Trois essais en économie du développement

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CONTENTS

INTRODUCTION

My thesis explores three questions. First, what determines human capital investments by households? Second, what determines the ability of households to use the full extent of their human capital, in particular what determines the labor supply of women? Third, how does increased access to credit impact households, and, once again, their ability to fully leverage their human capital? I have approached these questions empirically, conducting randomized field experiments in both rural and urban areas in Morocco and Djibouti, in the Maghreb Region and in Northeast Africa. Compared to other developing regions of the world, the Maghreb has not been as much studied using the methods that have been widely used in empirical development economics in the last two decades. By applying these methods to households in Morocco and in Djibouti, my thesis contributes to our understanding of the special challenges and opportunities faced by households in what is commonly considered part of the Arab world, especially with regard to their patterns for accumulating and using human capital.

In the following paragraphs, I succinctly present the genesis of and rationale behind each of the three articles of my thesis, their key methodological aspects as well as their main results.

The first article, *"Women at work: evidence from a randomized experiment in urban Djibouti,"* is on the determinants of households' ability to use the full extent of their human capital. Human capital may be acquired through both formal and informal education. For example, men and women with little or no formal education often learn handicraft from family members and peers. Alternatively, when they are in good health, they can take up manual work that requires little or no training. Accumulating these forms of human capital increases their potential for earning wages by working outside of home. But households in developing countries often do not use the full extent of their human capital. In particular, women labor force participation remains low. Is it due to limited work opportunities or are there other factors at play, namely social norms, that limit their willingness and ability to join the labor force (working outside of home) and exploit the extent of their human capital?

To explore these questions, we conducted a randomized field experiment. Women were randomly assigned offers to be employed in a workfare program. The offered wages were high compared to female earnings from self-employment activities and working conditions were set to facilitate women's participation (e.g. daily work schedules of four hours, no time or monetary costs of transportation to arrive to the work site and scheduled breaks to take care of young children). The targeted women had very low levels of literacy and only one out of ten was participating in the labor market at the time the program started. Women turned out to be highly interested in the job offers made available to them as evidenced by the almost universal take-up of the program. But they reverted back to inactivity once the employment offers were not available anymore. These results lead us to infer that the main barrier to labor force participation is not some form of prevailing social norms but rather the lack of suitable employment opportunities . Also, the lack of effect on women's employment in the medium term suggests that the intervention itself did not induce any change in prevailing attitudes towards women's work.

The second article, "*Turning a Shove into a Nudge? A "Labeled Cash Transfer for Education,*" is on the determinants of human capital investment by household. In particular, it examines the role of different standard features of conditional cash transfers programs. Even if a large body of literature has already shown the ability of these programs to increase household's investments in education and health, little was known at the time this study was conceived about the contribution of some of their standard features such as the conditionality, whether the designated beneficiary matters as well as the cost-benefits of various targeting mechanisms. A randomized control trial was thus designed to examine the effects of small transfers, made to fathers and targeted to poor communities where all households were eligible to the transfers. The program implicitly endorses education through an enrollment procedure that takes place at schools. Within the same experimental setting, the following variants to program design were introduced: i) making the transfers conditional on school attendance, ii) designating the mother as the recipient of the cash transfers, and iii) both together.

Large impacts of cash transfers labeled for education are detected in school participation. These effects may be due in part to an endorsement effect. Parents may have interpreted the introduction of the program as a positive signal about the value of education as suggested by the large increase observed in parental beliefs about returns on education, especially for girls. These large impacts occurred independently of the variants of program design examined. Making transfers to mothers, as opposed to the fathers, leads to almost no differences in school participation. Making the transfers conditional on child attendance, as opposed to simply labeling them, slightly decreases effects on school participation. In a context where pupil absenteeism is low to begin with, the labeled version of the cash transfer program (i.e., with no conditionality on attendance) thus ended up being more cost effective than standard cash transfers conditional on attendance.

The third article, *"Estimating the impact of microcredit on those who take it up: evidence from a randomized experiment in Morocco,"* studies how improved access to credit impacts households and their human capital use. At the time this study was conceived no other study had rigorously measured the impacts of microcredit lending. It was launched in parallel to another randomized control trial that explored a similar question in urban areas of India. To examine the impacts of microcredit, a randomized experiment was designed in close collaboration with Al Amana, the largest microcredit institution in Morocco. Selected villages in rural areas of Morocco were matched in pairs with one of them randomly selected to be granted microcredit access earlier on, while the other would gain access two years later.

The results of the study showed that the take-up of microcredit loans was fairly low at 17 percent. Access to microcredit allowed households in the treatment villages to invest significantly more in their self-employment activities, mostly in agriculture and animal husbandry. Profits from these activities increased consequently, but the effects were very heterogenous and concentrated at the highest quartile of the profit distribution. No effects on total household income were documented as the labor supply of adults outside the household declined. Similarly, no effect on consumption could be detected. Last, no anticipation effects or externalities were detected among the non-borrowing households. While the results of this study suggest that microcredit constitute an important financial instrument for the poor, no evidence was found about it being a powerful instrument to boost household consumption, at least in the medium term.

CHAPTER 1

Women at work: evidence from a randomized experiment in urban Djibouti

*By Florencia Devoto, Emanuela Galasso and Stefanie Brodmann**

Abstract

What keeps women in some developing countries from participating in the labor market? Is it limited job opportunities or limiting social norms? We examined the effects of these two factors on the labor supply decisions of women in urban Djibouti. Women were randomly assigned offers to be employed in a workfare program. The offers were exclusively targeted at women; the work could be performed by any other household member; and the earnings were paid out into a bank account established for the person who performed the work. We find a net increase in labor supply of over 50 percentage points: 96 percent of the women accepted the offers and 73 percent of women performed the work themselves. We observed none of the longer-term effects on labor supply by women that we would have observed if the increases in women's employment had changed prevailing social norms on women working. Indeed, the women who received the temporary employment offer reverted back to non-participation in the labor market when the program ended. This suggests that, in urban Djibouti, what keeps women from participating in the labor market is not so much deterrent social norms but limited employment opportunities.

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1. Introduction

In many developing countries, only a small portion of women participate in the labor market.[1] To what extent could the low labor force participation of women be explained by limited job opportunities as opposed to prevailing social norms that limit them from working outside the home?

Limited job opportunities has long been seen as a key explanation. It has been empirically demonstrated that economic development and women's participation in the labor force are associated, following a U-shaped pattern (Boserup, 1970; Goldin, 1995). Given this empirical regularity, one would expect poor countries to be on the downward sloping part of this U-shaped curve, departing from high levels of female labor force participation (mostly in family-run agricultural businesses or in self-employment activities) as a subsistence strategy to contribute to households' income. Recent research suggests that this trajectory may be changing as new job opportunities suitable for women become available earlier in the development process (Heath and Jayachandran, 2016), leading to increased participation by women as economies develop.

More recently researchers have proffered explanations centering on the existence of social norms that limit the willingness and ability of women to work outside the home.[2] Traditional norms typically support the view that women should work at home while men should join the labor market. Gender norms about female labor force participation may reflect collective-held beliefs or individual preferences that are cultural specific and may internalized by both men and women. Empirical work in South Asia shows that these norms have not only limited the ability of poor women to seek wage-earning work outside of the home but also constrained their entrepreneurial choices (Jayachandran 2015; Field *et al* 2016a, 2016b). In historical perspective, even in developed countries such as the US (Goldin, 2006; Fortin,

[1] For example, in Northern-Africa and in India, female labor force participation rates are of 23 percent and 27 percent respectively, while the average for the group of lower-middle income countries is of 38 percent (ILO, 2015).

[2] Early theoretical developments focused on studying inequalities in human capital accumulation between men and women and the impacts of discrimination to explain the observed labor market disparities. More recent theoretical and empirical developments that explore gender-specific preferences and social norms are discussed in detail by Bertrand (2011).

2015), the concept of gender identity gradually evolved alongside with improvements in human capital and female labor force participation.

To test the relative influences of these two factors, we conducted an experiment in a poor urban area in Djibouti in North Africa. We offered women short-term employment in a workfare program and studied the effects of this improvement in job opportunities on their labor supply decisions.

The context of our study presents a set of characteristics that makes it of interest for testing these hypotheses. Women in Djibouti City have low levels of literacy,[3] and, with female employment rates of only 15 percent[4][5], their participation in the labor market is equally low. Among women with no formal education, the few who engage in the labor market do so through subsistence self-employment activities and through (very limited) casual informal work. Djibouti is characterized by prominent gender inequalities, which likely reflects society's views on women's and men's roles. Indeed, social interactions in Djibouti are mainly driven by principles of Islamic laws,[6] implying that men and women do not always have an equal say in decisions that concern the household. In the context of Muslim marriages, as established by the marriage contract, husbands are expected to provide for the family (food, clothing and lodging) in exchange for women's obedience, which includes, among other duties, childrearing and household work (Tucker, 2008). The agency of women towards taking advantage of employment activities outside the household may thus be limited.

We randomly offered women the opportunity for short-term employment in a workfare program. The program was introduced in response to high food prices after the country experienced a severe drought. The Djiboutian workfare program stands out due to its explicit gender focus: the program designates the woman as the principal recipient of the workfare

[3] In Djibouti City, around half of adult women are illiterate, 50 percent have no education and 20 percent completed primary school (DISED, 2016).

[4] World Bank (2015).

[5] Female labor force participation rate was of 25 percent in Djibouti in 2012, with around half of the active women being unemployed (DISED, 2013). Women's participation in the labor market in Djibouti is thus below the average rate of 38 percent recorded for the group of lower-middle income countries (ILO, 2015).

[6] Around 95 percent of the population in Djibouti is Muslim.

employment offer.[7] Women were offered 50 days of consecutive work to perform cleaning services (e.g. garbage collection, particularly plastic bags), light labor-intensive community works, and small artisanal projects. In exchange, they were provided wages equivalent to 80 percent of the minimum wage. The wage rate is substantially higher than earnings women could obtain from self-employment activities. Women in eligible households could delegate the offer to any other adult member of their household, male or female. In specific cases, delegation to adults outside their household was allowed. The wage was paid into a bank account established for the person who worked. The motivation for opening a bank account in the name of the worker originated in the fact that in some contexts where women have low bargaining power, the lack of control over earnings can constitute one of the potential explanations of women's decision to not enter the labor market (Field et al, 2016b).

Based on detailed high frequency data on the labor supply of beneficiary women, their husbands, and other adult members, we measure their labor supply response to the short-term employment offer. Time use data allow us to analyze the reallocation of household tasks traditionally performed by women (i.e. childcare and household chores) to other household members. We also look at how the job offer affects women's decision-making power, intra-household resource allocation decisions as well as the effects on women's and husbands' perceived well-being. We measure these effects at two points in time: during the implementation of the program as well as nine months after the program ended.

Our results show that offering improved job opportunities is highly effective in encouraging women's labor force participation. In the absence of casual labor markets for women, and with only a small fraction of women engaged in self-employment activities with low returns, the relatively attractive wage rate resulted in a sizeable labor supply response: 96 percent of households who were offered the short-term employment opportunity took it up. Women massively took up the workfare program leading to a net increase of 55 and 66 percentage points in women's employment and women's workfare employment respectively. In those instances where women were unable to participate for exogenous reasons (program rules forbade the participation of pregnant women, those with newborn babies below the age of 40

[7] Many workfare programs usually include gender recommendations for participation for equity reasons (e.g. India's NREGA prioritizes 1/3 of their work to women), though without binding quotas.

days, or those who were sick at the time of the work offer) or decided to not perform the work themselves due to reasons other than program rules, women accepted the offer and delegated to another person. The large take up of the short-term employment opportunity together with the significant net increases in women's labor supply are consistent with the explanation that, in this setting, limited job opportunities rather than limiting social norms act as an impediment to female labor force participation. Our experimental setting does not allow us however to claim that social norms are not to some extent a limiting factor to women's labor force participation, but, instead, that social norms do not appear to be binding when offered wages are high enough.

Did the intervention itself shift social norms about attitudes towards women's work or shifted the bargaining power of women within the household? The intervention substantially increased women's employment, and in so doing could have acted as a coordinating device for both men and women to agree on women working outside the house. Had social norms changed along dimensions that we did not capture in our survey, we would have observed women continuing to work after the program ended, but we did not. Once the short employment opportunity ended, women reverted mostly to inactivity. The female labor supply effects observed during the program faded in the medium-term, once the workfare employment opportunities were no longer available. The lack of sustained employment effects suggests that the intervention itself did not induce any change in prevailing attitudes towards women's work. We do not find evidence that women's decision-making power (proxied by self-reported women's perceived participation in household decisions) changed as a result of the intervention in the medium-term either. This was to be expected given the temporary nature of the employment offer.

When analyzing the heterogeneity of the labor supply results according to women's pre-program employment status, we show that most of the effects are driven by women who were inactive at the time of the baseline survey. While program participation is similar in the two groups, net effects on employment and on earnings are much weaker among women who were already participating in the labor force through self-employment activities. Women engaged in self-employment activities at baseline were marginally poorer than the inactive ones, conditional on both having very low literacy rates. At endline, they were less likely to go back to their self-employment activities after the program had ended. Given the low return of their activities, women may have been temporarily using their savings from the intervention to consume instead of going back to jobs that generate low earnings, or, alternatively, this behavior may represent a temporary response due to the existence of fixed costs that prevent women from restarting their activities immediately.

An interesting result is that most of the women who received the workfare job offer were able to keep control over their labor earnings. Only a small portion of women gave earnings over to their husbands, while the level of transfers husbands made to their wives were unaffected. Women also acted rationally when allocating the additional labor earnings. As expected due the transient nature of the program, women, most likely, saved most of the additional income gains, consuming only a small share in the immediate run.

The rest of this paper is structured at follows. In section 2, we present the context and the intervention. Section 3 presents our experimental design, followed by the data collection in Section 4. The empirical methods we use in the analysis are described in Section 5. Main results are discussed in Section 6. We then present an analysis of the heterogeneity of effects by initial women's employment status in Section 7. In section 8, we conclude.

2. Context and intervention

Djibouti is a small country in North Africa with limited economic diversification. Growth during the past decade has been driven by foreign direct investment and public sector-led infrastructure investment, with limited trickle-down effects on job creation and poverty reduction for large segments of the population. About one-fifth (23 percent in 2013) of the population lives in extreme poverty and one-third lives with less than 2 USD a day (World Bank, 2015). About 20 percent of the urban population suffers from severe or moderate food insecurity (World Food Program, 2013). These high poverty rates are matched with low human development outcomes compared to other lower middle-income countries. One-third of the children are underweight or stunted, and maternal and child mortality remains high (DISED, 2012). Only 60 percent of the population aged 15 and older is literate, and the primary net enrolment rate in primary and middle school is just above 50 per cent in urban areas (World Bank, 2015).

