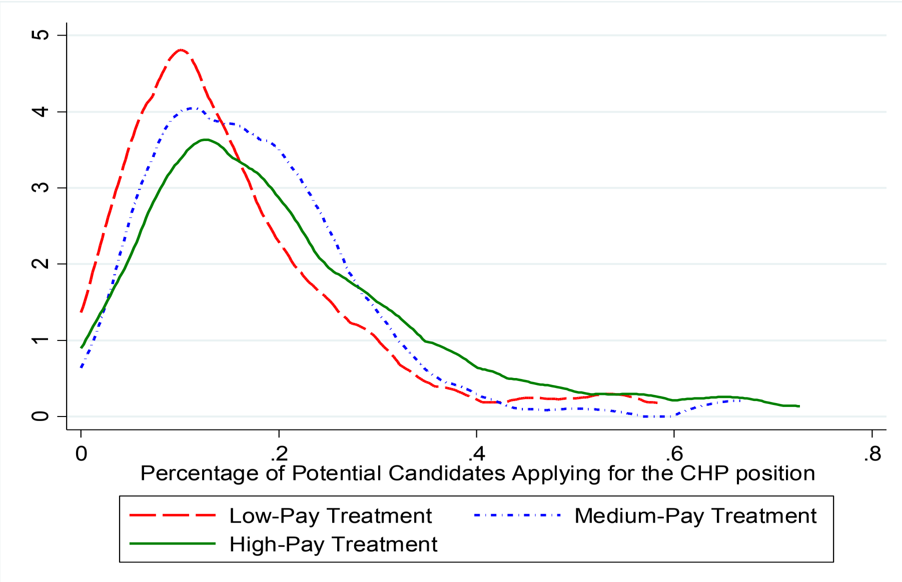
\+ Become highly trained on how to treat and prevent them

Figure II: Treatment Effects on the Expected Earnings Distribution



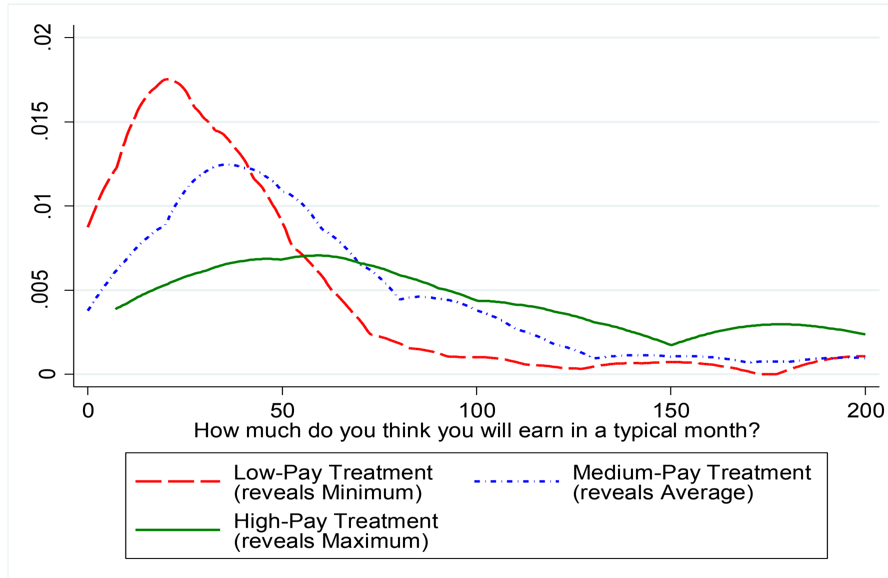
Notes: The expected earnings distribution is elicited by asking respondents to assign probabilities to the possibility that a CHP earns roughly Y per month, where Y are the values represented on the x-axis, Y={0,30,60,90,120,150,180,210} in thousands of UGX. The left panel presents differences in the perceived probability (y-axis) between the Medium-Pay and the Low-Pay Treatments. The right panel shows differences between the High-Pay and the Low-Pay Treatment. Both graphs present these differences controlling for a set of individual-level controls (number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size) and village-level fixed effects. The vertical lines represent the 90 percent confidence intervals based on standard errors clustered at the village level. Observations include all the respondents in Sample 1 of the Information Experiment.

Figure III: Kernel Density of the Size of the Application Pool



Notes: One observation per Microfinance Group (n=315). The Kolmogorov-Smirnov test for equality of distribution functions rejects equality of Low-Pay and High-Pay Treatments at the 5% level, rejects equality of the Low-Pay and Medium-Pay Treatments at the 10% level and does not reject equality of the High- and Medium-Pay Treatments. All microfinance members are potential applicants. Microfinance groups are formed by an average of 15 microfinance clients. Kernel density plot; Epanechikov Kernel with "optimal" width.

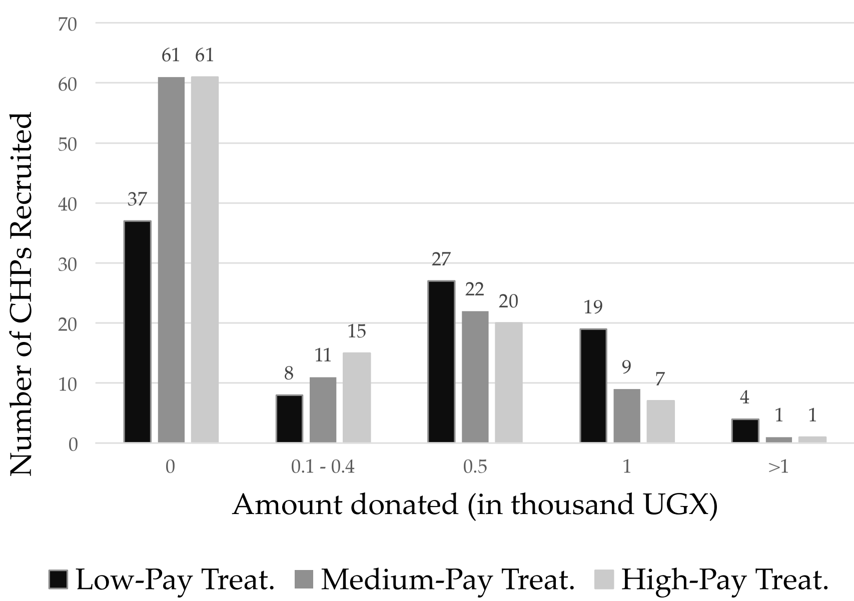
Figure IV: Kernel Density of CHP Expected Monthly Earnings



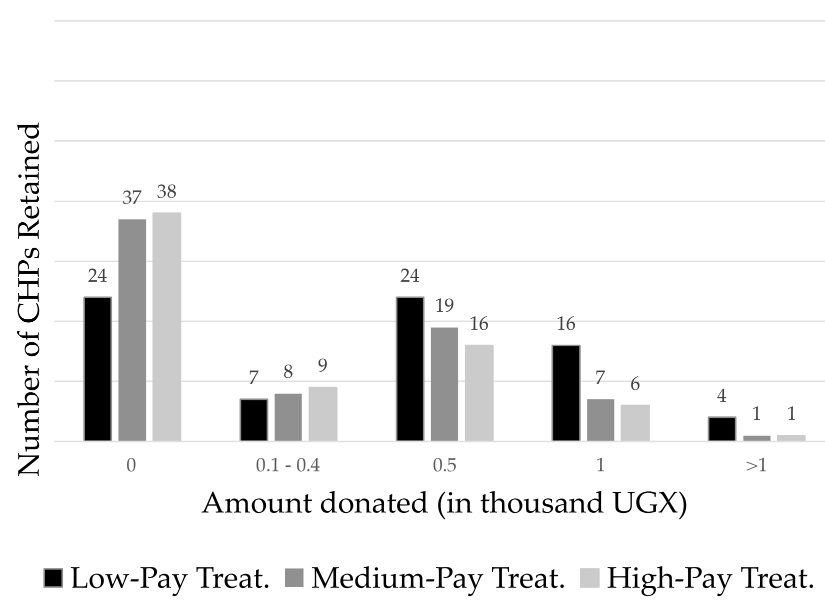
Notes: One observation per CHP (n=301). Expected monthly earnings are estimated by asking CHPs how much they "think they will earn per month from their CHP position in a typical month," before they started working. Expected monthly earnings are expressed in thousands of UGX and are truncated at the top at 5%, and at the bottom if the variable takes implausible values above 0 and under 500 UGX. Seventeen CHPs answered "I don't know" (4 in the Low-Pay, 6 in Medium-Pay, and 7 in the High-Pay Treatments), and these observations are treated as missing. The Kolmogorov-Smirnov test for equality of distribution functions rejects equality of all pairwise distributions at the 1% level. Kernel density plot; Epanechikov Kernel with "optimal" width.

Figure V: Distribution of Donations to Public Health NGO, by Treatment

Part A: Sample of CHPs initially recruited (n=301)

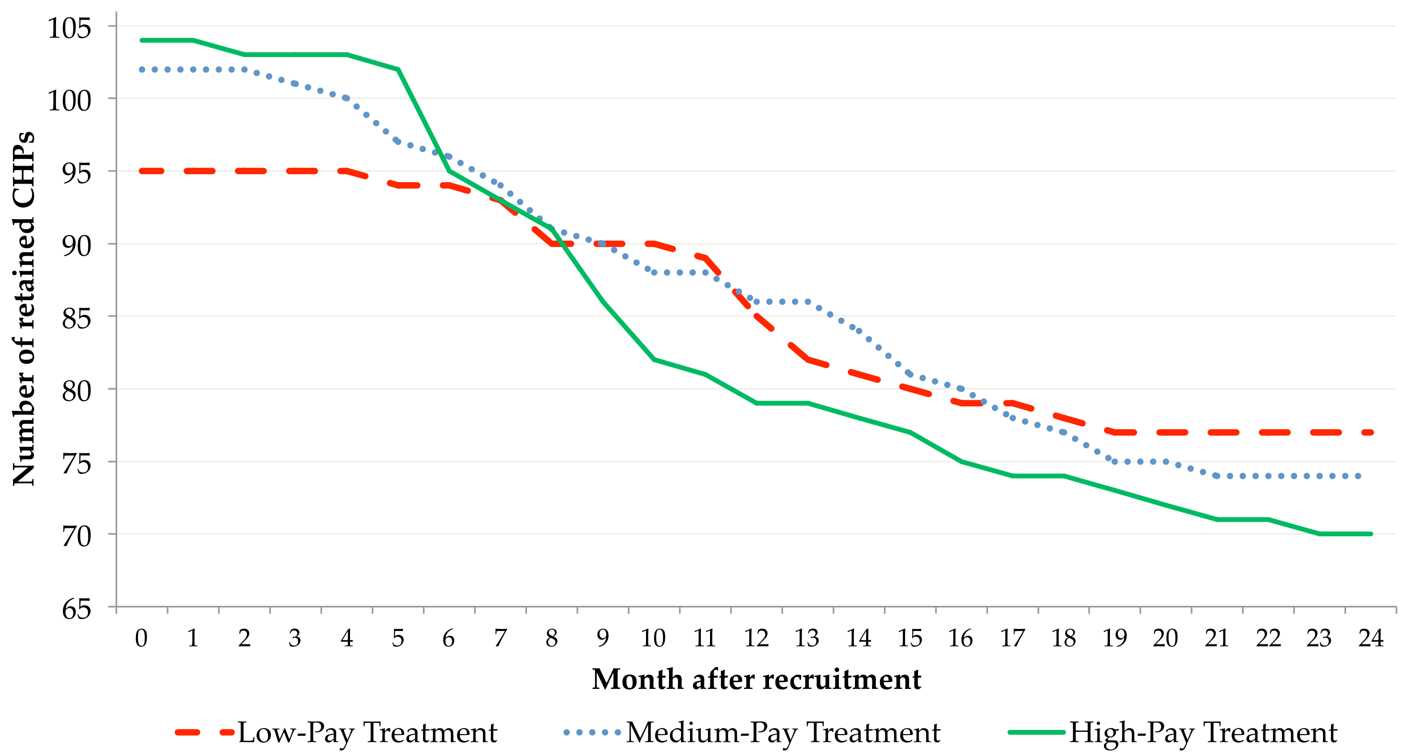


Part B: Sample of CHPs retained two years after recruitment (n=220)



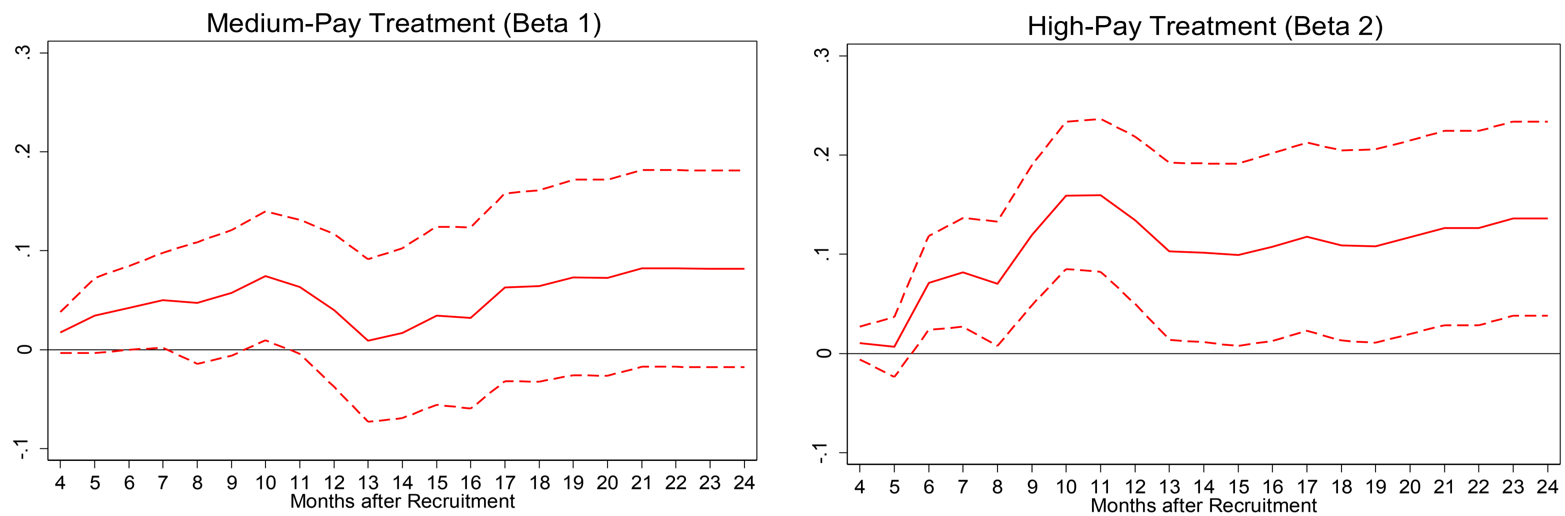
Notes: The amount donated is estimated by playing a contextualized dictator game in which the CHPs were asked, privately and confidentially, for voluntary donations to a public health NGO. Upon arriving at the training and before playing the game, the CHPs were given 3.5 thousand UGX as payment for showing up to the training.

Figure VI: Number of Workers Retained over Time, by Treatment



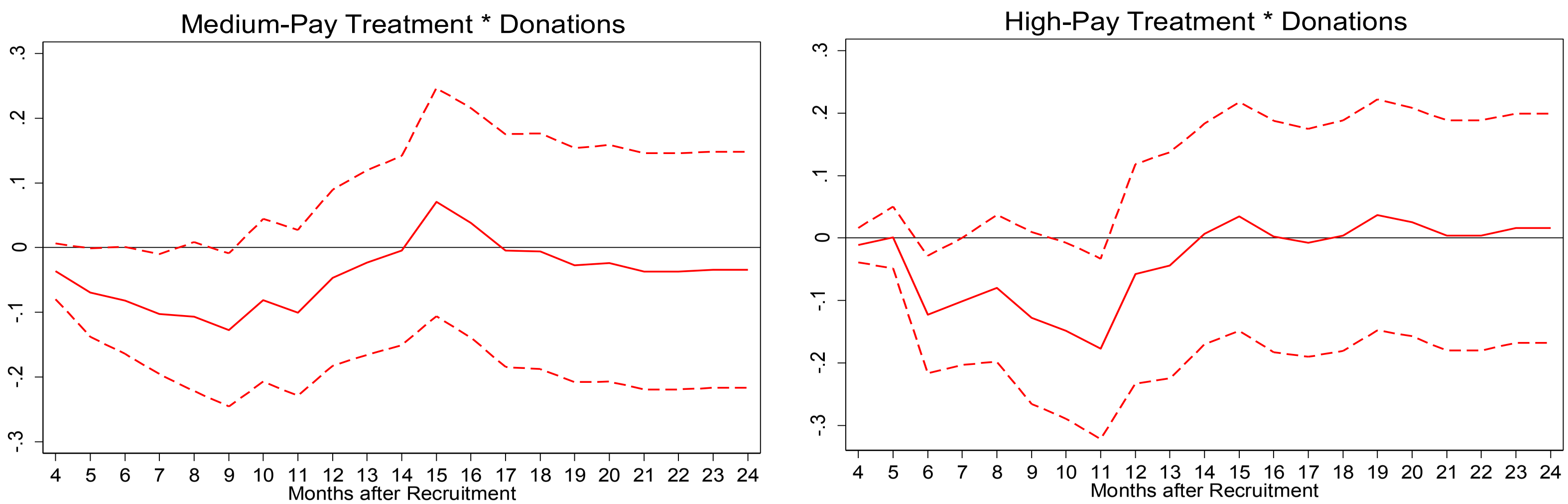
Notes: The graph represents the number of retained CHPs (those who did not drop out) over time. Month 0 is the month of the recruitment in the microfinance group. Month 1 is the month following recruitment MONTH, etc. In month 0, the figure represents the total number of recruited agents. CHPs are trained 2 months the recruitment.

Figure VII: Treatment Effects on Dropout Rate over time



Notes: The graphs present the estimated coefficients from a CHP-level regression in which the dependent variable = 1 if the CHP has dropped out (is not retained) t months after recruitment, where t={4,5… 24} is presented on the x-axis. The dependent variable is regressed on the treatment dummies (with the Low-Pay Treatment as the omitted treatment) with an OLS regression, controlling for branch fixed effects. The left panel shows the point estimates of the Medium-Pay Treatment. The right-panel shows the estimates of the High-Pay treatment. The dashed lines represent the 90 percent confidence intervals based on robust standard errors. The graph does not show data for the period prior to month 4 because the difference in retention across treatments before month 4 is very small.

Figure VIII: Differential Effect of the Treatments on the Dropout Rate over Time with Respect to Donations



Notes: The graphs present the estimated coefficients from a CHP-level regression in which the dependent variable=1 if the CHP has dropped out (is not retained) t months after recruitment, where t={4,5… 24} is presented on the x-axis. The dependent variable is regressed on the treatment dummies, donations, and the treatments dummies interacted with donations, controlling for branch fixed effects. The left panel shows the point estimates of the interaction term: Medium-Pay Treatment * Donations. The right-panel shows the estimates of High-Pay Treatment * Donations. The dashed lines represent the 90 percent confidence intervals based on robust standard errors. The graph does not show data for the period prior to month 4 because the difference in retention across treatments before month 4 is very small. Donations are estimated by playing a contextualized dictator game in which the CHPs were asked, privately and confidentially, for voluntary donations to a public health NGO.

Table I: Summary Statistics and Balance Checks of the Information Experiment

Randomization at the individual level

	All Respondents, Sample 1+2		Joint Test Sample 1 (see notes)		Joint Test Sample 2 (see notes)	
VARIABLES	**mean**	**s.d.**	**F-stat**	**p**	**F-stat**	**p**
Number of Respondents	***6844***		***3374***		***3470***	
Age	43.92	16.37	1.44	0.24	1.80	0.16
Married	0.77	0.42	0.38	0.69	0.80	0.45
Highest education level completed	5.69	3.81	0.62	0.54	2.68	0.07
Self-employed in a non-farming activity	0.19	0.40	0.20	0.82	1.59	0.20
Number of work hours per week	34.99	25.06	1.07	0.34	0.22	0.80
Number of children under age 5	0.68	0.82	4.47	0.01	1.78	0.17
Number of rooms in the house	2.87	1.16	0.89	0.41	0.28	0.76
Has ever volunteered in the health sector	0.07	0.26	1.12	0.33	0.70	0.50
The most important feature of a job is…						
"to have a positive impact on the community" (community driven)	0.30	0.46	0.36	0.70	1.48	0.23
"to earn money" (money driven)	0.59	0.49	1.06	0.35	0.31	0.73
"to earn respect from the community" (respect driven)	0.11	0.32	0.55	0.58	0.96	0.38
Has borrowed money from a microfinance institution in the last year	0.06	0.24	2.08	0.13	0.18	0.84
P-value for joint test of significance			***0.25***		***0.41***	

Notes: The first two columns report means and standard deviations. “Joint Test” reports the F-statistic and p-value from a joint test of the significance of the set of treatment dummies in explaining each variable, using robust standard errors. Sample 1 refers to the sample of respondents who answered questions on the expected earnings distribution, sample 2 refers to the sample of respondents who answered questions on expected monetary and non-monetary aspects of the job. For more details on the two samples, see details in the paper. The joint test of significance, reported at the bottom of the table, uses a multinomial logit model to test whether the covariates jointly predict treatment assignment, using robust standard errors. The null hypothesis is that all covariates together have no predictive power in predicting treatment assignment. "Highest education level completed" takes value 0 for "no education", 1 for "primary school-year 1" (...) 7 for "primary school-year 7", 8 "secondary school-year 1" (...) 13 "secondary school-year 6". "Self-employed in a non-farming activity" equals one if the main earnings activity is a non-farming self-employment activity rather than involvement in agriculture. "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect (similar definitions for "money driven" or "respect driven"). "Has borrowed money from a microfinance institution in the last year" equals one if the respondent has taken a loan from any microfinance institution in the year before the survey was administered. The resuts are robust to using errors clustered at the village level.

Table II: Summary Statistics and Balance Checks of the Recruitment Experiment

Randomization at the microfinance group (village) level

VARIABLES	# obs.	mean	s.d.	Joint Test: F-stat	Joint Test: p	Largest difference across pairs of treatments: Diff	Largest difference across pairs of treatments: p
Part 1: Village data							
Number of households in the village	315	182.95	100.48	0.75	0.47	16.05	0.22
Distance to BRAC branch office (in walking minutes)	315	41.12	40.61	0.09	0.91	-2.42	0.66
Distance to nearest public health clinic/hospital (in walking minutes)	315	50.09	54.78	0.02	0.98	-1.08	0.87
Distance to nearest drugshop (in walking minutes)	315	22.55	24.81	0.91	0.40	-4.57	0.19
Distance to nearest source of drinking water (in walking minutes)	315	19.48	22.48	0.71	0.49	3.78	0.24
Village receives radio broadcast	315	0.88	0.33	0.00	1.00	0.00	0.96
Village receives television broadcast	315	0.70	0.46	1.74	0.18	0.12	0.07
Village receives newspapers	315	0.62	0.49	0.39	0.68	-0.05	0.43
Number of BRAC microfinance clients (potential candidates for CHP position)	315	15.44	3.95	0.36	0.70	0.46	0.40
Part 2: BRAC microfinance-clients data (potential candidates for the CHP position)							
Age	4799	34.42	8.34	0.43	0.65	0.33	0.36
Married	4796	0.73	0.44	0.09	0.91	0.01	0.67
Highest education level completed	4789	5.90	3.59	1.07	0.34	0.29	0.18
Self-employed in a non-farming activity	4757	0.66	0.47	0.26	0.77	-0.03	0.51
Weekly earnings (in thousands of UGX)	4267	72.02	264.33	0.00	1.00	0.98	0.95
Number of work hours per week	4735	55.05	27.83	0.54	0.58	-2.16	0.32
Number of children under age 5	4298	1.48	1.12	0.63	0.53	0.06	0.28
Number of rooms in the house	4674	3.09	1.66	0.30	0.74	0.09	0.46
Has ever volunteered in the health sector	4863	0.08	0.27	0.19	0.83	0.01	0.57
The most important feature of a job is…							
"to have a positive impact on the community" (community driven)	4751	0.42	0.49	0.65	0.52	0.04	0.31
"to earn money" (money driven)	4751	0.37	0.48	0.68	0.51	-0.04	0.25
"to earn respect from the community" (respect driven)	4751	0.21	0.41	0.07	0.93	0.01	0.70
Owns a shop	4639	0.16	0.36	1.22	0.30	-0.03	0.17
Has ever sold health-related products	4863	0.09	0.28	0.61	0.54	0.02	0.27

Joint test of significance: p-value = 0.95

Notes: The first three columns report the number of observations, means and standard deviations. “Joint Test” reports the F-statistic and p-value from a joint test of the significance of the set of treatment dummies in explaining each variable, using robust standard errors in part 1 and standard errors clustered at the microfinance group (village) level in part 2. The last two columns report the largest difference in means and the lowest p-value from the associated t-test between pairs of treatment groups, using robust standard errors in part 1 and standard errors clustered at the microfinance group (village) level in part 2. The joint test of significance, reported at the bottom of the table, uses a multinomial logit model to test whether the covariates, aggregated at the village level, jointly predict treatment assignment, using robust standard errors. The null hypothesis is that all covariates together have no predictive power in predicting treatment assignment. "Distance to BRAC branch office" calculates the number of walking minutes from the center of the community to the closest BRAC office (similar definition for the other distance measures). "Highest education level completed" takes value 0 for "no education", 1 for "primary school-year 1" (...) 7 for "primary school-year 7", 8 "secondary school-year 1" (...) 13 "secondary school-year 6". "Self-employed in a non-farming activity" equals one if the main earnings activity is a non-farming self-employment activity rather than involvement in agriculture. "Weekly earnings" equal earnings in a typical week from all working activities, i.e., the main activity and other side activities, expressed in thousands of UGX and truncated at the top at 1%. "Number of work hours per week" is the total number of hours worked on these activities in a typical week. "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect (similar definitions for "money driven" or "respect driven"). "Has ever sold health-related products" equals one if the respondent has ever sold products such as medicine, soap, fortified oil, iodized salt, etc. The number of observations changes from one variable to another in part 2 because of the presence of missing values.

Table III: Treatment Effects on Perceived Earnings

	(1)	(2)	(3)	(4)	(5)	(6)
	Survey Questions		*Calculated from Elicited Expected Distribution of Earnings*			
VARIABLES	Expected earnings in a "typical month"	Expected earnings per hour of work in a "typical month"	Expected average earnings	Expected median earnings	Expected S.D. in earnings	Expected average earnings/ expected S.D. in earnings
Medium-Pay Treatment	5.5364**	0.2016	2.5943	2.2693	-0.7232	0.2345**
	(2.39)	(0.13)	(1.68)	(1.95)	(0.77)	(0.11)
High-Pay Treatment	33.2167***	0.7658***	8.3172***	9.8436***	0.4867	0.3127***
	(3.11)	(0.15)	(1.70)	(1.88)	(0.65)	(0.12)
Mean dep var in Low-Pay Treatment	87.886	2.575	116.852	114.404	47.648	2.789
Obs. (Info Experiment)	3,031	2,717	2,715	2,715	2,715	2,715
R-squared	0.354	0.387	0.406	0.407	0.365	0.238
p-value Med=High	0.000	0.000	0.001	0.000	0.088	0.528
p-value Low=Med=High	0.000	0.000	0.000	0.000	0.230	0.014

Notes: OLS estimates. Errors clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1. All regressions include village fixed effects and control for number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size. Results are robust to not adding the controls. All variables are expressed in thousands of UGX and are truncated at the top at 1%. The results remain consistent if I do not truncate the variables. "Expected earnings in a typical month" asks the respondents in Sample 2 how much they believe a CHP earns "in a typical month" (similar for "expected earnings per hour of work"). The variables in the last 4 columns are calculated from the elicited distribution of expected earnings from each respondent of Sample 1. Differences in the number of observations are explained by the presence of two separate samples (Samples 1 and 2) with different samples size and and by missing values in the dependent variables.

Table IV: Treatment Effects on other Perceived Job Attributes

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Job is perceived as a "private goal" (CHPs do the job for the money) more than a "social goal" (CHPs do the job to improve health conditions)	Perceived proportion of time allocated to sales (vs. delivery of health services)	Expected number of work hours in a "typical week"	Perceived difficulty in selling products to community	Perceived difficulty in improving people's health behavior	Own perceived ability
Medium-Pay Treatment	-0.0033	0.0027	-0.0638	-0.0285	-0.0315	0.0286
	(0.02)	(0.01)	(0.34)	(0.04)	(0.05)	(0.13)
High-Pay Treatment	0.0692***	0.0450***	0.1665	0.0008	-0.0236	0.1144
	(0.02)	(0.01)	(0.35)	(0.04)	(0.05)	(0.14)
Mean dep var in Low-Pay Treatment	0.403	0.461	14.081	1.827	2.536	6.004
Obs. (Info Experiment)	3,067	3,014	2,769	3,055	3,056	2,901
R-squared	0.282	0.293	0.384	0.266	0.217	0.259
p-value Med=High	0.002	0.000	0.544	0.444	0.867	0.483
p-value Low=Med=High	0.004	0.000	0.822	0.698	0.789	0.673

Notes: OLS estimates. Errors clustered at the village level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. All regressions include village fixed effects and control for number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size. Results are robust to not adding the controls. Outcomes variables on perceived difficulty (columns 4 and 5) are estimated on a scale of 1 to 4, with 1=very easy and 4=very difficult. Perceived ability is calculated by asking agents to rank themselves on a scale of 1 to 10, where 1 means "If 10 women were recruited I would be ranked the last (number 10) in terms of performance" and 10 means "I would be ranked the best (number 1)." Differences in the number of observations are explained by missing values in the dependent variables.

Table V: Treatment Effects on Applicants' Traits

Sample: Potential candidates (BRAC microfinance clients)

Dependent variable =1 if the potential candidate applies for the CHP position, =0 if the potential candidate does not apply

	(1)	(2)	(3)	(4)
	Prosocial preferences		*Interest in sales*	
TRAIT →	Has ever volunteered in the health sector	Community driven	Owns a shop	Has ever sold health-related products
TRAIT	0.2812***	0.0865***	0.0271	0.0503
	(0.05)	(0.02)	(0.03)	(0.04)
TRAIT * Medium-Pay Treatment	-0.0017	-0.0441	0.0233	0.1156*
	(0.06)	(0.03)	(0.04)	(0.06)
TRAIT * High-Pay Treatment	-0.1621**	-0.0882***	0.0566	0.1587**
	(0.06)	(0.03)	(0.05)	(0.06)
Mean of TRAIT in Low-Pay Treatment	0.079	0.427	0.166	0.081
Observations (Potential Candidate)	4,330	4,252	4,150	4,330
R-squared	0.229	0.210	0.203	0.215
*p-value Trait*Med=Trait*High*	0.013	0.204	0.488	0.519
*p-value Trait*Low=Trait*Med=Trait*High*	0.017	0.025	0.483	0.027

Notes: OLS estimates. Errors clustered at the microfinance-group level. *** p<0.01, ** p<0.05, * p<0.1. Whether a potential applicant (microfinance client) applies for the position is regressed on different "TRAITS" interacted with the treatments. All regressions include microfinance-group fixed effects and control for number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size. Results are robust to not adding the controls. "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect (similar definitions for "money driven" or "respect driven"). "Has ever sold health-related products" equals one if the respondent has ever sold products such as medicine, soap, fortified oil, iodized salt, etc. Number of observations varies from one column to the next due to missing values in dependent variables.

Table VI: Treatment Effects on Crowding Out and Crowding In of Applicants' Traits

Sample: Potential candidates (BRAC microfinance clients)

Dependent variable =1 if the potential candidate applies for the CHP position, =0 if the potential candidate does not apply

	(1)	(2)	(3)	(4)
	Prosocial preferences		***Interest in sales***	
TRAIT =1 if potential candidate… →	Has ever volunteered in the health sector	Is community driven	Owns a shop	Has ever sold health-related products
1(TRAIT=1)	0.2749***	0.0703***	0.0302	0.0834**
	(0.04)	(0.02)	(0.03)	(0.04)
1(TRAIT=1) * Medium-Pay Treatment	-0.0121	-0.0073	0.0188	0.0710
	(0.06)	(0.02)	(0.04)	(0.06)
1(TRAIT=0) * Medium-Pay Treatment	0.0197	0.0326*	0.0167	0.0114
	(0.01)	(0.02)	(0.02)	(0.01)
1(TRAIT=1) * High-Pay Treatment	-0.1078*	-0.0139	0.0738*	0.1318**
	(0.06)	(0.02)	(0.04)	(0.06)
1(TRAIT=0) * High-Pay Treatment	0.0550***	0.0768***	0.0320*	0.0303*
	(0.02)	(0.02)	(0.02)	(0.02)
Mean dep var for 1(TRAIT=0) in Low-Pay T.	0.028	0.195	0.061	0.029
Observations (Potential Candidate)	4,330	4,252	4,150	4,330
R-squared	0.135	0.115	0.109	0.121
*p-value Trait1*Med=Trait0*Med*	0.586	0.776	0.191	0.305
*p-value Trait1*High=Trait0*High*	0.007	0.002	0.369	0.090
*p-value Trait1*Med=Trait1*High*	0.106	0.158	0.955	0.319
*p-value Trait0*Med=Trait0*High*	0.032	0.033	0.331	0.268

Notes: OLS estimates. Errors clustered at the microfinance-group level. *** p<0.01, ** p<0.05, * p<0.1. Regressions control for branch fixed effects and for number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size. "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect (similar definitions for "money driven" or "respect driven"). "Has ever sold health-related products" equals one if the respondent has ever sold products such as medicine, soap, fortified oil, iodized salt, etc.

Table VII: Treatment Effects on the Size of the Applicant Pool

Sample: Microfinance Groups

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Number of applicants			=1 if at least one applicant		
Medium-Pay Treatment	0.4170**	0.4087**	0.4312**	0.0374	0.0335	0.0323
	(0.204)	(0.205)	(0.207)	(0.028)	(0.027)	(0.028)
High-Pay Treatment	0.7620***	0.7235***	0.7808***	0.0477*	0.0504*	0.0524*
	(0.227)	(0.226)	(0.231)	(0.026)	(0.027)	(0.027)
Number of potential candidates	0.0722***	0.0735***	0.0835***	0.0023	-0.0005	0.0005
	(0.021)	(0.024)	(0.025)	(0.003)	(0.003)	(0.003)
Microfinance-group level controls	No	Yes	Yes	No	Yes	Yes
Village-level controls	No	No	Yes	No	No	Yes
Mean of dep var in Low-Pay Treatment	2.219	2.219	2.219	0.933	0.933	0.933
Observations (Microfinance Groups)	315	315	315	315	315	315
R-squared	0.222	0.233	0.251	0.152	0.180	0.190
p-value Med=High	0.131	0.163	0.115	0.624	0.437	0.349
p-value Low=Med=High	0.003	0.006	0.003	0.188	0.166	0.141

Notes: OLS estimates. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Applicants are microfinance members who applied for the CHP position. All regressions include branch fixed effects. Microfinance group level controls include the number of potential candidates who have volunteered in the health center, the number of candidates who are community driven, the number who own a shop, and the number who have ever sold health-related products. The village-level controls include number of households in the village, distance to BRAC branch office, distance to nearest public health clinic/hospital, distance to nearest drugshop, distance to nearest source of drinking water, village receives radio broadcast, village receives television broadcast, village receives newspapers.

Table VIII: Treatment Effects on Traits of the Recruited Workers

Sample: Community Health Promoters (CHPs)

	Prosocial Preferences					*Interest in Sales*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Amount donated (dictator game)	=1 if donated more than 0 (50th percentile)	=1 if donated more than .5 (85th percentile)	Has ever volunteered in the health sector	Community driven	Owns a shop	Has ever sold health-related products
Medium-Pay Treatment	-0.2399***	-0.2152***	-0.2159***	-0.0212	-0.1702**	-0.0613	-0.0007
	(0.07)	(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.06)
High-Pay Treatment	-0.2573***	-0.2125***	-0.2623***	-0.1108*	-0.2677***	0.0584	0.0330
	(0.07)	(0.06)	(0.06)	(0.07)	(0.07)	(0.06)	(0.06)
Mean of dep var in Low-Pay Treatment	0.463	0.611	0.526	0.347	0.695	0.283	0.221
Observations (CHPs)	301	301	301	301	297	293	301
R-squared	0.255	0.309	0.312	0.046	0.174	0.143	0.156
p-value Med=High	0.703	0.965	0.406	0.151	0.146	0.045	0.546
p-value Low=Med=High	0.001	0.000	0.000	0.178	0.000	0.133	0.793

Notes: OLS estimates. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Regressions include branch fixed effects. Donations are estimated by playing a contextualized dictator game in which the CHPs were asked, privately and confidentially, for voluntary donations to a public health NGO. Amounts donated are expressed in throusands of UGX. "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect (similar definitions for "money driven" or "respect driven"). "Has ever sold health-related products" equals one if the respondent has ever sold products such as medicine, soap, fortified oil, iodized salt, etc. Results are robust to controlling for number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size. I do not include these controls because of the existence of missing values and this reduces the sample below 301.