Poor households with low educational attainment rely on labor earnings as their main source of income. With the public sector employing a large proportion of the educated population in the economy, and limited coverage of safety nets, labor participation remains very low, at about 40 percent, and participation rates are much lower among the poor (World Bank, 2015).

Women are at a severe disadvantage in the labor market. Official statistics show that women's labor force participation is only half of that of men (25 versus 54 percent)[8] and employment rates are even lower (15.5 percent for women and 43 percent for men). Higher education significantly improves women's opportunities for employment, mainly through employment in the public sector: about 43 percent of women with at least three years of university education have a job, but gender inequalities remain important (close to 70 percent of men with the same level of education are employed) (World Bank, 2015).

In this constrained environment, the Djibouti government launched a workfare program with support from the World Bank in the aftermath of high food prices and a drought. The objective was to provide a safety net to poor households faced by high unemployment and high food prices. The workfare program was first rolled-out in selected poor neighborhoods of Djibouti City and successively expanded to further urban and rural areas. Contrary to other settings, the workfare program has an explicit gender targeting, the primary target being households with pregnant women and children aged 0-2 as their target population. To be eligible to the workfare employment, households had to first enroll (and actively participate) in community-based activities aimed at promoting child and maternal nutrition. The interlinkage of the two interventions aimed to protect human capital investments that are crucial during the first 1,000 days by providing temporary income support. The self-targeting mechanism of workfare program aimed to maximize household net income gains by

[8] Female labor market participation rates are significantly lower in Djibouti and Somalia (32 percent) than in Eritrea (78 percent) and Ethiopia (77 percent), all countries belonging to region known as the Horn of Africa (ILO, 2015). In countries in the Middle-East and North Africa region, on average, female labor force participation reaches 22 percent.

attracting the poorest households who are willing to work at the statutory wage.[9] All households eligible to the welfare program were thus participating in nutrition promotion activities at the time the program was rolled-out. Women attended monthly group meetings (with a maximum of 20 women) held at walking distance venues from their homes. Each session (pregnant women and those with children 0-2 years of age were in different groups) started off with growth/weight monitoring by a community worker. The sessions which lasted for about two hours, included nutrition education, growth promotion, cooking sessions, and the distribution of nutritional supplements.[10]

The workfare component of the program included public works projects such as services (e.g. garbage collection, particularly plastic bags), light labor-intensive community works (street rehabilitation to improve traffic within and access to selected areas), and small artisanal projects. Communities were asked about their preferences with regard to the community works they wished to perform.

Employment offers in the workfare program were given preferentially to women. The women had the option of delegating the offer to any other adult member of their household, male or female who would then perform the work. Just before a group of public works projects was about to be launched, the research team communicated to program managers the list of women randomly assigned to that group. Female facilitators who ran the nutrition sessions attended by the pool of women in our sample were in charge of calling the selected women to a meeting where the public works jobs were offered to them. By the end of the meeting, women had to decide whether they wanted to take the job themselves or preferred to delegate it to another household member. While delegation was meant, by program design, to occur only in favor of other household members, in specific cases, delegation to adults outside the beneficiary's household was allowed. In this meeting, program officials also

[9] Growing evidence suggests that safety nets and social transfers might not only play the role of redistribution to the poor, but also may play an important investment role for long term human capital accumulation (US evidence Hoynes Miller, Simon 2015, Hoynes, Schanzenbach, and Almond 2016).

[10] This community approach is based on positive deviance and reinforcement of good behaviors, i.e. mothers in the community whose children are healthy and growing well despite living in the same harsh economic and environmental conditions as their peers. If a problem is detected during the sessions, the family will subsequently receive a home visit to provide more individualized counseling, and/or referral to the nearest health clinic.

collected the required administrative documents to issue the work contracts. After two weeks, on average, of various administrative procedures, the public works projects were started.

The works were planned to minimize the risk of health hazards to women, and avoid a crowding out effect on time spent on nurturing care and breastfeeding. Pregnant women in their last trimester of pregnancy as well as women with children in their first month after birth and a half were required to delegate the public works offer to another household member. Pregnant women and lactating women in their first six months were offered a handicraft project on a preferential basis, and whenever they did light community work, the implementing agency enforced breastfeeding breaks and the use of protective gear.

The household member who participated in the workfare component got paid through a bank account opened in her/his name. By opening bank accounts in the name of the workfare beneficiary, the program aimed at ensuring women's control over their own earnings. Indeed, in some contexts where women have low bargaining power, the lack of control over earnings can constitute one of the potential explanations of women's decision to not enter the labor market (Field at al, 2016b).

The public works program lasted for 50 days and provided a daily wage of 1,000 DJF (corresponding to about 80 percent of the minimum wage or about 5.6 USD). The gross income transfer is quite substantial in such context of high inactivity and unemployment. The median woman in our sample does not receive any labor income since only a small proportion of them perform paid work. Conditional on being employed, the offered weekly remuneration was 35 percent higher than women's median weekly labor income (with all work pooled together) and almost 100 percent higher than the median weekly earnings from women's self-employment activities. Potential earnings from workfare participation are less significant when compared to husbands' labor income: the hourly wage offered by the program is equivalent or lower than the one husbands obtain from day work and salaried employment and work hours offered are significantly lower. When we consider the median household in our sample, the weekly wage offered by the public works program was of the same order of magnitude as the weekly household labor income, which represents a significant potential increase in household's income from labor.

3. Experimental design

They study took place in a poor neighborhood of urban Djibouti City that was about to receive the public works program. The study covers all households in that neighborhood with a pregnant woman and/or children younger than 2 years old, that had registered for participation and had been assigned to a nutrition session group. As such, eligibility is established at the time a household joined the nutrition meetings. A total of 1,055 eligible households were identified based on program administrative data and a total of 1,011 households were successfully interviewed (96 percent response rate).

Figure 1 summarizes the experimental design. The evaluation exploits the gradual rollout of the public works activities within the neighborhood with a randomized assignment of the timing of offer to participate in a public works activity. The phase-in design of the intervention itself consisted of 250 public works positions set up every 6 months. Out of a list containing all eligible households surveyed at baseline (1,011 in total), households were randomly assigned to 4 groups: 257 households were randomly selected to receive the offer to participate in the public works program between May and September 2014 (Group A), 247 to receive the offer between November 2014 and May 2015 (Group B), 253 to receive it after June 2015 (Group C) and the other 254 after February 2016 (Group D)[11]. We stratified by nutrition groups women were assigned to. This implies that stratification was done at the geographical level[12] and by women status (pregnant or/and with child 0-2 years old). The 504 households that were given the opportunity to work with priority, groups A and B, constitute the treatment group. The other 507 households, groups C and D, constitute the control group, which received the intervention on average twelve months later than the treatment group (or, equivalently, nine months after the intervention in the corresponding treatment group ended).

There might be two potential threats to the validity of this experiment. First, the gradual rollout of the intervention might have generated anticipation effects, by the fact that women

[11] Randomization of the sample into the 4 groups has been done by the researchers after the baseline survey and before the program was announced. The list of selected households to receive the job offer in each group of public works projects was made available to program managers just before the projects started and its implementation was closely followed by the research team. There is thus no scope for expectations to receiving the program being systematically different across treatment and control groups.

[12] For program implementation, Hayabley district was divided in five geographical areas and women were assigned to nutrition sessions taking place within the area they lived.

know they will get the workfare offer at some point in time (even if they do not know when exactly). As a result, women could decide to delay their engagement in self-employment activities while attending to the workfare offer to come. Alternatively, they could access credit anticipating the increase in income due to workfare future participation. The second potential threat depends on the existence of general equilibrium effects: with a large enough program, the intervention might crowd-out the labor supply of those already (self-)employed, and, as a consequence, reduce competition in specific markets.[13] Our data suggest that most probably these effects are not at play since the median income from self-employment is similar at midline and endline (in the control group). Since time may also affect outcomes, we cannot affirm with certainty that our design is no affected by anticipation effects or externalities but, if there is a bias in our estimates, most likely, it is marginal[14].

4. Data

A baseline survey was administered to all eligible households in first quarter of 2014, immediately before the start of the intervention. A midline survey was conducted while the public works were taking place. This survey allows us to identify contemporaneous or short-term effects of the workfare employment (i.e. while the household is offered to participate in the temporary improved job opportunities). An endline survey was conducted once treatment households had already finalized the 50 days of work with the aim of measuring the effects of public works in the medium term, 9 months after the public works had ended[15].

While the baseline survey was administered to the entire sample at once, just before the public works program was announced, the midline and endline surveys were administered in a staggered fashion for the different groups, to align to the randomized offer of the program. Each treatment group was interviewed with its corresponding randomized control group both at midline - during the intervention - as well as at endline, after the workfare had ended.

[13] Imbert and Papp (2015) show that the rollout of India's workfare program (the Mahatma Gandhi National Rural Employment Guarantee Scheme) impacted private sector wages.

[14] In the next section we present the different survey instruments used at baseline, midline and endline. Employment and earnings were collected as part of the household survey at baseline, while employment diaries administered during three consecutive weeks were used to collect data on these outcomes. When comparing the level of these outcomes over time, it is important to keep in mind that the change in the survey instrument affected the measurement precision of the outcome.

[15] Or, equivalently, 12 months after the public works were launched.

Group A was interviewed with group C, and group B was interviewed with group D. The endline surveys for the treatment groups and their corresponding randomized control groups took place before the latter got offered the intervention.

Three types of data were collected. (i) A comprehensive *household survey* was administered both at baseline and endline. Selected modules of the survey were also administered at midline. (ii) Detailed *employment diaries* during three consecutive weeks at midline and endline. (iii) Finally, *administrative data* was used to complement survey data. We collected program data program data on beneficiaries extracted from the program managing information system and data on payments and transactions obtained from the financial institution in charge of paying the program beneficiaries. A detailed timeline of the surveys and interventions is presented in Figure A1 of the Apppendix.

Household Survey

A baseline household survey was administered between January and March 2014 to the entire sample of 1,011 households selected for the study. The list of eligible households was identified based on the existing program administrative list of women beneficiaries that have joined the nutrition meetings by January 2014. Out of a total of 1,055 eligible households, a total of 1,011 households were interviewed (96 percent response rate). One third of the non-responses were due to the absence of the household and another third to the refusal of the beneficiary and her husband to be interviewed. A comprehensive baseline household survey was administered to beneficiary women, and another shorter survey to the husbands of these women. The woman beneficiary survey covered the following topics: household socioeconomic characteristics, non-labor income, transfers, time use, durable assets, housing characteristics, household expenses, health and nutrition practices, food security, intra-household decision making, personality traits and well-being. The man survey covered: labor supply of household members and income from labor, time use, household expenses on items usually bought by male members (khat, cigarettes, transport, etc.), intra-household decision making, personality traits and well-being.

Selected modules of the baseline household survey were also administered at midline to the same respondents which allowed us to collect data on time use, expenditures, food diversity and security, school participation, program knowledge and public works delegation. If the beneficiary woman has delegated the public works to another household member or to another person not belonging to the household, the latter was not interviewed. We then recorded in the interview to the beneficiary women the income sharing rule agreed with the person to whom she has delegated. A household questionnaire similar to the one at baseline was administered to the beneficiary woman and her husband at endline.

Employment Diaries

The objective of the employment diaries was to get a more precise measure of the working status of the population we study due to the large importance of day work[16]. Employment diaries were administered to both the beneficiary woman and her husband during three consecutive weeks while the public works were taking place. Enumerators visited each household once a week and asked, for each of the seven days prior to the interview, whether the respondent had worked, the time she/he has worked and the type of work performed. Through the diaries, we also collected weekly data on labor income as well as on non-labor income. The beneficiary woman was also asked about labor force participation and earnings of other adult household members, for the whole 7 days preceding the home visit (as opposed to the daily frequency we used when collecting work data on the two main respondents). A module that measured intra-household cash transfers between the respondent and the rest of household members was also administered weekly as part of the diaries to capture contemporaneous income transfers across household members. At endline, we administered the same employment diaries during three consecutive weeks to the beneficiary woman and her husband in order to measure labor force participation with the same precision as at midline. We administered labor supply diaries together with time use information for women and men in the household, to measure women's and household labor supply responses to the intervention and to account for foregone income of participation. The change in the survey instrument as well in the timing of survey administration, between baseline and the following surveys, may have impacted the precision of measured labor outcomes.

Administrative Data

[16] We conceived them based on the labor diaries developed by Dupas *et al* (2017).

We use implementation data recorded in the managing information system at the ADDS. This database provides information on the individuals that joined the public works intervention, the type of project they were assigned, the period they worked and of each of the payments wired on her/his name to the accounts opened at the paying agency, the CEPEC. We could also obtain data on the financial transactions that occurred between the opening of the bank accounts and the time the endline survey was administered, for three fourths of the individual accounts opened at the CEPEC. We thus know, for each of these households, the time each payment was credited in the beneficiary account during the 50 days of work, its amount and the timing with which beneficiaries withdrew and/or saved this money in the account.

4.1. Baseline balance and attrition

Table 1 compares averages of a set of selected baseline characteristics and outcomes between the treatment and control groups. We observe that the treatment and control groups are relatively well balanced with respect to observable characteristics. There is a minor deviation in the age of the household head and the beneficiary woman, and a marginally smaller number of children 6-15. Moreover, the proportion of adult household members (excluding the beneficiary woman) who were inactive at baseline was slightly smaller in the treatment than in the control group, and a larger share of household members were working in daily paid jobs. In the analysis, we control for household's characteristics that are unbalanced at baseline (household composition, household members labor force participation and food per capita expenditures).

Data collected through this survey shows that in the sample of household of our study, a staggering share of women (82 percent) has no formal education, matched by an also large share of illiterate household heads (66 percent). In contrast, 77 percent of children 6-15 are formally enrolled in school. Labor force participation of these households is limited, with on average one main breadwinner working in the household, 50 percent as casual day workers and over 30 percent as wage-salaried workers and a negligible share of self-employed. The proportion of women employed or looking for work at baseline is very low, with only 13 percent active in the labor market. Employed women (11 percent) are mainly engaged in self-employment activities followed by casual work. Self-employment activities run by these women consist, almost exclusively, of selling food in the street, either as street vendors or at a shop[17]. Women who work usually belong to poorer households. They are more likely to be the household head, are older and rely significantly more on transfers from family members to cover the household needs (as compared to households where women do not work)[18]. A large share of women in our sample do not work on permanent basis though. When we analyze women's employment during a larger period of time, we find that almost 30 percent of women do engage temporarily in outside work[19]. This group of households are even poorer, as measured by their assets and expenditures[20], and are also more likely to receive transfers, suggesting that women's work, even if it does not generate significant earnings, is necessary to respond to household primary needs.

Food expenditure represents on average half of the total budget share. There is a statistically significant difference in average food per capita expenditures in the past 30 days, with the treatment group reporting slightly lower food per capita expenditures (10 percent at the mean of the control group). This imbalance is due to more observations at the right tail of the distribution (of food per capita expenditure) for the control group than for the treatment group and it disappears when we work with natural logs. In our analysis, we introduce a dummy to indicate that the household belongs to the 25 percent percentile of the food per capita expenditure distribution in order to control for this baseline unbalance.

It is interesting to document the extent of labor specialization and time use by gender within the household. If we exclude personal care (which includes sleep in the past 24 hours), women devote half of their time doing household chores, and about 20 percent of their time caring for other household members, and virtually no time doing work, defined as employment outside the household. Men in contrast spend half of their time working outside the household. Social activities within the neighborhood are important for both men and

[17] By shop we mean a fix stand.

[18] Table A5 in the appendix compares baseline characteristics of households with inactive women (at baseline) to those of households with active women.

[19] We compute this figure by identifying women in the control group that were employed at least once along the different surveys we conducted (i.e. baseline, midline and endline surveys). While at baseline we only identify 11 percent of employed women, this figure increases to almost 30 percent when we look at different points in time, showing that women's engagement in the labor market is not of a permanent nature. As for our baseline sample, the main occupation of this larger sample of women is in self-employment activities, followed by casual work.

[20] At baseline and compared to households in the control group where women never work.

women, with men (women) spending 25 percent (16 percent) of their time (outside of personal care) in social activities with neighbors and friends.

Women's mobility is constrained: half of women in our sample declare to have asked for their husband's permission last time they visited family or friends living in a different neighborhood of Djibouti City, while around 70 percent did in order to do so outside the city[21]. Women are freer to move within the neighborhood where the majority of women can decide alone about going to the grocer's, to the market or to visit family or friends[22]. Restrictions in women's mobility are consistent with formal law and customary Islamic norms (Tucker 2008). Data on time allocation also correspond to Muslim views on women's roles, who, among other duties, are responsible for childrearing and household work, while husbands are expected to provide for the family (food clothing and lodging).

Attrition

Table 2 presents attrition results for men and women with a regression for an indicator of not being interviewed at midline or endline as a function of treatment status (controlling for group and strata effects). On average, 7.5 percent of the women in the control group were not interviewed at midline, a fraction that increases to 11.4 percent at endline. Husbands are harder to interview given their daily work schedules and temporary absence from the household: about 28 percent of husbands could not be re-interviewed at midline and endline. The coefficient on treatment (relative to control) in the last column provides a test for differential attrition at midline and endline. The differential attrition by treatment status is a potential source of bias in program effectiveness, as the balance in observable and unobservable characteristics that ensues from the randomization of treatment status at baseline may get lost. There is sign of differential response at midline, with participant women 3.6 percentage points more likely to stay in the survey, and 3 percentage points less likely to stay in the (employment) survey at endline. The sign of women's non-response varies in the two surveys, which suggests that is not a full systematic behavior. In order to verify if these differences lead to unbalance with respect to observable characteristics, Tables

[21] These figures are conditional on having making the trip during the past 12 months. Around 70 percent of women did visit family or friends within Djibouti City, while 25 percent traveled to do so outside the city.

[22] Most women declare to have carried out these activities during the past 12 months, with 18 percent, 21 percent and 34 percent of women, asking for permission respectively, for the three activities mentioned.

A1 to A4 compare household baseline characteristics between control and treatment groups for the sample of households actually surveyed at each survey we administered. Overall, the imbalances we observe in the *postattrition* sample of the employment diaries administered to women (Table A1 and Table A3 in the appendix) correspond to those we had identified in our initial study sample (see Table 1). Therefore, the differential response of women to the employment survey does not yield additional imbalances with regard to observable characteristics. No differential attrition is observed in women's responses to the endline household survey or in any of the surveys conducted among husbands. In our analysis, we attempt at reducing any potential observable bias by controlling for a vector of baseline characteristics.

5. Empirical methods

We use the following reduced-form expression to estimate the effect of being offered the workfare program:

$$y_{igs} = \alpha + \beta T_{igs} + \delta X_{is} + \lambda_g + \tau_s + \varepsilon_{igs}$$

where y_{igs} is an outcome for household i in survey pair group g and strata s at time t and T_{igs} is a dummy equal to 1 if the household is offered the public works program. All regressions control for group effects (λ_g), strata fixed effects (τ_s) [23]and a vector of baseline (pre-determined) covariates (X_{is}).[24] The impact of the publics works offer is captured by β. We estimate this equation separately at two points in time: while the public works program takes place (midline) and after the program ended endline).

In order to improve precision of the estimates and to account for random imbalance on observable characteristics, all regressions include the following set of baseline regressors: age of the head and the beneficiary, number of household members, number of children aged 0-5, number of children aged 6-15, labor force participation variables for the beneficiary and the other adult household members and a dummy to indicate that the household belongs to

[23] Bruhn and McKenzie (2009).

[24] We do not cluster at the site level, given that the unit level of randomization is the household, and that the within-cluster dependence of the main outcomes is not meaningful: the baseline intra-cluster correlation for women's employment is 0.006 and for women's inactivity is 0.004.

the 25 percent percentile of the food per capita expenditure distribution. We use this equation to estimate effects presented in Tables 3 to 9. Results do not change when regressions do not control for baseline covariates.

We also provide evidence on the effects of the program by initial woman working status, comparing the effect of the intervention on women's decision to participate in the labor force, compared to measuring the behavioral responses of women who are already working (or looking for a job) to a new offer of temporary work. While women that were already working may be willing to join the workfare program if the remuneration or her aspects related to the job offer are attractive to them, the switch from one type of job to another may have smaller income gains due to crowding out than the switch from inactivity to work. In order to explore the heterogeneity of impact by initial women working status, we run the following regression:

$$y_{igs} = \alpha + \beta\, T_{igs} + \sigma\, BL_{is} + \gamma\, T_{igs} * BL_{is} + \delta\, X_{is} + \lambda_g + \tau_s + \varepsilon_{igs}$$

where BL_{is} is a dummy variable equal to 1 if the beneficiary woman of household i and strata s participated in the labor force at baseline. In this equation, the coefficient β estimates the impact of the work offer only on those households where women were inactive at baseline, while the sum of coefficients β + γ estimates the impact on those households where women were active (i.e. employed or were looking for a job) at baseline. Effects estimated in Tables 10.1 and 10.2 correspond to this specification.

6. Results

6.1. Program take-up

Table 3 present results on households take up of the offer to participate to the program. Take-up of the program by household members was almost universal by the time we administered the midline survey, with 95.9 percent of households accepting the offer in the treatment group (table 3, second row).

Take-up is mainly accounted for by women's participation: in 73.3 percent of cases, in the treatment households, women performed the workfare activities themselves, while in 22.4 percent of cases, women delegated the work. Delegation occurred mainly in favor of other females who do not belong to the same household (8.6 percent), followed by other male members of the household, mainly the husband (5.3 percent), and by other female members (4.2 percent). Among the group of women who accepted the offer but relied on someone else to perform the work, a significant proportion did so due to exogenous factors: (a) 25 percent of the instances of delegation were due to prohibitions imposed by the program on women who are in their third trimester of pregnancy, or that have children 0-40 days old; and (b) 15 percent of the women who delegated their participation in the program declared to have been sick at the time they received the job offer. Among the remaining cases of delegation, the caring for another household member was the main reason stated by women for not accepting the job offer (33 percent of the cases of delegation were due to childcare constraints while 15 percent were related to the caring for a sick household member).[25]

High participation in workfare work opportunities are also documented by Goldberg (2016) who finds take-up rates that exceeds 70 percent during the agricultural off-season in Malawi for both men and women even at very low wages.[26] Our results align with those of Goldberg as workfare employment opportunities are offered to a population with low opportunity cost in terms of outside work, for both inactive women and well as women previously employed in self-employment activities. But we document those increases in women's workfare participation only for offered wages that are relatively high compared to potential earnings women could obtain from self-employment activities. Our finding regarding female participation is striking in light of the presumed lower level of support for female labor force participation in Middle East and North Africa (Jayachandran, 2015). Field et al (2016b) find evidence that increased control over household resources (through financial literacy and female-owned bank accounts) can foster labor force participation in a context of strong norms against female work outside the household. They model social constraints to female employment as utility costs that might be internalized by both men and women in their household decision-making. Their results suggest that social norms internalized by husbands

[25] If we compute program participation net of delegation to a person outside the household, we still find that program participation (by a household member) is large at 85 percent in the treatment group. Moreover, even in the instances of delegation to a person outside the household, targeted women agreed with the person they delegated the work to on a share of the earnings to be paid to them.

[26] In this setting, the screening mechanism for reaching the poor through self-targeting that underlies public works programs around the world (Besley, Coate, 1992) does not bind. Eligible women are too poor and without access to independent income sources besides transfers from their husbands not to take the workfare offer.

in a context where women have low bargaining power may indeed limit female labor supply. Had prevailing social norms been binding in our context, on average or at least for an important subset of households in our sample, we would have detected less than full or heterogeneous take-up of the program. The large take-up by women of the workfare employment opportunities provides a first indication that it is mainly the lack of job opportunities what limits female labor force participation as opposed to prevailing social norms.

In addition to the high wage rate offered, two other features of the program may have contributed in making the job offer attractive to women. One could be the explicit gender labeling of the program. Designating women as the main recipient of the work offer and as the entry point to the entire household (by allowing delegation) may have encouraged women to take up the jobs themselves and explain in part the low take-up by male members of the household as well as the high delegation rates in favor of other women. Also, the favorable working conditions set by the program to facilitate women's participation (i.e. daily work schedules of four hours, no time or monetary cost associated with transportation to/from the work place and scheduled breaks to take care of young children) may have contributed to the reasons why inactive women were so eager to join the program, by easing the tension women face between outside work and childrearing and household work.

Interest in the program remained stable during its progressive rollout. No significant differences in take-up rates across the different waves of implementation that form our treatment group were detected (i.e. public works activities implemented between May 2014 and May 2015). Table 3 also shows that 3.9 percent of women in the control group stated that their household has joined the program. This is mostly due to administrative errors regarding the households selected to receive the offer and to a few cases of delegation in favor of women in the control group. Nevertheless, the differential in program take-up rates between the two groups is still very high at 92 percentage points.

To verify if program participation and delegation vary among households of different socio-economic levels, we computed proxy-means test (PMT) scores for the households in our study sample. No difference in take-up rates is observed among the different quantiles of the PMT distribution. The potential sizeable earning opportunity is valued across the entire poverty distribution. However, the extent of own participation (as opposed to delegation) decreases slightly with higher PMT scores, suggesting that households that are relatively better off and with a higher opportunity cost of their time might be willing to forego part of the potential incremental income through delegation.

6.2. Female and household labor supply response

Table 4 presents employment results at midline and endline for different household members: (a) for the beneficiary woman (self-declared); (b) for all the remaining adult household members including the husband (as declared by women); and (c) for the husband (self-declared). Employment status was computed based on data collected through employment diaries. We find that the contemporaneous effect on women's employment is substantial. The offer of the public works program at the time of the midline survey substantially increased the share of employed beneficiary women by 54.5 percentage points, raising their employment rate from 21.3 percent to 75.8 percent. Two thirds of newly employed women entered the labor force encouraged by the workfare intervention (i.e. they were inactive previously), while the remaining third were previously unemployed. The share of women employed in workfare represents 67.9 percent in the treatment group. The outcomes presented in this table measure actual work performed by women,[27] which explains the relatively smaller magnitude of women's workfare employment compared to the share of women who declare to have taken up the program her selves (74 percent as shown in table 3).[28] There is evidence of a temporary crowding-out effects among those previously employed, mainly among self-employed women who partially switched activities to work on the workfare projects, thus reducing their self-employment by 10 percentage points. Self-employed women were thus highly attracted by the new work offer and induced to temporarily substitute it for their self-employment activities: hourly wages offered under the public program were almost double the hourly self-reported income from self-employment in our control group.

[27] We created weekly dummies for each type of employment and then computed the average over the three weeks the employment diaries were administered.

[28] The difference between women's take up and household take up of 95.9 percent (Table 3) is explained by delegation of the workfare offer to another household members.

The net effect on women employment remains comparatively very large. Field *et al* (2016b) report impacts of 34 percentage points in women's labor force participation when female-owned bank accounts are opened and their utility is explained to women (compared to receive a bank account only). Bertrand *et al* (2017) document small effects on employment for a program that targeted low-skilled young men and women in Ivory Cost. These results are likely to be explained by a context of high informality, where wages were attractive also to young people already engaged in the labor market. As mentioned before, the magnitude of the effects found in our study are close to those reported by Goldberg (2016). The net effects on women's labor supply we document, support thus the hypothesis that the lack of job opportunities is an important factor limiting female labor force participation in the context of our study. We cannot claim however that prevailing social norms do not exert any influence on women's labor supply decisions in urban Djibouti. Instead, our findings suggest that when good enough job opportunities are made available, social norms are not bonding for most women. The gains in utility derived from higher earnings may have compensated the utility cost of overcoming social norms against female employment, to the extent they matter. Our results could potentially be also interpreted as an upper bound of the cost of overcoming these social norms. The cost of the prevailing social norms cannot be significantly higher than the wage differential offered by the workfare program, given the almost universal program take-up we document.

This increase in women employment is reflected in the time women spent working and in the earnings thus generated. On average, women in the treatment group increased their weekly work time by 14.4 hours (which represents a 180 percent increase) and their earnings by 2,986 FDJ. This increase in earnings is also substantial, almost tripling women's income from labor. Most of the increase in income is due to women previously inactive joining the workfare program. Back-of-the-envelope calculations, show that only 10 percent of women's labor income increase corresponds to a wage effect, while the remaining effect is originated in women who start to receiving labor earnings due to the program. This finding does not contradict the earlier statement we make about the attractiveness of the offer for a big proportion of employed women. Labor income distributions are right-skewed, which means that the program is more attractive to the median women than to the average women. While the average wage effects are modest, they may still be significant for employed women that have earnings below the mean of the labor income distribution.