Table IX: Treatment Effects on Dropping Out

Sample: Community Health Promoters (CHPs)

Dependent Variable =1 if the CHP has dropped out within two years of recruitment, =0 if CHP is retained

	(1)	(2)	(3)
Medium-Pay Treatment	0.0818	0.0391	0.0827
	(0.06)	(0.06)	(0.07)
High-Pay Treatment	0.1359**	0.0901	0.0953
	(0.06)	(0.06)	(0.07)
Amount donated (dictator game)		-0.1782***	-0.1353**
		(0.05)	(0.06)
Has ever volunteered in the health sector			-0.1299**
			(0.06)
Community driven			0.0460
			(0.06)
Owns a shop			0.0610
			(0.07)
Has ever sold health-related products			-0.2300***
			(0.07)
Controls	No	No	Yes
Mean of dep var in Low-Pay Treatment	0.189	0.189	0.189
Observations (CHPs)	301	301	271
R-squared	0.131	0.157	0.258
p-value Med=High	0.381	0.405	0.850
p-value Low=Med=High	0.071	0.332	0.288

Notes: OLS estimates. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include branch fixed effects. Column 3 additionally includes individual controls (number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size) and village-level controls (see list in Table II). "Amount donated" is estimated by playing a contextualized dictator game in which the CHPs were asked, privately and confidentially, for voluntary donations to a public health NGO. "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect. "Has ever sold health-related products" equals one if the respondent has ever sold products such as medicine, soap, fortified oil, iodized salt, etc.

Table X: Treatment Effects on Performance–BRAC Monthly Records

Sample: Community Health Promoters (CHPs)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Monthly sales profits			Number of households visited per month			Number of pre- and postnatal checks			Score given by BRAC to CHP overall performance (out of 10)		
Medium-Pay Treatment	-3.2843	-1.2695	-1.4013	-2.4506	0.6103	-1.1416	-0.8090	-0.1435	-0.2749	-0.6288*	-0.4406	-0.3312
	(2.53)	(2.49)	(2.85)	(3.51)	(3.56)	(3.65)	(0.66)	(0.66)	(0.70)	(0.34)	(0.34)	(0.39)
High-Pay Treatment	-4.8243	-2.6637	-2.9253	-8.0721**	-4.7896	-4.0527	-1.4528**	-0.7391	-0.4547	-0.8210**	-0.6228*	-0.6251
	(2.97)	(2.97)	(3.04)	(3.55)	(3.53)	(3.65)	(0.72)	(0.74)	(0.74)	(0.35)	(0.36)	(0.40)
Amount donated		8.3973***	8.4848***		12.7578***	10.9888***		2.7739***	3.1343***		0.7836**	0.9804**
		(2.79)	(2.91)		(3.69)	(4.02)		(0.69)	(0.70)		(0.31)	(0.39)
Has ever volunteered in the health sector			4.0673			4.0498			1.6478**			0.4169
			(2.63)			(3.47)			(0.71)			(0.35)
Community driven			-3.0813			-1.0483			-0.6795			-0.3345
			(2.68)			(3.44)			(0.68)			(0.36)
Owns a shop			4.0161			6.3491			-0.0151			-0.0268
			(4.10)			(4.19)			(0.75)			(0.42)
Has ever sold health-related products			0.8803			5.1064			1.2062			0.8421*
			(4.32)			(3.89)			(0.91)			(0.43)
Controls	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Mean in Low-Pay Treatment	38.396	38.396	38.396	52.807	52.807	52.807	9.801	9.801	9.801	6.758	6.758	6.758
Observations (CHPs)	301	301	271	301	301	271	301	301	271	297	297	268
R-squared	0.427	0.444	0.496	0.484	0.504	0.575	0.255	0.295	0.371	0.170	0.184	0.277
p-value Med=High	0.597	0.630	0.619	0.160	0.169	0.466	0.361	0.390	0.809	0.606	0.622	0.457
p-value Low=Med=High	0.220	0.665	0.630	0.076	0.300	0.535	0.130	0.573	0.823	0.042	0.188	0.293

Notes: OLS estimates. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include branch fixed effects. Column 3 of each outcome variable for individual controls (number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size) and village-level controls (see list in Table I). The first three outcome variables represent the average monthly value of different performance measures collected by BRAC in the period May 2013-April 2014. The "score given from BRAC to CHP overall performance" is a score out of 10 that each CHP supervisor gave to the CHP, considering all the performance dimensions, one year after the CHP recruitment. Monthly sales profits are expressed in thousands of UGX. "Amount donated" is estimated by playing a contextualized dictator game in which the CHPs were asked, privately and confidentially, for voluntary donations to a public health NGO. "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect. "Has ever sold health-related products" equals one if the respondent has ever sold products such as medicine, soap, fortified oil, iodized salt, etc.

Table XI: Treatment Effects on Household Targeting–Household Data

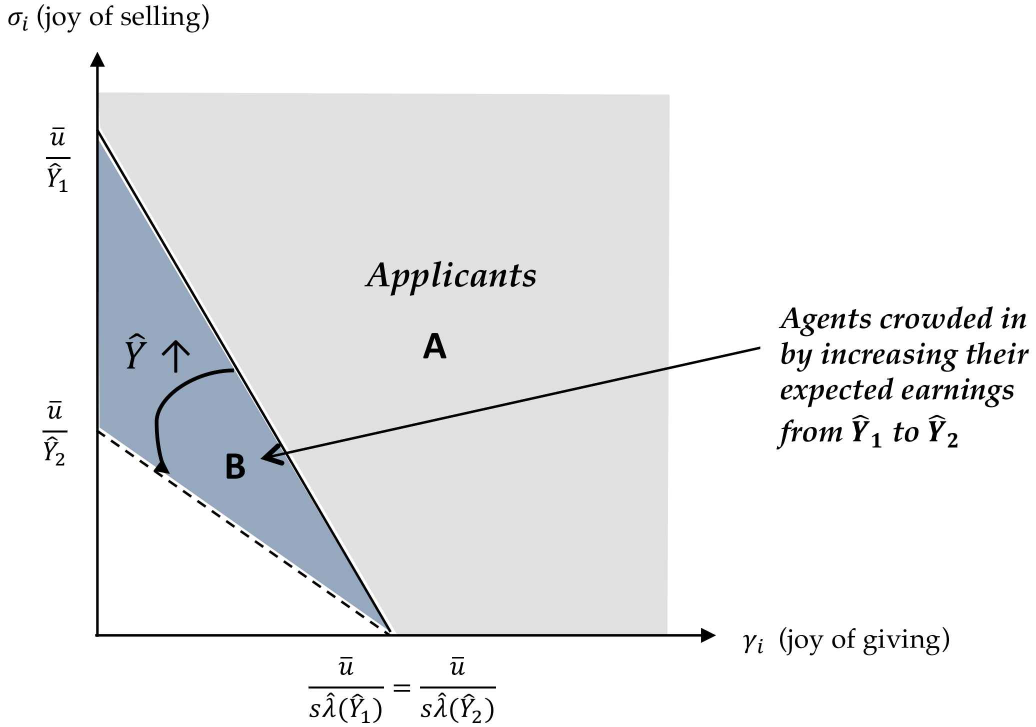
Sample: Households (CHPs' clients)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Household knows who the CHP is		Household has previously been visited by the CHP		Number of times household was visited by the CHP in the last month	
Medium-Pay Treatment	0.0312	0.0347	0.0506	0.0599*	0.1205	0.0829
	(0.03)	(0.03)	(0.03)	(0.03)	(0.15)	(0.18)
High-Pay Treatment	0.0236	0.0434	0.0395	0.0713**	-0.0909	-0.0871
	(0.03)	(0.03)	(0.03)	(0.03)	(0.11)	(0.13)
Priority Household		0.0809**		0.1051**		0.0648
		(0.04)		(0.05)		(0.17)
Priority Household * Medium-Pay Treatment		-0.0119		-0.0420		0.1619
		(0.05)		(0.06)		(0.34)
Priority Household * High-Pay Treatment		-0.1012*		-0.1551***		-0.1365
		(0.05)		(0.06)		(0.21)
Mean in Low-Pay Treatment for "Non-Priority"	0.611	0.612	0.538	0.538	0.995	0.985
% Households who are Priority	21.25	21.25	21.25	21.25	21.25	21.25
Observations (Households)	2,574	2,542	2,541	2,509	2,507	2,476
R-squared	0.323	0.328	0.277	0.282	0.128	0.131
p-value Low=Med=High	0.606	0.405	0.261	0.078	0.317	0.550
*p-value Priority*Low=Priority*Med =Priority*High*		0.108		0.019		0.579
p-value (Prioriy=Non-Priority) in Medium-Pay T.		0.508		0.951		0.726
p-value (Prioriy=Non-Priority) in High-Pay T.		0.593		0.869		0.347

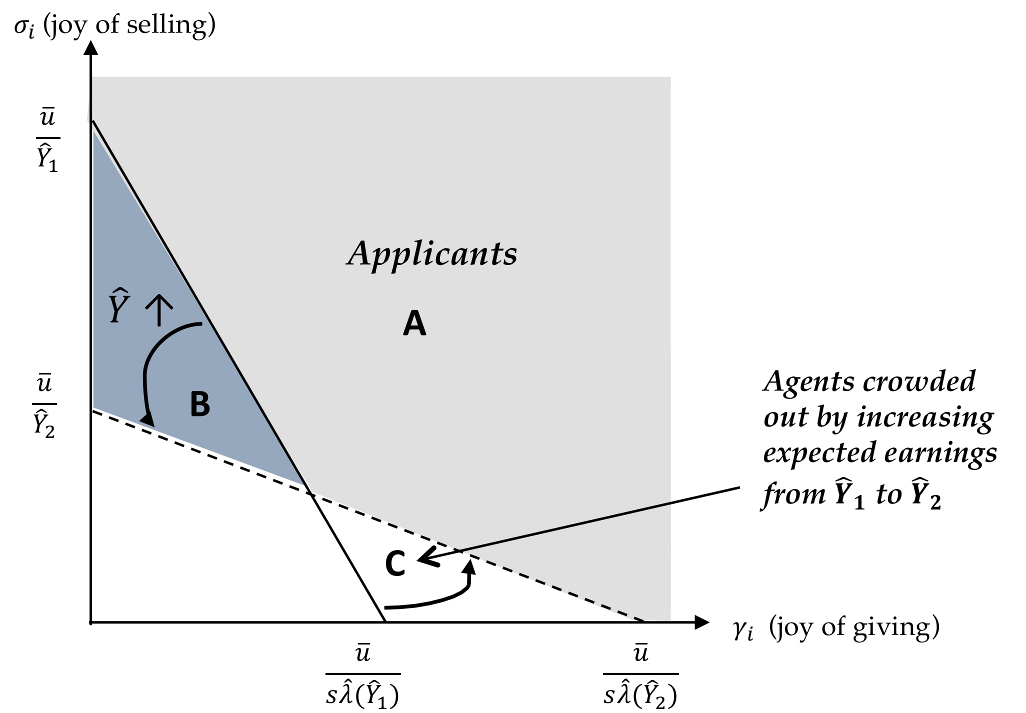
Notes: OLS estimates. Errors clustered at the village/CHP level. *** p<0.01, ** p<0.05, * p<0.1. All regressions include branch fixed effects and household-level controls (distance to CHP house, being a BRAC microfinance member, number of children under 5 years old, number of females of reproductive age, gender of the respondent). Priority Household equals 1 if anyone in the household was pregnant or delivered within 6 months of the survey. A household knows who the CHP is if the correct name is given to the question "who is the CHP serving this village?"

Figure A.I(a): The Effect of Increasing Expected Earnings on the Applicant Pool

No Inference Scenario: $\hat{\lambda}'(\hat{Y}) = 0$



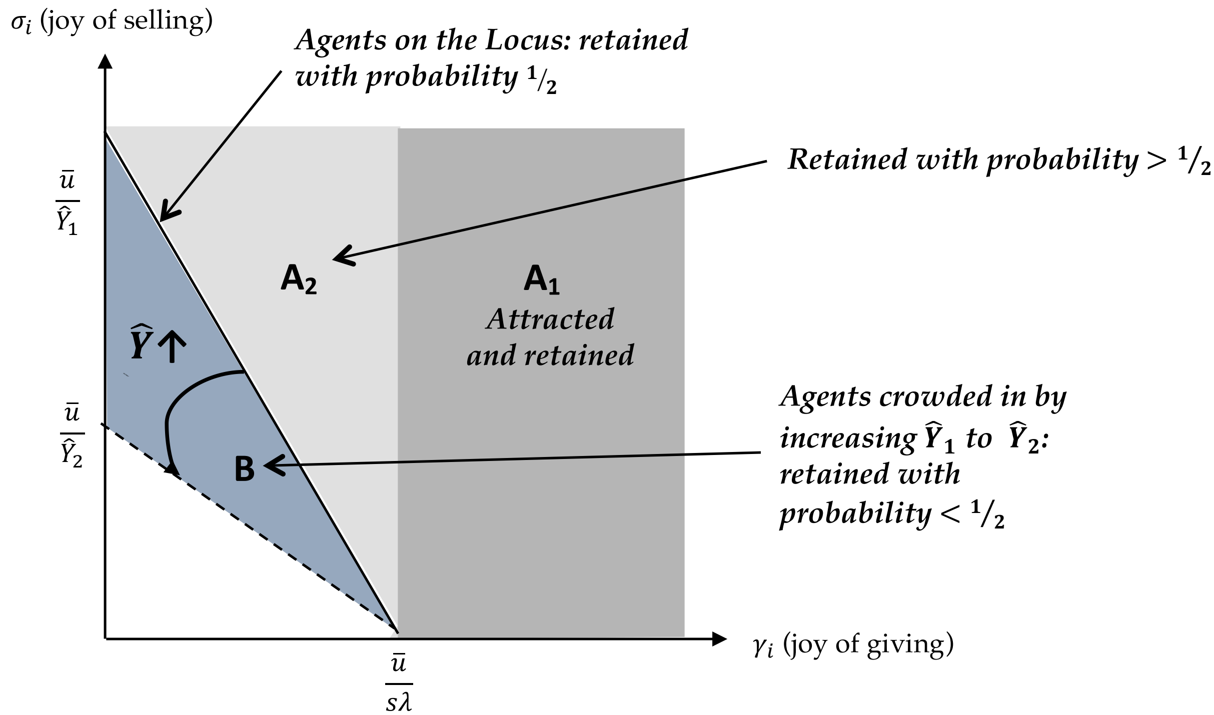
Inference Scenario: $\hat{\lambda}'(\hat{Y}) < 0$



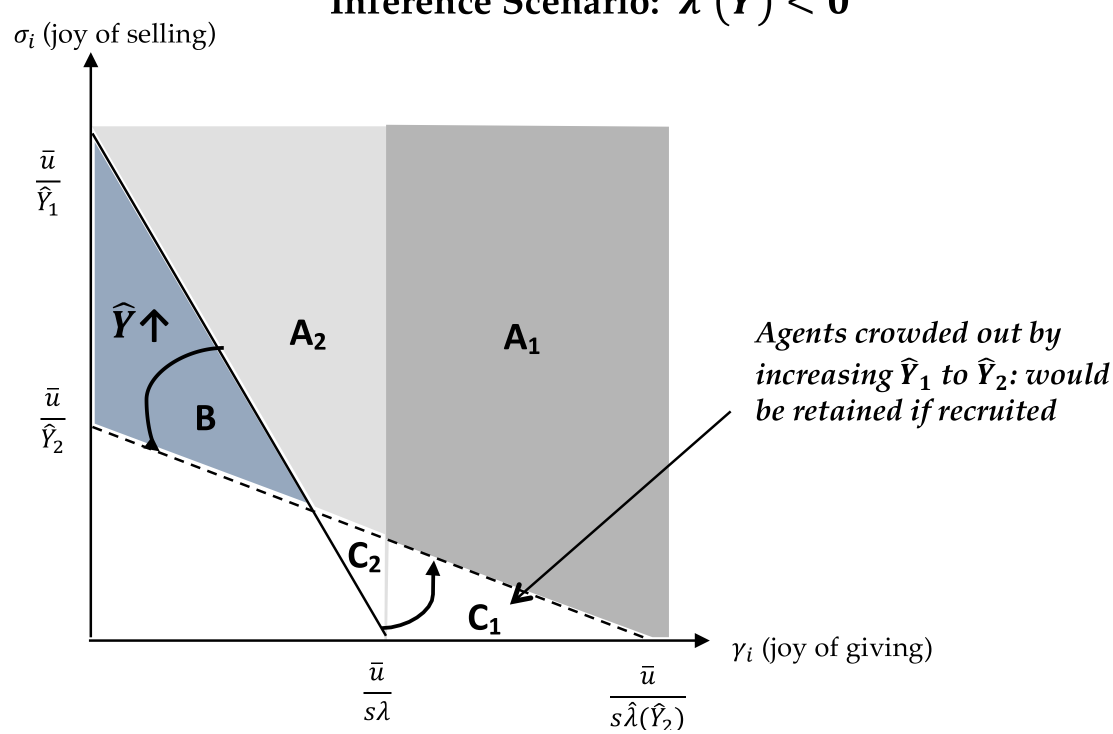
Notes: Any point in the figures corresponds to an agent with identity (σ_i, γ_i). The line in each figure represents the separation locus: all agents above or on the line apply for the position while all those below the line do not apply. In the bottom graph, the crossing point between the two separation lines is arbitrary and depends on the intensity of the signal $|\hat{\lambda}'(\hat{Y})|$.

Figure A.I(b): The Effect of Increasing Expected Earnings on the Pool of Retained Workers

No Inference Scenario: $\hat{\lambda}'(\hat{Y}) = 0$



Inference Scenario: $\hat{\lambda}'(\hat{Y}) < 0$



Notes: I assume in this graph that $\hat{Y}_1$ is the level of expected earnings, which, in expectation, equals the average realized earnings: $\hat{Y}_1 = \mu(1-\lambda)$ and that $\hat{\lambda}(\hat{Y}_1) = \lambda$. See notes in Figure A.I(a) for more details.

Table A.I: Heterogeneous Treatment Effects on Perceived Job Attributes

VARIABLES	Expected earnings in a "typical month"				Job perceived as "private goal" (vs. "social goal")[++]			
TRAIT →	Highest education level completed	Has ever volunteered in the health sector	Community driven	Borrowed money from a microfinance institution in the last year	Highest education level completed	Has ever volunteered in the health sector	Community driven	Borrowed money from a microfinance institution in the last year
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
High-Pay Treatment	3.5090	5.5269**	4.7773*	5.2327**	-0.0348	-0.0082	-0.0007	-0.0013
	(4.15)	(2.62)	(2.76)	(2.47)	(0.03)	(0.02)	(0.02)	(0.02)
Medium-Pay Treatment	30.3289***	33.4630***	27.5221***	33.0722***	0.0416	0.0654***	0.0852***	0.0674***
	(4.66)	(3.27)	(3.39)	(3.19)	(0.04)	(0.02)	(0.03)	(0.02)
TRAIT	0.4820	-7.2974	-21.2353***	4.9750	-0.0117***	-0.2073***	-0.0585	-0.0326
	(0.59)	(6.49)	(5.04)	(7.86)	(0.00)	(0.06)	(0.04)	(0.06)
TRAIT* Medium-Pay Treat.	0.3608	-2.8752	2.0511	2.9160	0.0055	0.0064	-0.0112	-0.0199
	(0.61)	(8.95)	(5.08)	(10.43)	(0.01)	(0.07)	(0.04)	(0.08)
TRAIT* High-Pay Treat.	0.5169	-5.1444	18.3049***	2.6054	0.0049	0.0129	-0.0548	0.0329
	(0.71)	(9.70)	(6.53)	(10.37)	(0.01)	(0.07)	(0.05)	(0.09)
Mean in Low-Pay Treatment	87.886	87.886	87.886	87.886	0.403	0.403	0.403	0.403
Obs. (Info Experiment)	3,031	3,031	3,031	3,031	3,067	3,067	3,067	3,067
R-squared	0.354	0.355	0.367	0.354	0.282	0.291	0.287	0.282
p-value Med=High	0.824	0.809	0.017	0.978	0.906	0.927	0.356	0.494
p-value Med=High=Min	0.734	0.867	0.018	0.951	0.517	0.984	0.509	0.790

[++] Job is perceived as "private goal" if respondents believe CHPs do the job to earn money and "social goal" if they believe CHPs do it to improve health conditions in the community.

Notes: OLS regression using Sample 2 of the Information Experiment. Errors clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1. Regressions control for village fixed effects and for number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size. Expected earnings are expressed in thousands of UGX and are measured by asking the respondents how much they think a CHP earns in a typical month. The earnings variables are truncated at the top at 1%. The results remain consistent if I do not truncate the variable. "Highest education level completed" takes value 0 for "no education", 1 for "primary school-year 1" (...) 7 for "primary school-year 7", 8 "secondary school-year 1" (...) 13 "secondary school-year 6". "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect. "Has borrowed money from a microfinance institution in the last year" equals one if the respondent has taken a loan from any microfinance institution in the year before the survey was administered.

Table A.II: Treatment Effects on Perceived Attributes of the Job−Matched Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Expected earnings in a "typical month"	Expected Earnings per hour of work in a "typical month"	Expected average earnings	Expected median earnings	Expected S.D. in earnings	Expected average earnings/ expected S.D. in earnings	Job is perceived as "private goal" (vs. "social-goal")[++]	Perceived % of time allocated to sales (vs. delivery of health services)	Expected number of work hours in a "typical week"	Perceived difficulty in selling products to community	Perceived difficulty in improving people's health behavior	Own perceived ability
Medium-Pay Treatment	8.6301*	0.2112	3.7350	3.8629	-2.1052	0.4309**	0.0037	0.0305*	-0.7633	-0.0228	0.0118	0.5287**
	(4.43)	(0.21)	(2.90)	(3.46)	(1.33)	(0.21)	(0.04)	(0.02)	(0.61)	(0.08)	(0.09)	(0.26)
High-Pay Treatment	47.0776***	0.9878***	9.3365***	10.8733***	1.0256	0.4087*	0.1321***	0.0641***	0.3998	0.0280	0.0161	0.1237
	(4.93)	(0.23)	(3.44)	(4.01)	(1.37)	(0.23)	(0.04)	(0.02)	(0.68)	(0.07)	(0.09)	(0.28)
Mean in Low-Pay Treat.	87.886	2.575	116.852	114.404	47.648	2.789	0.403	0.461	14.081	1.827	2.536	6.004
Obs. (Info Experiment)	2,242	2,033	2,013	2,013	2,013	2,013	2,264	2,244	2,068	2,265	2,267	2,144
R-squared	0.460	0.472	0.546	0.528	0.476	0.386	0.374	0.417	0.460	0.395	0.307	0.410
p-value Med=High	0.000	0.001	0.092	0.096	0.016	0.941	0.002	0.022	0.099	0.482	0.964	0.046
p-value Med=High=Min	0.000	0.000	0.027	0.027	0.046	0.031	0.001	0.001	0.225	0.777	0.982	0.044

[++] Job is perceived as "private goal" if respondents believe CHPs do the job to earn money, and "social goal" if they believe CHPs do it to improve health conditions in the community.

Notes: Weighted OLS regression where weights are determined by a matching approach: respondents from the Recruitment Experiment are paired with their 5 nearest-neighbors in the Information Experiment (with replacement) based on number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size. Errors clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1. All regression include village fixed effects. Expected earnings are expressed in thousands of UGX and are measured by asking the respondents how much they think a CHP earns and how many hours she works in a typical month. The earnings variables are truncated at the top at 1%. The results remain consistent if I do not truncate the variable. Outcomes variables on perceived difficulty are estimated on a scale of 1 to 4 with 1=very easy and 4=very difficult. Perceived ability is calculated by asking agents to rank themselves on a scale of 1 to 10, where 1 means "if 10 women were recruited, I would be ranked last (number 10) in terms of performance" and 10 means "I would be ranked first (number 1)."

Table A.III: Treatment Effects on Other Applicants' Traits

Sample: Potential Candidates

Dependent variable =1 if the potential candidate applies for the CHP position, =0 if the potential candidate does not apply

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
TRAIT →	Money driven	Respect driven	Highest education level completed	Self-employed in a non-farming activity	Number of work hours per week	Earnings per hour worked	Number of rooms in the house	Number of children under 5 years old	Degree of risk aversion
TRAIT	-0.0783***	-0.0246	0.0344***	0.0136	0.0000	0.0017	0.0200***	0.0057	-0.0465
	(0.02)	(0.03)	(0.00)	(0.02)	(0.00)	(0.00)	(0.01)	(0.01)	(0.05)
TRAIT * Medium-Pay Treatment	0.0681**	-0.0232	0.0007	-0.0540	0.0000	-0.0021	-0.0016	-0.0114	0.1039
	(0.03)	(0.04)	(0.00)	(0.04)	(0.00)	(0.00)	(0.01)	(0.01)	(0.07)
TRAIT * High-Pay Treatment	0.0843**	0.0189	0.0020	0.0309	0.0002	-0.0026	-0.0061	-0.0209	0.0380
	(0.03)	(0.04)	(0.00)	(0.04)	(0.00)	(0.00)	(0.01)	(0.01)	(0.07)
Mean of TRAIT in Low-Pay Treatment	0.365	0.208	5.792	0.670	54.699	1.624	3.110	1.447	0.301
Observations (Potential Candidate)	4,252	4,252	4,330	4,330	4,330	3,850	4,330	3,857	4,061
R-squared	0.209	0.207	0.203	0.203	0.202	0.210	0.203	0.214	0.208
*p-value Trait*Low=Trait*Med*	0.658	0.289	0.789	0.051	0.822	0.854	0.639	0.483	0.322
*p-value Trait*Low=Trait*Med=Trait*High*	0.021	0.565	0.906	0.139	0.952	0.582	0.793	0.278	0.273

Notes: OLS estimates. Errors clustered at the microfinance-group level. *** p<0.01, ** p<0.05, * p<0.1. Whether a potential applicant (microfinance client) applies for the position is regressed on different "TRAITS" interacted with the treatments. All regressions include microfinance-group fixed effects and control for number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size. Results are robust to not adding the controls. A respondent is money driven if she reports that earnings money is more important as a job characteristics than "having a positive impact on others in the community" or earnings respect (similar definition for "respect driven"). "Highest education level completed" takes value 0 for "no education", 1 for "primary school-year 1" (...) 7 for "primary school-year 7", 8 "secondary school-year 1" (...) 13 "secondary school-year 6". "Self-employed in a non-farming activity" equals one if the main earnings activity is a non-farming self-employment activity rather than involvement in agriculture. "Number of work hours per week" is the total number of hours worked on these activities in a typical week. Earnings per hour worked equal "Weekly earnings" divided by the number of hours worked per week, where weekly earnings equal earnings in a typical week from all working activities, i.e., the main activity and other side activities, expressed in thousands of UGX. Risk aversion is measured by asking respondents what the minimum amount of money is that they would accept from a person they trust instead of playing a game with a 50-50 chance of earnings 100 thousand UGX or nothing.

Table A.IV: Determinants of Application and Appointment

	(1)	(2)
	=1 if potential candidate applies for the CHP position	=1 if applicant is appointed as CHP by BRAC
Sample	***Potential Candidates***	***Applicants***
Has ever volunteered in the health sector	0.2335***	0.1300***
	(0.03)	(0.05)
Community driven	0.0158	0.0828**
	(0.01)	(0.04)
Owns a shop	-0.0251	-0.0285
	(0.02)	(0.05)
Has ever sold health-related products	0.1615***	0.1012*
	(0.03)	(0.06)
Age	-0.0001	0.0048***
	(0.00)	(0.00)
Married	-0.0022	-0.0024
	(0.01)	(0.04)
Highest education level completed	0.0222***	0.0240***
	(0.00)	(0.01)
Self-employed in a non-farming activity	-0.0138	0.0251
	(0.01)	(0.03)
Number of work hours per week	-0.0004*	-0.0017***
	(0.00)	(0.00)
Number of rooms in the house	0.0123***	0.0104
	(0.00)	(0.01)
Observations	4,075	703
R-squared	0.268	0.447

Notes: OLS estimates. Errors clustered at the microfinance-group level. *** p<0.01, ** p<0.05, * p<0.1. Microfinance Group Fixed Effects included in all the regressions. The first column includes the sample of potential candidates (microfinance clients); the second column includes the sample of applicants. "Highest education level completed" takes value 0 for "no education", 1 for "primary school-year 1" (...) 7 for "primary school-year 7", 8 "secondary school-year 1" (...) 13 "secondary school-year 6". "Self-employed in a non-farming activity" equals one if the main earnings activity is a non-farming self-employment activity rather than involvement in agriculture. "Number of work hours per week" is the total number of hours worked on these activities in a typical week. "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect (similar definitions for "money driven" or "respect driven"). "Has ever sold health-related products" equals one if the respondent has ever sold products such as medicine, soap, fortified oil, iodized salt, etc.

Table A.V: Correlations between Traits

	Has ever volunteered in the health sector	Comm-unity driven	Owns a shop	Has ever sold health-related products	Highest education level completed	Self-employed in a non-farming activity	Number of work hours per week	Earnings per hour worked	Number of rooms in the house
Part A. Pool of potential candidates (*n=4863)*									
Has ever volunteered in the health sector	1								
Community driven	0.060***	1							
Owns a shop	0.047***	0.031**	1						
Has ever sold health-related products	-0.001	0.016	0.527***	1					
Highest education level completed	0.161***	-0.006	0.192***	0.191***	1				
Self-employed in a non-farming activity	-0.011	0.005	0.092***	0.064***	0.010	1			
number of work hours per week	-0.044***	-0.081***	0.227***	0.165***	0.125***	0.171***	1		
Earnings per hour worked	0.048***	-0.002	-0.01	-0.014	0.009	-0.001	-0.132***	1	
Number of rooms in the house	0.080***	0.033**	0.118***	0.077***	0.054***	-0.032**	-0.028*	0.051***	1
Part B. Pool of selected Community Health Promoters *(n=301)*									
Amount donated (dictator game)	0.138**	0.041	0.013	-0.034	0.000	0.051	-0.024	-0.026	0.106*

Notes: OLS estimates. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Branch Fixed Effects included in all regressions. Columns 1-3 additionally include individual controls (number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size). "Highest education level completed" takes value 0 for "no education", 1 for "primary school-year 1" (...) 7 for "primary school-year 7", 8 "secondary school-year 1" (...) 13 "secondary school-year 6." "Self-employed in a non-farming activity" equals one if the main earnings activity is a non-farming self-employment activity rather than involvement in agriculture. "Number of work hours per week" is the total number of hours worked on these activities in a typical week. Earnings per hour worked equal "weekly earnings" divided by the number of hours worked per week, where weekly earnings equal earnings in a typical week from all working activities, i.e., the main activity and other side activities, expressed in thousands of UGX.

Table A.VI: Treatment Effects on the Size of the Applicant Pool, by Trait

Sample: Microfinance Groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
TRAIT →	Ever volunteered in the health sector	Never volunteered in the health sector	Community driven	Not community driven	Owns a shop	Does not own a shop	Ever sold health-related products	Never sold health-related products
OUTCOME VARIABLE →	***Number of applicants with TRAIT***							
Medium-Pay Treatment	0.0426	0.3690*	-0.0179	0.3625**	0.0049	0.3971**	0.1243	0.2873
	(0.101)	(0.209)	(0.150)	(0.174)	(0.106)	(0.192)	(0.085)	(0.204)
High-Pay Treatment	-0.0603	0.7726***	-0.2205	0.8932***	0.1954	0.4058**	0.2014**	0.5109**
	(0.099)	(0.225)	(0.148)	(0.193)	(0.127)	(0.200)	(0.099)	(0.223)
Mean of dep var in Low-Pay	0.533	1.686	1.229	0.971	0.495	1.629	0.343	1.876
Observations (Groups)	315	315	315	315	315	315	315	315
R-squared	0.114	0.178	0.168	0.211	0.239	0.216	0.227	0.213
p-value Med=High	0.257	0.076	0.186	0.009	0.089	0.966	0.441	0.322
p-value Low=Med=High	0.520	0.003	0.265	0.000	0.195	0.057	0.103	0.068
OUTCOME VARIABLE →	***=1 if at least one applicant with TRAIT***							
Medium-Pay Treatment	0.0578	0.0470	-0.0412	0.1602**	0.0576	0.0690	0.0955	0.0633
	(0.069)	(0.053)	(0.058)	(0.065)	(0.061)	(0.051)	(0.062)	(0.046)
High-Pay Treatment	-0.0093	0.1423***	-0.1035*	0.3180***	0.1297**	0.0712	0.0888	0.0702
	(0.068)	(0.048)	(0.061)	(0.059)	(0.063)	(0.050)	(0.062)	(0.047)
Mean of dep var in Low-Pay	0.400	0.800	0.724	0.514	0.343	0.800	0.295	0.829
Observations (Groups)	315	315	315	315	315	315	315	315
R-squared	0.114	0.113	0.217	0.189	0.239	0.133	0.198	0.115
p-value Med=High	0.320	0.030	0.310	0.007	0.246	0.963	0.916	0.871
p-value Low=Med=High	0.562	0.006	0.242	0.000	0.120	0.297	0.230	0.268

Notes: OLS estimates. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Applicants are microfinance members who applied for the CHP position. All regressions include branch fixed effects and control for average number of work hours per week in the microfinance group, proportion of microfinance clients who are self-employed in a non-farming activity, average age in the group, proportion marital status, average highest education level completed, average house size. "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect (similar definitions for "money driven" or "respect driven"). "Has ever sold health-related products" equals one if the respondent has ever sold products such as medicine, soap, fortified oil, iodized salt, etc.

Table A.VII: Treatment Effects on Expected Earnings of the Recruited Workers

Sample: Community Health Promoters (CHPs)

Dependent variable = Expected earnings ("How much do you think you will earn from CHP activities in a typical month?")

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TRAIT →	-	Amount donated	Has ever volunteered in the health sector	Community driven	Owns a shop	Has ever sold health-related products	Highest education level completed
Medium-Pay Treatment	27.9000***	28.7225***	25.6920**	25.1815**	35.3089***	33.8808***	56.1239
	(7.93)	(9.40)	(9.99)	(10.05)	(10.10)	(8.79)	(34.54)
High-Pay Treatment	73.3344***	69.7337***	74.2387***	75.0010***	78.0420***	83.0592***	28.7099
	(10.45)	(10.60)	(12.09)	(14.29)	(14.20)	(12.47)	(30.80)
TRAIT		0.0658	3.2350	9.5869	11.3188	28.3847**	1.3912
		(6.43)	(10.29)	(9.31)	(10.64)	(12.87)	(1.76)
TRAIT * Medium-Pay Treatment		-3.7334	6.8698	7.4214	-30.6992*	-27.2047	-3.1784
		(17.51)	(16.75)	(16.48)	(17.50)	(19.78)	(3.64)
TRAIT * High-Pay Treatment		17.3936	-2.0546	4.1890	-18.2003	-42.3279**	4.8078
		(43.03)	(26.48)	(20.92)	(20.89)	(21.32)	(3.18)
Mean of dep var in Low-Pay Treat.	39.59	39.59	39.59	39.59	39.59	39.59	39.59
Observations (CHPs)	286	286	286	283	278	286	286
R-squared	0.233	0.235	0.234	0.241	0.236	0.242	0.252
*p-value Med*Trait=High*Trait*		0.643	0.747	0.892	0.589	0.539	0.058
*p-value Low*Trait=Med*Trait =High *Trait*		0.896	0.904	0.898	0.201	0.100	0.141

Notes: OLS estimates. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Branch Fixed Effects included in all regressions. Expected monthly earnings are estimated by asking CHPs how much they "think they will earn per month from their CHP position in a typical month." This was asked before they started working (at the beginning of the training sessions). Expected monthly earnings are expressed in thousands of UGX and are truncated at the top at 5% and at the bottom if the variable takes implausible values above 0 and under 500 UGX. Results remain similar if I use different truncation rules or if I do not truncate at all. Seventeen CHPs answered "I don't know." "Amount donated" is estimated by playing a contextualized dictator game in which the CHPs were asked, privately and confidentially, for voluntary donations to a public health NGO. Upon arriving at the training and before playing the game, the CHPs were given 3.5 thousand UGX as payment for showing up to the training. "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect (similar definitions for "money driven" or "respect driven"). "Highest education level completed" takes value 0 for "no education", 1 for "primary school-year 1" (...) 7 for "primary school-year 7", 8 "secondary school-year 1" (...) 13 "secondary school-year 6".