Table 4 shows the participation in the program of other household members (second panel) and of the husbands (third panel). These are consistent with the results on participation stated by women (see Table 3). We can observe the effects of delegation through increased participation in the program of other adult members and of the husband. For the rest of household members, there is a positive effect of 3.3 percentage points in workfare employment. Those who participated in the public works program as a result of delegation also reduced their self-employment activities, leading to no significant changes in their level of participation in the labor market. When we look specifically at the labor supply of the husband, we observe similar effects (5.5 percentage points increase in workfare employment) to those observed for other household members (including the husband), but note that it is mostly unemployed husbands who joined the program. This most probably originates in the fact that employed husbands obtain on average the same hourly wage as the one offered by the workfare program but for full-time work. It is thus unsurprising to not observe any effect on total time worked and earnings for the rest of household members, given the short-term nature of the intervention.

Once the workfare employment opportunities are not available any more, at the time of our endline survey, we observe that most women withdraw from labor market with 65 percent returning to inactivity, 20.4 percent of the targeted women employed and 12 percent unemployed. Women in the treatment group are thus not more likely than those in the control group to remain active when work opportunities ended. Offered wages together with the favorable working conditions set by the program to facilitate women's participation (i.e. daily work schedules of four hours, no time or monetary cost associated with transportation to/from the work place and scheduled breaks to take care of young children) may explain why they do not continue working after the end of the intervention as they are not able to find similar working terms outside of the program. The program did not induce further social change in the medium-term either. The intervention itself, by significantly increasing the participation of women in the workfare jobs, could have acted as a coordination device for both men and women to agree on women working outside the house. Had this happened, we should have observed an increase in female labor force participation after the program ended. However, our medium-term results do not provide any evidence on social change taking place due to the program.

6.3. Impact on Time Use

Table 5 presents results from time use data for both beneficiary women and their husbands, for the day preceding the survey. In the absence of the public works offer, we observe that women allocate most of their time to household tasks and to caring for other household members[29] (67 percent of daily time, net of personal time).[30] Wage-earning/paid work represents only 8 percent of their daily time, consistent with the low level of employment we observe among women in the control group.

Overall, our results indicate that total work time remains unchanged once the workfare offer is introduced. Women do not experience a "second shift" at home (i.e. additional household work) when they spend significantly more time working outside the household. Women continue to perform a significant portion of household tasks (around 250 minutes a day), but the increased time spent working outside appears offset by a commensurate reduction in time allocated to other activities. Women who were offered the workfare increased their time spent on paid work by around 250 percent (from 55 to 195 minutes a day). This effect is much larger than the one we observe in Table 4, and leads to the following question: if women increased time spent on paid work by so much, did they reduce time spent on other activities, and if so, which ones? The largest effect is found in the time women allocate to household chores (a 17 percent decrease compared to the control group, which accounts for almost 80 percent of the increase in paid work time). Increased paid employment is also marginally offset by reductions in time previously allocated to personal care.

A further interesting question relates to how household tasks are reallocated when women engage in paid work. There are no effects on time spent by husbands on household chores

[29] This activity category reports exclusive time spent in the caring for other household members.
[30] We subtract from the total number of day minutes, the time spent in sleeping.

(as shown in the second panel of Table 5), indicating that, most probably, other household members engage in such activities while women are working.[31]

In the second panel of Table 5, we report results on time use data for the husband. These results are consistent with our findings for husband's employment: the time husbands spend working is only marginally affected by the workfare offer, by 10 percent. The husband of the beneficiary woman partially substitutes personal care time for the time needed for participation in the public works program (in those instances when they receive the delegation to work).

All the time use responses are temporary, reverting to no differences between the treatment and control groups at endline, nine months after the program has ended. It is interesting to note though that grandmothers in the treatment group spend less time taking care of the younger household children at the time of the endline survey. There is thus an intertemporal compensation for the additional time they spent in childcare while women were participating in the workfare activities.

6.4. Net income gains, expenditures, savings and loans

In Table 6 we present results regarding the program's impacts on total household income as well as on income sources. The boost of woman employment due to the public works program leads to a substantial short-term increase in household total income of about 38 percent. This increase comes exclusively from the increase in woman's labor income. Women's labor income increases by about DJF 2,986 per week as a result of the workfare program at the time of the midline survey, almost tripling women's income. This increase is consistent with the effect on the hours worked by women and the hourly rate paid by the

[31] Alternatively, one could assume that the household, as a whole, reduces time spent on such activities. Some indications of what might be happening can be derived from time allocated to child care. In the third panel of Table 5, we observe that the time spent by beneficiary women with the youngest children decreases, to account for the time spent by the mother working outside of the household (a reduction of 130 minutes per day), as young children are mainly taken care of by grandmothers, other female adult members and siblings, followed by neighbors while mothers are at work. Incidentally, it is also possible that during the time spent with children by these other household members, the latter also get organized to perform the household tasks beneficiary women used to do previously. Therefore, women most probably find other female members who take over these tasks, allowing them to reduce the time spend in them.

workfare program[32]. Such increase in earnings allows women to increment their participation in total household income very significantly, from 17 percent to almost 40 percent.

With a differential of 66 percentage points of women in the treatment group who declared to have worked in the public works activities at the time of the midline survey (Table 4), the estimated increase in women's earnings translates into net income gains of about DJF 4,525 for the average participant, or 75 percent of the full weekly wage transfer of DJF 6,000. Forgone income (of around 25 percent) reflects our previous findings on women's employment where we observe some crowding-out of women's self-employment activities by the workfare program. Total household income increases by a similar amount than women's earnings in the short term, reflecting a very limited crowding out of other income sources in response to the public works participation (which also reflects our findings regarding the lack of effects on total labor supply of other household members).

Net income gains derived from the program are also quite substantial compared to those reported for other workfare interventions. Bertrand *et al* (2017) report foregone income of about 60 percent of the transfer for a workfare program implemented in Ivory Cost to address the unemployment of young men and women in urban and semi-urban areas. Estimates of foregone income from large scale programs such as the *Jefas y Jefas* in Argentina (Galasso and Ravallion 2004) and the National Rural Employment in Bihar (Murgai et al 2015) are around one-third of the total wages earned through the program. In our case, the program is targeting a population that would not have carried out any type of work activity[33] in the absence of the program. The low opportunity cost coupled with high wages that effectively encouraged participation explain the comparatively larger net income gains of the program we study.

[32] The intention-to-treat estimates presented in Table 3 show that women in the treatment group work 14.4 more hours than those in the control group. Theoretically, total income effect of workfare would be of around 3,600 FDJ (14.4 hours x 1,000 FDJ daily pay / 4 hours of work per day). But, when we look at midline data of women who worked in the workfare program, most of them declared 5 hours of work per day (to be exact 4.8 hours per day on average, conditional on workfare employment). This means that women reported the total time they spent in the work sites since their arrival to their departure, including time spent in breaks. If we consider daily shifts of 4.8 hours, which translates into an hourly wage of 208 FDJ, total income increase would be of 3,000 FDJ, which corresponds to the magnitude of the effect we detect.

[33] Table 4 shows that only 20 percent of the target women are employed in the control group at the time the program took place, mostly in self-employment activities.

Table 7 shows results on expenditures, savings, insurance and loans. The income gain from the public works is partially spent, with 9 percent increase in total expenditures and 12 percent increase in food expenditures[34]. The increase in expenditures represents thus around one third of the incremental income earned as a result of program participation.[35] Our results suggest that the short-term increase in income was, most probably, largely saved, slowly smoothing consumption over time in the months following the end of public works. These results echo the behavioral responses of households in China; most of the short-term income gains from an anti-poverty intervention were saved, most likely in light of the uncertainty about the long-term sustainability of the project (Ravallion and Chen, 2007). As in many other low-income settings (Rosenzweig 1993; Fafchamps and Lund, 2003), enrolment into informal insurance schemes was one of the means chosen by women seeking to smooth consumption, at least for a subset of the households. We also observe that by the time of the endline survey, households in the treatment group have been reimbursing more mortgages or loans taken to repair their homes and, likely, reduced their debts at the grocers.[36]

6.5. Impact on intra-household allocations

An interesting aspect related to overall results when it comes to household income is who keeps control of the additional household earnings. Based on the weekly data we collected on intra-household transfers, we find that the increase in women's income leads to only moderate increases in intra-household transfers in the treatment group, suggesting that women, for the most part, keep control of the earnings they obtain through workfare (second panel of Table 6). This result is consistent with the Islamic norms whereby women have control over their own assets and income (Tucker, 2008).

[34] Of note is that we are not able to detect these effects for the level of per capita expenditures, but when we use natural logs of per capita expenditures the impacts become detectable. The logarithmic transformation spreads out data more evenly, neutralizing some outliers present at the right tail of the distribution in the control group. It also spreads out data clustered at the lower levels, where lay the effects on per capita expenditures.

[35] When we analyze the heterogeneity of impacts on total household income (at the time of the midline survey), we observe that impacts are statistically significant for all the percentiles we report (10 percent, 25 percent, 50 percent, 75 percent and 90 percent), but they are shown to be substantially larger at the percentiles 10 percent and 25 percent of the income distribution (results available upon request).

[36] A caveat for these findings is that the magnitudes of the different effects we are able to detect do not add up to the total increase observed in labor income. Obtaining accurate estimates of expenditures is methodologically less challenging than estimating savings. Respondents have often no incentives to provide precise answers about savings, which is a sensitive topic and less apparent to the rest of the community. These methodological aspects together with our endline results suggesting a reduction in household indebtedness lead us to conjecture that increased income was mostly saved.

Women in the treatment group are 6.2 percentage points more likely to give money to their husbands. When they do, they give almost the equivalent to the full public works weekly payment.[37] We also observe that husbands do not modify their transfers of cash to women once the latter have much higher earnings. No effect is thus detected in the proportion of women that declare to have received cash from their husbands during the preceding week or in the amounts they had received. Women are thus able to hold on to a much larger share of the household (cash) resources on account of their participation in the program. Husbands in our context do not capture women's additional earnings, at least in the short term, as found by Erten and Keskin (2015) for women with increased education and earnings in Turkey. Not surprisingly, given the trend observed in the household labor supply, we find no impact on income or intra-household transfers at the time of the endline survey.

Did holding own bank accounts help women in taking control over their earnings? Administrative data we obtained from the paying agency shows that women did not use these accounts to save. Almost the full amount program beneficiaries have been paid had been debited by the time of the survey. This finding is not surprising since these women are not familiar with the financial institutions and did not received any training on the potential uses of these accounts. These results echo the findings by Field et al (2016b), who show that providing access to a bank account without an enhanced training on how to use it does not result in greater use of those accounts. This is especially salient in our setting, where women have very low levels of literacy.

We next document the effects of the program on women's perceived decision-making power. When looking at the effects of the intervention on women's bargaining power (Table 8), we do not find strong evidence of changes in women's decision-making power due to the workfare employment offer. While some marginal effects are detected contemporaneously to workfare employment (i.e. in the share of expenditures in women's personal goods – clothing), these increases are dwarfed by the share of expenditures on men's personal goods. The lack of significant impacts on women's decision power is consistent with the temporary nature of the employment offer.

[37] Table 6 shows a DJF 305 increase in transfers to the husband done by additional 6.2 percentage points by women. This leads to an actual transfer of around DJF 5000 (weekly payment is of DJF 6000).

Our contemporaneous results show that both women and men experience neither a change in mental well-being nor in self-esteem due to the workfare program (Table 9), as captured by a mental health scale and a self-esteem scale[38]. As expected given the absence of effects contemporaneously to the public works activities, there are no statistically different effects between households who were offered the program and those who did not receive the offer along these dimensions in the longer-term.

7. Heterogeneity of effects by baseline employment status

In this section, we discuss the heterogeneity of effects by women employment status at baseline. Of note is that at that time, most of the women in our sample were inactive, with only 13 percent of women employed or looking for a job. Table 10.1 presents the results which are contemporaneous to program implementation (i.e. *while* the public works activities were taking place) while table 10.2 presents the medium-term results after the program had ended (Table 10.2).

The analysis of take-up rates shows that program participation by households in the treatment group is very large, regardless of women's initial employment status. The program is also equally effective in engaging both inactive and active women to participate directly in the public works activities, increasing workfare employment by around 65 percentage points in the two groups. This result confirms that the workfare offer is attractive even for employed women, with many of them switching from self-employment activities to workfare employment. The crowding out of self-employment activities is the largest in the group of active women: active women are 31.5 percentage points less likely to be engaged in a self-employment activity than women in their corresponding control group.

Not surprisingly, the effects of the program on women's employment and on earnings are much more pronounced in the group of women that were not engaged initially in the labor force. We find that the increase in employment among the inactive women is almost double the one observed for previously active women (58 percentage points and 30 percentage points respectively). Similarly, while earnings increase by 280 percent among the inactive

[38] We used the five-item Mental Health Inventory (MHI-5) developed by Veit and Ware and the self-esteem scale developed by Rosenberg. Data on the MHI-5, a measure of overall emotional functioning, were collected at midline for part of the sample and at endline for the whole sample.

women, they only do so by less than half for the active ones (the -negative- differential compared to the group of inactive women is also significant). These increases translate into 40 percent net income gains for households with inactive women, while no changes in household income are observed for women active at baseline. Interestingly, in households with inactive women, husbands who were unemployed are more likely to join the program (3.5 percentage points decrease in unemployment due to publics works participation). This result seems to suggest that a small portion of households prioritized husband's employment when the program becomes available. No similar effect is detected in the employment status of husbands with active spouses.

When we proceed to analyzing how households allocate the differential gains in earnings, we observe that households where women were not engaged initially in the labor force are the ones allocating the additional income partly to consumption, mainly food, and saving the rest to smooth consumption over the months following the end of the program.

Nine months after the program ends, women who were originally inactive reverted back to their initial employment status (no significant difference is observed compared with the labor supply of women in the control group). As we conclude when we analyzed these findings for the whole sample of our study, existing employment opportunities (in the absence of the program) seem thus to offer potential wages that are below their reservation wage and/or amenities that are not compatible with other women's duties, prompting women to discontinue their engagement in the labor market. Consistently, we do not observe any difference in women's earnings or in total income between households with inactive women in the treatment and control groups. There are some positive effects on the repayment of mortgages and of house-related loans (i.e. for maintenance, repairing or construction).

The story is markedly different for women who were already participating in the labor force when the program was launched. Exposure to the program discouraged women's labor participation in the longer-term, with women shifting away from employment. Around one third of women who would have been running a self-employment activity (we observe a negative effect of 14.7 percentage points in self-employment for this group) do not immediately re-engage in these activities after the workfare employment ends. The decrease in labor force participation is mirrored by a decrease in the numbers of hours they work and in women's earnings. Given the low returns on their activities, women may be using their savings from the intervention to consume instead of working. Poor households may prefer to withdraw, even if temporarily, from certain types of jobs when there is a change in their set of income opportunities, even when they are not time-constrained. For example, Crepon et al (2016) show that when households in rural Morocco increase their profits from self-employment activities as a response to microcredit access, they decrease significantly their participation in casual work. Alternatively, this behavioral response could be due to the existence of fixed costs associated to these activities that prevents women from restarting them immediately.

8. Conclusion

Low labor force participation rates of women in developing countries could be explained by the lack of attractive job opportunities and by social norms that deter women from working outside of the home. In an experiment in a poor area of urban Djibouti, a context where most women are not in the labor force, we randomly varied women's access to job opportunities in a workfare program. This allowed us to examine, directly, the influence of job opportunities, and indirectly, the influence of social norms. We find that women are unambiguously willing to enter the labor market when offered sufficiently attractive job opportunities: 95 percent accepted the employment offer and over 70 percent came forward to do the work, even when they could have delegated it to any other adult member of their household, male or female. Certainly, the relatively high wage rate influenced women's decisions to take up the job. Another enabling factor may have been given by the part-time nature of the work compatible with their regular household responsibilities, and time breaks during the working hours. These work arrangements may have helped ease the tension among the competing responsibilities, so that the women did not have to tradeoff between work inside the home and wage-earning work outside the home. We find evidence that women had control over their own earnings from the work, that they rationally saved a substantial fraction of their earnings. But once the program ended and the employment opportunity it presented was no longer available, women reverted back to non-participation inactivity or to their previous low levels of employment. Since women do take up the offer and do show up to work outside the home, but revert back to the low labor supply in the long terms, we infer that the main barrier to labor force participation by these women is not in the prevailing social norms but rather in the lack of suitable employment opportunities.

Though in our context, the lack of opportunity dominated social norms as the key deterrent, we cannot claim that it is the limiting factor in developing countries at large. Field et al. (2016) show that traditional views on women's work internalized by husbands can also be relevant in shaping women's labor supply. Community-based activities allowing women to benefit from peer support may also be relevant for inducing women to take on revenue-earning work (Field et al, 2014). Nevertheless, policies that focus exclusively on the role of social norms or, more generally, on the social and cultural determinants of women's labor supply, could be leaving unexploited an important policy margin. Our results suggest that policies that encourage economic development, by diversifying the economy and increasing work opportunities, are important in promoting labor force participation by women. Future research should help identify the relative importance of the wage rate and of enabling job features and benefits in activating women's labor supply in different contexts. Tailoring programs to women's needs, might play a substantial role in increasing women's participation, as opposed to more complex attempts at understanding and trying to externally shift social norms.

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Figure 1: Evaluation Design

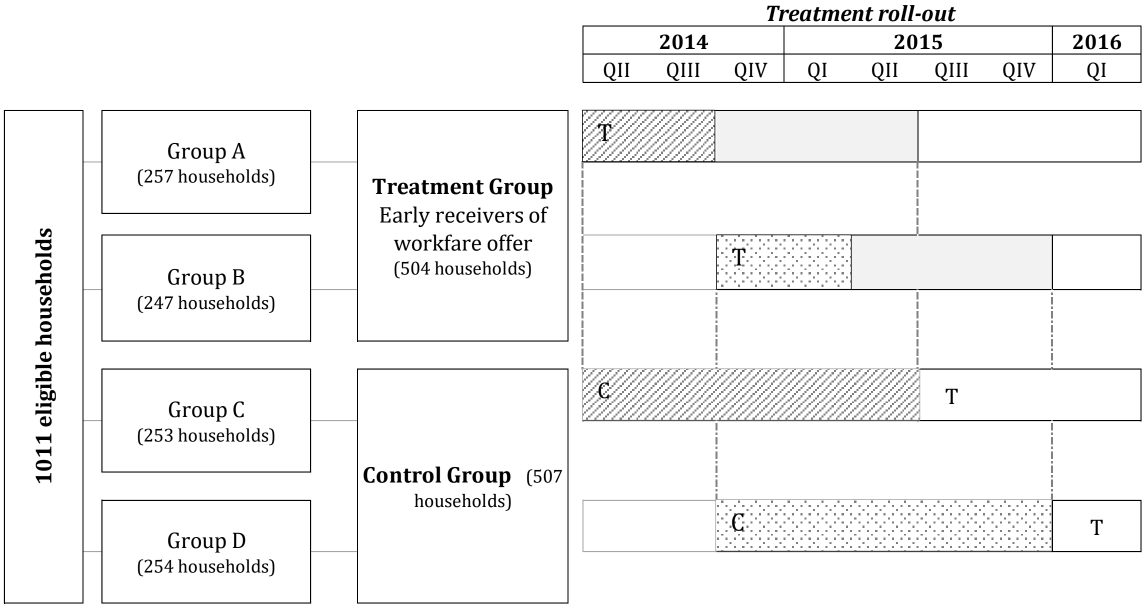

Table 1. Baseline Summary Statistics: entire sample

	Obs	Control Group			Treatment - Control		
		Obs	Mean	St. Dev.	Coeff.		*p-value*
Household characteristics							
Pregnant woman or child 0-3	1011	507	0.970	0.170	0.004		*0.727*
Number of HH members	1011	507	6.9	2.7	-0.3		*0.118*
Number of children 0-5	1011	507	1.8	0.8	0.0		*0.434*
Number of children 6-15	1011	507	2.2	1.8	-0.2	*	*0.055*
Number of adults >15	1011	507	3.0	1.6	-0.1		*0.396*
Male HH head	997	500	0.966	0.181	-0.022	*	*0.083*
Age of HH head	995	500	40.4	8.5	-1.3	**	*0.017*
Head with no education	970	486	0.656	0.475	-0.003		*0.927*
Age of Woman Beneficiary	1005	504	33.4	6.7	-1.0	**	*0.021*
Woman benef with no education	1000	501	0.824	0.381	0.009		*0.712*
Share of children 6-15 in school (cond on a child 6-15)	747	383	0.773	0.324	-0.004		*0.866*
Beneficiary woman was … in last 7 days							
Inactive	955	477	0.874	0.332	-0.001		*0.946*
Unemployed	955	477	0.021	0.143	0.010		*0.331*
Employed	955	477	0.105	0.307	-0.009		*0.656*
Day worker	955	477	0.036	0.186	-0.008		*0.499*
Self-employed	955	477	0.057	0.231	-0.005		*0.731*
Salaried	955	477	0.008	0.091	0.007		*0.348*
Other work relationship	955	477	0.002	0.046	-0.002		*0.275*
Share of adult members who were … in last 7 days (excludes beneficiary)							
Inactive	950	476	0.363	0.377	-0.042	*	*0.091*
Unemployed	950	476	0.034	0.134	0.011		*0.286*
Employed	950	476	0.603	0.390	0.031		*0.231*
Day worker	950	476	0.347	0.415	0.060	**	*0.031*
Self-employed	950	476	0.035	0.170	-0.013		*0.172*
Salaried	950	476	0.218	0.374	-0.023		*0.342*
Other work relationship	950	476	0.003	0.048	0.002		*0.619*
Income & transfers							
Income from labor in last 7 days (in FDJ)	953	476	8,427	9,459	-753		*0.186*
Log of income from labor in last 7 days	724	358	8.98	1.04	-0.09		*0.245*
HH had non-labor income in last 12 months	1001	500	0.250	0.433	-0.005		*0.848*
HH made a transfer in last 12 months	1001	500	0.104	0.306	-0.003		*0.861*
Expenditures							
Per capita total expenditures in last 30 days	958	476	14,294	11,264	-929		*0.142*
Of which: Food expenditures	958	476	6,992	7,054	-855	**	*0.020*
Health & education	958	476	1,515	1,803	68		*0.669*
Other expenditures	958	476	5,787	6,629	-142		*0.704*
Share of food in HH expenditures	958	476	0.494	0.145	-0.006		*0.554*
Share of health and education in HH expenditures	958	476	0.115	0.112	-0.009		*0.206*
Share of other items in HH expenditures	958	476	0.390	0.136	0.015		*0.107*
Share of households with PMT score above the median	997	500	0.520	0.500	-0.043		*0.178*
Food security							
Is concerned about not having enough food in last 7 days	1001	500	0.31	0.46	0.04		*0.190*
Index of food insecurity in last 7 days	1001	500	1.10	1.68	0.13		*0.235*

Notes: Unit of observation: Household. Sample: all households surveyed at baseline. Column 5: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, controlling for strata dummies. ***, **, * indicate significance at 1, 5 and 10 percent.

Data source: Baseline Household Survey conducted in January-March 2014.

Table 2. Attrition

		Control Group			Treatment - Control		
Panel A. Midline Survey	Obs	Obs	Mean	St. Dev.	Coeff.		*p-value*
Woman not surveyed at midline	1011	507	0.075	0.264	-0.036	**	0.014
Husband not surveyed at midline	1011	507	0.276	0.448	-0.025		0.379
Panel B. Endline Survey							
Woman not surveyed at endline HH survey	1011	507	0.114	0.319	0.020		0.341
Woman not surveyed at endline employment diaries	1011	507	0.093	0.290	0.034	*	0.086
Husband not surveyed at endline HH survey	1011	507	0.215	0.411	-0.003		0.897
Husand not surveyed at endline employment diaries	1011	507	0.286	0.452	-0.013		0.655

Notes: Unit of observation: Household respondents: beneficiary woman and husband. Sample: all households surveyed at baseline. Column 5: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, controlling for strata dummies. ***, **, * indicate significance at 1, 5 and 10 percent.

Data source: Panel A: Midline Employment Diaries. Panel B: Employment Diaries and Household Survey conducted at Endline.

Table 3. Take-up & delegation

Panel A. Midline Survey: Take-up	Obs	Control Group			Treatment - Control		
Woman's response		Obs	Mean	St. Dev.	Coeff.		p-value
HH heard about the program	940	464	0.933	0.250	0.054	***	*0.000*
HH took-up	948	467	0.039	0.193	0.920	***	*0.000*
Woman delegated PWs	948	467	0.011	0.103	0.213	***	*0.000*
Woman worked in PW	952	469	0.021	0.145	0.712	***	*0.000*
Husband worked in PW	745	367	0.005	0.074	0.060	***	*0.000*

Panel B. Midline Survey: Delegation	Treatment Group		
Woman's response	Obs	Mean	St. Dev.
Delegated PW to:			
A female HH member	476	0.042	0.201
A male HH member	476	0.053	0.223
A female non HH member	476	0.086	0.281
A man non HH member	476	0.021	0.144

Notes: Unit of observation: Household. Panel A: Column 5: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, controlling for strata dummies, survey-group dummies, and a vector of baseline household-level controls. Household-level controls: age of the head and the beneficiary, number of household members, number of children aged 0-5, number of children aged 6-15, a dummy equal to 1 if woman is active, share of members who are inactive and a dummy equal to 1 if the household belongs to the top 25 percentile of food per capita distribution. Panel B: summary statistics for the treatment group. ***, **, * indicate significance at 1, 5 and 10 percent.

Data source: Midline Household Survey.

Table 4. Employment

	Midline Survey						Endline Survey					
		Control Group			Treatment - Control			Control Group			Treatment - Control	
	Obs	Obs	Mean	St dev	Coeff.	p-value	Obs	Obs	Mean	St dev	Coeff.	p-value
Average over 3 weeks												
Beneficiary woman												
Inactive	950	467	0.552	0.427	-0.354 ***	*0.000*	875	450	0.629	0.423	0.045	*0.107*
Unemployed	950	467	0.234	0.340	-0.194 ***	*0.000*	875	450	0.127	0.266	-0.012	*0.503*
Employed	952	469	0.213	0.383	0.545 ***	*0.000*	897	460	0.237	0.400	-0.033	*0.188*
Worked as day worker	952	469	0.014	0.100	0.006	*0.335*	897	460	0.014	0.107	0.001	*0.855*
Worked as salaried	952	469	0.021	0.136	-0.010	*0.185*	897	460	0.015	0.114	-0.001	*0.942*
Self-employed	952	469	0.160	0.337	-0.106 ***	*0.000*	897	460	0.200	0.379	-0.028	*0.242*
Worked in PW program	952	469	0.017	0.125	0.662 ***	*0.000*	897	460	0.000	0.000	0.000	*0.000*
Worked in other work relationship	952	469	0.007	0.061	-0.004	*0.273*	897	460	0.008	0.071	-0.003	*0.304*
Hours worked	952	469	7.9	17.6	14.4 ***	*0.000*	897	460	10.0	20.5	-2.2 *	*0.072*
Labor income (in FDJ)	942	461	1,438	4,018	2,986 ***	*0.000*	895	458	1,101	3,334	-219	*0.235*
Will look for a job or start a self-empl act. in next 6 months	n.a	n.a	n.a	n.a	n.a	n.a	897	460	0.285	0.387	0.006	*0.824*
Share of adult members ... over the past 7 days *(excludes woman beneficiary and includes husband)*												
Inactive	913	450	0.436	0.388	-0.003	*0.869*	893	457	0.516	0.414	-0.019	*0.418*
Unemployed	913	450	0.037	0.119	-0.013 *	*0.054*	893	457	0.014	0.066	0.006	*0.216*
Employed	913	450	0.540	0.376	0.006	*0.761*	893	457	0.516	0.37	0.022	*0.301*
Worked as day worker	913	450	0.250	0.353	0.013	*0.542*	893	457	0.263	0.352	0.005	*0.807*
Worked as salaried	913	450	0.249	0.357	-0.023	*0.270*	893	457	0.226	0.339	0.018	*0.398*
Self-employed	913	450	0.044	0.168	-0.021 **	*0.023*	893	457	0.030	0.125	0.000	*0.988*
Worked in PW program	913	450	0.000	0.000	0.033 ***	*0.000*	893	457	0.000	0.000	0.000	*0.000*
Worked in other work relationship	913	450	0.007	0.06	-0.001	*0.834*	893	457	0.005	0.056	-0.002	*0.421*
Hours worked	913	450	26.8	27.0	0.6	*0.728*	893	457	28.9	27.1	0.3	*0.885*
Labor income (in FDJ)	905	444	6,394	6,054	357	*0.349*	893	457	7,772	6,674	-10	*0.983*
Number of adults other than woman beneficiary							740	375	8.885	0.868	-0.013	*0.822*
Husband												
Inactive	743	367	0.071	0.227	0.002	*0.908*	724	361	0.079	0.231	0.011	*0.514*
Unemployed	743	367	0.089	0.234	-0.028 *	*0.084*	724	361	0.050	0.169	-0.020 *	*0.092*
Employed	745	367	0.839	0.320	0.024	*0.298*	726	362	0.864	0.306	0.006	*0.796*
Worked as day worker	745	367	0.458	0.448	-0.024	*0.436*	726	362	0.474	0.465	-0.036	*0.257*
Worked as salaried	745	367	0.339	0.454	0	*0.990*	726	362	0.354	0.464	0.034	*0.291*
Self-employed	745	367	0.045	0.198	-0.001	*0.952*	726	362	0.047	0.202	0.003	*0.831*
Worked in PW program	745	367	0.003	0.043	0.055 ***	*0.000*	726	362	0.000	0.000	0.000	*0.000*
Worked in other work relationship	745	367	0.000	0.000	0.002	*0.224*	726	362	0.004	0.059	-0.002	*0.488*
Hours worked	744	367	39.9	24.6	1.4	*0.408*	726	362	44.3	25.1	1.7	*0.379*
Labor income (in FDJ)	733	296	7,854	10,251	-1,210	*0.149*	724	360	10,221	7,810	192	*0.765*

Notes: Unit of observation: Household. Column 5 and 11: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, controlling for strata dummies, survey-group dummies, and a vector of baseline household-level controls. Same household-level controls as in Table 3. ***, **, * indicate significance at 1, 5 and 10 percent.