Table A.VIII: Treatment Effects on Other Traits of Recruited Workers

Sample: Community Health Promoters (CHPs)

	PROSOCIALITY MEASURES WITH CONTROLS			*OTHER TRAITS*									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
VARIABLES	Amount donated (dictator game)	Has ever volunteered in the health sector	Community driven	Money driven	Respect driven	Highest education level completed	Self-employed in a non-farming activity	# hours of work per week	Earnings per hour of work	# rooms in the house	# children under 5 years old	Degree of risk aversion	Reference dependence
Medium-Pay Treatment	-0.2077***	0.0149	-0.1274*	0.1272**	0.0430	-0.7255*	-0.0250	-0.6207	-0.5685	0.1758	0.0533	0.0257	-0.2164
	(0.07)	(0.07)	(0.07)	(0.06)	(0.05)	(0.42)	(0.06)	(3.89)	(1.09)	(0.26)	(0.17)	(0.04)	(0.34)
High-Pay Treatment	-0.2252***	-0.0939	-0.2584***	0.2580***	0.0097	-0.1407	-0.0026	2.4073	-0.0862	-0.2492	-0.1672	0.0455	0.3392
	(0.07)	(0.07)	(0.07)	(0.06)	(0.05)	(0.39)	(0.06)	(4.20)	(1.07)	(0.24)	(0.17)	(0.03)	(0.31)
Mean in Low-Pay Treat.	0.463	0.347	0.695	0.168	0.137	9.284	0.653	53.323	2.407	3.720	1.384	0.270	5.000
Observations (CHPs)	282	282	278	297	297	301	299	295	259	292	267	284	299
R-squared	0.261	0.133	0.197	0.201	0.163	0.092	0.218	0.173	0.036	0.248	0.073	0.250	0.210
p-value Med=High	0.709	0.097	0.064	0.039	0.499	0.159	0.703	0.457	0.329	0.110	0.168	0.585	0.094
p-value Low=Med=High	0.004	0.188	0.002	0.000	0.667	0.197	0.906	0.744	0.597	0.261	0.357	0.423	0.232

Notes: OLS estimates. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Branch Fixed Effects included in all regressions. Columns 1-3 additionally include individual controls (number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size). "Highest education level completed" takes value 0 for "no education", 1 for "primary school-year 1" (...) 7 for "primary school-year 7", 8 "secondary school-year 1" (...) 13 "secondary school-year 6." "Self-employed in a non-farming activity" equals one if the main earnings activity is a non-farming self-employment activity rather than involvement in agriculture. "Number of work hours per week" is the total number of hours worked on these activities in a typical week. Earnings per hour worked equal "weekly earnings" divided by the number of hours worked per week, where weekly earnings equal earnings in a typical week from all working activities, i.e., the main activity and other side activities, expressed in thousands of UGX. Risk aversion is measured by asking respondents what the minimum amount of money is that they would accept instead of playing a game with a 50-50 chance of earnings 100 thousand UGX or nothing. Reference dependence is calculated by asking the respondents how many lotteries they are willing to play out of 7 hypothetical lotteries with a 50% chance of winning 5K UGX, 50% chance of losing X K, where X =0,1,2,3,4,5,6 (see more details on why i use this as a measure of reference dependence in the paper).

Table A.IX: Treatment Effects on Selection in the Candidate Pool

Sample: Applicants

Dependent variable =1 if the applicant is appointed for the CHP position, =0 if applicant is not appointed

	(1)	(2)	(3)	(4)
	Prosocial Preferences		***Interest in sales***	
TRAIT →	Has ever volunteered in the health sector	Comm-unity driven	Owns a shop	Has ever sold health-related products
TRAIT	0.1886	0.2950**	0.2995**	0.3153
	(0.16)	(0.13)	(0.15)	(0.20)
TRAIT * Medium-Pay Treatment	-0.1309	-0.1718	-0.4425**	-0.2751
	(0.21)	(0.18)	(0.20)	(0.25)
TRAIT * High-Pay Treatment	-0.0521	-0.1555	-0.1215	-0.2049
	(0.21)	(0.17)	(0.19)	(0.24)
Mean of TRAIT in the Low-Pay T.	0.240	0.558	0.233	0.155
Observations (Applicants)	749	735	716	749
R-squared	0.381	0.393	0.396	0.383
*p-value Trait*Med=Trait*High*	0.690	0.922	0.078	0.731
*p-value Trait*Low=Trait*Med=Trait*High*	0.820	0.587	0.067	0.544

Notes: OLS estimates. Errors clustered at the microfinance-group level. *** p<0.01, ** p<0.05, * p<0.1. Whether an applicant is appointed is regressed on different "TRAITS" interacted with the treatments. Microfinance Group Fixed Effects included in all the regressions and regressions control for number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size. Results remain robust to not adding the controls. "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect (similar definitions for "money driven" or "respect driven"). "Has ever sold health-related products" equals one if the respondent has ever sold products such as medicine, soap, fortified oil, iodized salt, etc.

Table A.X: Treatment Effects on Dropout Rate
Proportional Hazard Rate Model

Sample: Community Health Promoters (CHPs)

Dependent Variable = Log of the Hazard Rate of dropping out

	(1)	(2)	(3)
Medium-Pay Treatment	0.4590	0.1394	0.4941
	(0.31)	(0.32)	(0.38)
High-Pay Treatment	0.7229**	0.3629	0.6979*
	(0.29)	(0.32)	(0.40)
Amount donated (dictator game)		-1.5187***	-1.5057**
		(0.55)	(0.64)
Has ever volunteered in the health sector			-0.8791**
			(0.37)
Community driven			0.2582
			(0.33)
Owns a shop			0.2124
			(0.41)
Has ever sold health-related products			-1.3802***
			(0.53)
Controls	No	No	Yes
Observations (CHPs)	6,192	6,192	5,598
p-value Med=High	0.300	0.395	0.530
p-value Low=Med=High	0.0479	0.470	0.207

Notes: Proportional Hazard Rate (Cox Regressions). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Coefficients are not exponentiated. All regressions include branch fixed effects. Controls include number of work hours per week, self-employed in a non-farming activity, age, marital status, highest education level completed, house size, and the list of all village-level controls (see Table I). The results remain robust to using other parametric hazard rate regressions (e.g., Weibull, exponential). "Amount donated" is estimated by playing a contextualized dictator game in which the CHPs were asked, privately and confidentially, for voluntary donations to a public health NGO. "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect (similar definitions for "money driven" or "respect driven"). "Has ever sold health-related products" equals one if the respondent has ever sold products such as medicine, soap, fortified oil, iodized salt, etc.

Table A.XI: Heterogeneous Treatment Effects on Dropout Rate

Sample: Community Health Promoters (CHPs)

Dependent Variable =1 if the CHP has dropped out within two years of recruitment, =0 if CHP is retained

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
TRAIT →	Amount donated (dictator game)	Has ever volunteered in the health sector	Community driven	Owns a shop	Has ever sold health-related products	Reference dependence	Earnings per hour worked
Medium-Pay Treatment	0.0478	0.0797	0.0806	0.1015	0.1000	0.1131	0.0462
	(0.08)	(0.08)	(0.10)	(0.07)	(0.07)	(0.10)	(0.08)
High-Pay Treatment	0.0878	0.0791	0.1028	0.2009***	0.1947***	0.2123*	0.1134
	(0.08)	(0.07)	(0.10)	(0.07)	(0.07)	(0.12)	(0.07)
TRAIT	-0.1738***	-0.1857**	-0.0033	0.1085	0.0295	0.0212	-0.0028*
	(0.06)	(0.08)	(0.09)	(0.09)	(0.11)	(0.01)	(0.00)
Medium-Pay Treatment * TRAIT	-0.0342	-0.0060	-0.0079	-0.0444	-0.0814	-0.0049	0.0077
	(0.11)	(0.11)	(0.13)	(0.14)	(0.14)	(0.02)	(0.02)
High-Pay Treatment * TRAIT	0.0158	0.1525	0.0670	-0.1982	-0.2376*	-0.0167	0.0078
	(0.11)	(0.12)	(0.13)	(0.13)	(0.14)	(0.02)	(0.01)
Mean of dep var in Low-Pay Treat.	0.189	0.189	0.189	0.189	0.189	0.189	0.189
Observations (CHPs)	301	301	297	293	301	299	259
R-squared	0.157	0.157	0.137	0.146	0.145	0.136	0.142
p-value Med=High	0.597	0.994	0.793	0.181	0.195	0.440	0.440
p-value Low=Med=High	0.510	0.497	0.562	0.025	0.018	0.171	0.296

Notes: OLS estimates. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include branch fixed effects. "Amount donated" is estimated by playing a contextualized dictator game in which the CHPs were asked, privately and confidentially, for voluntary donations to a public health NGO. "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect. "Has ever sold health-related products" equals one if the respondent has ever sold products such as medicine, soap, fortified oil, iodized salt, etc. Reference dependence is measured by asking the respondents how many lotteries they are willing to play out of 7 hypothetical lotteries with a 50% chance of winning 5 thousand UGX, 50% chance of losing X thousand UGX, where X =0,1,2,3,4,5,6 (see Section 6 of the paper for more explanations of this measure). Earnings per hour worked equal "weekly earnings" divided by the number of hours worked per week, where weekly earnings equal earnings in a typical week from all working activities, i.e., the main activity and other side activities, expressed in thousands of UGX.

Table A.XII: Treatment Effects on Performance using Alternative Samples−BRAC Monthly Records

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Monthly sales profits			Number of households visited per month			Number of pre- and postnatal checks			Score given by BRAC to CHP performance		
Part 1: *Sample restricted to the 267 CHPs active in May 2013; outcome variables = performance in May 2013*												
Medium-Pay Treatment	4.412	4.3817	4.6329	4.0549	5.4180*	2.1241	0.2378	0.1882	0.0017	-0.4639	-0.3856	-0.4138
	(5.36)	(5.29)	(5.42)	(2.90)	(3.01)	(2.61)	(0.38)	(0.39)	(0.37)	(0.32)	(0.32)	(0.34)
High-Pay Treatment	-5.0483	-5.0810	-6.9057	-0.1329	1.3377	-0.7052	0.2654	0.2118	0.0376	-0.3439	-0.2594	-0.4081
	(4.55)	(4.54)	(5.14)	(2.14)	(2.10)	(2.11)	(0.39)	(0.41)	(0.40)	(0.31)	(0.32)	(0.33)
Amount donated		-0.1337	4.5189		6.0152**	3.5683		-0.2189	-0.3926		0.3455	0.3815
		(5.53)	(6.44)		(2.80)	(2.89)		(0.28)	(0.36)		(0.25)	(0.31)
Volunteered in the health sector			0.5283			-2.8248			0.2213			0.7366**
			(4.91)			(2.69)			(0.40)			(0.30)
Community driven			-3.3037			-2.2397			0.0440			-0.3956
			(5.25)			(2.17)			(0.32)			(0.29)
Owns a shop			4.9930			2.5602			-0.3886			0.0238
			(6.03)			(2.75)			(0.40)			(0.36)
Sold health-related products			1.0135			-0.6026			0.8239			0.5349
			(6.23)			(2.90)			(0.52)			(0.40)
Mean in Low-Pay T.	48.861	48.861	48.861	51.761	51.761	51.761	7.489	7.489	7.489	6.859	6.859	6.859
Observations (CHPs)	267	267	256	267	267	256	267	267	256	267	267	256
R-squared	0.255	0.255	0.265	0.744	0.749	0.764	0.711	0.711	0.729	0.252	0.255	0.287
p-value Med=High	0.084	0.085	0.065	0.115	0.118	0.228	0.943	0.951	0.930	0.723	0.709	0.987
p-value Low=Med=High	0.208	0.211	0.170	0.266	0.185	0.481	0.754	0.848	0.995	0.310	0.461	0.354
Part 2: *Sample incudes all 315 Microfinance Groups; outcome variables = average monthly performance in the period May 2013-April 2014; the 14 microfinance groups in which no CHP was recruited are assigned a zero average monthly performance*												
Medium-Pay Treatment	-2.4494			-0.3128			-0.2757			-0.1789		
	(2.79)			(3.92)			(0.68)			(0.40)		
High-Pay Treatment	-3.4956			-5.6087			-0.8927			-0.4416		
	(2.94)			(3.96)			(0.76)			(0.42)		
Mean in Low-Pay T.	38.084			53.076			9.857			6.764		
Observations (MF Groups)	315			315			315			311		
p-value Med=High	0.721			0.208			0.385			0.526		
p-value Low=Med=High	0.464			0.309			0.486			0.567		

Notes: OLS estimates. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include branch fixed effects. Sales profits are expressed in thousands of UGX. The "score given by BRAC to CHP performance" is a score out of 10 that each CHP supervisor gave to the CHP, considering all the performance dimensions, one year after their recruitment. "Amount donated" is estimated by playing a contextualized dictator game in which the CHPs were asked, privately and confidentially, for voluntary donations to a public health NGO. "Has ever volunteered in the health sector" equals one if the respondent has ever worked as a health volunteer. A respondent is community driven if she reports that "having a positive impact on others in the community" is more important as a job characteristics than earnings money or earnings respect. "Has ever sold health-related products" equals one if the respondent has ever sold products such as medicine, soap, fortified oil, iodized salt, etc.

Table A.XIII: Household Data−Summary Statistics and Balance Checks

Sample: Households (CHPs' clients)

				Joint test		**Largest difference across pairs of treatments**	
VARIABLES	**# obs**	**mean**	**s.d.**	**F-stat**	**p**	**Diff**	**p**
Priority household: at least one household member is pregnant or has delivered in the 6 months prior to the survey	2852	0.21	0.41	0.78	0.46	-0.03	0.23
Distance from CHP house (in walking minutes)	2715	13.59	11.97	0.04	0.96	-0.39	0.77
Number of household members who are less than 5 years old	2858	1.95	1.28	0.12	0.88	-0.04	0.65
Number of female household members of reproductive age (15-49 years old)	2855	1.51	1.19	1.49	0.23	-0.13	0.10
At least one household member is a BRAC microfinance member	2865	0.45	0.50	2.15	0.12	0.07	0.04
Respondent is a female	2789	1.09	0.28	3.16	0.04	-0.04	0.01

Joint test of significance: p-value= 0.40

Notes: The first three columns report number of observations, means, and standard deviations. "Joint test" reports the F-statistic and p-value from a joint test of the significance of the set of treatment dummies in explaining each variable. The remaining columns report the largest difference in means (and lowest p-value) from the associated t-test between pairs of treatment groups. The joint test of significance, reported at the bottom of the table, uses a multinomial logit model to test whether the covariates jointly predict treatment assignment. The null hypothesis is that all covariates together have no predictive power in predicting treatment assignment.

Table A.XIV: Treatment Effects on Performance Controlling for CHPs' Traits–Household Data

Sample: Households (CHPs' clients)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Household knows who the CHP is			Household has previously been visited by the CHP			Number of times household was visited in the last month		
Medium-Pay Treatment	0.0326	0.0442	0.0381	0.0517	0.0608*	0.0499	0.1168	0.1289	0.1696
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.16)	(0.17)	(0.16)
High-Pay Treatment	0.0220	0.0342	0.0392	0.0388	0.0486	0.0436	-0.1168	-0.1041	-0.0413
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.11)	(0.12)	(0.12)
Amount donated by the CHP		0.0477*	0.0018		0.0388	-0.0079		0.0511	0.0187
		(0.03)	(0.03)		(0.03)	(0.03)		(0.15)	(0.20)
CHP has ever volunteered in the health sector			0.0755**			0.0458			0.3163**
			(0.03)			(0.03)			(0.15)
CHP is community driven			0.0147			-0.0131			0.1053
			(0.03)			(0.03)			(0.13)
CHP owns a shop			-0.0112			0.0030			-0.1323
			(0.03)			(0.03)			(0.15)
CHP has ever sold health-related products			0.0798**			0.0298			-0.0206
			(0.04)			(0.04)			(0.16)
Mean in Low-Pay Treatment	0.581	0.581	0.581	0.496	0.496	0.496	1.002	1.002	1.002
Observations (Households)	2,542	2,542	2,460	2,509	2,509	2,428	2,476	2,476	2,395
R-squared	0.327	0.328	0.336	0.279	0.280	0.283	0.131	0.131	0.136
p-value Med=High	0.724	0.741	0.475	0.672	0.689	0.325	0.220	0.233	0.355
p-value Low=Med=High	0.588	0.400	0.969	0.244	0.165	0.834	0.107	0.107	0.152

Notes: OLS estimates. Errors clustered at the village/CHP level. *** p<0.01, ** p<0.05, * p<0.1. All regressions include branch fixed effects and household-level controls (distance to CHP house, being a BRAC microfinance member, number of children under 5 years old, number of females of reproductive age, gender of the respondent, priority household). A household knows who the CHP is if the correct name is given to the question "who is the CHP serving this village?"

Table A.XV: Treatment Effects on Performance on the Job–Household Data

Sample: Households located in the 255 villages in which CHPs were active at the time of the household survey

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Household knows who the CHP is				Household has previously been visited by the CHP				Number of times household was visited in the last month			
Medium-Pay Treatment	0.0457	0.0512	0.0519	0.0428	0.0682**	0.0715**	0.0658*	0.0743**	0.2130	0.2070	0.2791	0.1534
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.17)	(0.18)	(0.18)	(0.20)
High-Pay Treatment	0.0304	0.0361	0.0431	0.0502	0.0414	0.0449	0.0394	0.0753**	-0.0621	-0.0685	0.0195	-0.0589
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.13)	(0.13)	(0.15)	(0.15)
Amount donated by the CHP		0.0225	-0.0319			0.0140	-0.0407			-0.0255	-0.0756	
		(0.03)	(0.03)			(0.04)	(0.04)			(0.16)	(0.21)	
CHP has ever volunteered in the health sector			0.0632*				0.0274				0.2368	
			(0.03)				(0.04)				(0.16)	
CHP is community driven			0.0389				0.0057				0.1959	
			(0.03)				(0.03)				(0.14)	
CHP owns a shop			0.0007				0.0174				-0.1569	
			(0.03)				(0.03)				(0.16)	
CHP has ever sold health-related products			0.0553				0.0032				-0.1565	
			(0.04)				(0.04)				(0.17)	
Priority Household				0.0698*				0.0962**				0.0255
				(0.04)				(0.04)				(0.18)
Priority Household*Medium-Pay Treatment				0.0180				-0.0263				0.2562
				(0.05)				(0.06)				(0.39)
Priority Household*High-Pay Treatment				-0.1041*				-0.1669***				-0.1709
				(0.06)				(0.06)				(0.22)
Mean in Low-Pay Treatment	0.607	0.607	0.607	0.607	0.519	0.519	0.519	0.519	1.065	1.065	1.065	1.065
Observations (Households)	2,223	2,223	2,140	2,195	2,192	2,192	2,110	2,164	2,193	2,193	2,111	2,166
R-squared	0.333	0.333	0.341	0.339	0.282	0.282	0.285	0.288	0.127	0.127	0.133	0.130
p-value Med=High	0.632	0.299	0.778	0.820	0.110	0.431	0.194	0.976	0.096	0.247	0.229	0.272
p-value Low=Med=High	0.347	0.639	0.330	0.305	0.426	0.109	0.439	0.050	0.249	0.095	0.126	0.547

Notes: OLS estimates. Errors clustered at the village/CHP level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. All regressions include branch fixed effects and household-level controls (distance to CHP house, being a BRAC microfinance member, number of children under 5 years old, number of females of reproductive age, gender of the respondent, priority household). A household knows who the CHP is if the correct name is given to the question "Who is the CHP serving this village?"

Table A.XVI: Treatment Effects on Health Outcomes and Health Knowledge–Household Data

Sample: Households (CHPs' clients)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	=1 if respondent knows the drug(s) that treat the health problem			% of the causes of the health problem mentioned by the respondent that are correct			=1 if any household member has suffered from the health problem in the last 6 months		
Health problem →	Malaria	Diarrhea	Worms	Malaria	Diarrhea	Worms	Malaria	Diarrhea	Worms
Medium-Pay Treatment	-0.0374*	0.0022	-0.0040	-0.0233	0.0076	-0.0130	-0.0140	0.0254	0.0198
	(0.02)	(0.04)	(0.03)	(0.02)	(0.01)	(0.02)	(0.03)	(0.03)	(0.03)
High-Pay Treatment	0.0015	-0.0793**	-0.0247	-0.0109	0.0146	0.0058	0.0168	0.0163	0.0537**
	(0.02)	(0.04)	(0.03)	(0.02)	(0.01)	(0.02)	(0.03)	(0.03)	(0.03)
Mean of dep var in Low-Pay T.	0.899	0.331	0.774	0.776	0.873	0.816	0.696	0.229	0.146
Observations (Households)	1,519	1,519	1,519	1,467	1,423	1,325	1,500	1,496	1,491
R-squared	0.144	0.318	0.236	0.612	0.226	0.332	0.170	0.102	0.066
p-value Med=High	0.064	0.030	0.474	0.466	0.538	0.210	0.565	0.732	0.117
p-value Low=Med=High	0.089	0.047	0.685	0.376	0.477	0.437	0.288	0.640	0.135

Notes: Weighted OLS estimates. Errors clustered at the village/CHP level. *** p<0.01, ** p<0.05, * p<0.1. Data include all households who know the CHPs. Weights in the regression are proportional to the number of households who declare knowing the CHPs. This ensures that villages in which only a few households know the CHP are given less weight, as these households are more likely to be a relative of the CHP and are less representative of the whole village. All regressions include branch fixed effects and household-level controls (distance to CHP house, being a BRAC microfinance member, number of children under 5 years old, number of females at reproductive age, gender of the respondent, being a priority household).

Table A.XVII: IV Regressions

CHP monthly expected earnings instrumented with the treatment dummies

Part A: BRAC Monthly Records

	(1)	(2)	(3)	(4)	(5)
	CHP has dropped out within two years of recruitment	Monthly sales profits	Number of households visited per month	Number of pre- and postnatal checks	Score given by BRAC to CHP overall performance (out of 10)
Expected Earnings (in s.d.)	0.1489**	-5.0963	-9.0333**	-1.5746*	-0.9221**
	(0.07)	(3.33)	(4.08)	(0.84)	(0.39)
Mean of dep.var.	0.269	34.65	48.71	8.953	6.283
Observations (CHPs)	286	286	286	286	282
F stat: 1st stage	26.74	26.74	26.74	26.74	27.19

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. All regressions include branch fixed effects. Expected warnings are expressed in thousands of UGX and are truncated at the top at 1% and at the bottom if the variable takes implausible values above 0 and under 500 UGX. They are measured in standard deviations. The results remain consistent if I truncate it at 5% in the top or if I do not truncate the variable at all. The F stat at the bottom of the table reports the Kleibergen-Paap rk Wald F statistic from the first stage.

Part B: Household Data

	(1)	(2)	(3)	(4)	(5)	(6)
	Household knows who the CHP is		Household has previously been visited by the CHP		Number of times household was visited in the last month	
Expected Earnings (in s.d.)	0.0064	0.0249	0.0196	0.0520	-0.1177	-0.0969
	(0.03)	(0.04)	(0.03)	(0.04)	(0.12)	(0.13)
Priority Household		0.1307**		0.1848**		0.2539
		(0.06)		(0.07)		(0.29)
Expected Earnings*Priority Household		-0.0908		-0.1537**		-0.1983
		(0.06)		(0.07)		(0.23)
Mean of dep.var.	2,432	2,400	2,404	2,372	2,372	2,341
Observations (HHs)	0.612	0.612	0.538	0.538	0.985	0.985
F stat: 1st stage	22.57	13.52	23.08	13.20	22.19	13.15

Notes: Errors clustered at village (CHP) level. *** p<0.01, ** p<0.05, * p<0.1. Columns 2, 4, 6 ,8 use the following instrumental variables: treatments dummies and treatment dummies interacted with Priority Household. For full list of controls and other details, see Table XI. The F stat at the bottom of the table reports the Kleibergen-Paap rk Wald F statistic from the first stage.

Part C: Household Data - Health Knowledge and Health Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	=1 if respondent knows the drug(s) that treat the health problem			% the causes of the health problem mentioned by the respondent that are correct			=1 if any household member has suffered from the health problem in the last 6 months		
	Malaria	Diarrhea	Worms	Malaria	Diarrhea	Worms	Malaria	Diarrhea	Worms
Expected Earnings (in s.d.)	0.0161	-0.1170**	-0.0177	-0.0015	0.0201	0.0169	0.0377	0.0246	0.0702*
	(0.03)	(0.05)	(0.04)	(0.02)	(0.02)	(0.02)	(0.05)	(0.03)	(0.04)
Mean of dep.var.	1,408	1,408	1,408	1,359	1,322	1,225	1,392	1,388	1,383
Observations (HHs)	0.875	0.297	0.744	0.775	0.886	0.809	0.687	0.215	0.153
F stat: 1st stage	10.08	10.08	10.08	9.928	9.548	8.630	9.877	9.808	9.712

Notes: IV estimates. Errors clustered at village (CHP) level. *** p<0.01, ** p<0.05, * p<0.1. Data include all households who know the CHPs. The table shows the point estimates for a weighted IV regression. For the full list of controls and other details, see Table A.XVI. The F stat at the bottom of the table reports the Kleibergen-Paap rk Wald F statistic from the first stage.

Chapter 2
Social Connections and the Delivery of Development Programs

1. Introduction

Governments and NGOs often recruit agents from local communities to deliver development programs. Examples of these community-based positions include microfinance group leaders, community health workers, agriculture extension workers, seed farmers, to mention a few. While the ability to recruit from the community is key for the scalability and sustainability of many interventions, organizations face the challenge that these agents are embedded in existing social networks and this might determine which beneficiaries are targeted. Indeed, a rich literature shows that individuals have social preferences and that these depend on social connections, so that agents might favor individuals they are connected to rather than those who are more deserving. Understanding the effect of social connections on the targeting choices of delivery agents has therefore implications for both equity and efficiency.

This paper studies how social connections shape the delivery of an agriculture extension program implemented by BRAC in Uganda. In a context characterized by low agriculture productivity, the program trains and employs female agents chosen from the communities to provide advice, training and sell improved seeds to poor women farmers. As time and seeds are limited, the agents need to choose which farmers to target. BRAC's guidelines prioritize poorer and less productive farmers. Our aim is to test whether social connections shape targeting at the expense of this goal.

Our evaluation is based on a two-level randomization design. First, we randomize the allocation of the agriculture extension program at the community level to evaluate whether the program is beneficial. Second, within each treatment community, we randomly select one delivery agent out of two eligible candidates in each community. This generates variation in social connections which is orthogonal to unobservable traits common to farmers who are socially connected to potential agents. The estimated effect of social connections thus captures social preferences without capturing unobservable traits that determine program returns.

Our main finding is that, relatively to unconnected farmers, farmers only connected to the selected delivery agents are more likely to benefit from the program while farmers only connected to the agent who ran but was not selected are less likely to benefit. We make precise the conditions under which the evidence implies that agents put positive weight on the utility of farmers connected to them (altruism), or negative weight on the utility of farmers connected to the rival candidate (spite), or both.

The analysis proceeds as follows. To begin with we identify the effect of the agriculture program on adoption of improved seeds and agriculture productivity with the cluster randomized controlled trial we designed with BRAC. The trial randomises the roll-out of the program, whereby 109 communities are treated in 2012 and a further 59 are kept as controls until 2015. Two years after the introduction of the program, we find that farmers in the treatment communities are more likely to have received advice and training on agriculture than in control communities. Their adoption rate of BRAC improved seeds increased by 6.2 percentage points without any reduction in the adoption of non-BRAC improved seeds, and their agriculture rate of returns increase by 1.15, i.e. each dollar of input leads to an additional 1.15 dollar of output. This corresponds to a 50% increase in rate of returns. These effects are in line with earlier studies (Beaman et al. 2014; Duflo et al. 2008).

Having established that the program is beneficial and, thus, that distortions from the optimal allocation of seeds matter, we zoom in on treatment communities to unpack the effects of social connections. To deliver the program, BRAC relies on agents, called extension workers or EW for short, chosen among eligible candidates in the community. BRAC identified two EW candidates in each community based on the number of acres of land they own, their agricultural knowledge, business skills and popularity. One of the two candidates is then randomly appointed and the data shows that the two candidates (selected and non-) are similar to each other. Prior to the randomisation, we measure social connections between each of the sample farmers and each of the two candidates. Post randomization we show that farmers connected to the selected EW share similar traits with those connected to the non-selected EW, and both differ from those connected to neither. The design thus creates exogenous variation in the connection to the selected and the non-selected EW candidate, conditional on being connected to one of the two candidates. This allows us to identify the causal effect of social connections on program delivery.

We find that, compared to farmers connected to none of the candidates, those connected to the selected EW are significantly more likely to receive advice, training and BRAC improved seeds from the EW. In contrast, those connected to the non-selected EW are significantly less likely to receive all three services: they are worse-off than those connected to neither candidate. The difference between these estimates captures the causal effect of social connections. In other words, while farmers connected to a candidate might have unobservable traits that affect the returns from adoption, these drop out from the difference between knowing the selected and the non-selected EW. The fact that this is positive and precisely estimated indicates that social preferences affect targeting choices. These can take the form of altruism towards one's friends or spite towards one's rival's friends. We use our estimates to bound the effect of altruism and spite conditional on unobservable traits.

To corroborate this evidence we exploit the intuition that the strength of social preferences should be increasing in the social distance between the selected candidate and the farmers who are connected to the non-selected candidate, while the effect of unobservables should not. Although we do not have data on the full network, we can proxy for social distance by the connections between the two candidates: if the two know each other, the distance between the selected EW and the farmers connected to the non-selected EW is at most degree two. Another proxy of social distance we use is the percentage of farmers in the village that know both candidates, as this increases the chances that the distance between the selected EW and the farmers connected to the non-selected EW is at most degree two. We find that the difference between being connected to the selected EW and being connected to the non-selected EW is positive and precisely estimated only when social distance is likely to be above two. This is driven by the fact that the negative effects of the connections to the non-selected candidate materializes only when social distance is high, while the positive effect of knowing the selected EW holds irrespective of the social distance between the selected EW and farmers connected to her rival.

These results are consistent with two, not necessarily mutually exclusive, explanations: (1) the delivery agent discriminates against farmers who are connected to their rival, (2) the farmers boycott the delivery agent, although this can only explain why they do not get seeds, as that hurts the EW, but not why they do not ask for advice/go to training. In either case, the findings indicate negative social preferences or "spite" as agents are willing to bear a cost to damage others: the agent gives up on sales commission, the farmers give up on buying good quality seeds and get some low quality seeds from the market instead.

Finally, we analyze whether farmers connected to the losing EW candidate can compensate by buying improved seeds from other sources so that ultimately productivity is unchanged. The results suggest that they tend to adopt significantly more improved seeds from local markets, which, however, have been shown to be very low quality (Bold et al. 2015). Ultimately, productivity and returns increase for farmers connected to the winning candidate and drop for farmers connected to the losing candidate relative to farmers connected to neither.

A straightforward implication of these results is that variations in connections shape adoption rates at the village level. To test this, we exploit natural variation in the relative size of farmers connected to the winners and losers and we find that aggregate adoption increases with the percentage of farmers connected to the winner while it decreases with the percentage connected to the loser. Although village-level social connections may not be orthogonal to other village characteristics, these results are suggestive evidence that both the network of the selected and the non-selected EW, and the overlap between the two networks, impact aggregate adoption rate.

This paper contributes to two strands of the literature. First, it contributes to the existing research on the targeting of development programs. In the context of Below Poverty Line card allocation in India, Niehaus and Atanassova (2013) show that conditioning household card-eligibility on more poverty indicators may worsen targeting when the program is implemented by corruptible agents: while less eligible households pay larger bribes and are less likely to obtain cards, the targeting rule becomes harder to enforce. Other related studies find that the targeting of antipoverty programs depends on whether the beneficiaries of the program are identified by the community itself or identified according to a number of pre-determined criteria (Alatas et al. 2013), whether the program is implemented by the government vs. an NGO (Banerjee et al. 2009), or on a number of village characteristics (Galasso and Ravallion 2005; Bardhan and Mookherjee 2006). This paper complements these existing studies by showing that who is chosen as the delivery agent strongly predicts which households are then targeted and, though this, how successful the program is.

Second, this paper complements studies of social networks and technology diffusion. In the context of the diffusion of pit-planting in Malawi, BenYishay and Mobarak (2014) find that selecting 'average farmers' as delivery agent works better than selecting government-employed extension workers or 'lead farmers'. Farmers indeed appear to be most convinced by the advice of others who face agricultural conditions that are comparable to the conditions they face themselves. In the same empirical setting, Beaman et al. (2014) show that selecting agriculture extension workers through a complex network-theory targeting rule is more effective in fostering adoption than using other simpler selection strategies, i.e. minimizing "geographic distance" between the delivery agent and the population, asking the community to select these agents or using a simple network-theory targeting rule. Finally, Banerjee et al. (2013) find that the adoption of a microfinance product in India is higher in villages where the injection point occupy a central position in the social network. While all three studies identify the position of the delivery agent in the social network as a determinant of technology adoption, our paper is the first to show that the adoption rates may be shaped not only by the position of the selected candidate but also by the position of the non-selected candidate in the network.

The rest of the paper proceeds as follows: Section 2 discusses the context of the experiment and the empirical design; Section 3 identifies the effect of the BRAC agriculture program on training, advice and adoption of seeds. Section 4 investigates the individual and aggregate effects of social connections on program delivery. Section 5 concludes.

2. Context, Research Design and Data

2.1. The Agriculture Program

Agricultural productivity in sub-Saharan Africa is extremely low. Many argue that growth in agricultural yields in Asia and the stagnation in Africa can be explained by increased use of modern technologies in Asia and continued low use in Africa (Morris et al. 2007). As a large fraction of the population in Africa depends on agriculture, interventions that raise agricultural productivity are considered to have the potential to reduce poverty and promote growth.

In such a context, BRAC's agriculture program provides extension services seeking to improve agriculture productivity by promoting the usage of beneficial farming techniques and by increasing the availability of high-quality improved seeds. Given the large proportion of poor marginalized farmers involved in subsistence agriculture in Uganda and BRAC's objective to empower women, the program targets poor women farmer. Launched in August 2008, it operates today in 41 districts of Uganda, engages more than 800 extension workers, and reaches 40,000 women farmers (Barua 2011).

BRAC selects Extension Workers (EW) among poor marginalized women based on the following criteria: being a woman farmer, permanent resident in the village, age strictly above 22. During the recruitment process, BRAC additionally favors candidates who own 1 to 3 acres of land, who are respected in the community, with knowledge about agriculture and with business skills. Once selected, all EWs receive six days of training in crop production techniques, adoption of improved seeds and pest control, as well as follow-up monthly refresher courses.

After the initial training, EWs are posted to their villages where they are made responsible for 3 non-remunerated tasks: (1) train 15-20 farmers at the beginning of each growing season on good agriculture practices and usage of improved seeds, (2) provide advice on agriculture to farmers on a daily basis by visiting their homes, (3) set up a demonstration plot using learned techniques and BRAC improved seeds.[57] EWs are remunerated for selling improved seeds of the two main crops in West-Uganda, i.e. maize and beans. These seeds, which

[57] The BRAC's program analyzed in this paper differs from the standard BRAC program implemented in other existing districts. The standard program enlists two separate extension workers per community known as the model farmer and the community agriculture promoter (CAP). While model farmers are made responsible for setting up a demonstration plot and for training general farmers at the beginning of each season, CAPs are in charge of selling improved seeds to the community. In our experiment, the two agriculture extension positions are merged into one (we call it the "Extension Worker" position) in which the worker not only trains, provides advice and has a demonstration plot but also sells improved seeds. BRAC also recruits "Community Livestock promoters" (CLP) who promote and sell poultry vaccines and medicines. Given that the selection of these CLPs was not randomized in the experiment (see details below) and that we observe in our endline data that basically none of the interviewed households buy seeds from these CLPs, our analysis do not focus on them.

are produced in BRAC's production farm,[58] are bought by the EWs at the wholesale price and sold to the community at a markup[59]. Although BRAC's agriculture program increases the availability of improved seeds, the quantity produced in their production farm is limited and cannot supply all households in the villages. As a consequence, EWs face an allocation problem.

All the EWs of a given branch are supervised by the same person called the Program Assistant (PA). PAs visit each of the EWs they supervise once a week in the field. During these visits, they monitor the EW, sell improved seeds to the EW and also sell BRAC seeds directly to the community. Our data indicate that among the farmers who have adopted BRAC seeds at endline, 44% purchased them from the EW, 46% from the PA while the rest purchased from the branch or from BRAC's Community Livestock Promoter, i.e. an agent in charge of promoting poultry vaccines. Although the EW is the most important person for training and advice to farmers, both the EW and the PA play an important role in selling seeds.

Two characteristics of the Ugandan context are important to be emphasized here. First, improved seeds are rare in rural Uganda and are usually sold at a higher price than BRAC's one. A survey we conducted at baseline in the 71 local markets of our experiment confirms this: maize improved seeds are found in 3 markets only and are sold at a median price of 2,500 UGX per kilogram, i.e. 200 UGX higher than BRAC's price. Second, the improved seeds that are available in local markets and shops of rural Uganda tend to have poor quality. In a recent study conducted in 120 local shops/ markets of rural Uganda, Bold et al. (2015) find that the most popular high-yield variety maize seeds contain less than 50% authentic seeds and document that such low quality results in negative average returns. As BRAC improved seeds have better quality and a lower cost, we consider BRAC and non-BRAC improved seeds as two distinct products in the analysis.

In addition to the agriculture program, BRAC also runs a microfinance program that provides access to small loans for women engaged in farm or non-farm self-employed activities. By forming groups of 20 to 25 members who are responsible for each other's loans, BRAC microfinance clients get either 3 or 12.5 months loans, with weekly repayment starting from one week after the disbursement and with a 25% interest rate. As marginalized farmers are very often credit constrained, the existence of a microfinance program may complement the agriculture program by enabling them to buy the improved seeds. We test this in Section 3.2.

[58] In a small sub-sample of the villages, they also sell cabbage, tomato and G-nuts improved seeds.

[59] Maize seeds are for instance bought from BRAC at 2,000 UGX per kilogram and sold to the community at 2,300 UGX per kilogram

2.2 Research design, sample and data

The aim of our research is to understand the role of social connections in shaping the delivery of an agriculture extension program. To do so, the empirical design is divided in two experiments. The first experiment tests whether BRAC's agriculture extension program increases adoption of improved seeds and agriculture productivity. Having established whether the program is beneficial and, thus, whether distortions from the optimal allocation of seeds matter, the second experiment, which is our core experiment, tests the role of social connections in program delivery.

Experiment 1: Evaluating the Agriculture Program

During BRAC's expansion in four new branches of West-Uganda,[60] we collaborated with them to randomize the roll-out of their programs across communities between 2012 and 2015. Each community is formed by one to five villages merged together to avoid contamination issues. Of our initial sample of 214 villages, 92 villages indeed share a trading center, market, school, church, mosque, health center or are located in the same valley or hillside. To avoid contamination, we considered these 92 villages as 46 single units and defined a total of 168 communities to include in our experiment.

Stratifying by branch, size of the community, percentage of farmers and distance to the closest market, the randomization assigns communities to one of three groups. The first treatment group (51 communities) received both the agriculture extension and the microfinance programs in 2012. The second treatment group (58 communities) received the agriculture extension program in 2012 while the microfinance program started only in 2015. The control group (59 communities) received neither of the programs until 2015.

As extension services were non-existent at baseline and communities that did not receive a program in 2012 were not informed that the program would start in 2015 (no anticipation effect),[61] comparing treatments and control allows us to identify the effect of BRAC agriculture extension program on adoption of improved seeds and agriculture productivity. Adding the microfinance program in a sub-sample of the communities helps us evaluating the importance of credit constraints in the adoption process.[62]

The data are constructed as follows: (1) all the 25,384 female household heads located in the 168 communities of our study are interviewed at census, (2) census data are used to draw a random sample of 27 households per community included in our 2012 baseline survey

[60] The four branches are Kabale and Muhanga (in Kabale district), and Rukungiri and Buyanja (Rukungiri district). Both Kabale and Rukungiri are 'chief towns' of their respective districts, and tend to have more trade and business activities than Muhanga and Buyanja.

[61] In our baseline data, only 14% of the respondents report having received advice from an agriculture extension worker in the last cropping season.

[62] Microfinance is not available in these communities at baseline: only 2% of the sample we interviewed at baseline report having borrowed money from a microfinance institution in the last year.

(total of 5,158 respondents),[63] (3) 93% of these baseline households were interviewed again at endline in 2014 (4,366 households). Attrition rates are thus low and, as shown in Table A0, balanced across treatments. Finally, note that the proportion of respondents who are engaged in agriculture is large: 75% in the census data and 85% in the baseline data. The analysis in this paper restricts to the 4,692 respondents who are involved in agriculture at baseline.

The program effects we identify are causal under the assumptions that treatment assignment is orthogonal to characteristics of the community that affect the outcomes of interest. In support of this assumption, Table 1 presents means and standard deviations for a number of baseline variables, for each treatment group separately. Reassuringly, most of these baseline characteristics do not differ significantly between treatments and control communities, indicating that the randomization yields a sample that is balanced.

Baseline summary statistics indicate that 93% of the respondents know about the existence of improved seeds and 73% believe they have positive returns (Table 1). Although knowledge and perceived returns are high, two third of the farmers have never used improved seeds: 60% because of reported seeds unavailability and 35% because of seeds being too expensive for them. Among the one third of the respondents who have used improved seeds at baseline, 80% bought them from local markets or shops, while the remaining 20% bought them from government extension workers or from the National Agricultural Advisory Services (not reported).

To measure farmer's productivity, we use three variables: 1) kilograms of beans/maize produced per acre cultivated and hour worked, 2) total output value (including all crops) per acre cultivated and hour worked,[64] 3) rate of returns, defined as [(value of output - value of input) / value of input] where the input value includes expenses incurred for buying inputs, renting land, hiring workers and for the own opportunity cost of time.[65]

The average farmer of our sample cultivates 1.2 acres of land, works 482 hours on agriculture per cropping season and has 50% probability of being engaged in commercial (vs. subsistence) farming, i.e. selling at least 5% of the agriculture output value. The average farmer moreover produces 0.9 kg of maize per hour worked and acre cultivated, corresponding to yields of 1,462 kg per hectare per year. This number is in line with the 2012 FAO data and the Uganda National Household Survey data (1300 and 1900 kg of maize/hectare over a

[63]The random draw is stratified by household's microfinance eligibility status, i.e. resident in the village for at least one year, business owner, interested in receiving microfinance, and with poverty score between 24 and 71%. This is done to make sure to include, in our baseline sample, households who are then eligible to access BRAC loans once the microfinance program starts and assess potential complementarities between the microfinance and the agriculture extension program.

[64]The total output value equals the quantity of each crop produced multiplied by the median market price of each crop unit in the branch. Extreme outliers, that we define being above or below 2 standard deviations from the mean, are removed from this calculation. The results are robust to alternative cleaning strategies.

[65]We assume that the opportunity cost of working on one's own plot of land equals hours worked on own land multiplied by the branch median hourly wage paid to work on someone else land. Result are robust to considering the median hourly salary for working on one's own non-farm business.

year respectively).[66] The average output value of our sample farmers equals 2,750 UGX per hour worked and acre cultivated and the average rate of returns over the season are estimated at 6.5%. Finally, half of the baseline farmers have completed primary school, they own more land on average than the land they cultivate (2.1 acres owned while 1.2 acres cultivated on average) and own a total of 18 personal and business assets.

Experiment 2: Evaluating the role of social connections

The second experiment of this paper took place within the treatment communities and was implemented in 3 consecutive steps. Among the 109 treated communities, BRAC identified 60 clusters, each composed of either 1, 2 or 3 communities and each served by a single EW. The 60 clusters were created by considering the proximity between the treatment communities and making sure that the EW could easily reach each of the program recipients. In all of these clusters, BRAC then selected two EW candidates based on the following criteria: being a woman farmer, permanent resident in the village, age above 22, preferably with 1 to 3 acres of land owned, respected in the community, with knowledge about agriculture and business skills. After confirming that both candidates of the cluster were interested in accepting the position, we randomly appointed one of the two. The empirical design is illustrated in Figure 1.

Two surveys were conducted after the selection of the two EW candidates but before random appointment: 1) the 120 EW candidates were interviewed on their agriculture knowledge and productivity, 2) the baseline households were asked about their social connections with the two candidates of their own cluster. More specifically, we asked each household at baseline whether they knew each of the 2 candidates and if they did: how frequently did they talk to them, whether they would turn to them for advice on agriculture, and which relationship they had with them (friend, relative, neighbor, input provider or someone they know).

Throughout the paper, we use the most conservative measure of social connection, i.e. whether a farmer knows the EW candidate at baseline, irrespective of the exact nature of the relationship. We narrow this definition to farmers who typically discuss about agriculture with the EW candidate as a robustness check (see Section 4.2). The percentage and number of sample farmers connected to the EW candidates is presented in Table 2: 53% know both candidates, 15% know the winner only, 10% the loser only and 22% neither. Overall, 68% of farmers are thus connected to the winning candidate and 63% are connected to the losing candidate.

Balance checks for the EW randomization are reported in Tables 3 and 4: Table 3 compares the 60 winning candidates to the 60 losing ones, while Table 4 compares respondents connected to the winner and the loser. Both tables provide evidence that the randomization yielded a

[66] FAO, "Area Cultivated and Output of Maize in Uganda", http://faostat.fao.org/site/339/default.aspx. For UNHS, see http://www.ajfand.net/Volume12/No7/Okoboi10505.pdf

balance sample. The selected and non-selected EW candidates are indeed similar to each other in terms of their connections in the cluster, their level of poverty and their involvement in agriculture (column 4, Table 3). Note that both of them are bigger farmers than the average non-EW candidate farmer, i.e. they spend more hours on farming, cultivate more land and are more likely to be engaged in commercial farming. Similarly, respondents connected only to the winning EW candidate are comparable to those connected only to the loser (column 4, Table 4) but are both more productive farmers than agents connected to neither (columns 5 and 6).

3. The Effects of the Agriculture Program

3.1. The Effect of the Agriculture Program on Adoption and Productivity

We evaluate the agriculture program with:

$$y_{ij1} \quad = \quad \alpha + \beta y_{ij0} + \gamma T_j + X_j \delta + \varepsilon_{ij}$$

where $y_{ij1}\,(y_{ij0})$ is the outcome of interest for farmer i in community j at endline in 2014 (baseline in 2012), $T_j = 1$ for treatment communities (treatment one or two) and X_j is a vector of community-level stratification variables. Errors are clustered at the community level.

Table 5 provides evidence on program impact. Respondents in the treatment group are 3.6 and 3.8 percentage points more likely than in the control group to have received advice on agriculture in the last season and to have been trained by the EW in the last year. While receiving these services does not affect perceived returns of improved seeds (which were high at baseline already), the program fosters the adoption of BRAC seeds by 6.2 percentage points after 2 years without reducing the adoption of non-BRAC improved seeds. These effects are precisely estimated and are in line with other existing studies: Beaman et al. (2014) find an adoption increase of pit-planting in Malawi of 10% after 3 years.

Three points are of note. First, the fact that the effect on adoption rate is about twice as large as the effect on receiving training suggests spillovers from training. Second, half of the adoption of BRAC improved seeds in the treatment communities happens through the EWs while the other half buys from other BRAC source, i.e. mainly from the EWs supervisors -the PAs- and also rarely from the Community Livestock Promoter. This indicates that EWs play an important role in selling seeds. Third, almost no respondent in the control communities has received training, advice or has bought improved seeds from BRAC (bottom of Table 5). This provides evidence that the control group has not been contaminated.

Columns 7 and 9 of Table 5 show that the agriculture program increases the likelihood that a household is engaged in commercial farming by 11% while the number of hours worked on agriculture decreases, suggesting an increase in agriculture productivity. Moreover, the agriculture rate of returns significantly increase by 1.15, i.e. each dollar of input leads to an additional 1.15 dollar of output. This corresponds to a 50% increase in rate of returns over a season in comparison to average rate of returns in the control. This effect is in line with the 36% increase in rate of returns calculated for fertilizers in Kenya by Duflo et al. (2008). Finally, "output value per acre cultivated and hour worked" increases but the effect is not precisely estimated.

3.2. The Constraints Relaxed by the Agriculture Program

Having established that the program "works", we investigate next which constraints to technology adoption are relaxed by the program. In Table A1, we restrict the analysis to respondents who have never adopted improved seeds at baseline and interact the presence of the agriculture program with the baseline self-reported constraints to adoption. The results indicate that the lack of availability of high-quality improved seeds is the key constraint relaxed by the program: respondents who reported not having access to improved seeds at baseline are significantly more likely to adopt at endline. As already noted, knowledge about the existence, the benefits and the risks of improved seeds is high at baseline and the program does not consequently relax an information constraint. Finally, the respondents who reported not adopting improved seeds at baseline because these are too expensive are more likely to use both BRAC and non-BRAC improved seeds at endline.

In Table A2, we investigate whether a number of farmer behavioral characteristics and biases limit investment in improved seeds. First, we interact the treatment status of a community with a measure of the respondent's discount rate to test whether myopic farmers are less likely to adopt than more forward looking farmers. Next, we interact the treatment dummy with a measure of farmers' risk aversion, which may predict adoption of risky or uncertain agriculture technologies (Liu 2011). Finally, we test whether present-biased farmers under-adopt beneficial technologies. As discussed in Duflo et al. (2015), this can happen because of the existence of a lag between the harvest time when cash is most available and the planting season when improved seeds are purchased. We find that neither the discount factor nor present-biasedness predict adoption. In contrast, risk aversion matters: agents who report taking risks in agriculture are more likely to adopt improved seeds.[67]

[67] The discount rate is calculated by asking respondents 4 questions: whether they would prefer earning 30k UGX today or Xk UGX in 1 month where X=30, 40 or 50. The index takes value 0 if the respondent is always willing to wait for whatever amount of X, value 1 if she is willing to wait only for X=40 or 50, value 3 if she is willing to wait only for X=50, and value 4 if she is never willing to wait. To measure how much farmers are willing to take risk in agriculture we asked: "In agriculture and livestock, would you say that you are someone who likes to take risks, or do you try to avoid risks? Please rate yourself between 0 and 10, where 0 means you never like to take risks, and 10 means you are always ready to take risks". To measure present-biasedness, we asked each respondent 2 hypothetical questions: (1) whether she would prefer earning 30k UGX today or Xk UGX in 1 month where X=30, 40 or 50; (2) we repeated this question changing the time frame to 6 and 7

Finally, to understand the relevance of credit constraints in technology adoption and the necessity to address multiple constraints simultaneously (Jack 2011), we randomly assigned half of the agriculture extension communities to a complementary BRAC microfinance program. Table A3 shows that the introduction of the microfinance program did not affect the take-up of improved seeds, indicating that credit constraints that can be alleviated by microcredit are not important in our context. The absence of an effect can be explained either by the absence of a credit constraint at baseline or, more realistically, by the fact that microfinance does not properly relax the credit constraint. As discussed in Field and Pande (2008), the rigid contract offered by microfinance institutions, i.e. clients repay loans in weekly installments beginning shortly after disbursement,[68] may limit investments in new technologies.

4. Social Connections and Program Delivery

4.1. Identification

To estimate the effect of social connections on program delivery, we restrict the sample to the 60 treatment clusters and the 3,042 farmers who reside there. We estimate the following specification:

$$y_{ic1} \quad = \quad \alpha + \eta y_{ic0} + \gamma^W W_i + \gamma^L L_i + X_c \delta + \varepsilon_{ic}$$

where y_{ic1} (y_{ic0}) is the outcome of interest for farmer i in cluster c at endline (baseline), $W_i = 1$ if farmer i knows the winning EW candidate at baseline and $L_i = 1$ if farmer i knows the losing EW candidate at baseline. X_c is a vector of controls that includes branch fixed effects and the stratification variables used for treatment allocation in Experiment 1. It also includes the distance in kilometers between the house of the respondent and the house of the winning EW to control for differences in targeting due to geographical proximity. We cluster standard errors at the cluster level (level of the EW selection randomization).

In comparison to the previous section, this section identifies the program effect on each group of farmers with the same connection status relative to the program effect on unconnected farmer but it does not identify the effect of the program as a whole.

The parameters of interest, γ^W and γ^L, measure the effect of being connected to the winning and losing candidate relatively to farmers connected to neither candidate. Note that $\widehat{\gamma^W} = s^W + u$ while $\widehat{\gamma^L} = s^L + u$ where s^W(or s^L) measure the effect of the social preferences of the selected EW on farmers connected to her (or to the non-selected EW) and u is the

months. A respondent is considered "present biased" if she prefers waiting 1 extra month to get more money in the future but not willing to wait today.

[68]BRAC follows this model: microfinance clients repay weekly starting from the week after the disbursement.

unobservable trait shared by all farmers connected to a candidate EW. Given that winning status is determined randomly, $(\widehat{\gamma^W} - \widehat{\gamma^L})$, which we report at the foot of the table, identifies the causal effect of social preferences from being connected to the winner relative to being connected to the loser, clean of unobservable traits shared by farmers connected to an EW candidate, that is $(\widehat{\gamma^W} - \widehat{\gamma^L}) = s^W - s^L$. The null hypothesis is that connections do not affect targeting $(\widehat{\gamma^W} - \widehat{\gamma^L}) = 0$.

While $\widehat{\gamma^W}$ and $\widehat{\gamma^L}$ do not identify social preferences when connected and unconnected farmers have different unobservable traits ($u \neq 0$), we can identify the social preferences "frontier" $s^W = \widehat{\gamma^W} - \widehat{\gamma^L} + s^L$, which, assuming $s^W \geq 0$ and $s^L \leq 0$ allows us to derive bounds for s^W and s^L. The graphical representation of the identification strategy is presented in Figure 2.

4.2. Results: Individual Outcomes

The Effect of Social Connections on Program Delivery

Table 6 shows that social connections shape the delivery of this program. Indeed, $\widehat{\gamma^W} - \widehat{\gamma^L}$ (reported at the foot of the table)[69] is positive and precisely estimated throughout so that relatively to those connected to the losing candidate, farmers who are connected to the winner benefit more. In particular, agents connected only to the winner are 6 and 7 percentage points more likely to have received advice and training from the EW respectively, 12 percentage points more likely to believe improved seeds are beneficial and 5 percentage points more likely to adopt BRAC seeds from the EW.

Interestingly, $\widehat{\gamma^W}$ is always positive while $\widehat{\gamma^L}$ is always negative and both are precisely estimated. This indicates that, relative to farmers connected to neither candidate, farmers connected to the selected EW are more likely to receive advice, training and adopt seeds, while farmers who are connected to the non-selected candidate are less likely.

The results are thus consistent with three sets of social preferences. At one extreme, the EW might put positive weight on the utility of farmers connected to them (altruism) and zero weight on the utility of farmers connected to the rival candidate. In this case, $\widehat{\gamma^L} = u \Rightarrow u < 0$, namely the unobservable traits of connected farmers are such that, absent social preferences, they are less likely to receive the program. This might be the case if connected farmers are less needy and the EW optimally targets the neediest. In this case $s^W = \widehat{\gamma^W} - \widehat{\gamma^L} > 0$, implying that farmers connected to the winner are more likely to receive the program than unconnected farmers although they should actually be less likely based on their traits alone.

At the other extreme, the EW might put zero weight on the utility of farmers connected

[69] We report both the coefficient and the standard errors. These are obtained by regressing outcome variables on "whether the respondent know the selected EW" controlling for the number of connections to potential candidates (0,1 or 2). This is simply a reparametrization of the specification.

to them and negative weight on the utility of farmers connected to the rival candidate (spite). In this case, $\widehat{\gamma^W} = u \Rightarrow u > 0$, namely the unobservable traits of connected farmers are such that, absent social preferences, they are more likely to receive the program. This might be the case if connected farmers are more receptive and more willing to adopt. In this case $s^L =-(\widehat{\gamma^W} - \widehat{\gamma^L}) < 0$ implying that farmers connected to the loser are less likely to receive the program than unconnected farmers although they should actually be more likely based on their traits alone.

For intermediate parameter values, the EW exhibits both altruism for her connected farmers and spite for those connected to her rival, and these preferences must satisfy $s^W = \widehat{\gamma^W} - \widehat{\gamma^L} + s^L$. For instance, for the adoption of improved seeds from the EW, the first case implies s^W =0.5, $s^L = 0$, the second case s^W =0, $s^L = -0.5$, in the third $s^W \in (0, 0.5)$, $s^L \in (-0.5, 0)$.

The fact that social connections predict adoption of BRAC seeds from the EW but not from other BRAC sources (the PAs) suggests that $u = 0$ and $\widehat{\gamma^W} = s^W$; $\widehat{\gamma^L} = s^L$. While EWs may have an incentive not to follow BRAC's guidelines, i.e. target their friends at the expense of farmers with higher returns to adoption, there is no reason to believe that the PAs have the same incentive. They are indeed located in the branch office, a more urban area of the district and outside the villages of our experiment, and have thus no personal connection with most of the sample farmers at baseline. As PAs are equally likely to target connected and unconnected farmers (Column 6, Table 6), we can conclude that these farmers have similar unobserved characteristics ($u = 0$). This provides suggestive evidence that the coefficients we estimate, $\widehat{\gamma^W}$ and $\widehat{\gamma^L}$, measure the causal effect of a connection to the winning and losing candidate respectively.

To provide further evidence on the existence of spite, we exploit variation in the strength of social preferences that is uncorrelated with u. We exploit the intuition that the EW is more likely to have negative social preferences towards farmers connected to her rival (i.e. $\widehat{\gamma^L}< 0 \Rightarrow s^L < 0$) if these are socially distant from her, while u should be the same regardless of social distance. While we only measure degree 1 connections between farmers and EW candidates we can proxy degree 2 connections by measuring whether the EWs are connected to each other (in which case the other EW acts as a link) or whether the two groups of farmers -those connected to the winner and those connected to the loser- overlap (in which case the overlapping farmers can serve as link).

We have three measures of connections between the two candidates: whether both EW candidates belong to the local farmer association, live in the same community and know each other.[70] To measure network overlap, we use a dummy that equals one if the percentage of

[70]Pairwise correlations between these measures equal: 31% for belonging to the same farm association and living in the same cluster, 26% for belonging to the same association and knowing the other candidate, and 24% for living in the same community and knowing the other candidate. Information on whether the two EW

households who know both candidates is above the cluster-level median. We then estimate:

$$
\begin{aligned}
y_{ic1} \quad = \quad & \alpha + \eta y_{ic0} + \beta 1(Dummy_c = 1) + \gamma^{W0} W_i * 1(Dummy_c = 0) + \gamma^{L0} L_i * 1(Dummy_c = 0) \\
& + \gamma^{W1} W_i * 1(Dummy_c = 1) + \gamma^{L1} L_i * 1(Dummy_c = 1) + X_c \delta + \varepsilon_{ic}
\end{aligned}
$$

where $Dummy_c = 1$ if the degree between the selected EW and the farmers connected to the non-selected EW in cluster c is likely to be 2, while 0 if the degree is likely to be more than 2. Errors are clustered at the level of the EW randomization, i.e. the cluster.

The differences (γ^{L0} - γ^{L1}) and (γ^{W0} - γ^{W1}) test whether the effect of social connections varies with social distance.[71] ($\gamma^{W1} - \gamma^{L1}$) estimates the effect of social connections when the selected EW and the farmers connected to the non-selected EW are likely to have a degree 2. ($\gamma^{W0} - \gamma^{L0}$) estimates the effect of social connections when the selected EW and the farmers connected to the non-selected EW are likely to have a degree higher than 2.

Table 7 shows that social connections matter for program delivery only in the latter case ($Dummy_c = 0$). With training, advice or adoption as an outcome variable, ($\widehat{\gamma^{W0}} - \widehat{\gamma^{L0}}$) is indeed positive and significant (see foot of the Table, p-values 2), while ($\widehat{\gamma^{W1}} - \widehat{\gamma^{L1}}$) is not statistically different than zero (p-values 1).

The findings also show that $\widehat{\gamma^{L0}} < 0$ and $\widehat{\gamma^{L1}} \approx 0$, in line with negative social preferences towards socially distant farmers. Reassuringly, connections to the winning EW candidate seem to matter irrespective of the social distance between the EW and the farmers connected to the non-selected EW: $\widehat{\gamma^{W0}} > 0$ and $\widehat{\gamma^{W1}} > 0$, i.e. agents always put a positive weight on the utility of their friends. These results are robust to the four alternative measures of social distance.

Three appendix tables present further results and robustness checks. In Table A4, we benchmark against total program effect for the variables that take value zero in the control group, i.e. "receiving advice/ training from BRAC" and "adoption of BRAC seeds". The results suggest that agents connected to the winning EW benefit the most, isolated agents benefit somewhat, while agents connected to the losing EW do not benefit from the program. In Table A5, we replicate the social network analysis without assuming linearity in the number of connections, i.e. the effect of knowing both candidates is allowed to differ from the sum of the effect of knowing the selected and the non-selected candidate. Results remain similar. Finally, Table A6 shows that our results are robust to a less conservative measure of connection, i.e. whether a farmer typically discusses about agriculture with the EW candidate.

candidates know each other is available only for 23 clusters out of 60 (available only if at least one of the two candidates is included in the baseline survey that asks about connections to EW candidates).

[71] This holds under the assumption that the average unobserved characteristics u is equal in clusters where $Dummy_c = 1$ from clusters where $Dummy_c = 0$.

The Effect of Social Connections on Performance

The previous section indicates that agents only connected to the candidate who ran but was not selected are less likely to adopt BRAC seeds. We analyze next whether these farmers can compensate by taking other actions so that total production is unchanged, e.g. buy improved seeds from non BRAC sources, work more hours, cultivate more land. Results are reported in Table 8.

Column 1 indicates that agents connected to non-selected EW are more than twice as likely to buy improved seeds from local markets and shops (3 percentage points increase) than agents connected to the selected EW. They also spend 97 more hours per cropping season working on their land (19% higher). Relative to unconnected farmers, farmed connected to the losing candidate work 36 hours (7%) more while agents connected to the winning candidate work 61 hours (12%) less. In other words, the probability of adopting BRAC improved seeds at endline seems to be negatively related with the number of hours worked on agriculture. No differences are found on the number of acres of land cultivated.

Ultimately, it is important to understand whether working more hours and buying more non-BRAC seeds helps farmers compensate for the lower adoption of BRAC seeds in terms of agriculture productivity. Results indicate that it does not: the number of kilograms of beans and maize (the two main crops in the areas of the study) produced per acre of land and hour worked is 0.47 and 0.54 lower for agents connected only to the loser vs. only to the winner. Rate of returns are also lower: being connected to the non-appointed person decreases the returns by 50%.

Who is chosen as the delivery agent and who is not chosen but applied strongly affects which households benefit from the program: friends of the selected delivery agents benefit the most while friends of the non-selected agent benefit the least. Relative to unconnected farmers, yields, output and rate of returns are 50%, 33% and 40% higher for farmers who know the winner while they are 30%, 10%, 10% lower for those who know the lose". This indicates that knowing the right person helps while knowing the wrong one hurts.

4.3. Results: Aggregate Outcomes

Given earlier results that social connections matter only when EW candidates are socially distant and/or network overlap is small, we now exploit natural variation in group sizes to quantify the effect of social connections on village wide adoption rates. Intuitively these should be higher when the size of the in-group is large and when the out-group is small because the former is actively targeted and the latter is shunned. Although this variation is not orthogonal to other village-level characteristics and we cannot identify causal effects, this helps us get a sense of the magnitude. As these are likely to be biased upward, a small magnitude indicates that social connections do not matter much to explain aggregate outcomes.

Figure 3 shows cluster-level variation in group sizes, with the percentage of respondents who know the non-selected EW candidate at baseline on the Y axis and the percentage of respondents who know the selected EW candidate on the X axis. The figure indicates that there is a significant amount of variation in social connections across clusters.

Figure 4 shows large differences in program delivery across clusters, e.g nobody adopts BRAC improved seeds in 10 clusters out of 60, while adoption is above 10% in other 8 clusters. Similarly, the number of farmers who received advice or training vary significantly from one cluster to another: in 25 clusters no-one receives those services, while in 10 clusters more than 10% of the farmers receives them.

We estimate the effect of social connections by regressing aggregate outcome variables on the percentage of households in the cluster who know the winning EW candidate and the percentage who know the losing candidate (see Table 9). Keeping size of the out-group constant, one inter-quartile range increase in the in-group is associated with a 6% increase in aggregate training rate, a 5% increase in the aggregate adoption of BRAC seeds and a 7% decrease in the adoption of lower-quality non-BRAC seeds. All these effects are precisely estimated. In contrast, by keeping the size of the in-group constant, an additional inter-quartile range in the out-group decreases training rate and adoption of BRAC seeds by 4% and 3% respectively, while the adoption of non-BRAC seeds increases by 8%. Table A7 replicates Table 7 at the aggregate level. Results are consistent with before: social connections matter only when EW candidates are socially distant and/or network overlap is small. This is suggestive evidence that the size of both the in-group and out-group shape aggregate adoption. More work is needed to assess causality.

5. Conclusions

We study how social connections shape the delivery of an agriculture extension program. In each community, we identified two candidates for the position and randomly appointed one of the two. We find that both the connections to the selected extension agent and to the non-selected agent matter: relatively to unconnected farmers, farmers connected only to the selected delivery agents are more likely to benefit from the program while farmers connected only to the non-selected agent are shunned.

We show that these results are consistent with in-group favoritism and out-group discrimination: the negative effects of the connections to the non-appointed candidate matter only when the social distance between the selected delivery agent and the farmers connected to the non-selected candidate is above degree two. This indicates that agents not only put a positive weight on the utility of their friend, they also put a negative weight on the utility of their rival's friends when these are socially distant from them. As a consequence, both positive and negative social preferences shape program delivery.

Although the agriculture extension program we analyze is representative of many other community-based development programs, two considerations about the specific features of our context are important to inform the external validity of the results. First, the setting we study is one with excess supply of delivery agents. With limited labor supply, negative social preferences might be less relevant: connections to the non-selected candidate do not matter as this person may simply not exist. Second, the total profits that BRAC extension workers make from the sale of improved seeds are relatively low. In the presence of stronger incentives, agents may not be willing to bear a cost to favor their friends or damage their friend's rival, i.e. they may not give up on a high sales commission. As illustrated in Bandiera et al. (2009), performance-based incentives may change the agent's stake in the delivery of public goods.

The findings of this paper have key policy implications for the selection of delivery agents. In their recruitment decisions, organizations should take into account social connections of the beneficiaries with all candidates for the position. Appointing the most-connected agent in the village may not always be the optimal solution: if this person has a "rival" who is herself well-connected and with little network overlap, then the organization could benefit from recruiting someone less popular. Because who is chosen as delivery agent affects which beneficiaries are then targeted, improving the selection process of these agents has the potential to substantially improve the delivery of development programs.

FIGURE 1: EMPIRICAL DESIGN

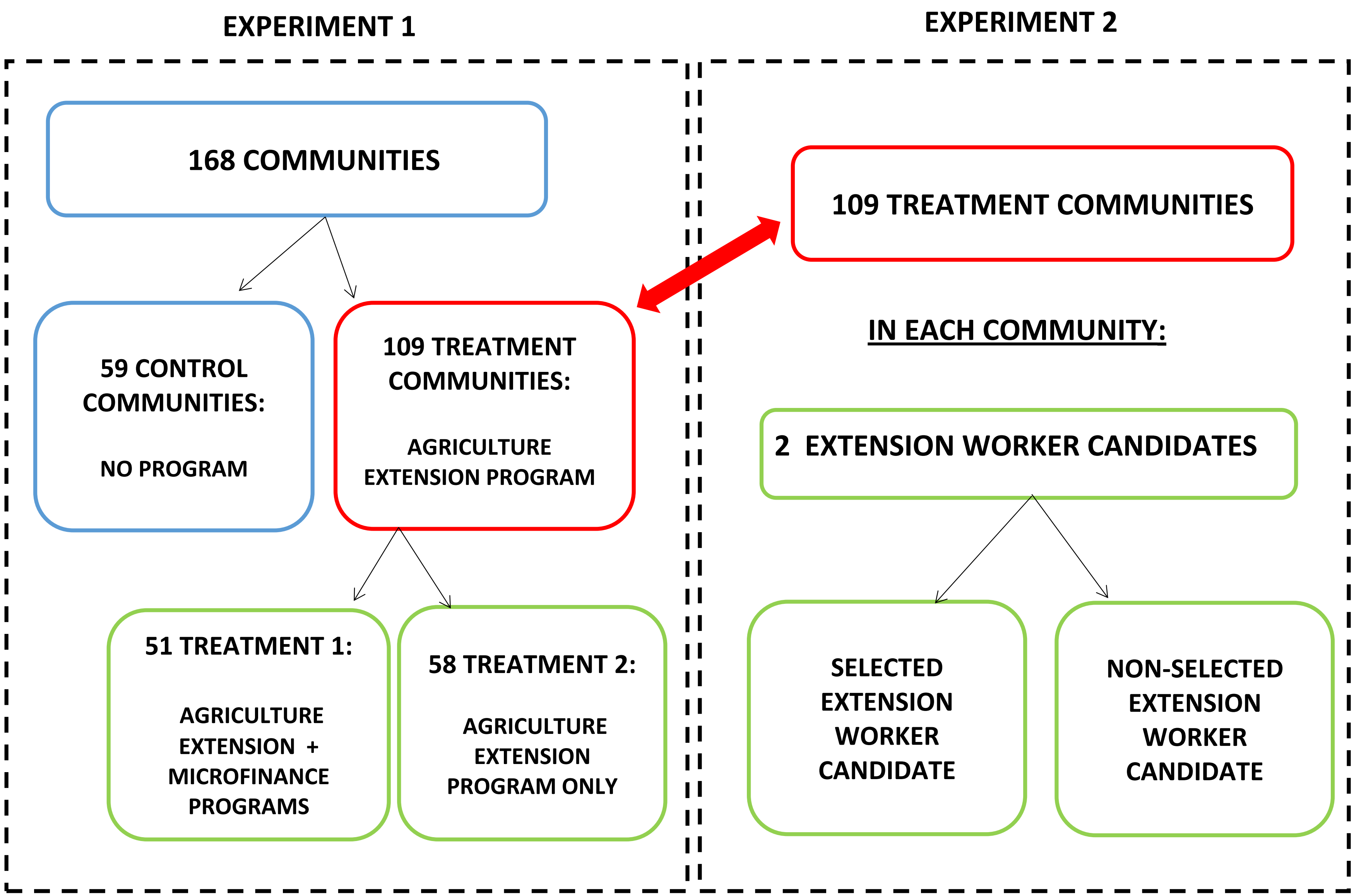


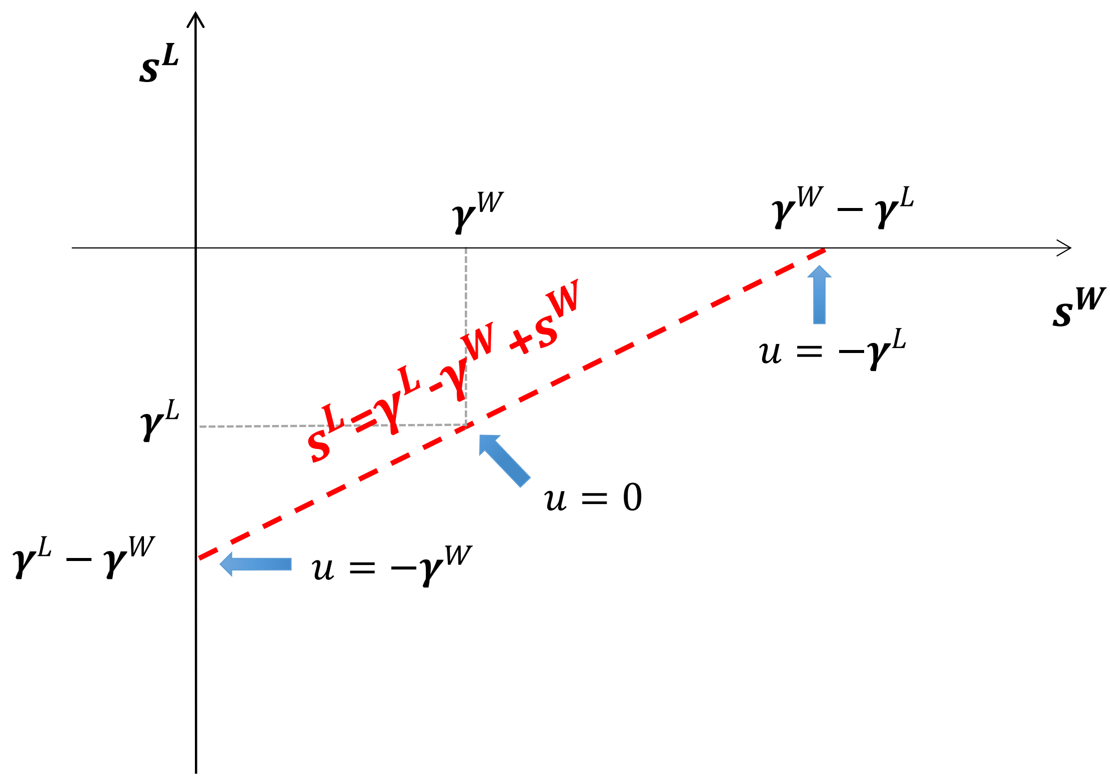
FIGURE 2: GRAPHICAL REPRESENTATION OF IDENTIFICATION STRATEGY

Assumptions: $\gamma^L = s^L + u\,;\quad \gamma^W = s^W + u\,;\quad s^L \leq 0\,;\; s^W \geq 0$

γ^L and $\gamma^W \rightarrow$ estimated effect of connection to losing / winning candidate

s^L and $s^W \rightarrow$ true social preferences

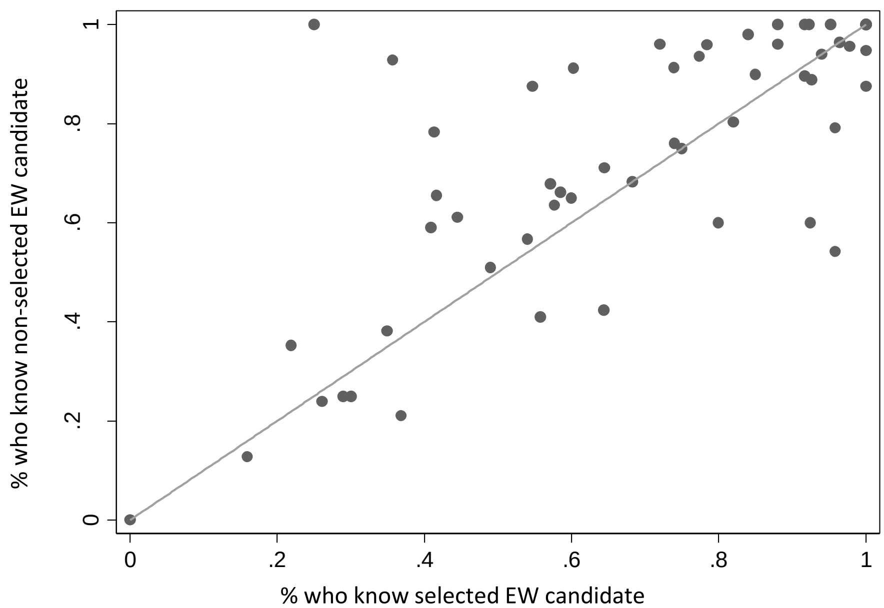
$u \rightarrow$ measurement error or omitted variable



Note: This figure illustrates the true effect of "being connected to the losing candidate" on the Y axis and the true effect of "being connected to the winning candidate on the X axis". The possible combinations of these two effects are illustrated by the dashed red line. The intersection between the dashed red line and the X axis represents the extreme case in which the true effect of being connected to the winning candidate is 0. The intersection between the Y axis and the dashed red line represents the other extreme: no effect of social connection to the losing candidate. In the specific case in which u=0 (no omitted variables and measurement error), then the estimated coefficients equal the true effects.