Data source: Employment Diaries cinducted at Midline and at Endline.

Table 5. Time use

	Midline Survey						Endline Survey					
		Control Group			Treatment - Control			Control Group			Treatment - Control	
	Obs	Obs	Mean	St dev	Coeff.	p-value	Obs	Obs	Mean	St dev	Coeff.	p-value
Average over 2 weeks												
Minutes spent by beneficiary woman in:												
Personal care	944	463	738	102	-19 ***	*0.004*	880	447	734	120	9	*0.271*
Study	944	463	0	0	0	*0.277*	880	447	1	13	-1	*0.389*
Chores	944	463	364	133	-109 ***	*0.000*	880	447	349	154	9	*0.375*
Caring others	944	463	107	87	-9	*0.102*	880	447	100	114	2	*0.767*
Work	944	463	55	130	140 ***	*0.000*	880	447	67	158	-19 **	*0.044*
Social	944	463	95	100	1	*0.857*	880	447	117	114	-3	*0.691*
Other	944	463	78	77	-3	*0.543*	880	447	72	96	2	*0.812*
Minutes spent by husband in:												
Personal care	731	360	698	139	-21 **	*0.042*	684	340	685	150	-8	*0.497*
Study	731	360	0	2	1	*0.327*	684	340	0	0	2	*0.155*
Chores	731	360	6	46	-4	*0.137*	684	340	2	14	0	*0.715*
Caring others	731	360	22	44	9 **	*0.029*	684	340	16	51	-2	*0.619*
Work	731	360	377	220	36 **	*0.027*	684	340	415	225	4	*0.819*
Social	731	360	181	152	-8	*0.476*	684	340	205	158	-12	*0.344*
Other	731	360	154	120	-19 **	*0.033*	684	340	117	110	16 *	*0.081*
Minutes the youngest HH child was cared for by:												
Beneficiary woman	944	463	1280	304	-128 ***	*0.000*	880	447	1277	387	18	*0.445*
Grandmother or female HH member adult	944	463	28	90	44 ***	*0.000*	880	447	39	170	-22 **	*0.026*
HH member girl (<15)	944	463	37	110	33 ***	*0.000*	880	447	21	116	-8	*0.237*
Female neighbor	944	463	5	46	33 ***	*0.000*	880	447	5	44	-2	*0.510*
Male adult	944	463	11	41	8 **	*0.020*	880	447	11	81	-3	*0.522*
Other	944	463	14	106	10	*0.109*	880	447	12	113	3	*0.653*

Notes: Unit of observation: Household. Column 5 and 11: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, controlling for strata dummies, survey-group dummies, and a vector of baseline household-level controls. Same household-level controls as in Table 3. ***, **, * indicate significance at 1, 5 and 10 percent.

Data source: Household Survey conducted at Midline and at Endline.

Table 6. Income & Transfers

	Midline Survey						Endline Survey					
		Control Group			Treatment - Control			Control Group			Treatment - Control	
	Obs	Obs	Mean	St dev	Coeff.	p-value	Obs	Obs	Mean	St dev	Coeff.	p-value
Average over 3 weeks												
Labor & Non-labor income in last 7 days												
Total income (in FDJ)	944	463	8,312	7,365	3,191 ***	*0.000*	896	459	9,122	7,643	-129	*0.789*
Amount (in FDJ) of woman's labor income	942	461	1,438	4,018	2,986 ***	*0.000*	895	458	1,101	3,334	-219	*0.235*
Amount (in FDJ) of other HH members' labor income	905	444	6,394	6,054	357	*0.349*	893	457	7,772	6,674	-10	*0.983*
HH had non-labor income	952	469	0.109	0.245	-0.047 ***	*0.001*	895	458	0.043	0.161	-0.005	*0.659*
Amount (in FDJ) of non-labor income	944	463	748	3,102	-111	*0.563*	895	458	287	1,437	91	*0.399*
Intra-HH transfers in last 7 days *(as declared by woman)*												
Woman gave money to husband	951	468	0.067	0.201	0.062 ***	*0.000*	896	459	0.032	0.116	0.001	*0.919*
Amount (in FDJ) woman gave to husband	951	468	88	344	305 ***	*0.000*	896	459	184	1,251	28	*0.754*
Husband gave money to beneficiary woman	951	468	0.574	0.404	-0.035	*0.155*	896	459	0.549	0.415	-0.014	*0.588*
Amount (in FDJ) husband gave to woman	951	468	7,015	8,704	159	*0.770*	896	459	7,445	9,892	454	*0.508*

Notes: Unit of observation: Household. Column 5 and 11: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, controlling for strata dummies, survey-group dummies, and a vector of baseline household-level controls. Same household-level controls as in Table 3. ***, **, * indicate significance at 1, 5 and 10 percent.

Data source: Employment Diaries conducted at Midline and at Endline.

Table 7. Expenditures, Savings, Insurance and Loans

	Midline Survey						Endline Survey					
		Control Group			Treatment - Control			Control Group			Treatment - Control	
	Obs	Obs	Mean	St dev	Coeff.	p-value	Obs	Obs	Mean	St dev	Coeff.	p-value
Expenditures over the past 30 days												
Per capita HH expenditures (FDJ)												
Total	914	443	13,462	16,469	-649	*0.428*	879	446	10,106	7,317	683	*0.267*
Durables	914	443	274	2,573	-153	*0.231*	879	446	93	747	138	*0.120*
Non-durables	914	443	13,188	16,123	-496	*0.534*	879	446	10,013	7,224	545	*0.367*
Food	914	443	7,502	13,807	-339	*0.618*	879	446	4,666	2,774	49	*0.765*
Ln of per capita HH expenditures:												
Total	914	443	9.22	0.71	0.09 **	*0.021*	879	446	9.04	0.59	0.04	*0.300*
Non-durables	914	443	9.21	0.70	0.10 **	*0.013*	879	446	9.03	0.59	0.03	*0.365*
Food	909	439	8.60	0.74	0.12 ***	*0.005*	879	446	8.31	0.55	0.04	*0.253*
Home Durables												
Index of home durables	n.a	n.a	n.a	n.a	n.a	n.a	1011	507	0.05	2.21	0.02	*0.854*
Savings & Insurance												
The HH has any type of savings or insurance	963	475	0.221	0.415	0.062 **	*0.021*	903	462	0.160	0.316	-0.007	*0.712*
Loans												
The household buys at the gocery store at credit	684	335	0.412	0.493	0.014	*0.701*	895	458	0.285	0.376	-0.03	*0.230*
Amount owed to the grocer (in FDJ)	682	335	5,574	11,270	-32	*0.977*	894	457	4,688	16,489	-1,333	*0.122*
A HH member has an outstanding loan	684	335	0.057	0.232	-0.011	*0.528*	895	458	0.032	0.143	-0.002	*0.862*
HH reimbursed a mortgage or house-related loan in last 30 days	n.a	n.a	n.a	n.a	n.a	n.a	767	386	0.023	0.151	0.029 **	*0.035*

Notes: Unit of observation: Household. Column 5 and 11: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, controlling for strata dummies, survey-group dummies, and a vector of baseline household-level controls. Same household-level controls as in Table 3. ***, **, * indicate significance at 1, 5 and 10 percent.

Data source: Household Survey conducted at Midline and at Endline.

Table 8. Barganing Power

	Midline Survey						Endline Survey					
		Control Group			Treatment - Control			Control Group			Treatment - Control	
	Obs	Obs	Mean	St dev	Coeff.	p-value	Obs	Obs	Mean	St dev	Coeff.	p-value
Panel A. Expenditures												
Share of expenditures over the past 30 days												
Clothes and shoes for:												
Husband	914	443	0.003	0.011	0.000	*0.984*	879	446	0.004	0.014	0.000	*0.976*
Beneficiary Woman	914	443	0.006	0.014	0.002 **	*0.040*	879	446	0.008	0.032	-0.001	*0.602*
Khat and Tobacco for male adults (incl. husband)	820	398	0.060	0.098	-0.003	*0.702*	777	385	0.071	0.121	0.004	*0.681*
Panel B. Woman's participation in HH decisions												
Indexes of woman's participation in HH decisions												
Index 1: Woman took decisions alone	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	882	449	0.000	1.000	0.086	*0.210*
Index 2: Woman took decisions jointly with other members	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	882	449	0.000	1.000	-0.019	*0.769*

Notes: Unit of observation: Household. Column 5 and 11: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, controlling for strata dummies, survey-group dummies, and a vector of baseline household-level controls. Same household-level controls as in Table 3. ***, **, * indicate significance at 1, 5 and 10 percent.

Data source: Household Survey conducted at Midline and at Endline.

Table 9. Well-Being

	Midline Survey						Endline Survey					
		Control Group			Treatment - Control			Control Group			Treatment - Control	
	Obs	Obs	Mean	St dev	Coeff.	p-value	Obs	Obs	Mean	St dev	Coeff.	p-value
Beneficiary woman												
Self-esteem indicator (Rosenberg Scale)	n.a	n.a	n.a	n.a	n.a	n.a	805	419	21.29	3.16	0.04	*0.873*
Mental Health indicator	428	209	14.21	3.24	0.15	*0.644*	793	411	14.65	2.88	-0.26	*0.225*
Husband												
Self-esteem indicator (Rosenberg Scale)	n.a	n.a	n.a	n.a	n.a	n.a	422	303	21.55	3.13	-0.37	*0.277*
Mental Health indicator	306	154	13.64	4.06	0.70	*0.137*	612	308	14.77	2.97	-0.20	*0.441*

Notes: Unit of observation: Household. Midline survey sample: households belonging to groups 3 and 4. Column 5 and 11: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, controlling for strata dummies, survey-group dummies, and a vector of baseline household-level controls. Same household-level controls as in Table 3. ***, **, * indicate significance at 1, 5 and 10 percent.

Data source: Household Survey conducted at Midline and at Endline.

Table 10.1. Midline Survey. Heterogeneity: effects on households depending on women's working status at baseline

	Midline Survey												
		Control Group			Treatment			Treatment * Active Woman at Baseline			Treatment + Treatment * Active Woman = 0		
	Obs	Obs	Mean	St dev	Coeff.		p-value	Coeff.		p-value	Coeff.		p-value
Program participation													
HH heard about the program	940	408	0.934	0.249	0.049	***	*0.000*	0.034		*0.390*	0.083	**	*0.025*
HH took-up	948	411	0.041	0.199	0.917	***	*0.000*	0.015		*0.722*	0.932	***	*0.000*
Woman delegated PWs	948	411	0.012	0.110	0.219	***	*0.000*	-0.046		*0.455*	0.173	***	*0.003*
Employment													
Beneficiary Woman													
Inactive	950	411	0.589	0.416	-0.378	***	*0.000*	0.225	***	*0.005*	-0.153	**	*0.041*
Unemployed	950	411	0.238	0.337	-0.203	***	*0.000*	0.068		*0.207*	-0.135	***	*0.008*
Employed	952	413	0.172	0.352	0.579	***	*0.000*	-0.296	***	*0.000*	0.283	***	*0.000*
Worked as day worker	952	413	0.015	0.106	0.004		*0.576*	0.017		*0.391*	0.021		*0.262*
Worked as salaried	952	413	0.019	0.127	-0.008		*0.329*	-0.024		*0.285*	-0.032		*0.134*
Self-employed	952	413	0.121	0.299	-0.076	***	*0.000*	-0.247	***	*0.000*	-0.323	***	*0.000*
Worked in PW program	952	413	0.017	0.124	0.665	***	*0.000*	-0.028		*0.675*	0.637	***	*0.000*
Worked in other work relationship	952	413	0.0	0.1	-0.003		*0.464*	0.0		*0.243*	-0.015		*0.130*
Hours worked	952	413	6.1	15.5	16.0	***	*0.000*	-13.6	***	*0.000*	2.4		*0.406*
Husband													
Inactive	743	321	0.065	0.217	-0.001		*0.971*	0.008		*0.881*	0.007		*0.884*
Unemployed	743	321	0.09	0.24	-0.035	**	*0.043*	0.076		*0.122*	0.041		*0.369*
Employed	745	321	0.844	0.319	0.034		*0.161*	-0.087		*0.209*	-0.053		*0.413*
Hours worked	744	321	40.3	24.7	1.9		*0.303*	-6.0		*0.268*	-4.0		*0.425*
Share of other HH members													
Inactive	913	395	0.428	0.387	-0.012		*0.596*	0.086		*0.187*	0.074		*0.223*
Unemployed	913	395	0.031	0.101	-0.008		*0.269*	-0.031		*0.148*	-0.039	*	*0.051*
Employed	913	395	0.559	0.374	0.006		*0.797*	0.019		*0.778*	0.025		*0.692*
Labor & Non-labor income in last 7 days													
Total income	944	409	8,087	7,257	3,462	***	*0.000*	-3,677	**	*0.011*	-216		*0.873*
Amount of woman's labor income	942	407	1,147	3,646	3,218	***	*0.000*	-1,832	**	*0.014*	1,385	**	*0.047*
Amount of other HH members' labor income	905	391	6,533	6,117	390		*0.354*	-1,714		*0.166*	-1,324		*0.253*
Amount (in FDJ) of non-labor income	944	409	701	3,073	-95		*0.641*	-149		*0.805*	-244		*0.665*
Per capita HH expenditures (FDJ) over the past 30 days													
Log of per capita HH expenditures													
Total	914	388	9.23	0.72	0.10	**	*0.019*	-0.16		*0.195*	-0.06		*0.599*
Non-durables	914	388	9.21	0.71	0.10	**	*0.012*	-0.16		*0.188*	-0.06		*0.626*
Food	909	384	8.60	0.74	0.12	***	*0.007*	-0.12		*0.353*	0.00		*0.997*
Savings & Insurance													
The HH has any type of savings or insurance	963	419	0.224	0.418	0.060	**	*0.033*	0.012		*0.882*	0.072		*0.349*
Loans													
The household buys at the gocery store at credit	684	290	0.417	0.494	-0.006		*0.872*	0.179		*0.119*	0.173		*0.106*
A HH member has an outstanding loan	684	291	0.062	0.241	-0.019		*0.319*	0.052		*0.341*	0.033		*0.516*

Notes: Unit of observation: Household. Column 5 and 7: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, a dummy for active woman interacted with treatment, a dummy equal to 1 if woman was active at baseline (not shown), controlling for strata dummies, survey-group dummies, and a vector of baseline household-level controls. Household-level controls: age of the head and the beneficiary, number of household members, number of children aged 0-5, number of children aged 6-15 and a dummy equal to 1 if the household belongs to the top 25 percentile of food per capita distribution. ***, **, * indicate significance at 1, 5 and 10

Data source: Employment Diaries and Household Survey conducted at Midline.