FIGURE 3: CLUSTER-LEVEL VARIATION IN SOCIAL CONNECTIONS TO EXTENSION WORKERS' CANDIDATES

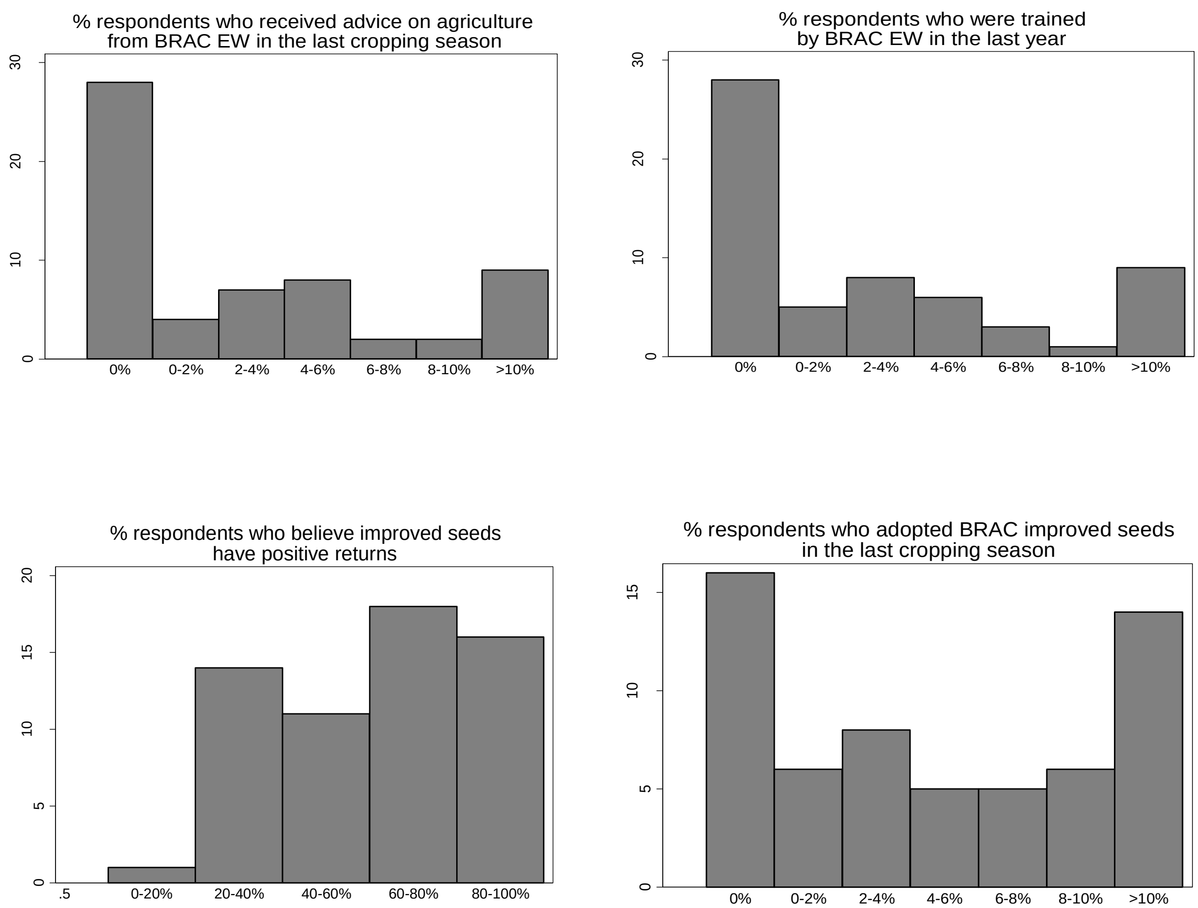
Sample: treatment communities only



Note: The figure plots, for each cluster, the % of respondents who know the non-selected EW candidate at baseline on the Y axis and the % of respondents who know the selected EW candidate at baseline on the X axis. Each dot represents one of the 60 treatment clusters. Any dot on the 45 degree line has the same % of respondents who know the selected and non selected EW candidate ."Knows (non) selected EW candidate" equals 1 if the respondents knows this person independently of whether she knows the other candidate or not.

FIGURE 4: CLUSTER-LEVEL VARIATION IN PROGRAM DELIVERY

Sample: treatment communities only



Note: The figures show the endline cluster-level variation in the percentage of respondent who received BRAC services, who believe improved seeds have positive returns and who adopted improved seeds at endline. "Received advice on agriculture from EW " equals 1 if the respondent received an advice from the BRAC extension worker at any time during the past season (during trainings, household visits, etc.). "Was trained by EW " equals 1 if the respondent attended at least one training organized by the extension worker in the past year. Beliefs about the returns of improved seeds equal zero for agents who do not know about the existence of improved seeds. Respondents who adopted improved seeds at endline were asked the sources of these seeds. Sources include BRAC extension worker, BRAC branch office, markets, shop or government extension worker .

TABLE 1: BALANCE CHECKS ON HOUSEHOLD CHARACTERISTICS AT BASELINE

Sample: treatment and control communities

	Treatment 1: Agriculture+ Microfinance Programs	Treatment 2: Agriculture Program only	Control	p-values T1= Control	p-values T2 = Control	p-values T1 = T2
Number of observations	*1490*	*1552*	*1650*			
Advice, knowledge and adoption of improved seeds:						
Knows about existence of improved seeds	0.931	0.926	0.945	0.401	0.312	0.829
	(0.254)	(0.261)	(0.228)			
Believes improved seeds have positive returns	0.680	0.724	0.760	0.078	0.378	0.312
	(0.467)	(0.447)	(0.427)			
Has ever adopted improved seeds	0.308	0.291	0.368	0.274	0.131	0.735
	(0.462)	(0.455)	(0.482)			
Agriculture activity in the last cropping season:						
Hours worked	488.653	452.767	504.220	0.623	0.098	0.161
	(341.302)	(309.705)	(349.616)			
Acres of land cultivated	1.205	1.278	1.109	0.164	0.026	0.362
	(1.360)	(1.440)	(1.164)			
Engaged in commercial agriculture	0.517	0.583	0.515	0.951	0.019	0.038
	(0.500)	(0.493)	(0.500)			
Kg of beans produced per acre cultivated and hour worked	0.862	0.915	0.786	0.482	0.159	0.652
(conditional on cultivating beans)	(1.944)	(1.872)	(1.720)			
Kg of maize produced per acre cultivated and hour worked	0.914	0.886	0.899	0.927	0.922	0.852
(conditional on cultivating maize)	(1.776)	(1.701)	(1.942)			
Output value per acre cultivated and hour worked	2.488	3.209	2.529	0.931	0.196	0.197
(in thousand UGX)	(8.059)	(9.926)	(7.595)			
Rate of returns: (output value - input value) / input value	5.498	7.436	5.696	0.886	0.305	0.254
	(24.937)	(33.425)	(22.418)			
Other characteristics:						
Acres of land owned	2.156	2.214	2.044	0.511	0.328	0.719
	(3.571)	(3.216)	(3.746)			
Total number of assets owned	18.268	18.265	18.819	0.380	0.369	0.995
	(8.309)	(8.671)	(8.309)			
Completed primary school	0.452	0.479	0.437	0.537	0.072	0.288
	(0.498)	(0.500)	(0.496)			

Notes: Columns 1, 2 and 3 show means and standard deviations in parentheses. Column 4, 5 and 6 reports the p-value of the test of equality of means based on standard errors clustered at the community level. The p-values are robust to controlling for stratification variables. Beliefs about the returns of improved seeds equal zero for agents who do not know about the existence of improved seeds. "Engaged in commercial agriculture" equals 1 if the ratio between revenues and output value is above the median of 5%. "Kg of maize (beans) produced per hour worked and acre cultivated" calculates the production of maize (beans) in kg per acre and hour worked dedicated to maize (beans) cultivation. This variable is missing for agents who did not grow maize (beans) in the last cropping season. The total output value equals the quantity of each crop produced multiplied by the median market price of each crop unit in the branch. Quantity and value of production are truncated above and below two standard deviations from the mean. Results are robust to alternative cleaning strategies. "Rate of returns" divide the difference between the total production and input value by the input value. The input value includes expenses incurred for buying inputs, land and labor costs (hiring workers and own opportunity cost of time). The opportunity cost of working on one's own plot of land equals hours worked on own land multiplied by the branch median hourly wage paid to work on someone else land. Result are robust to considering the median hourly salary for working on one's own non-farm business. Number of assets owned calculates the total number of personal and business assets owned by the household.

TABLE 2: CONNECTIONS TO SELECTED AND NON-SELECTED EXTENSION WORKER CANDIDATES AT BASELINE

<u>Sample: treatment communities only</u>

Selected EW candidate	**Non-selected EW candidate**		
	Knows who the EW candidate is	Does not know who the EW candidate is	Total
Knows who the EW candidate is	**53%** N=1,384	**10%** N=267	**63%** N=1,651
Does not know who the EW candidate is	**15%** N=401	**22%** N=557	**37%** N=958
Total	**68%** N=1,785	**32%** N=824	**100%** N=2,609

Note: Table shows percentage and number of respondents in treatment clusters who know the selected and/ or non-selected EW candidates are at baseline. The randomization of the EW selection takes place within the treatment communities only and is done at the cluster level. Each cluster is composed of 1, 2 or 3 treatment communities.

TABLE 3: BALANCE CHECKS ON CHARACTERISTICS OF EXTENSION WORKER CANDIDATES AT BASELINE

Sample: treatment communities only

	Selected EW candidate	Non-selected EW candidate	Average farmer in treatment communities	p-value [Selected EW= Non-selected EW]
Number of observations	*60*	*60*	*3042*	
Measures of connections, as reported by households				
% respondents who know the EW candidate	0.703	0.738	-	0.476
	(0.271)	(0.270)	-	
% respondents who typically discuss agriculture with the EW candidate	0.524	0.548	-	0.651
	(0.286)	(0.292)	-	
Belongs to the community farm association	0.769	0.691	-	0.366
	(0.425)	(0.466)	-	
Belongs to BRAC microfinance group	0.350	0.383	-	0.708
	(0.481)	(0.490)	-	
Agriculture activity in the last cropping season:				
Hours worked	564.870	529.107	497.073	0.301
	(160.376)	(198.752)	(136.694)	
Acres of land cultivated	1.583	1.763	1.155	0.430
	(1.086)	(1.359)	(0.307)	
Engaged in commercial agriculture	0.875	1.000	0.532	0.334
	(0.354)	(0.000)	(0.161)	
Other characteristics:				
Acres of land owned	2.949	2.873	2.004	0.864
	(2.508)	(2.313)	(0.697)	
Total number of assets owned	42.817	39.550	18.630	0.565
	(32.333)	(29.670)	(2.944)	
Completed primary school	0.617	0.533	0.483	0.360
	(0.490)	(0.503)	(0.137)	

Notes: Columns 1 and 2 show means and standard deviations in parentheses for the selected and the non-selected EW candidate respectively. Column 3 shows the means and standard deviations in parentheses for the average farmer in treatment communities (treatment 1 or 2). Column 4 reports the p-value of the test of equality of means in column 1 and 2 based on robust standard errors. The randomization of the EW selection takes place within the treatment communities only and is done at the cluster level. Each cluster is composed of 1, 2 or 3 treatment communities. "Engaged in commercial agriculture" equals 1 if the ratio between revenues and output value is above the median of 5%. Number of assets owned calculates the total number of personal and business assets owned by the household.

TABLE 4: BALANCE CHECKS ON HOUSEHOLD CHARACTERISTICS BY CONNECTIONS TO EXTENSION WORKER CANDIDATES

Sample: treatment communities only

	Knows selected EW candidate	Knows non-selected EW candidate	Knows none of the EW candidates	p-value: selected EW = non selected EW	p-value: selected EW = none	p-value: non selected EW = none
Number of observations	*1651*	*1785*	*557*			
Advice, knowledge and adoption of improved seeds:						
Knows about existence of improved seeds	0.935	0.926	0.942	0.291	0.443	0.048
	(0.247)	(0.261)	(0.233)			
Believes improved seeds have positive returns	0.716	0.714	0.705	0.705	0.174	0.280
	(0.451)	(0.452)	(0.457)			
Has ever adopted improved seeds	0.357	0.351	0.228	0.663	0.857	0.752
	(0.479)	(0.477)	(0.420)			
Agriculture activity in the last cropping season:						
Hours worked	481.576	473.722	471.840	0.766	0.127	0.006
	(354.844)	(343.467)	(251.654)			
Acres of land cultivated	1.213	1.229	1.197	0.859	0.340	0.148
	(1.319)	(1.445)	(0.934)			
Engaged in commercial agriculture	0.549	0.559	0.541	0.278	0.371	0.727
	(0.498)	(0.497)	(0.499)			
Kg of beans produced per acre cultivated and hour worked	0.957	0.969	0.687	0.879	0.102	0.108
(conditional on cultivating beans)	(1.982)	(2.035)	(1.554)			
Kg of maize produced per acre cultivated and hour worked	1.014	1.039	0.429	0.756	0.012	0.021
(conditional on cultivating maize)	(1.870)	(1.940)	(0.714)			
Output value per acre cultivated and hour worked	3.366	3.243	1.443	0.653	0.067	0.073
(in thousand UGX)	(9.890)	(10.020)	(3.107)			
Rate of returns: (output value - input value) / input value	7.788	7.408	3.345	0.544	0.133	0.301
	(33.384)	(32.180)	(20.435)			
Other characteristics:						
Acres of land owned	2.079	2.123	2.205	0.909	0.274	0.235
	(2.789)	(3.185)	(2.206)			
Total number of assets owned	18.143	18.127	19.172	0.964	0.231	0.219
	(8.387)	(8.448)	(8.782)			
Completed primary school	0.446	0.456	0.482	0.278	0.287	0.955
	(0.497)	(0.498)	(0.500)			

Notes: "Knows (non) selected EW candidate" equals 1 if the respondents knows this person independently of whether she knows the other candidate or not. Columns 1 and 2 show means and standard deviations in parentheses. Column 3 reports the p-value of the test of equality of means based on standard errors clustered at the community level. s and standard deviations in parentheses. Column 4, 5 and 6 report the p-values of tests of equality of means based on standard errors clustered at the community level. The p-values are robust to controlling for stratification variables or to clustering at the cluster level. Beliefs about the returns of improved seeds equal zero for agents who do not know about the existence of improved seeds. "Engaged in commercial agriculture" equals 1 if the ratio between revenues and output value is above the median of 5%. "Kg of maize (beans) produced per hour worked and acre cultivated" calculates the production of maize (beans) in kg per acre and hour worked dedicated to maize (beans) cultivation. This variable is missing for agents who did not grow maize (beans) in the last cropping season. The total output value equals the quantity of each crop produced multiplied by the median market price of each crop unit in the branch. Quantity and value of production are truncated above and below two standard deviations from the mean. Results are robust to alternative cleaning strategies. "Rate of returns" divide the difference between the total production and input value by the input value. The input value includes expenses incurred for buying inputs, land and labor costs (hiring workers and own opportunity cost of time). The opportunity cost of working on one's own plot of land equals hours worked on own land multiplied by the branch median hourly wage paid to work on someone else land. Result are robust to considering the median hourly salary for working on one's own non-farm business. Number of assets owned calculates the total number of personal and business assets owned by the household.

TABLE 5: THE EFFECTS OF THE AGRICULTURE EXTENSION PROGRAM

Sample: treatment and control communities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
				Adopted improved seeds in the last cropping season from...			Agriculture activity in the last cropping season						
	Received advice on agriculture from EW in the last cropping season	Was trained by EW in the last year	Believes improved seeds have positive returns	BRAC EW	Other BRAC source	Non BRAC source	Hours worked	Acres of land cultivated	Engaged in commercial agriculture	Kg of beans produced per acre cultivated and hour worked	Kg of maize produced per acre cultivated and hour worked	Output value per acre cultivated and hour worked (thousand UGX)	Rate of returns
Treatment	0.0361***	).0379***	-0.0422	0.0277***	0.0341***	0.0195	-17.6430	0.0866	0.0550*	-0.0740	0.0100	0.0470	1.1503**
	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(18.73)	(0.07)	(0.03)	(0.08)	(0.12)	(0.11)	(0.51)
Observations	4,336	4,339	4,000	4,269	4,269	4,285	3,493	4,246	4,259	2,505	982	3,299	3,150
R-squared	0.018	0.021	0.142	0.015	0.025	0.085	0.103	0.068	0.062	0.010	0.025	0.013	0.017
Mean Dep. Var. in Control	0.001	0.000	0.685	0.000	0.001	0.077	541.400	1.163	0.551	0.697	0.555	1.017	2.059

Note: OLS regressions. Errors clustered at the community level. *** p<0.01, ** p<0.05, * p<0.1. Treatment communities=1 if communities are in Treatment 1 or 2 (with ot without complementary microfinance program). Regressions control for community stratification variables (branch fixed effects, percentage of farmers, number of habitants, distance to closest market). Columns 4-6 control for "ever adoption of improved seeds" while the rest of the columns control for the baseline value of the outcome variable (ANCOVA). "Received advice on agriculture from EW " equals 1 if the respondent received an advice from the BRAC extension worker at any time during the past season (during trainings, household visits, etc.). "Was trained by EW " equals 1 if the respondent attended at least one training organised by the extension worker in the past year. Beliefs about the returns of improved seeds equal zero for agents who do not know about the existence of improved seeds. Respondents who adopted improved seeds at endline were asked the sources of these seeds. Sources include BRAC extension worker, BRAC branch office, markets, shop or government extension worker. "Engaged in commercial agriculture" equals 1 if the ratio between revenues and output value is above the median of 5%. "Kg of maize (beans) produced per hour worked and acre cultivated" calculates the production of maize (beans) in kg per acre and hour worked dedicated to maize (beans) cultivation. This variable is missing for agents who did not grow maize (beans) in the last cropping season. The total output value equals the quantity of each crop produced multiplied by the median market price of each crop unit in the branch. This variable is truncated above and below two standard deviations from the mean. Results are robust to alternative cleaning strategies. "Rate of returns" divide the difference between the total production and input value by the input value. The input value includes expenses incurred for buying inputs, land and labor costs (hiring workers and own opportunity cost of time). The opportunity cost of working on one's own plot of land equals hours worked on own land multiplied by the branch median hourly wage paid to work on someone else land.

TABLE 6: THE EFFECTS OF SOCIAL CONNECTIONS ON PROGRAM DELIVERY

Sample: treatment communities only

	(1)	(2)	(3)	(4)	(5)
				Adopted improved seeds in the last cropping from…	
	Received advice on agriculture from EW in the last cropping season	Was trained by EW in the last year	Believes improved seeds have positive returns	EW	Other BRAC source
Knows selected EW candidate	0.0266*	0.0376**	0.0528*	0.0300**	0.0016
	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)
Knows non-selected EW candidate	-0.0295*	-0.0305*	-0.0684**	-0.0225*	-0.0146
	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)
Observations	2,411	2,410	2,213	2,369	2,369
R-squared	0.016	0.022	0.166	0.015	0.029
Mean Dep. Var. for "Knows none"	0.020	0.014	0.426	0.012	0.012
p-value [knows both = knows none]	0.798	0.495	0.601	0.486	0.158
Knows selected EW - knows non-selected EW	0.0561*	0.0682**	0.1212***	0.0525**	0.0162
	(0.03)	(0.03)	(0.04)	(0.02)	(0.02)

Notes: Sample restricts to treatment communities only and the omitted group are those who know none of the EW potential candidates. Foot of the table reports coefficient for "Knows selected EW - knows non selected-EW" estimated from a regression in which outcome variable is regressed on "Knows selected EW" controlling for number fo connections to candidates. All the regressions are OLS regressions. Errors clustered at the Cluster level. *** p<0.01, ** p<0.05, * p<0.1. Regressions control for community stratification variables (branch fixed effects, percentage of farmers, number of habitants, distance to closest market) and for the distance in km between the house of the household and the house of the selected EW. Results are robust to dropping stratification variables. Columns 4-6 control for "ever adoption of improved seeds" while the rest of the columns control for the baseline value of the outcome variable (ANCOVA). "Knows (non) selected EW candidate" equals 1 if the respondents knows this person independently of whether she knows the other candidate or not. "Received advice on agriculture from EW " equals 1 if the respondent received an advice from the BRAC extension worker at any time during the past season (during trainings, household visits, etc.). "Was trained by EW " equals 1 if the respondent attended at least one training organized by the extension worker in the past year. Beliefs about the returns of improved seeds equal zero for agents who do not know about the existence of improved seeds. Respondents who adopted improved seeds at endline were asked the sources of these seeds. Non BRAC sources are either PAs or CLPs.

TABLE 7: THE EFFECTS OF SOCIAL CONNECTIONS ON PROGRAM DELIVERY BY SOCIAL DISTANCE

Sample: treatment communities only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent Variable →	Received advice on agriculture from EW in the last season	Was trained by EW in the last year	Adopted improved seeds in the last cropping season from EW	Received advice on agriculture from EW in the last season	Was trained by EW in the last year	Adopted improved seeds in the last cropping season from EW	Received advice on agriculture from EW in the last season	Was trained by EW in the last year	Adopted improved seeds in the last cropping season from EW	Received advice on agriculture from EW in the last season	Was trained by EW in the last year	Adopted improved seeds in the last cropping season from EW
Dummy →	EW candidates are both part of the local farm association			EW candidates live in the same community			EW candidates know each other			% of households who know both EW candidates is above median		
1(**Dummy**=1)	-0.0023	-0.0049	-0.0082	-0.0152	-0.0078	-0.0046	-0.0316	-0.0201	-0.0287	-0.0516***	-0.0514***	-0.0361***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Knows selected EW * 1(**Dummy**=1)	0.0301	0.0400	0.0344	0.0430	0.0485*	0.0396*	0.0119	0.0188	0.0089	0.0440**	0.0525**	0.0464**
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)
Knows non-selected EW * 1(**Dummy**=1)	-0.0135	-0.0141	-0.0056	-0.0189	-0.0114	-0.0023	0.0101	0.0051	0.0244**	-0.0007	0.0026	-0.0004
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Knows selected EW * 1(**Dummy**=0)	0.0200	0.0322**	0.0229**	0.0179	0.0296*	0.0196	0.0628	0.0616	0.0604**	0.0222	0.0331*	0.0239*
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.04)	(0.04)	(0.03)	(0.02)	(0.02)	(0.01)
Knows non-selected EW * 1(**Dummy**=0)	-0.0452**	-0.0468**	-0.0398**	-0.0479*	-0.0513**	-0.0415**	-0.0647**	-0.0583	-0.0574***	-0.0336*	-0.0360**	-0.0274*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.02)	(0.01)
Observations	2,411	2,410	2,369	2,306	2,307	2,267	1,138	1,138	1,120	2,411	2,410	2,369
R-squared	0.022	0.027	0.022	0.021	0.030	0.027	0.032	0.042	0.043	0.018	0.024	0.017
Mean Dep. Var. for "Knows none"	0.020	0.014	0.012	0.020	0.014	0.012	0.020	0.014	0.012	0.020	0.014	0.012
(1) p-value [knows EW = knows non EW] when Dummy=1	0.342	0.241	0.316	0.149	0.157	0.116	0.960	0.713	0.371	0.135	0.100	0.121
(2) p-value [knows EW = knows non EW] when Dummy=0	0.061	0.023	0.008	0.070	0.024	0.041	0.064	0.113	0.010	0.102	0.044	0.061
(3) p-value [knows EW & Dum1 = knows EW & Dum0]	0.724	0.295	0.642	0.424	0.557	0.085	0.221	0.104	0.001	0.132	0.061	0.350
(4) p-value [knows non EW&Dum1 = knows non EW&Dum0]	0.322	0.791	0.189	0.349	0.185	0.455	0.046	0.335	0.088	0.385	0.467	0.155
# clusters with Dummy=1		29			33			16			30	
# clusters with Dummy=0		29			27			7			30	

Notes: Sample restricts to treatment communities only. OLS regressions. Errors clustered at the Cluster level. *** p<0.01, ** p<0.05, * p<0.1. Regressions control for community stratification variables (branch fixed effects, percentage of farmers, number of habitants, distance to closest market) and for the distance in km between the house of the household and the house of the selected EW. Results are robust to dropping stratification variables. Columns with adoption as outcome variables control for "ever adoption of improved seeds" while the rest of the columns control for the baseline value of the outcome variable (ANCOVA). The "dummy" variable in the right hand side of the regression is indicated in the box on the top of the table. In the first three columns, the dummy equals 1 if both EW candidates belong to the local farmer association. In columns 4-6, the dummy equals 1 if the EW candidates live in the same community. In columns 7-9, dummy equals 1 if the EW candidates know each other (they are either friends, relatives, neighbours, or are in any other type of relationship). Note that data on whether EW candidates know each other is available for only a subsample of the treatment clusters. In columns 10-12, the "dummy" variable equals 1 if the percentage of households who know both the selected and non selected candidates is above the cluster-level median of 67%. "Received advice on agriculture from EW " equals 1 if the respondent received an advice from the BRAC extension worker at any time during the past season (during trainings, household visits, etc.). "Was trained by EW " equals 1 if the respondent attended at least one training organised by the extension worker in the past year. "Knows (non) selected EW candidate" equals 1 if the respondents knows this person independently of whether she knows the other candidate or not.

TABLE 8: THE EFFECTS OF SOCIAL CONNECTIONS ON PERFORMANCE

Sample: treatment communities only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		Agriculture activity in the last cropping season						
	Adopted improved seeds in the last cropping from non BRAC source	Hours worked	Acres of land cultivated	Engaged in commercial agriculture	Kg of beans produced per acre cultivated and hour worked	Kg of maize produced per acre cultivated and hour worked	Output value per acre cultivated and hour worked (in thousand UGX)	Rate of returns
Knows selected EW candidate	0.0060	-61.9686***	-0.0684	-0.0001	0.2682**	0.3385***	0.2908**	0.4872
	(0.01)	(16.44)	(0.07)	(0.03)	(0.11)	(0.10)	(0.11)	(0.43)
Knows non-selected EW candidate	0.0302***	35.9845**	0.0402	0.0372	-0.2077**	-0.2071**	-0.0925	-0.1173
	(0.01)	(16.13)	(0.08)	(0.04)	(0.09)	(0.10)	(0.11)	(0.36)
Observations	2,383	1,956	2,366	2,370	1,392	564	1,845	1,764
R-squared	0.095	0.103	0.059	0.069	0.028	0.070	0.019	0.014
Mean Dep. Var. for "Knows none"	0.025	504.900	1.488	0.624	0.529	0.377	0.861	1.194
p-value [knows both = knows none]	0.020	0.193	0.831	0.430	0.399	0.225	0.084	0.599
Knows selected EW - knows non-selected EW	-0.0241*	-97.9530***	-0.1086	-0.0373	0.4760**	0.5457***	0.3833**	0.6045
	(0.01)	(25.93)	(0.08)	(0.05)	(0.20)	(0.17)	(0.19)	(0.38)

Notes: Sample restricts to treatment communities only and the omitted group are those who know none of the EW potential candidates. Foot of the table reports coefficient for "Knows selected EW - knows non selected-EW" estimated from a regression in which outcome variable is regressed on "Knows selected EW" controlling for number fo connections to candidates. All the regressions are OLS regressions. Errors clustered at the Cluster level. *** p<0.01, ** p<0.05, * p<0.1. Regressions control for unit stratification variables (branch fixed effects, percentage of farmers, number of habitants, distance to closest market), for the baseline value of the outcome variable (ANCOVA) and for the distance in km between the house of the household and the house of the selected EW. Results are robust to dropping stratification variables. "Engaged in commercial agriculture" equals 1 if the ratio between revenues and output value is above the median of 5%. "Kg of maize (beans) produced" calculates the production of maize (beans) in kg. This variable is missing for agents who did not grow maize (beans) in the last cropping season. "Total kg produced" calculates the total number of kg produced summing all crops grown in that last season. The total production value equals the quantity of each crop produced multiplied by the median market price of each crop unit in the branch. Quantity and value of production are truncated above and below two standard deviations from the mean. Results are robust to alternative cleaning strategies. "Rate of returns" divide the difference between the total production and input value by the input value. The input value includes expenses incurred for buying inputs, land and labor costs (hiring workers and own opportunity cost of time). The opportunity cost of working on one's own plot of land equals hours worked on own land multiplied by the branch median hourly wage paid to work on someone else land. Result are robust to considering the median hourly salary for working on one's own non-farm business.

TABLE 9: THE AGGREGATE EFFECT OF SOCIAL CONNECTIONS ON PROGRAM DELIVERY

Sample: treatment communities only
Data aggregated at cluster level

	(1)	(2)	(3)	(4)	(5)
	% who received advice on agriculture from EW in the last cropping season	% who were trained by EW in the last year	% who believe improved seeds have positive returns	% who adopted improved seeds in the last cropping season from…	
				BRAC EW	Other BRAC source
% who know selected EW candidate	0.0925	0.1419*	0.0829	0.1084*	0.0055
	(0.06)	(0.07)	(0.24)	(0.06)	(0.03)
% who know non-selected EW candidate	-0.0883*	-0.1098**	-0.0441	-0.0732*	0.0002
	(0.05)	(0.05)	(0.21)	(0.04)	(0.03)
Observations	60	60	60	60	60
R-squared	0.187	0.235	0.555	0.193	0.307
Mean Dep. Var. for "% who know none =1"	0.048	0.032	0.333	0.048	0.000
p-value [% who know selected EW candidate = % who know non-selected EW candidate]	0.061	0.029	0.771	0.066	0.922

Note: OLS regressions. Sample restricts to treatment communities only. Data aggregated at the cluster level. Robust standard errors. *** p<0.01, ** p<0.05, * p<0.1. Regressions control for: branch fixed effects, unit-level stratification variables (percentage of farmers, number of habitants, distance to closest market) and baseline value of the dependent variables (ANCOVA). Results are robust to dropping stratification variables. "% who received advice on agriculture from EW " equals the % of respondents in the cluster who received an advice from the BRAC extension worker at any time during the past season (during trainings, household visits, etc.). "% trained by EW " is the % who attended at least one training organised by the extension worker in the past year. "% who know (non) selected EW candidate" equals the % of respondents who know this person independently of whether they know the other candidate or not.

TABLE A0: ATTRITION

Sample: treatment and control communities

	Attrition: Interviewed at baseline but not at endline
Treatment 1: Agriculture Program only	0.0191
	(0.01)
Treatment 2: Agriculture + Microfinance Programs	0.0156
	(0.01)
Observations	4,692
R-squared	0.001
% Attrition in Control	0.0582
p-value [Ag= Ag +Microfinance]	0.824

Note: "Treatment2" combines the agriculture extension program with BRAC's microfinance program. OLS regressions. Errors clustered at the unit level. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A1: THE CONSTRAINTS RELAXED BY THE AGRICUTURE EXTENSION PROGRAM

Sample: treatment and control communities

Only respondents who have never adopted improved seeds at baseline

	(1)	(2)
	Adopted improved seeds in the last cropping season from…	
	BRAC source	Non BRAC source
Treatment * never adopted improved seeds because not available	0.0465***	-0.0139
	(0.02)	(0.02)
Treatment * never adopted improved seeds because too expensive	0.0794***	0.0957***
	(0.03)	(0.03)
Treatment * never adopted improved seeds because do not know about their existence	0.0047	0.0644
	(0.02)	(0.04)
Treatment * never adopted improved seeds because low returns	0.0232	0.0648
	(0.04)	(0.06)
Treatment * never adopted improved seeds because too risky (low yield in bad season)	-0.0059	-0.0139
	(0.02)	(0.05)
Observations	967	967
R-squared	0.059	0.082

Notes: OLS regressions. Errors clustered at the unit level. *** p<0.01, ** p<0.05, * p<0.1. Regressions control for unit stratification variables (branch fixed effects, percentage of farmers, number of habitants, distance to closest market) and for "ever adoption of improved seeds". Never adopters were asked at baseline the reasons why they never adopted and could give up to 3 different answers. These self reported reasons are interacted with the treatment dummy. Respondents who adopted improved seeds at endline were asked the sources of these seeds. Sources include BRAC extension worker, BRAC branch office, markets, shop or government extension worker.

TABLE A2: THE EFFECTS OF THE AGRICULTURE EXTENSION PROGRAM BY DISCOUNT FACTOR, RISK AVERSION AND PRESENT BIASEDNESS

Sample: treatment and control communities

	(1)	(2)	(3)	(4)	(5)	(6)
	Adopted improved seeds in the last cropping season from…					
	BRAC source	Non BRAC source	BRAC source	Non BRAC source	BRAC source	Non BRAC source
Treatment	0.0719***	0.0219	0.0688***	0.0227*	0.0394**	0.0039
	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)
Discount rate (scale 1 to 4)	0.0025	0.0008				
	(0.00)	(0.01)				
Treatment * Discount rate (scale 1 to 4)	-0.0009	-0.0006				
	(0.01)	(0.01)				
Risk lover (scale 0 to 10)			-0.0003	-0.0021		
			(0.00)	(0.00)		
Treatment * Risk lover (scale 0 to 10)			0.0051*	0.0025		
			(0.00)	(0.00)		
Present biased (dummy)					-0.0050	0.0215
					(0.01)	(0.02)
Treatment * Present biased (dummy)					0.0004	-0.0162
					(0.02)	(0.02)
Observations	4,253	4,253	4,221	4,221	4,230	4,230
R-squared	0.040	0.085	0.040	0.084	0.041	0.084

Notes: OLS regressions. Errors clustered at the unit level. *** p<0.01, ** p<0.05, * p<0.1. Regressions control for unit stratification variables (branch fixed effects, percentage of farmers, number of habitants, distance to closest market) and for "ever adoption of improved seeds". The discount rate is calculated by asking respondents 4 questions: whether they would prefer earning 30k UGX today or Xk UKG in 1 month where X=30, 40 or 50. The index takes value 0 if the respondent is always willing to wait, for whatever amount of X, value 1 if she is willing to wait only for X=40 or 50, value 3 if she is willing to wait only for X=50, and value 4 if she is never willing to wait. "Risk lover" variable asks respondents "In agriculture and livestock, would you say that you are someone who likes to take risks, or do you try to avoid risks? Please rate yourself between 0 and 10, where 0 means you never like to take risks, and 10 means you are always ready to take risks". To measure present-biasedness, we asked each respondent 2 hypothetical questions: (1) whether they would prefer earning 30k UGX today or Xk UKX in 1 month where X=30, 40 or 50; (2) we repeated this question changing the time frame to 6 and 7 months. A respondent is considered "present biased" if she prefers waiting 1 extra month to get more money in the future but not willing to wait today.

TABLE A3: THE EFFECTS OF A COMPLEMENTARY MICROFINANCE PROGRAM

Sample: treatment and control communities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(1)	(2)	(10)	(11)
				Adopted improved seeds in the last cropping season from…			Agriculture activity in the last cropping season						
	Received advice on agriculture from EW in the last cropping season	Was trained by EW in the last year	Believes improved seeds have positive returns	BRAC EW	Other BRAC source	Non BRAC source	Hours worked	Acres of land cultivated	Engaged in commercial agriculture	Kg of beans produced per acre cultivated and hour worked	Kg of maize produced per acre cultivated and hour worked	Output value per acre cultivated and hour worked	Rate of returns
Treatment 1: Agriculture Program only	0.0440***	0.0448***	-0.0623*	0.0332***	0.0323***	0.0237*	-22.6443	0.0659	0.0647*	-0.0479	0.1078	0.0004	1.0172*
	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(20.91)	(0.09)	(0.04)	(0.09)	(0.15)	(0.11)	(0.61)
Treatment 2: Agriculture + Microfinance Programs	0.0281***	0.0309***	-0.0224	0.0223***	0.0359***	0.0153	-12.5291	0.1070	0.0453	-0.1018	-0.1047	0.0956	1.2871*
	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(22.30)	(0.08)	(0.03)	(0.08)	(0.13)	(0.14)	(0.66)
Observations	4,336	4,339	4,000	4,269	4,269	4,285	3,493	4,246	4,259	2,505	982	3,299	3,150
R-squared	0.020	0.023	0.144	0.016	0.025	0.085	0.104	0.068	0.062	0.011	0.027	0.013	0.017
Mean Dep. Var. in Control	0.001	0.000	0.685	0.000	0.001	0.077	541.400	1.163	0.551	0.697	0.555	1.017	2.059
p-value [Ag= Ag +Microfinance]	0.176	0.265	0.210	0.331	0.713	0.500	0.639	0.620	0.567	0.465	0.144	0.421	0.724

Note: "Treatment2" combines the agriculture extension program with BRAC's microfinance program. OLS regressions. Errors clustered at the unit level. *** p<0.01, ** p<0.05, * p<0.1. Regressions control for unit stratification variables (branch fixed effects, percentage of farmers, number of habitants, distance to closest market). Columns 4-6 control for "ever adoption of improved seeds" while the rest of the columns control for the baseline value of the outcome variable (ANCOVA). "Received advice on agriculture from EW " equals 1 if the respondent received an advice from the BRAC extension worker at any time during the past season (during trainings, household visits, etc.). "Was trained by EW " equals 1 if the respondent attended at least one training organized by the extension worker in the past year. Beliefs about the returns of improved seeds equal zero for agents who do not know about the existence of improved seeds. Respondents who adopted improved seeds at endline were asked the sources of these seeds. Sources include BRAC extension worker, BRAC branch office, markets, shop or government extension worker. "Engaged in commercial agriculture" equals 1 if the ratio between revenues and output value is above the median of 5%. The total production value equals the quantity of each crop produced multiplied by the median market price of each crop unit in the branch. This variable is truncated above and below two standard deviations from the mean. Results are robust to alternative cleaning strategies. "Rate of returns" divide the difference between the total production and input value by the input value. The input value includes expenses incurred for buying inputs, land and labor costs (hiring workers and own opportunity cost of time). The opportunity cost of working on one's own plot of land equals hours worked on own land multiplied by the branch median hourly wage paid to work on someone else land.

TABLE A4: THE EFFECTS OF THE AGRICULTURE EXTENSION PROGRAM BY SOCIAL CONNECTIONS

Sample: treatment and control communities

	(1)	(2)	(3)	(4)
			Adopted improved seeds in the last season from…	
	Received advice on agriculture from EW in the last cropping season	Was trained by EW in the last year	BRAC Extension Worker	Other BRAC source
Omitted Group	*Control*	*Control*	*Control*	*Control*
Treatment * Knows selected EW candidate	0.0438***	0.0526***	0.0400***	0.0197
	(0.02)	(0.02)	(0.01)	(0.01)
Treatment * Knows non-selected EW candidate	-0.0052	-0.0084	-0.0067	0.0147
	(0.01)	(0.01)	(0.01)	(0.01)
Treatment * Knows none of the EW candidates	0.0203***	0.0161***	0.0143**	0.0208***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	3,962	3,964	3,905	3,905
R-squared	0.021	0.028	0.021	0.025
Mean Dep. Var. in Control	0.001	0.000	0.000	0.001
p-value [Treatment*knows EW = Treatment*knows non EW]	0.087	0.036	0.045	0.847
p-value [Treatment*knows EW = Treatment*knows none]	0.155	0.025	0.055	0.945
p-value [Treatment*knows non EW = Treatment*knows none]	0.092	0.098	0.111	0.694

Note: Sample includes treatment and control group with the control group as the omitted group. OLS regressions. Errors clustered at the Cluster level. *** p<0.01, ** p<0.05, * p<0.1. Regressions control for: unit-level stratification variables (branch fixed effects, percentage of farmers, number of habitants, distance to closest market) and baseline value of the dependent variables (ANCOVA). Results are robust to dropping stratification variables."Knows selected EW candidate" equals 1 if the respondents knows this person independently of whether she knows the non-selected candidate or not. "Received advice on agriculture from EW " equals 1 if the respondent received an advice from the BRAC extension worker at any time during the past season (during trainings, household visits, etc.). "Was trained by EW " equals 1 if the respondent attended at least one training organized by the extension worker in the past year.

TABLE A5: THE EFFECTS OF SOCIAL CONNECTIONS - OTHER SPECIFICATION

Sample: treatment communities only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	Received advice on agriculture from EW in the last cropping season	Was trained by EW in the last year	Believes improved seeds have positive returns	Adopted improved seeds in the last cropping season from…			Hours worked	Acres of land cultiv-ated	Engaged in commer-cial agric-ulture	Kg of beans produced per acre cultivated and hour worked	Kg of maize produced per acre cultivated and hour worked	Output value per acre cultiv-ated and hour worked	Rate of returns
				BRAC EW	Other BRAC source	Non BRAC source							
Knows ONLY selected EW candidate	0.0565**	0.0658***	0.0227	0.0415**	0.0166	0.0389**	-18.2674	-0.0197	0.0155	0.3135	0.2378	0.1822	-0.2139
	(0.02)	(0.02)	(0.04)	(0.02)	(0.02)	(0.02)	(34.04)	(0.17)	(0.06)	(0.23)	(0.21)	(0.21)	(0.90)
Knows ONLY non-selected EW candidate	-0.0054	-0.0076	-0.0917**	-0.0132	-0.0026	0.0566***	70.0984**	0.0794	0.0498	-0.1742*	-0.2846***	-0.1763	-0.6550
	(0.01)	(0.01)	(0.04)	(0.01)	(0.01)	(0.02)	(28.09)	(0.18)	(0.04)	(0.09)	(0.10)	(0.16)	(0.60)
Knows BOTH EW candidates	0.0048	0.0145*	-0.0227	0.0105	-0.0092	0.0447***	-15.1898	-0.0157	0.0411	0.0705	0.0984	0.1719	0.1929
	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.02)	(18.78)	(0.15)	(0.05)	(0.08)	(0.09)	(0.11)	(0.66)
Observations	2,411	2,410	2,213	2,369	2,369	2,383	1,956	2,366	2,370	1,392	564	1,845	1,764
R-squared	0.018	0.024	0.166	0.016	0.029	0.097	0.105	0.059	0.069	0.028	0.070	0.019	0.014
Mean Dep. Var. for "Know none"	0.020	0.014	0.426	0.016	0.012	0.025	504.900	1.488	0.624	0.529	0.377	0.861	1.194
p-value [know only EW = know only non EW]	0.045	0.016	0.005	0.016	0.463	0.220	0.002	0.155	0.507	0.031	0.008	0.070	0.362
p-value [knows both = know only EW]	0.044	0.035	0.205	0.016	0.217	0.678	0.922	0.958	0.644	0.203	0.472	0.957	0.633
p-value [knows both = know only non EW]	0.485	0.166	0.091	0.016	0.592	0.354	0.002	0.229	0.808	0.017	0.002	0.022	0.197
p-value [knows both = know only EW+ know only non EW]	0.055	0.067	0.441	0.016	0.260	0.025	0.187	0.718	0.730	0.763	0.549	0.560	0.411

Notes: Sample restricts to treatment communities only and the omitted group are those who know none of the EW potential candidates. OLS regressions. Errors clustered at the Cluster level. *** p<0.01, ** p<0.05, * p<0.1. "Knows only (non) selected EW candidate" equals 1 if the respondents knows this person and does not know the other candidate. Regressions control for unit stratification variables (district fixed effects, percentage of farmers, number of habitants, distance to closest market) and for the distance in km between the house of the household and the house of the selected EW. Results are robust to dropping stratification variables. Columns 4-6 control for "ever adoption of improved seeds" while the rest of the columns control for the baseline value of the outcome variable (ANCOVA). "Received advice on agriculture from EW " equals 1 if the respondent received an advice from the BRAC extension worker at any time during the past season (during trainings, household visits, etc.). "Was trained by EW " equals 1 if the respondent attended at least one training organized by the extension worker in the past year. Beliefs about the returns of improved seeds equal zero for agents who do not know about the existence of improved seeds. Respondents who adopted improved seeds at endline were asked the sources of these seeds. Sources include BRAC extension worker, BRAC branch office, markets, shop or government extension worker . "Engaged in commercial agriculture" equals 1 if the ratio between revenues and output value is above the median of 5%. "Kg of maize (beans) produced per hour worked and acre cultivated" calculates the production of maize (beans) in kg per acre and hour worked dedicated to maize (beans) cultivation. This variable is missing for agents who did not grow maize (beans) in the last cropping season. The total output value equals the quantity of each crop produced multiplied by the median market price of each crop unit in the branch. Quantity and value of production are truncated above and below two standard deviations from the mean. Results are robust to alternative cleaning strategies. "Rate of returns" divide the difference between the total production and input value by the input value. The input value includes expenses incurred for buying inputs, land and labor costs (hiring workers and own opportunity cost of time). The opportunity cost of working on one's own plot of land equals hours worked on own land multiplied by the branch median hourly wage paid to work on someone else land. Results are robust to considering the median hourly salary for working on one's own non-farm business.

TABLE A6: THE EFFECT OF SOCIAL CONNECTIONS - OTHER MEASURE OF CONNECTIONS

Sample: treatment communities only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
				Adopted improved seeds in the last cropping season from...									
	Received advice from EW in the last season	Was trained by EW in the last year	Believes improved seeds have positive returns	BRAC EW	Other BRAC source	Non BRAC source	Hours worked	Acres of land cultiv-ated	Engaged in commer-cial agric-ulture	Kg of beans produced per acre cultivated and hour worked	Kg of maize produced per acre cultivated and hour worked	Output value per acre cultivated and hour worked	Rate of returns
Discuss agriculture with EW candidate	0.0307**	0.0367**	0.0114	0.0306**	0.0081	0.0070	-54.6938***	-0.0648	-0.0123	0.1638*	0.1644	0.3329***	0.7914*
	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.01)	(16.04)	(0.07)	(0.03)	(0.09)	(0.10)	(0.10)	(0.42)
Discuss agriculture with non-EW candidate	-0.0103	-0.0033	-0.0187	-0.0034	-0.0118	0.0217	7.7742	-0.0035	0.0476	-0.0974*	0.0134	-0.0120	0.0763
	(0.01)	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)	(15.97)	(0.06)	(0.03)	(0.05)	(0.11)	(0.10)	(0.43)
Observations	2,163	2,163	1,982	2,127	2,127	2,140	1,755	2,123	2,128	1,260	485	1,659	1,580
R-squared	0.017	0.023	0.176	0.015	0.027	0.097	0.108	0.059	0.073	0.020	0.050	0.017	0.019
Mean Dep. Var. for "discuss with none"	0.018	0.012	0.456	0.008	0.017	0.040	509.100	1.440	0.604	0.560	0.415	0.870	1.228
p-value [discuss with EW = discuss with non EW]	0.096	0.130	0.503	0.089	0.335	0.447	0.018	0.478	0.232	0.036	0.363	0.038	0.118
p-value [discuss with both = discuss with none]	0.070	0.011	0.820	0.036	0.702	0.125	0.018	0.508	0.357	0.462	0.158	0.014	0.235

Notes: Sample restricts to treatment communities only and the omitted group are those who know none of the EW potential candidates. OLS regressions. Errors clustered at the Cluster level. *** p<0.01, ** p<0.05, * p<0.1. Regressions control for community stratification variables (district fixed effects, percentage of farmers, number of habitants, distance to closest market) and for the distance in km between the house of the household and the house of the selected EW. Results are robust to dropping stratification variables. Columns 4-6 control for "ever adoption of improved seeds" while the rest of the columns control for the baseline value of the outcome variable (ANCOVA). "Received advice on agriculture from EW " equals 1 if the respondent received an advice from the BRAC extension worker at any time during the past season (during trainings, household visits, etc.). "Was trained by EW " equals 1 if the respondent attended at least one training organized by the extension worker in the past year. Beliefs about the returns of improved seeds equal zero for agents who do not know about the existence of improved seeds. Respondents who adopted improved seeds at endline were asked the sources of these seeds. Sources include BRAC extension worker, BRAC branch office, markets, shop or government extension worker. "Engaged in commercial agriculture" equals 1 if the ratio between revenues and output value is above the median of 5%. "Kg of maize (beans) produced per hour worked and acre cultivated" calculates the production of maize (beans) in kg per acre and hour worked dedicated to maize (beans) cultivation. This variable is missing for agents who did not grow maize (beans) in the last cropping season. The total output value equals the quantity of each crop produced multiplied by the median market price of each crop unit in the branch. Quantity and value of production are truncated above and below two standard deviations from the mean. Results are robust to alternative cleaning strategies. "Rate of returns" divide the difference between the total production and input value by the input value. The input value includes expenses incurred for buying inputs, land and labor costs (hiring workers and own opportunity cost of time). The opportunity cost of working on one's own plot of land equals hours worked on own land multiplied by the branch median hourly wage paid to work on someone else land. Results are robust to considering the median hourly salary for working on one's own non-farm business.

TABLE A7: THE AGGREGATE EFFECTS OF SOCIAL CONNECTIONS BY SOCIAL DISTANCE AND NETWORK

Sample: treatment communities only

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dependent Variable →	% who received advice on agriculture from EW in the last season	% who were trained by EW in the last year	% who adopted improved seeds in the last season from EW	% who received advice on agricultur e from EW in the last season	% who were trained by EW in the last year	% who adopted improved seeds in the last season from EW	% who received advice on agriculture from EW in the last season	% who were trained by EW in the last year	% who adopted improved seeds in the last season from EW	% who received advice on agricultur e from EW in the last season	% who were trained by EW in the last year	% who adopted improved seeds in the last season from EW
Dummy →	=1 if EW candidates are both part of the local farm association			=1 if EW candidates live in the same community			=1 if EW candidates know each other			=1 if percentage of households who know both EW candidates is above median		
Dummy	-0.0034	-0.0171	-0.0231	-0.0664	-0.0635	-0.0825*	0.0192	0.0143	0.0227	-0.1078	-0.2158	-0.1981
	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.09)	(0.15)	(0.17)	(0.15)
% who know selected EW * 1(**Dummy**=1)	-0.0352	-0.0123	0.0046	0.2491	0.2832	0.3342	0.1086	0.0459	0.1672	0.5315*	0.6819*	0.7039**
	(0.07)	(0.07)	(0.06)	(0.21)	(0.26)	(0.23)	(0.16)	(0.22)	(0.20)	(0.27)	(0.35)	(0.31)
% who know non-selected EW * 1(**Dummy**=1)	0.0475	0.0534	0.0496	-0.1986	-0.1998	-0.2162	-0.1132	-0.0382	-0.1126	-0.4052	-0.4162	-0.4481*
	(0.08)	(0.09)	(0.07)	(0.19)	(0.23)	(0.20)	(0.12)	(0.18)	(0.16)	(0.24)	(0.29)	(0.25)
% who know selected EW * 1(**Dummy**=0)	0.1378*	0.1990**	0.1398	0.0201	0.0597	0.0046	-0.1720	-0.1214	-0.0019	0.0460	0.0733	0.0312
	(0.08)	(0.09)	(0.09)	(0.03)	(0.04)	(0.02)	(0.20)	(0.23)	(0.31)	(0.04)	(0.05)	(0.03)
% who know non-selected EW * 1(**Dummy**=0)	-0.1139**	-0.1467**	-0.0992*	-0.0985*	-0.1189**	-0.0810**	0.1707	0.1353	0.0597	-0.0579	-0.0810**	-0.0452*
	(0.05)	(0.06)	(0.05)	(0.05)	(0.05)	(0.03)	(0.16)	(0.20)	(0.25)	(0.04)	(0.03)	(0.03)
Observations	58	58	58	60	60	60	23	23	23	60	60	60
R-squared	0.215	0.273	0.211	0.252	0.300	0.345	0.353	0.364	0.411	0.298	0.351	0.368
Mean Dep. Var. for "% know none =1"	0.048	0.032	0.048	0.048	0.032	0.048	0.048	0.032	0.048	0.048	0.032	0.048
(1) p-value [% knows EW = % knows non EW] when Dummy=1	0.554	0.670	0.726	0.265	0.321	0.209	0.433	0.834	0.428	0.060	0.074	0.037
(2) p-value [% knows EW = % knows non EW] when Dummy=0	0.037	0.016	0.086	0.092	0.018	0.020	0.348	0.546	0.910	0.127	0.035	0.038
(3) p-value [% knows EW & Dum1 = % knows EW & Dum0]	0.064	0.048	0.192	0.285	0.729	0.514	0.230	0.662	0.561	0.082	0.234	0.034
(4) p-value [% knows non EW &Dum1 = % knows non EW &Dum0]	0.064	0.051	0.097	0.610	0.390	0.162	0.360	0.571	0.664	0.147	0.084	0.107
# clusters with Dummy=1	29			33			16			30		
# clusters with Dummy=0	29			27			7			30		

Notes: OLS regressions. Sample restricts to treatment communities only. Data aggregated at the cluster level. Robust standard errors. *** p<0.01, ** p<0.05, * p<0.1. Regressions control for community stratification variables (branch fixed effects, percentage of farmers, number of habitants, distance to closest market). Results are robust to dropping stratification variables. Columns with adoption as outcome variables control for "% ever adoption of improved seeds" while the rest of the columns control for the baseline value of the outcome variable (ANCOVA). The "dummy" variable in the right hand side of the regression is indicated in the box on the top of the table. In the first three columns, the dummy equals 1 if both EW candidates belong to the local farmer association. In columns 4-6, the dummy equals 1 if the EW candidates live in the same community. In columns 7-9, dummy equals 1 if the EW candidates know each other (they are either friends, relatives, neighbours, or are in any other type of relationship). Note that data on whether EW candidates know each other is available for only a subsample of the treatment clusters. In columns 10-12, the "dummy" variable equals 1 if the percentage of households who know both the selected and non selected candidates is above the cluster-level median of 67%. "Received advice on agriculture from EW " equals 1 if the respondent received an advice from the BRAC extension worker at any time during the past season (during trainings, household visits, etc.). "Was trained by EW " equals 1 if the respondent attended at least one training organised by the extension worker in the past year. "Knows (non) selected EW candidate" equals 1 if the respondents knows this person independently of whether she knows the other candidate or not.

Chapter 3
Movement Restrictions and Education, Evidence from Palestine

1. Introduction

Education plays a key role not only as an indicator of human capital but also as a central component of economic development. Understanding its determinants has therefore always been recognized as a first order economic question. In this paper, I study the extent to which movement restrictions limit education. Examples of movement obstacles abound, e.g. countries in conflict or in post-conflict often block access to specific locations due to security concerns. Assessing the effect of movement restrictions on education in these countries is important not only to understand the evolution of human capital itself but also to evaluate the role that mobility constraints plays in intensifying conflicts through the weakening of the educational system.[72]

In this paper, I base the evidence on the West Bank Separation Barrier that was built in 2003 by the Government of Israel to enhance their security. While the Barrier (or the "Wall") was constructed to limit movements between Israel and the West Bank, it also constitutes a major movement obstacle within the West Bank: the Barrier was built within the West Bank cutting off individuals located in-between the Green Line and the Barrier. Restricted opening hours at the Gates of the Barrier, overcrowding, along with the multiple layers of checks and security procedures at these Gates have made the crossing long and difficult. One third of the West Bank population is estimated to be affected by the Wall, i.e. they have limited access to schools, jobs, services on the other side of the Wall (UNOCHA 2010b).

To study the impact of movement restrictions on education, I use a difference-in-differences approach that compares education outcomes in areas affected by the Wall ("treatment group") to those not affected ("control group"), for individuals who were young enough to be in school

[72]In areas where unemployment is high and where young people participate in military activities for lack of alternative opportunities, education may provide a viable option for keeping them away from combat. They also prepare students in taking an active part in important civil processes.

when the Wall was constructed ("exposed cohorts") to individuals who were already out of school ("non exposed cohorts"). In the identification strategy, the exposure of an individual to the Wall is thus determined both by her age and her location of residence.

The main source of data is the 2009 Socioeconomic and Food Security cross-sectional survey (SEFSec) that contains information on the age, education and income of 25,744 individuals in the West Bank and indicates whether their residential location is affected by the Wall or not. An area is considered "affected" if it is located between the Green Line and the Wall, or outside but encircled by three or four sides by the Wall. While all individuals living in locations affected by the Wall are included in the "treatment group", I include only their closest match among those living in non-affected areas to the "control group". The matching approach selects control individuals for whom the distribution of income, age and number of schools is as similar as possible to the distribution in the treated group.

Throughout the paper, I consider three outcome variables: (1) whether an individual has dropped out from elementary school (vs. if she has completed elementary school), (2) whether she has dropped out from preparatory school, and (3) whether she has ever been enrolled in school. For each of these variables, I use information on the age of the respondent when the Wall was constructed in 2003 and the education system in Palestine to identify "exposed" cohorts. For drops out from elementary (preparatory) school, children are exposed if they are too young to have completed elementary (preparatory) school in 2003, i.e. less than 13 (17) years old. Similarly, only students who are too young to have ever attended school in 2003 are "affected", i.e. younger than 7 years old. For each outcome variable, I divide the exposed and unexposed cohorts in a "younger" and an "older" sub-cohort, e.g for the drop out from elementary school, the young exposed (unexposed) cohort is aged 4-8 (14-18) while the old one is 9-13 years old (19-23).

The analysis proceeds in three steps. First, I estimate difference-in-differences for two separate "experiments of interest": the young unexposed cohort is compared to the young exposed cohort in the first and to the old exposed cohort in the second. The distinction between these two experiments allows me to separately identify the shorter vs. longer-run effect of limited mobility on education, i.e. for the drop out variables the effect is identified for students who had nearly completed their studies when the Wall was constructed (old exposed cohort) to those who were further away from completion (young exposed cohort). Second, I present the results for a control experiment that compares the two unexposed cohorts to each other. The control experiment aims to support the parallel trend assumption, i.e. in the absence of movement restrictions, the treatment group would have followed the same time trend in education levels as the control group. Third, I conduct a number of robustness checks dealing with anticipation effects, migration issues, reversion to the mean and omitted variables.

The results show that movement restrictions negatively impact education. Using a logit regression model with location and year-of-birth fixed effects, I find that the construction of the Barrier raised the elementary and preparatory school drop out rate by 3.7 and 6 percentage points respectively when unexposed cohorts are compared to the young exposed cohorts. As an average of 7 and 11.5 out of 100 students abandon elementary and preparatory school respectively in the control areas, limiting the freedom of movement has a large negative effect on education as it raises the drop out rate by 50%. In contrast, the difference-in-differences coefficients are not significant when the unexposed cohorts are compared to the old exposed ones: students who were close to school completion did not give up school and decided to bear the travel/psychological cost of crossing the Wall. This result is consistent with the fact that the marginal benefit from attending school decreases with the number of years before completion. Finally, the Barrier is found to increase the share of individuals who have never attended school by 3.6 percentage points, i.e. 45% higher than in control areas. Reassuringly, the estimated coefficients are not significantly different than zero in the control experiment, indicating that the treatment and control groups were on parallel trends before the construction of the Wall.

To provide further evidence that the identified effects are causal, I show that the Barrier affected education only in areas where the Wall construction is completed while had no effect in districts where the Wall was initially planned but ended up not being built. Moreover, I find that refugees, who are allowed to enroll in UN schools located in refugee camps and have access to these schools even after the construction of the Wall, are less negatively affected than non-refugees.

Importantly, the impact of limited mobility is stronger for households defined as "food insecure", i.e. low income and low consumption. Food secure households, who are wealthier and can afford movings kids from a public to a private school if needed, are instead less affected. This indicates that movement restrictions lead not only to a decrease in average education but also to a potential increase in income inequality. Finally, movement restrictions are found to affect boys more than girls. This is consistent with existing anecdotal evidence that indicates that girls are granted permits to cross checkpoints more easily than boys who are more likely to be turned down on the grounds of security.

Besides its direct effect on the reduced access to schools, the Barrier could also affect education indirectly by reducing access to health facilities, land and work places.[73] Rather than only dropping out because they cannot access schools, children may quit school because their parents lost their job and they cannot afford school fees anymore or because their health deteriorated. Although I am not able to separate direct from indirect effects of limited mobility on education in this paper, existing literature indicates that indirect effects matter,

[73]Palestinians without a Jerusalem ID cannot access East Jerusalem where most of the specialized hospitals are located. To enter Jerusalem, they need a special short term permit that is often turned down for males between 15 and 30 on the grounds of security. Palestinians living in the West Bank also need a visitor permit to access their farming land in the Seam Zone.

i.e. Di Maio and Nandi (2008) analyze the relationship between child labor and schooling attendance in Palestine and show that the loss of the father's job raises significantly the probability that the children participate in the labor market and abandon school.[74]

While this study shows that movement restrictions are detrimental for education, the drop in education may have had a broader economic and social impact. How did the labor market adjust to a decrease in the supply of educated workers? Was the Wall construction able to deter violence or did its negative impact on education increase subsequent violence? Data I am collecting on the frequency of violent events in the West Bank will allow me to answer this question in a future study and will, hopefully, provide us with a deeper understanding of the effect of policies promoting freedom of movement on economic development.

This paper is related to the extensive literature that studies the determinants of education. Among the many papers that exist, those that are more closely related to this study, both in terms of topic and of identification strategy, are Lavy and Zablotsky (2011) and Duflo (2001). Lavy and Zablotsky (2011) analyze the effect of mother's education on fertility and education of children. Using the revocation of the Military Government of Arabs in Israel that imposed severe travel restrictions, they find that the change in access to schools led to a sharp decline in fertility. Duflo (2001) uses a similar difference-in-differences approach as the one in this paper to estimate the impact of school construction on education and earnings. This study contributes to the existing literature by providing, to the best of my knowledge, the first rigorous evaluation of the role of limited mobility in shaping education outcomes.

The rest of this paper is organized as follows. In Section 2, I discuss movement restriction in the West Bank and its theoretical impact on education. In Section 3, I describe the data, the education system in Palestine and present an overview of the identification strategy. Section 4 presents estimates of the impact of the Wall on education and its heterogeneous effect. Section 5 discusses threats to the identification strategy and robustness checks, and Section 6 concludes.

2. Movement Restrictions in the West Bank

Following the beginning of the second Intifada, the Israeli authorities implemented a comprehensive system of physical movement obstacles within the West Bank to enhance their security. As a result of this, a Barrier and other movement obstacles, i.e. checkpoints, roadblocks and earth mounds, were built to separate Israel from the West Bank.[75]

The construction of the Separation Barrier started in June 2002 in the North of the West Bank. By the end of 2003, 198 km of Wall were built in the Centre and the North and

[74] A survey of the PCBS on the impact of the Annexation Wall shows that 26 percent of the students interviewed abandoned school because of economic hardship (PCBS 2005).

[75] Among the 66 checkpoints, 65 percent are located along the Barrier and used by the Israeli authorities to control access to East Jerusalem and Israel.

60 km around East Jerusalem. The construction of the South part of the Wall started in October 2003 but was halted in 2004 due to financial constraints and concerns raised by the international community. Nowadays, the Wall is almost completed in the North and Center of the West Bank, while it is half incomplete in the South. Figure 1 shows a map of the parts of the Wall that are constructed, in construction or planned in July 2008. The map of the Wall has not changed much since the end of 2003: most of what is built today was built back then, i.e. the intensity of "movement restrictions" has remained more or less constant since the end of 2003.

According to the UN Office for the Coordination of Humanitarian Affairs (UNOCHA 2010b), over 674,000 Palestinians - approximately 30 per cent of the West Bank population - were directly affected by the Wall in 2003: 274,000 of them were living in areas between the Wall and the Green Line[76] and 400,000 were located East of the Wall with the necessity to cross it every day to access their farms, jobs, schools and services.

The only way to cross the Wall is through Wall Gates that have varying opening hours: some are always open while others open only a few days a week at specific hours. Every individual who crosses the Wall needs a permission issued from the Israeli Defense Forces and a Jerusalem ID if she wants to access East Jerusalem. Overcrowding, along with the multiple layers of checks and security procedures at these checkpoints have made the crossing long and difficult. Crossing one side to another of the Wall in other ways than through the Gates is basically impossible due to the presence of the Wall, layers of razor wire, military patrol roads, sand paths to trace footprints, trenches, a three-meter high electric fence, and buffer zones of 30 to 100 meters beside the Wall with prohibited access to Palestinians.

Anecdotal evidence suggests that the Wall has caused a disruption in the education system. A number of students and teachers who could easily access their school before the Wall was built, now have to cross checkpoints to reach the school and need an approved permission to do so. Students living in Ramallah and studying in Jerusalem must for instance take transport to Qalandia checkpoint, then transport to Ram checkpoint and finally get a third transport to school. In areas without checkpoints and where the Wall cuts straight through, children have been cut off from school, e.g. in Abu Dis students travel over 20 kilometers around the Wall to reach their classrooms. In places where the checkpoints open only at specific hours, teaching hours shorten to allow students to get back home. Even when checkpoints are open all the day, students and teachers are often late in school as passing through checkpoints can take a long time and parents may not have the time to bring their children themselves to school.

[76] Of these, 161,000 live in enclaves, 96,000 are inside the depth Barriers and 17,000 are closed in areas between the Wall and the Green line.

3. Data and Identification Strategy

3.1. Data

The main source of data of this paper is the 2009 Socioeconomic and Food Security survey (SEFSec) conducted by the Food and Agriculture Organization in collaboration with the Palestinian Central Bureau of Statistics (PCBS). The survey contains information on gender, age, education, income, labor force participation, food security of 25,744 individuals located in the West Bank. Education is reported in three ways, i.e. with their enrollment status, the number of years of education and the highest level of education completed.

Importantly, the data indicate whether the place of residence of the respondent is affected or not by the construction of the Wall. An area is considered "affected" if it is located between the Green Line and the Wall as constructed in 2003,[77] or outside but encircled by three or four sides by the Wall. Among the twelve districts of the West Bank, Jericho and Nablus are the least affected by the Wall while Qalqylia, Salfit and Jerusalem are the most exposed with over 80% of the households defined as "affected". The construction of the Wall began in 2002 and a large part of what is built today (see Figure 1) was concluded by the end of 2003.

While the SEFSec dataset includes information on the residence of all interviewed respondents in 2009, it does not include information on their precise migration history. As a consequence, I am not able to assess whether a respondent changed location between 2002, when the construction of the Wall started, until 2009, when the survey was done. Although I assume that the location remained the same in the analysis, I discuss migration issues in details in Section 5.2.

Finally, I merge the SEFSec data with a school database provided by the Palestinian Ministry of Education and Higher Education (MoEHE). This enables me to know: (a) the total number of schools located in each district, (b) who is admitted in the schools: age and gender, (c) the type of management of the schools: public school, private school or UNRWA school for refugees (United Nations Relief and Works Agency) .

3.2. Identification Strategy

The identification strategy relies on a difference-in-differences approach that compares education outcomes in areas affected by the Wall ("treatment group") to those not affected ("control group"), for individuals who were in school when the Wall was constructed ("exposed cohort") to individuals who were not ("non exposed cohort").[78] The identifying assumption is that,

[77]Only the sections of the Wall that were constructed in 2003 are considered in the definition of an "affected area" while those planned, but not constructed in 2003 are not.

[78]Note that although some sections of the Wall were planned in 2003 but were then not yet constructed in 2009, the context and the data do not allow me to use these areas as the control group. First, movement restrictions existed in these areas due to the presence of soldiers and fences along the line where the Wall was supposed to be constructed but then was not. Second, although the SEFSec database contains information on

in the absence of movement restrictions, treatment areas would have followed the same time trend in education levels as the control. I discuss and provide evidence that this assumption holds in Section 4.

Because areas affected by the Wall differ from the unaffected ones in terms of number of schools, age and income per capita of the population, I resort to a one-to-one matching approach in defining the control group. While all individuals living in locations affected by the Wall are included in the "treatment group", only their closest match among those living in non-affected areas is added to the "control group". The matching approach selects 9,433 individuals among the pool of 16,284 control individuals for whom the distribution of income, age and number of schools is as similar as possible to the distribution in the treated group. The matching is done without replacement and with common support. Results are robust to using different matching strategies, e.g. 5 nearest neighbors, matching with replacement.

The exposure of an individual to the construction of the Wall is determined jointly by her location and by whether she was enrolled in school or about to start school at the end of 2003. Although I do not have information on the education history of each respondent, I use information on the age of the respondent in 2003 and the education system in Palestine to identify cohorts likely to have been in school in 2003. The educational system in Palestine consists in elementary school from grade 1 to 6, preparatory school from grade 7 to 10 and secondary school from grade 11 to 12. The three education outcome variables I use in the paper are: 1) whether an individual has dropped out of elementary school (vs. if she has completed elementary school), 2) whether she has dropped out from preparatory school, 3) whether she has ever been enrolled in school. Given that dropout from secondary school is extremely low (see below), the analysis focuses on elementary and preparatory school exclusively.