Table 10.2. Endline Survey. Heterogeneity: effects on households depending on women's working status at baseline

	Endline Survey												
		Control Group			Treatment			Treatment * Active Woman at Baseline			Treatment + Treatment * Active Woman = 0		
	Obs	Obs	Mean	St dev	Coeff.		p-value	Coeff.		p-value	Coeff.		p-value
Employment													
Beneficiary Woman													
Inactive	827	365	0.672	0.403	0.030		*0.333*	0.114		*0.181*	0.144	*	*0.070*
Unemployed	827	365	0.132	0.267	-0.012		*0.551*	0.055		*0.327*	0.043		*0.409*
Employed	847	374	0.191	0.367	-0.019		*0.492*	-0.157	**	*0.044*	-0.176	**	*0.015*
Worked as day worker	847	374	0.007	0.069	0.010		*0.205*	-0.048	**	*0.024*	-0.038	*	*0.052*
Worked as salaried	847	374	0.012	0.106	-0.006		*0.514*	0.025		*0.301*	0.019		*0.388*
Self-employed	847	374	0.164	0.347	-0.018		*0.508*	-0.129	*	*0.084*	-0.147	**	*0.035*
Worked in PW program	847	374	0.000	0.000	0.000		*0.000*	0.000		*0.000*	0.000		*0.000*
Worked in other work relationship	847	374	0.008	0.071	-0.002		*0.499*	-0.007		*0.420*	-0.009		*0.260*
Hours worked	847	374	7.7	17.7	-0.9		*0.500*	-10.8	***	*0.003*	-11.7	***	*0.001*
Husband													
Inactive	690	296	0.073	0.226	0.006		*0.761*	-0.013		*0.812*	-0.007		*0.889*
Unemployed	690	296	0.040	0.155	-0.015		*0.245*	-0.060		*0.103*	-0.075	**	*0.029*
Employed	692	297	0.882	0.290	0.005		*0.832*	0.079		*0.241*	0.084		*0.182*
Share of other HH members													
Inactive	843	371	0.491	0.399	-0.005		*0.843*	-0.091		*0.210*	-0.096		*0.154*
Unemployed	843	371	0.011	0.047	0.010		*0.107*	-0.015		*0.362*	-0.005		*0.717*
Employed	843	371	0.543	0.363	0.019		*0.430*	0.054		*0.422*	0.073		*0.243*
Labor & Non-labor income in last 7 days													
Total income	846	373	9,117	7,055	-26		*0.962*	-2,032		*0.189*	-2,058		*0.152*
Amount of woman's labor income	845	372	781	2,151	-115		*0.581*	-973	*	*0.096*	-1,087	**	*0.045*
Amount of other HH members' labor income	843	371	8,104	6,693	113		*0.822*	-1,528		*0.279*	-1,416		*0.280*
Amount (in FDJ) of non-labor income	845	372	279	1,509	-35		*0.762*	443		*0.173*	407		*0.176*
Log of per capita HH expenditures													
Total	829	360	9.047	0.588	0.044		*0.300*	-0.166		*0.160*	-0.122		*0.266*
Non-durables	829	360	9.039	0.586	0.038		*0.362*	-0.160		*0.170*	-0.122		*0.260*
Food	829	360	8.307	0.541	0.034		*0.352*	-0.033		*0.746*	0.001		*0.991*
Savings & Insurance													
The HH has any type of savings or insurance	852	375	0.154	0.308	-0.007		*0.771*	-0.045		*0.472*	-0.052		*0.376*
Loans													
The household buys at the gocery store at credit	845	372	0.274	0.375	-0.016		*0.562*	-0.122		*0.113*	-0.138	*	*0.054*
A HH member has an outstanding loan	845	372	0.03	0.136	-0.002		*0.859*	-0.007		*0.819*	-0.009		*0.753*
HH reimbursed a mortgage or house-related loan in last 30 days	729	314	0.029	0.167	0.025	*	*0.096*	-0.010		*0.804*	0.015		*0.697*

Notes: Unit of observation: Household. Column 5 and 7: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, a dummy for active woman interacted with treatment, a dummy equal to 1 if woman was active at baseline (not shown), controlling for strata dummies, survey-group dummies, and a vector of baseline household-level controls. Same household-level controls as in Table 10.1. ***, **, * indicate significance at 1, 5 and 10

Data source: Employment Diaries and Household Survey conducted at Endline.

APPENDIX

Figure A1: Detailed Timeline

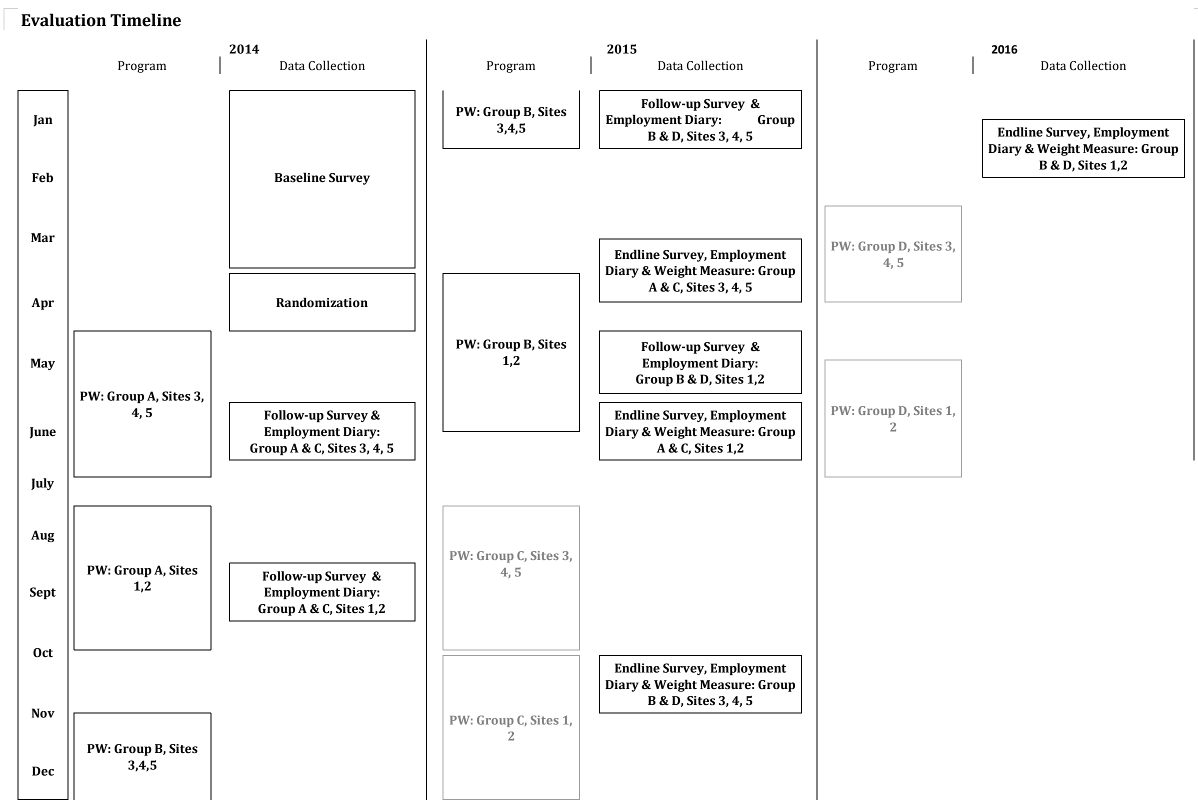

Table A1. Attrition: Woman Midline Survey

Panel A. Attrition rate		Control Group			Treatment - Control		
	Obs	Obs	Mean	St. Dev.	Coeff.		*p-value*
Woman not surveyed at midline	1011	507	0.075	0.264	-0.036	**	*0.014*

Panel B. Summary statistics of households surveyed at Woman Midline Survey		Control Group, surveyed at midline			Surveyed at midline X Treatment		
	Obs	Obs	Mean	St. Dev.	Coeff.		*p-value*
Household characteristics							
Pregnant woman or child 0-3	952	469	0.972	0.164	0.005		*0.645*
Number of HH members	952	469	7.0	2.7	-0.3	*	*0.090*
Number of children 0-5	952	469	1.8	0.7	0.0		*0.394*
Number of children 6-15	952	469	2.2	1.8	-0.2	*	*0.052*
Number of adults >15	952	469	3.0	1.7	-0.1		*0.299*
Male HH head	939	463	0.968	0.177	-0.027	**	*0.046*
Age of HH head	937	463	40.6	8.5	-1.3	**	*0.020*
Head with no education	914	450	0.664	0.473	-0.008		*0.808*
Age of Woman Beneficiary	947	467	33.5	6.8	-1.0	**	*0.026*
Woman benef with no education	942	464	0.821	0.384	0.021		*0.403*
Share of children 6-15 in school (cond on a child 6-15)	709	356	0.775	0.319	-0.005		*0.836*
Beneficiary woman was ... in last 7 days							
Inactive	898	440	0.873	0.334	-0.003		*0.890*
Unemployed	898	440	0.023	0.149	0.008		*0.456*
Employed	898	440	0.105	0.306	-0.005		*0.804*
Day worker	898	440	0.039	0.193	-0.009		*0.451*
Self-employed	898	440	0.052	0.223	-0.001		*0.954*
Salaried	898	440	0.009	0.095	0.008		*0.299*
Other work relationship	898	440	0.002	0.048	-0.002		*0.275*
Share of adult members who were ... in last 7 days (excludes beneficiary)							
Inactive	892	438	0.355	0.373	-0.030		*0.229*
Unemployed	892	438	0.034	0.136	0.010		*0.346*
Employed	892	438	0.611	0.387	0.020		*0.442*
Day worker	892	438	0.339	0.410	0.065	**	*0.024*
Self-employed	892	438	0.037	0.176	-0.014		*0.161*
Salaried	892	438	0.231	0.383	-0.035		*0.170*
Other work relationship	892	438	0.003	0.050	0.002		*0.662*
Income							
Income from labor in last 7 days (in FDJ)	895	438	8,517	9,191	-814		*0.160*
Log of income from labor in last 7 days	683	333	8.98	1.05	-0.07		*0.322*
Expenditures							
Per capita total expenditures in last 30 days	902	440	14,307	11,515	-1,021		*0.120*
Of which: Food expenditures	902	440	7,005	7,247	-890	**	*0.021*
Health & education	902	440	1,487	1,705	122		*0.452*
Other expenditures	902	440	5,815	6,804	-253		*0.517*
Log of per capita total expenditures in last 30 days	902	440	9.410	0.516	-0.045		*0.188*
Log of food expenditures in last 30 days	902	440	8.659	0.538	-0.059	*	*0.090*
Share of households with PMT score above the median	939	463	0.514	0.500	-0.040		*0.223*

Notes: Panel A: Unit of observation: Beneficiary Woman. Sample: all households surveyed at baseline. Panel B: Unit of observation: Household. Sample: all households surveyed at Woman Midline Survey. Column 5: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, controlling for strata dummies. ***, **, * indicate significance at 1, 5 and 10 percent.

Data source: Panel A: Midline Employment Diaries. Panel B: Baseline Household Survey.

Table A2. Attrition: Man Midline Survey

Panel A. Attrition rate		Control Group			Treatment - Control	
	Obs	Obs	Mean	St. Dev.	Coeff.	*p-value*
Husband not surveyed at midline	1011	507	0.276	0.448	-0.025	*0.379*
Panel B. Summary statistics of households surveyed at Man Midline Survey		Control Group, surveyed at midline			Surveyed at midline X Treatment	
	Obs	Obs	Mean	St. Dev.	Coeff.	*p-value*
Household characteristics						
Pregnant woman or child 0-3	745	367	0.967	0.178	0.005	*0.667*
Number of HH members	745	367	7.1	2.6	-0.4 *	*0.050*
Number of children 0-5	745	367	1.8	0.8	0.0	*0.774*
Number of children 6-15	745	367	2.3	1.8	-0.3 **	*0.029*
Number of adults >15	745	367	3.0	1.7	-0.1	*0.365*
Male HH head	733	361	0.989	0.105	-0.028 ***	*0.009*
Age of HH head	731	361	40.7	8.1	-1.6 ***	*0.009*
Head with no education	718	355	0.673	0.470	-0.012	*0.741*
Age of Woman Beneficiary	740	365	33.7	6.6	-1.3 **	*0.013*
Woman benef with no education	735	362	0.834	0.372	-0.001	*0.977*
Share of children 6-15 in school (cond on a child 6-15)	557	286	0.772	0.323	-0.011	*0.698*
Beneficiary woman was ... in last 7 days						
Inactive	720	357	0.871	0.336	-0.010	*0.700*
Unemployed	720	357	0.022	0.148	0.017	*0.198*
Employed	720	357	0.106	0.309	-0.007	*0.750*
Day worker	720	357	0.039	0.194	-0.015	*0.256*
Self-employed	720	357	0.050	0.219	0.008	*0.632*
Salaried	720	357	0.011	0.105	0.003	*0.727*
Other work relationship	720	357	0.003	0.053	-0.003	*0.307*
Share of adult members who were ... in last 7 days (excludes beneficiary)						
Inactive	721	358	0.345	0.369	-0.036	*0.196*
Unemployed	721	358	0.037	0.142	0.010	*0.413*
Employed	721	358	0.618	0.382	0.026	*0.377*
Day worker	721	358	0.370	0.416	0.054	*0.101*
Self-employed	721	358	0.036	0.177	-0.015	*0.183*
Salaried	721	358	0.211	0.369	-0.020	*0.469*
Other work relationship	721	358	0.001	0.018	0.004	*0.203*
Income						
Income from labor in last 7 days (in FDJ)	717	355	8,522	8,657	-1126 *	*0.071*
Log of income from labor in last 7 days	558	276	8.98	1.01	-0.11	*0.198*
Expenditures						
Per capita total expenditures in last 30 days	722	356	14,002	10,634	-704	*0.325*
Of which: Food expenditures	722	356	6,663	5,515	-410	*0.258*
Health & education	722	356	1,451	1,667	76	*0.599*
Other expenditures	722	356	5,888	7,313	-370	*0.433*
Log of per capita total expenditures in last 30 days	722	356	9.40	0.49	-0.04	*0.258*
Log of food expenditures in last 30 days	722	356	8.65	0.50	-0.04	*0.290*
Share of households with PMT score above the median	733	361	0.499	0.501	-0.011	*0.767*

Notes: Panel A: Unit of observation: Beneficiary Husband. Sample: all households surveyed at baseline. Panel B: Unit of observation: Household. Sample: all households surveyed at Man Midline Survey. Column 5: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, controlling for strata dummies. ***, **, * indicate significance at 1, 5 and 10 percent.