As only a small percentage of the students repeat classes,[79] I use fixed-age cut-offs for a non-repeating student in Palestine to estimate whether a respondent is likely to be in school or not in 2003. For drops out from elementary school, children are exposed if they have not yet completed elementary school in 2003. Given that children usually start elementary school at age 7, they are at risk of dropping out only if they are 13 years old or younger in 2003. Similarly for drops out from preparatory school, only children who are 17 or younger in 2003 are potentially at risk. Finally, only students who are too young to have ever attended school in 2003, i.e. 7 or younger, are affected by the Wall in their decision to enroll in school or not. Figure 2 illustrates the age cut-offs for the three variables of interest.

Two point are here of note. First, because the SEFsec survey was conducted six years after the Wall was built, using the number of years of education as an outcome variable

the locality code of the respondent, it does not provide me with the name of these locality for privacy reason (in other words, I do not know which code refers to which locality).

[79] In Palestine, students do not repeat classes until grade 4 of elementary school. After grade 4, students may be asked to repeat a class. The SEFSec sample indicates that among the respondents with 7 years of education, only 6 percent has not completed elementary school. A similar percentage is observed for preparatory school.

would overestimate my results. Students who were old enough in 2003 to have completed their education would indeed be compared to younger exposed children who may still be enrolled in school in 2009. Outcomes such as "Dropping out of school" and "Having never been enrolled in school" have the advantage of referring to decisions taken before the 2009 survey for both the exposed and unexposed cohorts.

Second, the reason I use the end of 2003 as the key date for defining "exposure" rather than June 2002, when the Wall construction started, is that I would otherwise allow for time variation in the exposure to the Wall, i.e. students who started school just before the end of 2003 are more intensively exposed to the Wall than those who were enrolled in 2002 but finished school before the end of 2003. Using 2003 as the cutoff thus ensures that each individual in the "exposed cohort" is equally exposed to the Wall. A caveat here is that my results are underestimated if the partial construction of the Wall in 2002 has affected negatively the education of "unexposed" students who were in school in 2002 but not in 2003. On the other hand, the results are overestimated if students who were about to finish school in 2002 had an extra incentive to study and complete schooling before the Wall was fully built. These potential anticipation effects are discussed in section 5.1.

3.3. Summary Statistics

Table 1 presents summary statistics for the treatment and the control group separately. Although respondents in both groups are similar in terms of their education, age and labor force participation, they strongly differ in whether they face mobility restrictions. While 26 percent of the treatment households report that mobility constraints are the major problem they face, only 17 percent report it as an obstacle in the control group. This difference, which is significant at the one percent level, indicates that I am capturing movement restrictions difficulties by separating the sample in areas affected and not by the Barrier.

Eight percent of the individuals in the sample have never attended school while the rate of elementary, preparatory and secondary school completion is 78, 51 and 23 percent respectively. The drop out rates from elementary, preparatory and secondary school equal 7, 11 and 1.5 percent respectively. Note that, throughout this paper, I define "drop-out rate" as the share of students who were enrolled but stopped going to school, and did not re-enroll afterwards. Finally, 63% of the sample is "food secure", i.e. both income or consumption are above \$4.7/adult equivalent/day while the rest is "food insecure".

The MoEHE school database indicates that there are an average of 7.3 schools per district in the West Bank. One third of these schools are female schools, one third are male schools and one third are mixed. Finally, 72% of the schools are public, 15% are private and 12% are UNRWA schools for refugees.

4. The Effect of Movement Restrictions on Education

4.1. Simple Difference-in-Differences

A first evidence of the effect of the Barrier on education is presented in Figure 3. The Figure presents the percentage of agents who dropped out from elementary and preparatory school and the percentage who have never enrolled in school by age in 2003, for the treatment and the control group separately. While the exposed cohorts tend to have higher probabilities of dropping out from elementary school in the treatment than in the control areas, the opposite is true for the unexposed cohorts. The same pattern is observed for the drops out from preparatory school and ever enrollment in school.

Tables 2 presents simple two-by-two DID estimates while Figure 4 presents the graphical representation of these results. The exposed and unexposed cohorts are divided into a younger and an older cohort, e.g. for the drop out from elementary school, the exposed cohort is divided into a "young" and "old" cohort aged 4-8 and 9-13 respectively while unexposed cohort are divided into age ranges 14-19 and 19-23. While a similar 5 years age interval is used for the drop out from secondary school, I use a 3 years age interval for the "never enrolled in school" variable.[80]

The effect of movement restrictions on education is estimated in Panel A and B by comparing education of the young and old exposed cohort respectively to the young unexposed one. Panel C presents a control experiment that compares education of the young unexposed cohort to the old unexposed cohort. The identifying assumption is satisfied if the DID estimate in Panel C is not significantly different than zero.

Columns (a) and (b) of Table 2 suggest that exposed cohorts abandon elementary and preparatory school more often in affected areas while the opposite is true for unexposed cohorts, leading to positive DID estimates. Importantly, the DID is positive and significant only for the young exposed cohorts in Panel A and B while non-significant for older exposed cohorts. These results are consistent with the expected cost of staying in school -i.e. crossing the checkpoints every day until school completion- increasing with the number of years before school completion. In other words, the findings indicate that the older cohorts are not affected by limited mobility because their expected cost of dropping out is larger than the benefits of finishing school while this is not the case for younger exposed cohorts.

Two points are here of note. First, most parts of the Wall that exist today were already built by the end of 2003, young and old exposed cohorts face a similar "intensity of travel restrictions". The difference in the reaction of these cohorts is hence not explained by increasing "exposure" to the Wall. Second, Panel D of Table A1 indicates the different in their reaction

[80] The reason is that the youngest children in Panel A (aged 2 in 2003) need to be at least 8 in 2009 to have been faced with the decision of enrolling in school in 2009. The DID would be underestimated if I include, in the exposed cohorts, children aged 0 and 1 in 2003 (too young to be enrolled in 2009).

is statistically different than zero: the young exposed cohort is significantly more negatively affected by movement restrictions than the old exposed cohort.

In Column (c) of Table 2, the results show that the Barrier increases the share of individuals who have never attended school. In contrast with the drop out results, the DID estimates are stronger for older exposed cohorts. This indicates that the effect of the Wall on enrollment was attenuated over time, i.e. kids who were about to start school when the Wall was constructed did not enroll while those in the same situation a few years later did. This could be explained by learning effects, e.g. parents "learn" about another school located on their side of the Wall and decide to enroll their kids in this school.

Importantly, the DID estimates are not significantly different than zero in Panel C for all the outcome variables. In other words, in the absence of movement restrictions before the construction of the Wall, treatment and control group were following parallel trends. This is clearly illustrated in Figure 4.

4.2. Difference-in-Differences with Fixed Effects and Controls

The difference-in-differences presented in the previous section can be generalized to a regression framework. As the education outcome variables of this paper are binary, I use a logit estimation that fits the data better than a linear estimation:

$$logit(Y_{ijl}) = \alpha + \beta_j + \gamma_l + T_j E_i \delta + X_i \zeta + \varepsilon_{ijl}$$

where Y_{ijl} is an education dummy variable for individual i living in district j and aged l in 2003, $T_j = 1$ if the district of residence is affected by the construction of the Wall (treatment group), $E_i = 1$ if the individual i is young enough in 2003 to belong to the exposed cohort, β_j are district fixed effects and γ_l are year of birth fixed effects. Finally, X_i is a vector of individual and household controls that are correlated with Y_{ijl}: gender, ownership of an Israeli/Jerusalem ID or not, refugee status (refugee or non-refugee), location of the house (urban, rural or refugee camp), distance to the closest school in kilometers, number of children in the household, number of parents who work in the household.

The parameter of interest is here the odds ratio which measures the extent to which the effect of living in an area affected by the Wall changes for exposed cohorts compared to non-exposed ones. The logit coefficient δ and the odds ratio are reported in Table 3 with different specifications: without fixed effects and without controls, with fixed effects and without controls, with fixed effects and with controls. Reassuringly, the magnitude and precision of the estimated effects are robust to adding fixed effects or control variables in the regression. This provides supporting evidence that the estimated effects are not biased by omitted variables. More discussion on potential omitted variables is discussed in Section 5.

When the old unexposed cohort is compared to the young exposed cohort (Panel A), the results suggest that the construction of the Wall increased the dropout rate from elementary and preparatory school by 1.68 and 1.60 times respectively (Columns a and b). However, the Wall did not affect the drop out rate of the old exposed cohort: these students, who were about to finish elementary/preparatory school when the Wall was constructed, were thus willing to bear the cost involved by limited mobility in order to complete their studies (Panel B). In Column (c), the impact of the Wall on the probability of having never attended school is significant only for the oldest exposed cohort. Finally, in all three Tables, DID estimates are not significantly different than zero in the control experiment (Panel C). This is reassuring as the contrary would have suggested that, irrespective of the Wall, education was deteriorating faster in affected localities, and the positive coefficients we identify may then have reflected this differential trend rather than the effect of the Wall.

In non-linear models, DID estimates may differ from δ as the cross partial derivative of $E(Y_{ijl})$ with respect to E_i and T_j is different from the derivative of $E(Y_{ijl})$ with respect to the multiplicative term E_iT_j. To address this potential issue, I calculate marginal effects manually using the correct cross partial derivative formula (Ai and Norton 2003).[81] Reported in Table 4, these marginal effects suggest that the construction of the Wall increased the drop-out rate from elementary and preparatory school by 3.7 and 5.9 percentage points respectively when comparing the youngest affected cohort to the unaffected one. As the average drop out rate in the West Bank is 7% for elementary school and 11% for preparatory school, these effects are large in magnitude: they correspond to a 50% increase in drop out rate. Moreover, the probability of having never attended school increased by 45% (3.6 percentage points) when older unaffected cohorts are compared to unaffected ones. Reassuringly, no significant effects are detected in the control experiment.

The second part of Table 4 calculates marginal effects for exposed and unexposed cohorts within the treatment areas separately. While the Wall increased drop out of young exposed cohorts, it had no effect on unexposed cohorts. Similarly, it increased the chances of having never attended school for old exposed cohorts while had no effect on unexposed cohorts.

Placebo tests and heterogeneous effects

To provide further evidence that the identified DID estimates are causal, I present the results of two placebo tests in Table 5: the first placebo test exploits variation in movement restrictions intensity across districts while the second exploits variation across households in access to schools after the Wall was constructed.

While the exact location of the Wall is certainly endogenous to a number of factors that are correlated with education, the order in which the Wall was constructed is probably less

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[81] $\frac{\partial^2 F(Y_{ijl})}{\partial A_j \partial T_i} = {}^1/_{1+e^{-(\alpha+\beta+\gamma+\delta+X_i\zeta)}} - {}^1/_{1+e^{-(\alpha+\beta+X_i\zeta)}} - + {}^1/_{1+e^{-(\alpha+\gamma+X_i\zeta)}} + + {}^1/_{1+e^{-(\alpha+X_i\zeta)}}$ where $F(Y_{ijl}) = {}^1/_{1+e^{-(\alpha+\beta_j+\gamma_l+A_jT_i\delta+X_i\zeta+\varepsilon_{ijl})}}$ is the logit cumulative distribution function

endogenous. As the Wall was gradually constructed from North to South, the Barrier was almost complete in the North in 2003 while it was largely incomplete in the South. As a consequence, the effect of movement restrictions is expected to be stronger in the North where families face more mobility obstacles. This prediction is confirmed by Part 1 of Table 5: the affected population living in the North is more than 3 times as likely to have dropped out and to have never enrolled in school than the unaffected population. This effect is smaller and not significant in the Center and in the South of the West Bank.[82]

Part 2 of Table 5 replicates the analysis using a more precise index of movement restriction intensity in the district rather than using its location. The Mayssun El-Attar Index (2009) equals ${}^{Wall_j}/_{Border_j} + {}^{InsideWall_j + Enclaves_j}/_{TotalArea_j}$ where $Wall_j$ is the length of the Wall (in km) in district j, $Border_j$ refers to the length of the Green Line border, $InsideWall_j$ is the size (in km^2) of the area between the Green Line and the Wall, $Enclaves_j$ is the size of the areas that have become enclaves. Again, Table 5 indicates that impact on education was weaker in areas less exposed to mobility obstacles (index equal to 0) while stronger in localities with an index bigger than one.

In the West bank, all the households with a registered refugee status are eligible for free basic education through UNRWA (United Nations Relief and Works Agency) schools. As refugees tend to live in refugee camps, close to where the UNRWA schools are located, most of them continue having access to UNRWA schools after the construction of the Wall and are thus less affected by the Wall.[83] Table 5 confirms this prediction: the negative impact of movement constraints is stronger for non-refugees than for refugees. In particular, it appears that non-refugees living in areas affected by the Wall drop out three times as much from elementary school as those living in unaffected areas (significance at 1 percent). In contrast, the education level of agents with the refugee status has been less impacted.

In the rest of this section, I explore whether there are heterogeneous effects of limited mobility by wealth and by gender. The results are reported in Table 6. Using "food security" a measure of wealth, I find that the effect of limited mobility is stronger "food insecure" than "food secure" households. Exposed food insecure children drop out from elementary and preparatory school 4 and 2.5 times more than unexposed food insecure children, while the effect on food secure households is positive but insignificant. This indicates that movement restrictions lead to an increase in income inequality as poor families seem to be more negatively affected than rich families. Richer parents may indeed have the option of sending their kids to a private school in case the public one is too difficult to reach or can afford to pay a driver to bring children to school without having to queue at the checkpoint on their own and losing their jobs.

[82]Note that the estimates for the Centre are higher and more significant when the locality of Ramallah is excluded from the sample.

[83]The UNRWA defines a refugee as someone: "whose normal place of residence was Palestine between June 1946 and May 1948, who lost her home and means of livelihood as a result of the 1948 Arab-Israeli War".

Finally, Table 6 shows that the effect of the Wall is stronger for boys than for girls. Exposed boys living in affected areas are more than twice as likely of having dropped out as in unaffected areas. For girls, the interaction term is positive but not significant. This result is consistent with existing anecdotal evidence that indicates that girls are granted permits to cross checkpoints more easily than boys who are turned down on the grounds of security.

4.3. Extended Difference-in-Differences

I extend results in the previous section by estimating the effect of limited mobility age-by-age with:

$$logit(Y_{ijl}) = \alpha + \beta_j + \gamma_l + \sum_{l=n}^{m} (T_j A_{il})\, \delta_l + X_i \zeta + \varepsilon_{ijl}$$

where $T_j = 1$ if district j is affected by the Wall, $A_{il} = 1$ if individual i is aged l in 2003 and $[n - m]$ is the "exposed" age range. In the regression, individuals in the oldest unexposed cohort in 2003 form the control group. The parameters of interests, δ_l, estimate the differential effect of the Barrier for agents aged l in 2003 in comparison to unexposed agents. δ_l are expected to be non-positive for $l > p$ where p is the youngest age for an individual to be considered unexposed.

Figure 5 plots smoothed interaction terms, i.e. for each $l : \frac{(\delta_{l-1}+\delta_l+\delta_{l+1})}{3}$ is plotted. The advantage of considering this smoothed version of the coefficients is that it shows more clearly the discontinuity in the trend for affected and unaffected cohorts. With drop out or with never enrollment as an outcome variable, the smoothed coefficients tend to be negative for the unexposed cohorts while positive for the exposed ones. Note that for drop out from preparatory schools, the interaction term sharply increases for exposed cohorts younger than 13. This again confirms the fact that only the youngest exposed students seem to have given up preparatory school as a consequence of mobility constraints. With "never enrollment in school", the estimated coefficients are negative for unexposed cohort older than 8, strongly increase for exposed cohort just below 8 while slowly decrease for younger exposed cohorts.

5. Threats to Identification and Robustness Checks

The interpretation of the coefficients relies on the identifying assumption that there are no omitted time varying and district-specific effects correlated with the construction of the Wall. Potential threats to the identification are discussed below.

5.1. Anticipation

This paper defines "exposed cohorts" as individuals who were in school at the end of 2003, rather than using June 2002 (start date for the construction) as the cutoff. While this ensures that each individual that belongs to the "exposed cohort" is equally exposed to the Wall, my results may be over- or underestimated by anticipation effects. More precisely, the results are underestimated if the partial construction of the Wall in 2002 has affected negatively the education of "unexposed" students who were in school in 2002 but not in 2003. On the other hand, the results are overestimated if students who were about to finish school in 2002 had an extra incentive to study and complete schooling before the Wall was fully built.

To alleviate these concerns, I replicate the analysis using two alternative experiments of interest in which old and young exposed cohorts are compared to the old unexposed cohort, rather than the young unexposed one. Because old unexposed individuals were already out of school in June 2002, their education was not directly affected by the Wall. They are moreover less likely to have quickly completed their schooling because of an anticipated nearby construction of the Wall. Panel E and F of Table A1 shows that the results remain robust to these two alternative experiment of interests. Results are also robust to replicating Table 2 using June 2002 as the cutoff for the definition of "exposure" (not reported).

5.2. Sample Selection: Migration

The SEFSec dataset includes information on the current residence of all interviewed households while does not include information on the precise migration history of each household. As a consequence, I assume in the analysis that the location of each respondent has not changed between 2002, when the construction of the Wall started, until 2009, when the survey was done. This assumption may not be satisfied if households living in affected areas anticipated that once the Wall was constructed, they would have limited access to education and decided to move to unaffected areas in the West Bank or outside the West Bank. Similarly, the assumption does not hold if households managed to move from affected to unaffected areas after 2003, once the Wall was built.

These movements are threat to the analysis if the educational composition of individuals living in areas affected or not by the Barrier changed as a result of the construction of the Wall. If the sample of households who migrated is non-random and would have been less affected by the Wall than the average sample household, then the estimated DID estimates are overestimated.[84] If, instead, agents who migrated are those who would have been more severely affected by the Wall then the effect I identify is underestimated.

The difficulty in getting formal permission to move both within the West Bank and outside of the West Bank[85] combined with the difficulty in finding a job in the West Bank (20% unemployment rate) and selling land/ houses in affected areas, strongly limits internal and

[84] This could be the case if wealthier households were more likely to migrate. The effect of the Wall construction on education is indeed found to be milder for richer households than poorer ones.

[85] Migrating to another area of the West Bank or to Jerusalem requires a permission that is usually hard to get. Moving out from the West Bank to neighboring countries, e.g. Jordan, Egypt, is hard as these countries have very strict criteria for granting residence and work permits.

external migration. This is confirmed by a number of existing surveys. The 2003 PCBS Household Survey (PCBS 2003) on the Impact of the Separation Wall asked a random sample of 5,148 persons located in affected and unaffected areas whether they recently moved and if, not, whether they were planning to. Only 5% of the sample reports having changed location in the past or wanting to move, with an equal proportion in the West and the East of the Barrier. Among this 5%, 72% reported having moved or wanting to move as a result of the Wall. Another survey of the PCBS (PCBS 2006) on the impact of the Separation Wall on forced migration indicates that only 0.8% of the population living in localities where the Barrier passes through have been displaced in 2003. Finally, the SEFSec survey asked each respondent included in the analysis whether she migrated in the six months before the interview and for what reasons. It turns out than among the 23,500 individuals interviewed, only 2% changed place of residence and this proportion is equal in Wall-affected and unaffected areas. Moreover, the income level, the educational outcomes and the age of the 2% who migrated is not statistically different from the rest of the population (not reported).

To provide further evidence that the DID estimates are not biased by sample selection issues, Table 7 (Part 3) replicates the analysis excluding from the sample all respondents who got married, had children and became "parent" in a new household since they went to school. Their probability of moving, before or after the Wall was built, is indeed higher than the respondents who still live with their parents (coded as being son or daughter in the household). With this restricted sample, I find the effect of limited mobility to be slightly stronger than before in the experiment of interest while not significant in the control experiment. This suggests that, if anything, migration leads to underestimate my results. Reassuringly, the respondents "included" in the sample have similar education level, wealth, gender as those "excluded" (not reported), indicating that these two groups do not seem to statistically differ on the extent to which they are affected by limited mobility. Results in Table A2 indicate that neither the number of years of schooling, nor the higher education level significantly predicts the chances of still living with parents.

5.3. Omitted Variables

Because the Barrier was built to protect Israel from attacks emanating from the West Bank, one eventuality is that the location of the Wall coincides with the areas of Palestine that are experiencing an increase in violence. If school attendance in affected areas diminishes as a result of violence increase and not only as a consequence of movement restrictions, the results of Section 4 are overestimated, i.e. parents keep their children at home because of their fear of violence rather than because schools are not accessible. Reassuringly, changes in violence level seem to be unrelated to the proximity to the Barrier. Nablus and Hebron, which are among the governorates less exposed to movement restrictions, are for instance the areas with the larger increase in deaths and buildings damaged in these last years (UNOCHA 2010b). Moreover, the fact that education is affected only in localities that are subject to movement restrictions (see Table 7) suggests that the estimates cannot be completely explained by the violence argument.

The DID estimates identified above could also be biased by the economic depression that took place in Palestine while the Wall was constructed. The results would be biased if this depression, which certainly negatively affected education, was more severe for exposed cohorts and in localities affected by the Wall. The PCBS Labor Force Surveys (2002 to 2010) however suggest that the young population has not suffered more from the economic crisis than the oldest cohort. Moreover, the 2009 SEFSec survey ask households whether "expenditures in education have been reduced as a result of economic depression" and the data show that fewer households did so in "Wall-affected" areas than in non-affected ones (11 percent against 14 percent).

The estimated effect of the Wall would be biased if the construction of the Barrier was accompanied by measures that had diverging impacts on education in different districts, i.e. new education law, new schools constructed, government investments in education, foreign aid in the instruction sector. Reassuringly, no education policy was implemented before SEFSec survey was conducted.[86] The MoEHE school database indicates that the number of schools did not change from 2003 and 2009 in areas affected by the Wall. Three schools were destroyed because they were on the Wall path, others were transformed into military barracks but this reduction in the number of schools is compensated by the construction of new schools.

5.4. Reversion to the Mean

Reversion to the mean is another possible threat to the identification, particularly for drops out from preparatory school: in the pre-Wall period, the drop out rate was lower in treatment areas than in control and was decreasing over time while it started increasing in treatment areas in the post-Wall period, surpassing the control (Figure 3, Panel 2). This change in the time trend can potentially be explained by reversion to the mean. To alleviate this concern, Table 7 presents the results for an alternative sample that excludes the localities of Ramallah and East Jerusalem (Al-Quds) which are the richest areas of the West Bank and are both part of the treatment group. While the exclusion of these two regions raises the average drop out level in treatment areas above the control ones in the pre-Wall period and eliminates the pre-Wall negative trend, the results remain similar. This provides suggestive evidence that reversion to the mean does not confound my results.

[86] The only new education regulation was adopted in 2010, after the SEFSec survey was conducted. It prescribes that all schools in Israel, including the Arabic schools in Jerusalem, pay teachers at least 4,500 shekels per month, they should all satisfy security measures and should provide 20 hours per week of special assistance to pupils with movement restrictions.

5.5. Quality of Education

The analysis has focused on the "quantity" of education, but "quality" of education is another important dimension to consider. The finding that limited mobility negatively affects the "quantity" of education would be less worrying if this is compensated by an improvement in the quality of education.

Although I do not have data to formally test this assumption, anecdotal evidence suggests that the reduction in quantity of education was coupled with a reduction in quality of education. To illustrate this, let us take the example of Shireens school, a high-quality private school that I visited when I was in Anata. The school is located close to the Shufat refugee camp just outside the Wall that separates Jerusalem from the West Bank, but inside the administrative boundaries of Jerusalem. Because of the Barrier, students who were coming from Jerusalem and who are now inside the Wall cannot attend this school anymore. The reduction in enrolled pupils forced the school to decrease the tuition fees and consequently lower the quality of education by reducing the number of teachers and accepting more children per class. Other anecdotal evidences suggest that: (1) school schedules have been interrupted due to strict opening hours at the Gates and, (2) overcrowding at the Gates has increased the absence of teachers from school.

6. Conclusions

In this paper, I study the role of movement restrictions in shaping schooling decisions. This is a relevant topic with implications for economic development and social changes. The evidence is based on the construction of the West Bank Separation Barrier in 2003 as a natural experiment. The exposure of an individual to the Barrier is determined both by her locality of residence and by whether she was in school or about to start school when the Barrier was built. Using a difference-in-differences approach, the results suggest that movement restrictions seriously deteriorate schooling levels: the probability of dropping out from elementary and preparatory school increased by 3.7 and 6 percentage points respectively, i.e. a 50% increase relative to localities with no movement restrictions, while the proportion of students who have never attended school increased by 3.6 percentage points.

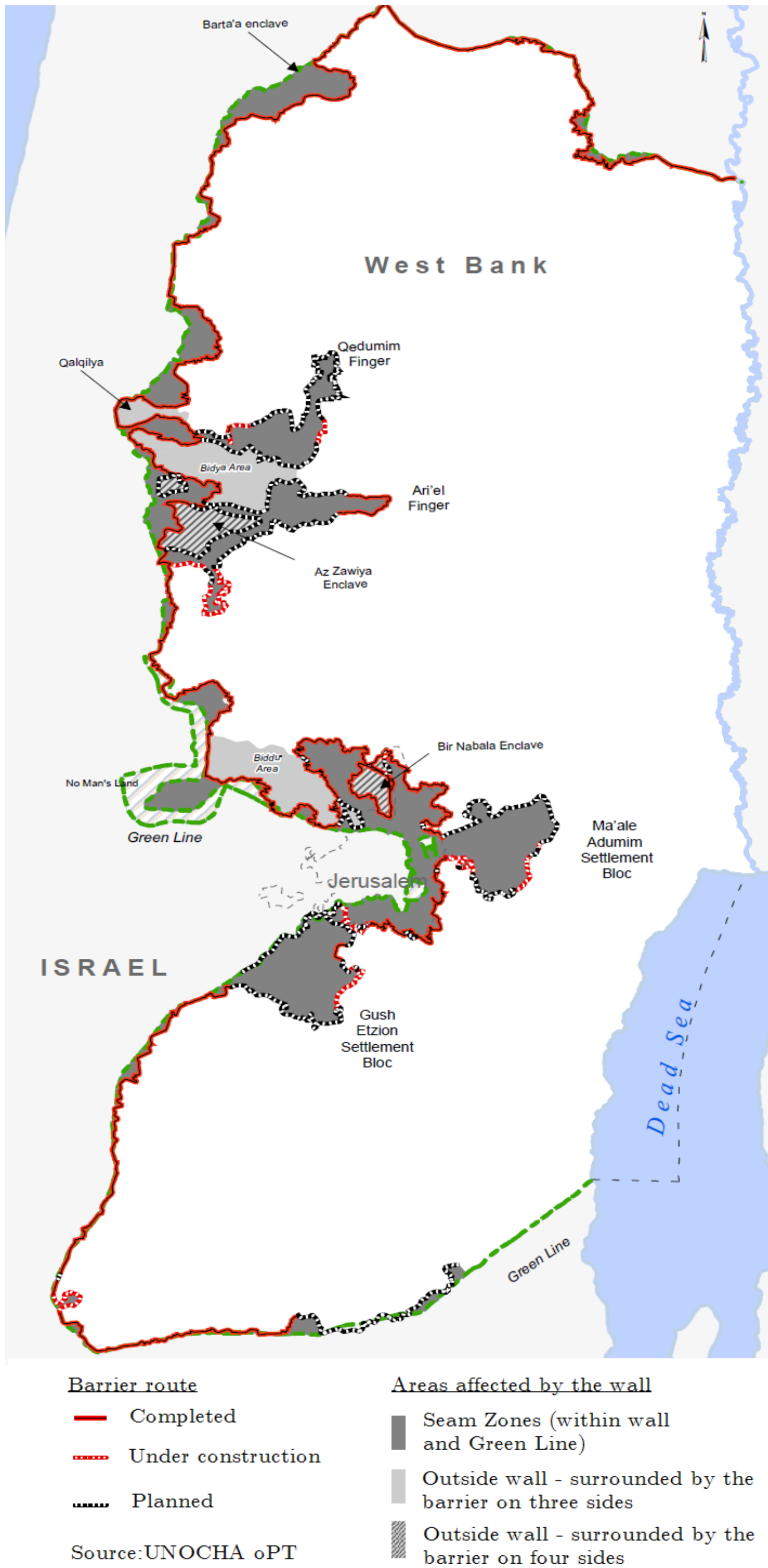
Heterogeneous effects of movement restrictions on education are detected. Students who were enrolled and about to finish school when the Wall was constructed did not give up school while students who were far from school completion were more likely to drop out. This indicates that the expected cost of staying in school -crossing the checkpoints every day- increases with the number of years before school completion. The results also show that the poorest children are the most likely to drop out, suggesting that movement restrictions not only deteriorate the average education level but may potentially increase income inequality.

Finally, boys are found to be more affected than girls. Reassuringly, the results are robust to a number of checks dealing with anticipation effects, migration issues, reversion to the mean and omitted variables.

Considerations about the specific features of the Palestinian context are important to inform the external validity of the results. First, Palestine is a part of the world with strong political tensions and the impact of limited mobility may hence be intensified by political fear, i.e. the Barrier may have an effect on education not only because of the inability to cross the Wall but also as a result of the fear of going through checkpoints. Second, the construction of a Separation Barrier is a specific type of movement restrictions. Examples of other movement obstacles abound, e.g. countries in conflict or in post-conflict often block access to specific locations due to security concerns. In the Democratic Republic of Congo and Liberia, for instance, families were forced to flee their homes and live in refugee camps where there is no access to colleges. In countries not in conflict, social and religious norms may also limit the freedom of movement, e.g. imposing women to be accompanied by a male family member when moving in public spaces.

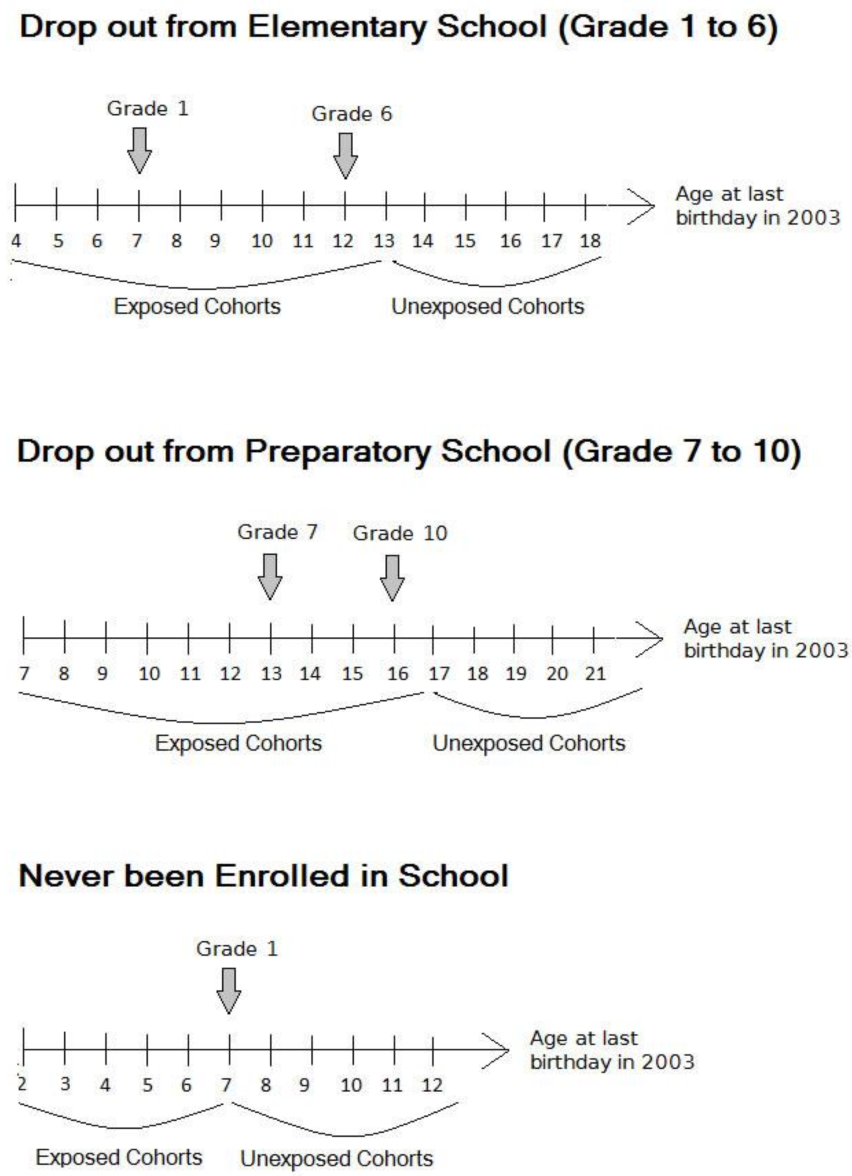
While this study shows that movement restrictions negatively impact education, more research is needed in order to better understand the effect of worse education on broader economic and social impact. This will inform us on the importance of adopting policies promoting freedom of movement to improve economic and social development.

Figure 1: Map of the West Bank - July 2008



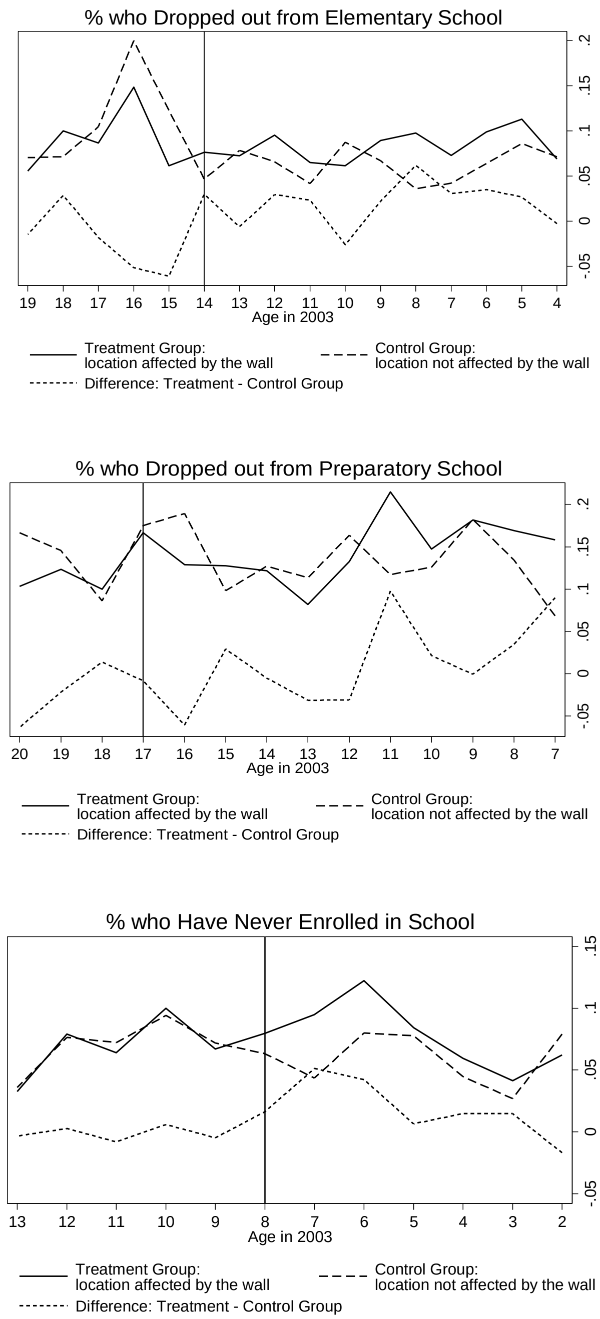
Notes: This map shows the parts of the Barrier that were constructed, under construction, or planned (not yet started) in July 2008. It also shows in grey all the areas of the West Bank that are considered affected by the Wall. An area is considered "affected" if it is located between the Green Line and the Wall as constructed in 2003 or outside but encircled by three or four sides by the Wall. Source: UNOCHA Occupied Palestinian Territory. Map can be found at: http://www.ochaopt.org/documents/barrierrouteprojections_july_2008.pdf

Figure 2: Education System and Cohorts Exposed or Unexposed to the Wall



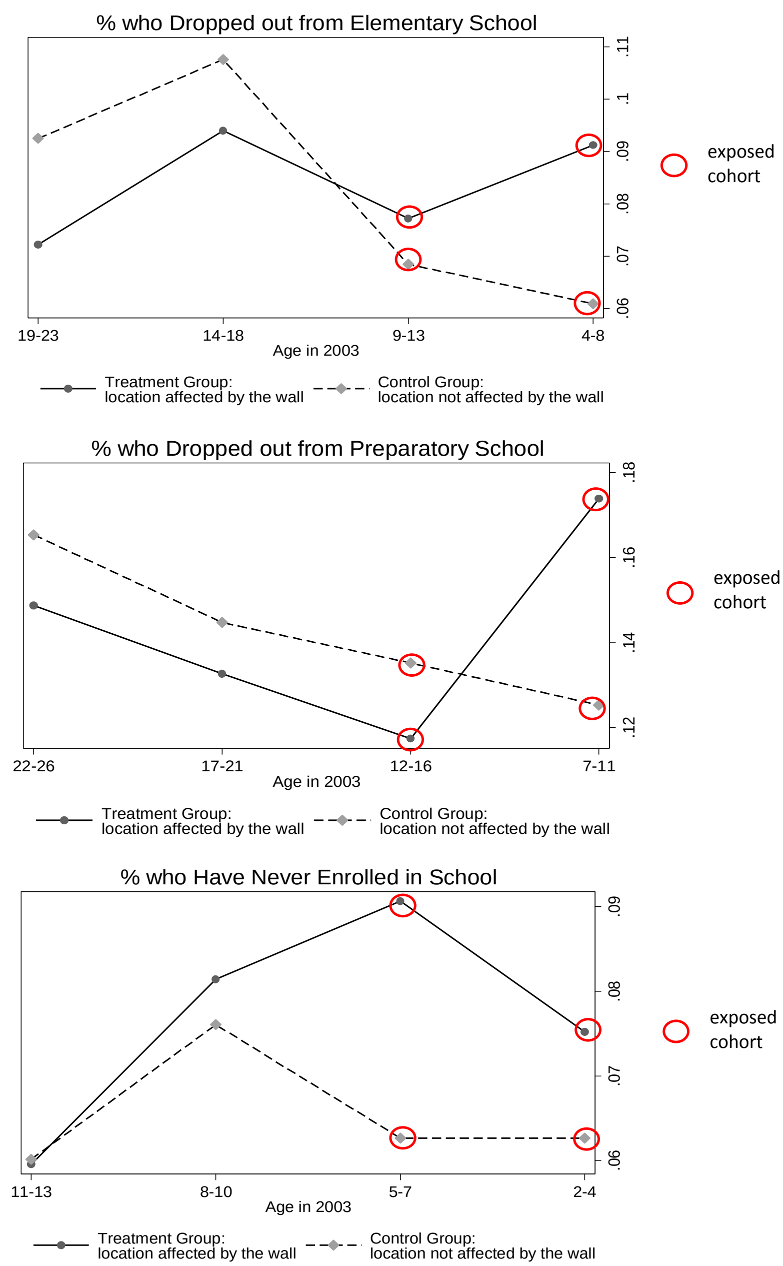
Notes: This figure indicates the age cut-offs (age of the respondent in 2003) above which a respondent is considered "exposed" to the Wall for three variables: whether a respondent dropped out from Elementary school (enrolled but never finished it), dropped out from Preparatory school (enrolled but never finished it), never been enrolled in school (never attended school and never enrolled in Elementary school). The educational system in Palestine consists in Elementary school from grade 1 to 6 and Preparatory school from grade 7 to 10. Children usually start school at 7 years old. For "drops out from Elementary school", children are exposed if they have not yet completed Elementary school in 2003, i.e. 13 years old or younger in 2003. For "drops out from Preparatory school", they are exposed if they have not finished Preparatory school in 2003, i.e. 17 or younger. For "never enrolled in school", they are exposed if they are too young to have ever attended school in 2003, i.e. 7 or younger.

Figure 3: Education by age, in Treatment and Control group



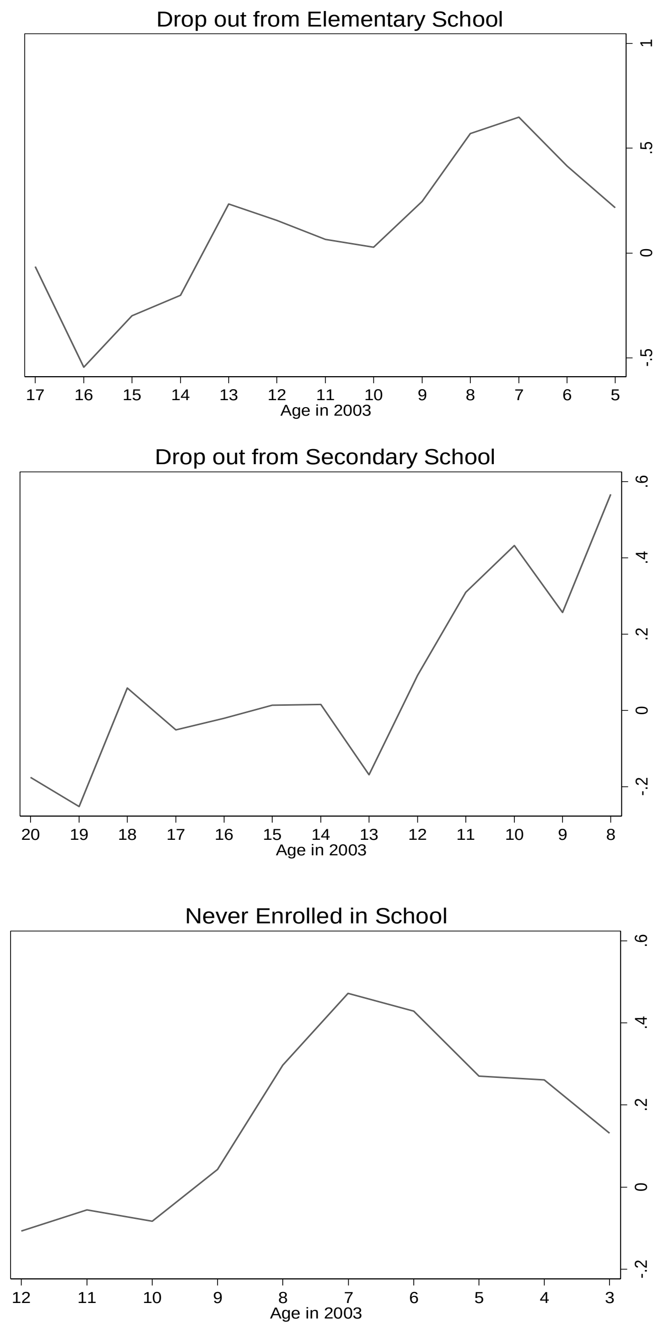
Notes: Figure shows average education outcome variable on Y axis and age of the respondent in 2003 on X axis, for treatment and control group separately and for the difference between the two groups. An area is in treatment group if it is considered “affected” by the Wall, i.e. located between the Green Line and the Wall or outside but encircled by three or four sides by the Wall. Areas in the control group are not "affected". The vertical line on the graphs indicates the age cut-offs. Agents at the right of this line are "exposed", those at the left belong to un-exposed cohort. 3 education variables are considered: whether a respondent dropped out from Elementary school (enrolled but never finished it), dropped out from Preparatory school (enrolled but never finished it), never been enrolled in school (never attended school and never enrolled in Elementary school).

Figure 4: Education for Exposed and Unexposed cohorts, in Treatment and Control group



Notes: Figure shows average education outcome variable on Y axis and different age-ranges on X axis, for treatment and control group separately. An area is in treatment group if it is considered “affected” by the Wall, i.e. located between the Green Line and the Wall or outside but encircled by three or four sides by the Wall. Areas in the control group are not "affected". Exposed cohorts are circled with a red circle. Both exposed and non-exposed cohorts are divided into a younger and an older cohort. 3 education variables are considered: whether a respondent dropped out from Elementary school (enrolled but never finished it), dropped out from Preparatory school (enrolled but never finished it), never been enrolled in school (never attended school and never enrolled in Elementary school).

Figure 5: Smooth difference-in-differences estimates, by Age



Notes: Figure plots "interaction term coefficients" obtained from regressing education outcome variable on age dummies interacted with a dummy for treatment group. In the regression, individuals in the oldest unexposed cohort form the control group. The coefficients estimate the differential effect of the Barrier for agents aged "a" in 2003 in comparison to unexposed agents. Here the coefficient is "smooth": calculates average between coefficients for age "a-1", "a" and "a+1". An area is in treatment group if it is considered “affected” by the Wall, i.e. located between the Green Line and the Wall or outside but encircled by three or four sides by the Wall. Areas in the control group are not "affected". 3 education variables are considered: whether a respondent dropped out from Elementary school (enrolled but never finished it), dropped out from Preparatory school (enrolled but never finished it), never been enrolled in school (never attended school and never enrolled in Elementary school).

Table 1: Descriptive Statistics- SEFSec Survey after Matching

	Treatment: Locations affected by the Wall	**Control:** Locations not affected by the Wall	P-value [Treatment = Control]
Respondent characteristics			
Movement restrictions are an obstacle	0.259	0.166	0.000
	(0.438)	(0.372)	
Never enrolled in school	0.080	0.072	0.084
	(0.271)	(0.258)	
Highest level of education completed…			
Elementary school	0.777	0.784	0.398
	(0.417)	(0.412)	
Preparatory school	0.501	0.528	0.009
	(0.500)	(0.499)	
Secondary school	0.225	0.239	0.101
	(0.418)	(0.426)	
Drop out from…			
Elementary school	0.071	0.068	0.379
	(0.258)	(0.251)	
Preparatory school	0.109	0.116	0.253
	(0.312)	(0.320)	
Secondary school	0.016	0.012	0.111
	(0.126)	(0.110)	
Has a work	0.284	0.275	0.256
	(0.451)	(0.446)	
Monthly household income per capita	493.705	472.954	0.024
	(407.088)	(732.976)	
Food security (scale 1 to 4)	2.829	2.697	0.000
	(1.207)	(1.201)	
Food insecure	0.699	0.613	0.000
	(0.459)	(0.487)	
Number of observations	*9433*	*9433*	

Notes: Columns 1 and 2 show respondent-level means and standard deviations in parentheses for treatment and control group. Column 3 reports the p-value of the test of equality of means based on robust standard errors. Households in the treatment group live in areas “affected” by the Wall, i.e. located between the Green Line and the Wall or outside but encircled by three or four sides by the Wall. Households in the "control group" live in areas defined as "not affected" by the Wall. While all sample individuals living in locations affected by the Wall are included in the “treatment group”, only a subsample of individuals initially sampled in non-affected areas are in the control group, i.e. those for whom the distribution of income, age and number of schools is as similar as possible to the distribution in the treated group. The matching is done without replacement and with common support. Results are robust to using different matching strategies, e.g. 5 nearest neighbors, matching with replacement. "Movement restrictions are an obstacle"=1 if respondent indicates that "in general movement restrictions represented an obstacle to me /my family during the past 6 months". "Dropped out from Elementary/Preparatory/Secondary school"=1 if student enrolled in the school but did not completing it. "Works"=1 if respondent works at least 1 hour per week and earns money for this. "Works"=0 if the respondent is not working, i.e. student, retired, looking for a job, disable, does not want to work. "Household income per capita" calculates the household average monthly family income during the past 6 months (in NIS) divided by the number of household members. "Food security" takes value 1 if the respondent is defined "food insecure" , 2 if "vulnerable", 3 if "marginally secure", 4 if "food secure". A respondent is food secure if both income and consumption are above $4.7/adult equivalent/day.

Table 2: Simple Difference-in-Differences Estimates

	(a) Drop Out from Elementary School				(b) Drop Out from Preparatory School				(c) Never enrolled in school			
	Age in 2003	**Treatment:** Locations affected by the Wall	**Control:** Locations not affected by the Wall	Difference	Age in 2003	**Treatment:** Locations affected by the Wall	**Control:** Locations not affected by the Wall	Difference	Age in 2003	**Treatment:** Locations affected by the Wall	**Control:** Locations not affected by the Wall	Difference
Panel A: Experiment of interest 1		n= 3,603				n= 2,772				n= 2,942		
Young exposed Cohort	4 - 8	0.093	0.063	0.031	7 - 11	0.169	0.126	0.043	2 - 4	0.074	0.060	0.014
		(0.0090)	(0.0070)	(0.0120)		(0.0130)	(0.0110)	(0.0180)		(0.0090)	(0.0080)	(0.0120)
Young unexposed Cohort	14 - 18	0.098	0.106	-0.008	17 - 21	0.130	0.145	-0.015	8 - 10	0.079	0.085	-0.006
		(0.0110)	(0.0110)	(0.0150)		(0.0140)	(0.0140)	(0.0200)		(0.0110)	(0.0110)	(0.0150)
Difference		-0.004	-0.043	**0.038****		0.039	-0.019	**0.059****		-0.005	-0.025	**0.020**
		(0.0140)	(0.0130)	**(0.0190)**		(0.0200)	(0.0180)	**(0.0270)**		(0.0140)	(0.0140)	**(0.0200)**
Panel B: Experiment of interest 2		n= 3,485				n= 2,438				n= 2,984		
Old exposed Cohort	9 - 13	0.079	0.074	0.005	12 - 16	0.121	0.141	-0.02	5 - 7	0.094	0.066	0.029
		(0.0090)	(0.0080)	(0.0120)		(0.0130)	(0.0130)	(0.0190)		(0.0100)	(0.0080)	(0.0130)
Young unexposed Cohort	14 - 18	0.098	0.106	-0.008	17 - 21	0.130	0.145	-0.015	8 - 10	0.079	0.085	-0.006
		(0.0110)	(0.0110)	(0.0150)		(0.0140)	(0.0140)	(0.0200)		(0.0110)	(0.0110)	(0.0150)
Difference		-0.019	-0.032	**0.013**		-0.009	-0.004	**-0.005**		0.015	-0.019	**0.034***
		(0.0140)	(0.0140)	**(0.0200)**		(0.0190)	(0.0200)	**(0.0280)**		(0.0150)	(0.0140)	**(0.0200)**
Panel C: Control experiment		n= 2,888				n= 2,025				n= 2,412		
Young unexposed Cohort	14 - 18	0.098	0.106	-0.008	17 - 21	0.130	0.145	-0.015	8 - 10	0.079	0.085	-0.006
		(0.0110)	(0.0110)	(0.0150)		(0.0140)	(0.0140)	(0.0200)		(0.0110)	(0.0110)	(0.0150)
Old unexposed Cohort	19 - 23	0.077	0.090	-0.014	22 - 26	0.154	0.155	-0.001	11 - 13	0.063	0.066	-0.003
		(0.0100)	(0.0110)	(0.0150)		(0.0170)	(0.0180)	(0.0240)		(0.0100)	(0.0100)	(0.0150)
Difference		0.021	0.015	**0.006**		-0.024	-0.010	**-0.014**		0.016	0.019	**-0.003**
		(0.0150)	(0.0160)	**(0.0220)**		(0.0220)	(0.0230)	**(0.0320)**		(0.0150)	(0.0150)	**(0.0210)**

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table reports means and standard deviations of 3 variables: (a) whether a respondent dropped out from Elementary school, (b) dropped out from Preparatory school, (c) never been enrolled in school. For each of these variables, columns report mean and standard deviations for the treatment group (location affected by the wall), the control group (location non affected by the wall) and the difference between the two groups. Rows report mean and standard deviation for exposed and unexposed cohort separately and for the difference between the two. Panel A compares young exposed and unexposed cohorts to each other, Panel B compares old exposed cohort to young unexposed cohort, Panel C compares young and old unexposed cohorts. The difference-in-differences, indicated in bold, calculates the difference of the difference. The educational system in Palestine consists in Elementary school from grade 1 to 6 and Preparatory school from grade 7 to 10. Children usually start school at 7 years old. For "drops out from Elementary school", children are exposed if they have not yet completed Elementary school in 2003, i.e. 13 years old or younger in 2003. For "drops out from Preparatory school", they are exposed if they have not finished Preparatory school in 2003, i.e. 17 or younger. For "never enrolled in school", they are exposed if they are too young to have ever attended school in 2003, i.e. 7 or younger. This is indicated in column "age in 2003". An area is in treatment group if it is considered “affected” by the Wall, i.e. located between the Green Line and the Wall or outside but encircled by three or four sides by the Wall. Areas in the control group are not "affected".

Table 3: Extended Difference-in-Differences Estimates

	(a) Drop Out from Elementary School				(b) Drop Out from Preparatory School				(c) Never enrolled in school			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Experiment of interest 1 - Young exposed vs Young unexposed cohort												
Coefficient	0.545**	0.561**	0.566**	0.492**	0.465**	0.465**	0.469**	0.465**	0.284	0.273	0.257	0.262
	(0.2370)	(0.2378)	(0.2398)	(0.2510)	(0.2224)	(0.2240)	(0.2259)	(0.2263)	(0.2832)	(0.2843)	(0.2846)	(0.2862)
Odds Ratio	1.724**	1.753**	1.761**	1.636**	1.592**	1.592**	1.599**	1.796**	1.329	1.314	1.293	1.299
	(0.4086)	(0.4168)	(0.4225)	(0.4106)	(0.3540)	(0.3566)	(0.3612)	(0.4250)	(0.3763)	(0.3735)	(0.3681)	(0.3718)
Panel B: Experiment of interest 2 - Old exposed vs Young unexposed cohort												
Coefficient	0.166	0.175	0.201	0.176	-0.030	-0.044	0.008	0.004	0.456*	0.456*	0.486*	0.467*
	(0.2399)	(0.2408)	(0.2428)	(0.2428)	(0.2394)	(0.2409)	(0.2434)	(0.2434)	(0.2722)	(0.2726)	(0.2729)	(0.2766)
Odds Ratio	1.181	1.191	1.222	1.193	0.971	0.957	1.008	1.004	1.577*	1.577*	1.626*	1.596*
	(0.2833)	(0.2868)	(0.2967)	(0.2999)	(0.2324)	(0.2306)	(0.2453)	(0.2592)	(0.4293)	(0.4299)	(0.4436)	(0.4414)
Panel 3: Control Experiment - Young unexposed vs. Old unexposed cohort												
Coefficient	0.081	0.059	0.025	0.043	-0.120	-0.146	-0.172	-0.211	-0.036	0.003	-0.001	0.020
	(0.2603)	(0.2623)	(0.2651)	(0.2811)	(0.2540)	(0.2568)	(0.2603)	(0.2619)	(0.3161)	(0.3165)	(0.3196)	(0.3220)
Odds Ratio	1.084	1.061	1.025	1.044	0.887	0.864	0.842	0.757	0.965	1.003	0.999	1.020
	(0.2823)	(0.2783)	(0.2717)	(0.2935)	(0.2254)	(0.2219)	(0.2191)	(0.2088)	(0.3051)	(0.3175)	(0.3193)	(0.3285)
Control variables												
Year of birth fixed effects	*No*	*Yes*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*Yes*	*No*	*Yes*	*Yes*	*Yes*
District fixed effects	*No*	*No*	*Yes*	*Yes*	*No*	*No*	*Yes*	*Yes*	*No*	*No*	*Yes*	*Yes*
Individual controls	*No*	*No*	*No*	*Yes*	*No*	*No*	*No*	*Yes*	*No*	*No*	*No*	*Yes*

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors. Logit regressions. The table reports coefficients and odds ratio for the interaction term between "dummy =1 of Exposed cohort" and "dummy=1 for Treatment Group". Three outcomes variables are considered: (a) whether a respondent dropped out from Elementary school, (b) dropped out from Preparatory school, (c) never been enrolled in school. For each of these variables, 4 specifications are presented. The first one includes no control, the second adds birth fixed effects, the third adds district fixed effects and the fourth adds a number of individual controls, i.e. gender, ownership of an Israeli/Jerusalem ID or not, refugee status (refugee or non-refugee), location of the house (urban, rural or refugee camp), distance to the closest school in kilometers, number of children in the household, number of parents who work in the household. Panel A compares young exposed and unexposed cohorts to each other, Panel B compares old exposed cohort to young unexposed cohort, Panel C compares young and old unexposed cohorts. For "drops out from Elementary school", children are exposed if they have not yet completed Elementary school in 2003, i.e. 13 years old or younger in 2003. For "drops out from Preparatory school", they are exposed if they have not finished Preparatory school in 2003, i.e. 17 or younger. For "never enrolled in school", they are exposed if they are too young to have ever attended school in 2003, i.e. 7 or younger. An area is in treatment group if it is considered “affected” by the Wall, i.e. located between the Green Line and the Wall or outside but encircled by three or four sides by the Wall. Areas in the control group are not "affected".

Table 4: Adjusted Difference-in-Differences Estimates

	(a) Drop Out from Elementary School	(b) Drop Out from Preparatory School	(c) Never enrolled in school
Panel A or B:	*Experiment of interest 1*	*Experiment of interest 1*	*Experiment of interest 2*
Adjusted coefficient	0.037**	0.059**	0.036*
	(0.0193)	(0.0266)	(0.0208)
Panel C:	*Control experiment*	*Control experiment*	*Control experiment*
Adjusted coefficient	-0.004	-0.029	0.001
	(0.0178)	(0.0357)	(0.0194)
Panel A or B:	*Experiment of interest 1*	*Experiment of interest 1*	*Experiment of interest 2*
Marginal effect for Exposed Cohort	0.036**	0.061**	0.033**
	(0.0144)	(0.0249)	(0.0160)
Marginal effect for Unexposed Cohort	-0.011	-0.021	-0.007
	(0.0196)	(0.0279)	(0.0186)
Year of birth fixed effects	*Yes*	*Yes*	*Yes*
District fixed effects	*Yes*	*Yes*	*Yes*
Individual controls	*Yes*	*Yes*	*Yes*

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors. The table reports coefficients for the interaction term between "dummy =1 for Exposed cohort" and "dummy=1 for Treatment Group" adjusted following Ai et al. 2003. Three outcomes variables are considered: (a) whether a respondent dropped out from Elementary school, (b) dropped out from Preparatory school, (c) never been enrolled in school. For the first two outcome variables, I report the Adjusted coefficient for experiment of interest 1 and for control experiment. Within the experiment of interest 1, I report the marginal effect for exposed and unexposed cohort separately. For "never enrollment in school", I report Adjusted coefficient for experiment of interest 2 and control experiment. Within the experiment of interest 2, I report the marginal effect for exposed and unexposed cohort separately. All specifications control for district and year of birth fixed effects and a list of individual control variables: i.e. gender, ownership of an Israeli/Jerusalem ID or not, refugee status, location of the house, distance to the closest school in kilometers, number of children in the household, number of parents who work in the household. For "drops out from Elementary school", children are exposed if they have not yet completed Elementary school in 2003, i.e. 13 years old or younger in 2003. For "drops out from Preparatory school", they are exposed if they have not finished Preparatory school in 2003, i.e. 17 or younger. For "never enrolled in school", they are exposed if they are too young to have ever attended school in 2003, i.e. 7 or younger. An area is in treatment group if it is considered “affected” by the Wall, i.e. located between the Green Line and the Wall or outside but encircled by three or four sides by the Wall. Areas in the control group are not "affected".

Table 5: Extended Difference-in-Differences by Subsamples

Panel A or B:	# obs.	(a) Drop Out from Elementary School *Experiment of interest 1*	(b) Drop Out from Preparatory School *Experiment of interest 1*	(c) Never enrolled in school *Experiment of interest 2*
PART 1: Split sample by areas of the West Bank (North, Center and South)				
Subsample = North of the West Bank	1,806			
Coefficient		1.264**	1.320***	1.182**
		(0.5620)	(0.4423)	(0.4787)
Odds Ratio		3.539**	3.108***	3.259**
		(1.9888)	(1.1295)	(1.5601)
Subsample = Center of the West Bank	1,588			
Coefficient		-0.272	-1.042	0.268
		(0.4045)	(0.6961)	(0.4697)
Odds Ratio		0.762	0.375*	1.308
		(0.3081)	(0.1917)	(0.6144)
Subsample = South of the West Bank	834			
Coefficient		0.597	0.582	-0.206
		(0.4372)	(0.5693)	(0.6521)
Odds Ratio		1.817	2.135	0.814
		(0.7945)	(1.0877)	(0.5308)
PART 2: Split sample by movement restriction intensity				
Subsample = strong intensity (index=1)	1,028			
Coefficient		1.058**	1.251***	1.301**
		(0.4146)	(0.4746)	(0.6260)
		(0.9144)	(0.6169)	(7.3517)
Odds Ratio		2.881**	3.495***	3.672**
Subsample = medium intensity (index >0 and <1)	1,091			
Coefficient		0.591	0.072	2.059**
		(0.5063)	(0.5741)	(0.9383)
Odds Ratio		1.806	1.074	7.835**
Subsample = weak intensity (index=0)	1,009			
Coefficient		-0.292	0.683	-0.265
		(0.5819)	(0.4347)	(0.4550)
Odds Ratio		0.747	1.979	0.767
		(0.4347)	(0.8604)	(0.3490)
PART 3: Split sample by refugee status				
Subsample = respondents with refugee status	1,121			
Coefficient		0.061	0.719	0.105
		(0.4139)	(0.4612)	(0.6460)
Odds Ratio		1.063	1.381	1.111
		(0.4399)	(0.5128)	(0.7175)
Subsample = respondents not with refugee status	3,107			
Coefficient		1.116***	0.160	0.527*
		(0.3586)	(0.4172)	(0.3171)
Odds Ratio		3.052***	1.801*	1.695*
		(1.0944)	(0.6133)	(0.5373)
Control variables				
Year of birth fixed effects		*Yes*	*Yes*	*Yes*
District fixed effects		*Yes*	*Yes*	*Yes*
Individual controls		*Yes*	*Yes*	*Yes*

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors. Logit regressions. The table reports coefficients and odds ratio for the interaction term between "dummy =1 of Exposed cohort" and "dummy=1 for Treatment Group". Each regression restricts to a subsample: North, Center or South area of the West Bank / areas with weak, medium or strong movement restrictions intensity / refugees and non refugees. An area has weak movement restriction intensity if the Mayssun El-Attar Index=0, medium if Mayssun El-Attar Index>0 and <1 and strong if Mayssun El-Attar Index=1. See explanation of the Mayssun El-Attar Index in the paper section 4.2. Three outcomes variables are considered: (a) whether a respondent dropped out from Elementary school, (b) dropped out from Preparatory school, (c) never been enrolled in school. For the first two outcome variables, I report the coefficient/ odds ratio for experiment of interest 1. For "never enrollment in school", I report coefficient/ odds ratio for experiment of interest 2 . For each of these variables, the specification include year of birth and district fixed effects plus a number of individual controls, i.e. gender, ownership of an Israeli/Jerusalem ID or not, refugee status (refugee or non-refugee), location of the house (urban, rural or refugee camp), distance to the closest school in kilometers, number of children in the household, number of parents who work in the household. An area is in treatment group if it is considered “affected” by the Wall, i.e. located between the Green Line and the Wall or outside but encircled by three or four sides by the Wall. Areas in the control group are not "affected".

Table 6: Extended Difference-in-Differences Estimates by Subsamples

		(a) Drop Out from Elementary School	(b) Drop Out from Preparatory School	(c) Never enrolled in school
Panel A or B:	# obs.	*Experiment of interest 1*	*Experiment of interest 1*	*Experiment of interest 2*
PART 1: Split sample by Food Security				
Subsample = Food Insecure	1,609			
Coefficient		1.349***	1.062*	0.238
		(0.4961)	(0.4312)	(0.4427)
Odds Ratio		3.852***	2.492**	1.269
		(1.8721)	(1.0880)	(0.5619)
Subsample = Food Secure	2,618			
Coefficient		0.233	0.164	0.587
		(0.2934)	(0.3045)	(0.3603)
Odds Ratio		1.262	1.688	1.798
		(0.3645)	(0.5968)	(0.6479)
PART 2: Split sample by Gender				
Subsample = Boys	2,186			
Coefficient		0.779**	0.783**	0.750**
		(0.3385)	(0.3927)	(0.3806)
Odds Ratio		2.180**	2.309**	2.116**
		(0.7256)	(0.8482)	(0.8053)
Subsample = Girls	2,186			
Coefficient		0.179	0.480	0.226
		(0.3571)	(0.3447)	(0.4047)
Odds Ratio		1.196	1.702	1.254
		(0.4273)	(0.6734)	(0.5074)
Control variables				
Year of birth fixed effects		*Yes*	*Yes*	*Yes*
District fixed effects		*Yes*	*Yes*	*Yes*
Individual controls		*Yes*	*Yes*	*Yes*

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors. Logit regressions. The table reports coefficients and odds ratio for the interaction term between "dummy =1 of Exposed cohort" and "dummy=1 for Treatment Group". Each regression restricts to a subsample: respondents defined as food secure/ food insecure and buys/girls. A respondent is food secure if both income and consumption are above $4.7/adult equivalent/day. Three outcomes variables are considered: (a) whether a respondent dropped out from Elementary school, (b) dropped out from Preparatory school, (c) never been enrolled in school. For the first two outcome variables, I report the coefficient/ odds ratio for experiment of interest 1. For "never enrollment in school", I report coefficient/ odds ratio for experiment of interest 2 . For each of these variables, the specification include year of birth and district fixed effects plus a number of individual controls, i.e. gender, ownership of an Israeli/Jerusalem ID or not, refugee status (refugee or non-refugee), location of the house (urban, rural or refugee camp), distance to the closest school in kilometers, number of children in the household, number of parents who work in the household. An area is in treatment group if it is considered “affected” by the Wall, i.e. located between the Green Line and the Wall or outside but encircled by three or four sides by the Wall. Areas in the control group are not "affected".

Table 7: Odd Ratios for Alternative Samples

	(a) Drop Out from Elementary School	(b) Drop Out from Preparatory School	(c) Never enrolled in school
1) Original Sample (as in table 2)			
Experiment of interest 1	1.636**	1.796**	1.596*
	(0.4106)	(0.4250)	(0.4414)
Control Experiment	1.044	0.757	1.020
	(0.2935)	(0.2088)	(0.3285)
2) Sample excluding East Jerusalem and Ramallah localities			
Experiment of interest 1	1.705**	1.383**	1.691*
	(0.4259)	(0.2013)	(0.4812)
Control Experiment	1.024	0.952	0.933
	(0.2828)	(0.1682)	(0.3090)
3) Sample excluding household heads and wives/husbands.			
Experiment of interest 2	1.798**	1.989**	1.609*
	(0.4761)	(0.6016)	(0.4425)
Control Experiment	0.679	0.946	1.120
	(0.2816)	(0.4703)	(0.3683)
Control variables			
Year of birth fixed effects	*Yes*	*Yes*	*Yes*
District fixed effects	*Yes*	*Yes*	*Yes*
Individual controls	*Yes*	*Yes*	*Yes*

Notes: *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors. Logit regressions. The table reports odds ratio for the interaction term between "dummy =1 of Exposed cohort" and "dummy=1 for Treatment Group". The results are presented for 3 samples: full sample (as in table 2), sample excluding East Jerusalem and Ramallah localities, sample excluding households heads and their wives/husbands. Three outcomes variables are considered: (a) whether a respondent dropped out from Elementary school, (b) dropped out from Preparatory school, (c) never been enrolled in school. For each of these variables, specification includes district/ year of birth fixed effects and a number of individual controls, i.e. gender, ownership of an Israeli/Jerusalem ID or not, refugee status, location of the house, distance to the closest school in kilometers, number of children in the household, number of parents who work in the household. Experiment of interest 1 compares young exposed and unexposed cohorts to each other, Experiment of interest 2 compares old exposed cohort to young unexposed cohort, Control Experiment compares young and old unexposed cohorts. For "drops out from Elementary school", children are exposed if they have not yet completed Elementary school in 2003, i.e. 13 years old or younger in 2003. For "drops out from Preparatory school", they are exposed if they have not finished Preparatory school in 2003, i.e. 17 or younger. For "never enrolled in school", they are exposed if they are too young to have ever attended school in 2003, i.e. 7 or younger. An area is in treatment group if it is considered “affected” by the Wall, i.e. located between the Green Line and the Wall or outside but encircled by three or four sides by the Wall.

Table A1: More Simple Difference-in-Differences Estimates

		(a) Drop Out from Elementary School				(b) Drop Out from Preparatory School				(c) Never enrolled in school			
		Age in 2003	**Treatment:** Locations affected by the Wall	**Control:** Locations not affected by the Wall	Difference	Age in 2003	**Treatment:** Locations affected by the Wall	**Control:** Locations not affected by the Wall	Difference	Age in 2003	**Treatment:** Locations affected by the Wall	**Control:** Locations not affected by the Wall	Difference
	Young exposed Cohort	4 - 8	0.093	0.063	0.031	7 - 11	0.169	0.126	0.043	2 - 4	0.074	0.060	0.014
Panel D	Old exposed Cohort	9 - 13	0.098	0.106	-0.008	12 - 16	0.121	0.141	-0.02	5 - 7	0.094	0.066	0.029
	Difference		-0.005	-0.043	**0.039***		0.048	-0.015	**0.063****		-0.020	-0.006	**-0.015**
	Young exposed Cohort	4 - 8	0.093	0.063	0.031	7 - 11	0.169	0.126	0.043	2 - 4	0.074	0.060	0.014
Panel E	Old unexposed Cohort	19 - 23	0.077	0.090	-0.014	22 - 26	0.154	0.155	-0.001	11 - 13	0.063	0.066	-0.003
	Difference		0.016	-0.027	**0.045****		0.015	-0.029	**0.044****		0.011	-0.006	**0.017**
	Old exposed Cohort	9 - 13	0.098	0.106	-0.008	12 - 16	0.121	0.141	-0.02	5 - 7	0.094	0.066	0.029
Panel F	Old unexposed Cohort	19 - 23	0.077	0.090	-0.014	22 - 26	0.154	0.155	-0.001	11 - 13	0.063	0.066	-0.003
	Difference		0.021	0.016	**0.006**		-0.033	-0.014	**-0.019**		0.031	0.000	**0.032**

Notes: *** p<0.01, ** p<0.05, * p<0.1. The table reports mean and standard deviations of 3 variables: (a) whether a respondent dropped out from Elementary school, (b) dropped out from Preparatory school, (c) never been enrolled in school. For each of these variables, columns report mean and standard deviations for the treatment group (location affected by the wall), the control group (location non affected by the wall) and the difference between the two groups. Rows report mean and standard deviation for exposed and unexposed cohort separately and for the difference between the two. Panel D compares young and old exposed cohorts, Panel E compares young exposed to old unexposed cohorts, Panel F compares old exposed cohort to old unexposed cohort. The difference-in-differences, indicated in bold, calculates the difference of the difference. The educational system in Palestine consists in Elementary school from grade 1 to 6 and Preparatory school from grade 7 to 10. Children usually start school at 7 years old. For "drops out from Elementary school", children are exposed if they have not yet completed Elementary school in 2003, i.e. 13 years old or younger in 2003. For "drops out from Preparatory school", they are exposed if they have not finished Preparatory school in 2003, i.e. 17 or younger. For "never enrolled in school", they are exposed if they are too young to have ever attended school in 2003, i.e. 7 or younger. This is indicated in column "age in 2003". An area is in treatment group if it is considered “affected” by the Wall, i.e. located between the Green Line and the Wall or outside but encircled by three or four sides by the Wall. Areas in the control group are not "affected".

Table A2: Effect of Education on the Probability of Staying in same Household

OUTCOME VARIABLE=1 if respondent lives with her parents

Explanatory variable	Years of education	Education level	Drop out from elementary	Drop out from preparatory	No school
Coefficient	-0.016	-0.042	0.219	-0.107	0.156
	(0.0197)	(0.0508)	(0.2400)	(0.2465)	(0.1914)
Control variables					
District fixed effects	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Individual characteristrics	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*
Household characteristrics	*Yes*	*Yes*	*Yes*	*Yes*	*Yes*

Notes: Individuals aged 19 to 35 are included in the regressions. *** p<0.01, ** p<0.05, * p<0.1. Robust standard errors. Logit regressions. The table reports coefficient from regressing whether an individual lives with her parents in 2009 on education variables. Specification includes district/ year of birth fixed effects and a number of individual controls, i.e. gender, ownership of an Israeli/Jerusalem ID or not, refugee status, location of the house, distance to the closest school in kilometers, number of children in the household, number of parents who work in the household.

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