Data source: Panel A: Midline Employment Diaries. Panel B: Baseline Household Survey.

Table A3. Attrition: Woman Endline Surveys

Panel A. Attrition rate		Control Group			Treatment - Control		
	Obs	Obs	Mean	St. Dev.	Coeff.		*p-value*
Woman not surveyed at endline hh survey	1011	507	0.114	0.319	0.020		*0.341*
Woman not surveyed at endline employment survey	1011	507	0.093	0.290	0.034	*	*0.086*

Panel B. Summary characteristics of households surveyed at endline		Control Group, surveyed at endline			Surveyed at endline X Treatment		
	Obs	Obs	Mean	St. Dev.	Coeff.		*p-value*
HHs surveyed at Woman Household Survey							
Household characteristics							
Pregnant woman or child 0-3	882	449	0.971	0.168	0.004		*0.684*
Number of HH members	882	449	7.1	2.7	-0.3	*	*0.071*
Number of children 0-5	882	449	1.8	0.7	0.0		*0.540*
Number of children 6-15	882	449	2.3	1.8	-0.2	**	*0.036*
Number of adults >15	882	449	3.0	1.7	-0.1		*0.341*
Male HH head	871	444	0.966	0.181	-0.014		*0.286*
Age of HH head	869	444	40.9	8.4	-1.5	***	*0.009*
Head with no education	849	432	0.662	0.474	-0.018		*0.579*
Age of Woman Beneficiary	877	447	33.6	6.7	-0.9	*	*0.057*
Woman benef with no education	872	444	0.827	0.379	0.013		*0.618*
Share of children 6-15 in school (cond on a child 6-15)	676	356	0.777	0.319	-0.007		*0.774*
Expenditures							
Per capita total expenditures in last 30 days	835	420	14,116	11,408	-941		*0.170*
Of which: Food expenditures	835	420	6,849	6,926	-732	*	*0.061*
Health & education	835	420	1,484	1,665	83		*0.621*
Other expenditures	835	420	5,784	6,956	-292		*0.480*
Share of food in HH expenditures	835	420	0.492	0.143	0.000		*0.969*
Share of health and education in HH expenditures	835	420	0.116	0.111	-0.009		*0.262*
Share of other items in HH expenditures	835	420	0.392	0.136	0.009		*0.342*
Share of households with PMT score above the median	871	444	0.495	0.501	-0.028		*0.422*
Food security							
Is concerned about not having enough food in last 7 days	874	444	0.309	0.462	0.037		*0.256*
Index of food insecurity in last 7 days	874	444	1.1	1.7	0.1		*0.446*
HHs surveyed at Woman Employment Diaries							
Beneficiary woman was ... in last 7 days							
Inactive	847	432	0.866	0.341	0.006		*0.785*
Unemployed	847	432	0.023	0.151	0.004		*0.693*
Employed	847	432	0.111	0.315	-0.011		*0.614*
Day worker	847	432	0.039	0.195	-0.016		*0.194*
Self-employed	847	432	0.058	0.234	0.000		*0.983*
Salaried	847	432	0.009	0.096	0.008		*0.333*
Other work relationship	847	432	0.002	0.048	-0.003		*0.264*
Share of adult members who were ... in last 7 days (excludes beneficiary)							
Inactive	841	430	0.354	0.372	-0.026		*0.319*
Unemployed	841	430	0.036	0.139	0.006		*0.564*
Employed	841	430	0.610	0.387	0.020		*0.469*
Day worker	841	430	0.348	0.410	0.055	*	*0.063*
Self-employed	841	430	0.034	0.170	-0.012		*0.226*
Salaried	841	430	0.224	0.382	-0.027		*0.294*
Other work relationship	841	430	0.003	0.051	0.002		*0.603*
Income & transfers							
Income from labor in last 7 days (in FDJ)	845	430	8,720	9,672	-957		*0.119*
Log of income from labor in last 7 days	655	331	8.981	1.059	-0.103		*0.197*
HH had non-labor income in last 12 months	889	455	0.251	0.434	-0.015		*0.616*

Notes: Panel A: Unit of observation: Beneficiary Woman. Sample: all households surveyed at baseline. Panel B: Unit of observation: Household. Sample: all households surveyed at Woman Endline Household Survey and at Woman Employment Diaries. Column 5: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, controlling for strata dummies. ***, **, * indicate significance at 1, 5 and 10 percent.

Data source: Panel A: Endline Household Survey and Endline Employment Diaries. Panel B: Baseline Household Survey.

Table A4. Attrition: Man Endline Surveys

Panel A. Attrition rate		Control Group			Treatment - Control		
	Obs	Obs	Mean	St. Dev.	Coeff.		*p-value*
Husband not surveyed at endline hh survey	1011	507	0.215	0.411	-0.003		*0.897*
Husband not surveyed at endline employment survey	1011	507	0.286	0.452	-0.013		*0.655*

Panel B. Summary characteristics of households surveyed at endline		Control Group, surveyed at endline			Surveyed at endline X Treatment		
HHs surveyed at Man Household Survey	Obs	Obs	Mean	St. Dev.	Coeff.		*p-value*
Household characteristics							
Pregnant woman or child 0-3	793	398	0.972	0.164	0.000		*0.985*
Number of HH members	793	398	7.0	2.6	-0.3		*0.174*
Number of children 0-5	793	398	1.8	0.7	0.0		*0.654*
Number of children 6-15	793	398	2.3	1.8	-0.2		*0.141*
Number of adults >15	793	398	2.9	1.6	0.0		*0.680*
Male HH head	783	393	0.962	0.192	-0.002		*0.867*
Age of HH head	782	393	40.5	8.3	-1.2	*	*0.056*
Head with no education	767	385	0.66	0.474	-0.013		*0.709*
Age of Woman Beneficiary	790	397	33.4	6.6	-0.6		*0.239*
Woman benef with no education	785	394	0.827	0.378	0.017		*0.525*
Share of children 6-15 in school (cond on a child 6-15)	606	312	0.772	0.329	-8E-04		*0.976*
Expenditures							
Per capita total expenditures in last 30 days	752	373	13,728	9,473	-602		*0.347*
Of which: Food expenditures	752	373	6,733	6,562	-649	*	*0.100*
Health & education	752	373	1,422	1,566	202		*0.266*
Other expenditures	752	373	5,574	4,496	-154		*0.642*
Share of food in HH expenditures	752	373	0.494	0.141	-0.001		*0.892*
Share of health and education in HH expenditures	752	373	0.112	0.108	-0.003		*0.724*
Share of other items in HH expenditures	752	373	0.393	0.134	0.004		*0.670*
Share of households with PMT score above the median	783	393	0.499	0.501	-0.022		*0.543*
HHs surveyed at Man Employment Diaries							
Beneficiary woman was ... in last 7 days							
Inactive	692	344	0.863	0.344	0.011		*0.667*
Unemployed	692	344	0.020	0.141	0.016		*0.218*
Employed	692	344	0.116	0.321	-0.027		*0.250*
Day worker	692	344	0.041	0.198	-0.027	**	*0.038*
Self-employed	692	344	0.058	0.234	0.005		*0.798*
Salaried	692	344	0.012	0.107	0.000		*0.957*
Other work relationship	692	344	0.003	0.054	-0.003		*0.262*
Share of adult members who were ... in last 7 days (excludes beneficiary)							
Inactive	693	345	0.341	0.370	-0.037		*0.195*
Unemployed	693	345	0.035	0.138	0.007		*0.581*
Employed	693	345	0.624	0.384	0.031		*0.313*
Day worker	693	345	0.360	0.413	0.074	**	*0.028*
Self-employed	693	345	0.037	0.180	-0.013		*0.269*
Salaried	693	345	0.226	0.385	-0.036		*0.211*
Other work relationship	693	345	0.001	0.018	0.004		*0.231*
Income & transfers							
Income from labor in last 7 days (in FDJ)	691	342	8,635	8,758	-991		*0.121*
Log of income from labor in last 7 days	541	266	8.972	1.092	-0.114		*0.216*
HH had non-labor income in last 12 months	719	357	0.238	0.427	-0.009		*0.792*
HH made extra-hh transfers in last 12 months	719	357	0.106	0.309	-0.007		*0.766*

Notes: Panel A: Unit of observation: Beneficiary Husband. Sample: all households surveyed at baseline. Panel B: Unit of observation: Household. Sample: all households surveyed at Man Endline Household Survey and at Man Employment Diaries. Column 5: coefficients from an OLS regression of the left-hand side variable on a treatment dummy, controlling for strata dummies. ***, **, * indicate significance at 1, 5 and 10 percent.

Data source: Panel A: Endline Household Survey and Endline Employment Diaries. Panel B: Baseline Household Survey.

Table A5. Baseline Summary Statistics by baseline women's employment status

	Obs	Woman Inactive at Baseline			Active - Inactive		
		Obs	Mean	St. Dev.	Coeff.		*p-value*
Household characteristics							
Pregnant woman or child 0-3	955	835	0.978	0.145	-0.027	*	*0.089*
Number of HH members	955	835	6.8	2.6	0.4		*0.106*
Number of children 0-5	955	835	1.8	0.8	-0.1	*	*0.068*
Number of children 6-15	955	835	2.1	1.8	0.3	*	*0.053*
Number of adults >15	955	835	2.9	1.5	0.2		*0.157*
Male HH head	955	835	0.963	0.189	-0.099	***	*0.000*
Age of HH head	955	835	39.2	9.4	0.7		*0.469*
Head with no education	955	835	0.636	0.481	0.022		*0.658*
Age of Woman Beneficiary	955	835	32.7	6.9	1.4	**	*0.050*
Woman benef with no education	955	835	0.826	0.379	0.019		*0.616*
Share of children 6-15 in school (cond on a child 6-15)	706	611	0.767	0.331	0.038		*0.309*
Beneficiary woman was … in last 7 days							
Inactive	955	835	1.000	0.000	-1.000		*0.000*
Unemployed	955	835	0.000	0.000	0.207	***	*0.000*
Employed	955	835	0.000	0.000	0.793	***	*0.000*
Day worker	955	835	0.000	0.000	0.257	***	*0.000*
Self-employed	955	835	0.000	0.000	0.414	***	*0.000*
Salaried	955	835	0.000	0.000	0.096	***	*0.000*
Other work relationship	955	835	0.000	0.000	0.009	***	*0.007*
Share of adult members who were … in last 7 days (excludes beneficiary)							
Inactive	946	832	0.327	0.373	0.091	**	*0.021*
Unemployed	946	832	0.030	0.131	0.083	***	*0.000*
Employed	946	832	0.643	0.389	-0.174	***	*0.000*
Day worker	946	832	0.387	0.437	-0.029		*0.521*
Self-employed	946	832	0.027	0.146	0.007		*0.654*
Salaried	946	832	0.224	0.379	-0.160	***	*0.000*
Other work relationship	946	832	0.002	0.038	0.013	**	*0.019*
Income & transfers							
Income from labor in last 7 days (in FDJ)	949	830	7,921	8,500	460		*0.607*
Log of income from labor in last 7 days	722	619	8.94	1.01	-0.19	*	*0.091*
HH had non-labor income in last 12 months	955	835	0.218	0.413	0.197	***	*0.000*
HH made a transfer in last 12 months	949	830	0.096	0.295	0.001		*0.963*
Expenditures							
Per capita total expenditures in last 30 days	941	822	13,840	9,738	-865		*0.388*
Of which: Food expenditures	941	822	6,593	5,784	-387		*0.506*
Health & education	941	822	1,536	2,474	97		*0.700*
Other expenditures	941	822	5,712	5,634	-575		*0.329*
Share of food in HH expenditures	941	822	0.491	0.147	0.018		*0.212*
Share of health and education in HH expenditures	941	822	0.109	0.111	0.013		*0.263*
Share of other items in HH expenditures	941	822	0.400	0.138	-0.031	**	*0.029*
Share of households with PMT score above the median	942	824	0.510	0.500	-0.070		*0.175*
Food security							
Is concerned about not having enough food in last 7 days	949	830	0.32	0.47	0.08		*0.100*
Index of food insecurity in last 7 days	949	830	1.12	1.70	0.28		*0.110*

Notes: Unit of observation: Household. Sample: all households surveyed at baseline. Column 5: coefficients from an OLS regression of the left-hand side variable on a dummy equal to 1 if woman was active at baseline, controlling for strata dummies. ***, **, * indicate significance at 1, 5 and 10 percent.

Data source: Baseline Household Survey

CHAPTER 2

Turning a Shove into a Nudge? A "Labeled Cash Transfer" for Education

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Abstract

Conditional Cash Transfers (CCTs) have been shown to increase human capital investments in developing countries, but their standard features make them expensive to administer: transfers are large, means-tested, given to mothers only, and conditioned on certain behaviors. In this paper, we use a randomized experiment conducted over 600 poor, rural communities in Morocco to estimate the impact of a government-run program that is an alternative to CCTs, a "labeled cash transfer" (LCT). The LCT is a small cash transfer, not conditional on school attendance but explicitly labeled as an education support program. The targeting was done at the community level rather than through means-testing at the household level, and the transfers were made to fathers. Over two years, the program led to large gains in school participation, at a fraction of the administrative and total costs of traditional CCT programs. Within our experimental design, we evaluate the effects of adding conditionality and focusing on mothers, but find few differences across these program variants, although the unconditional program leads to somewhat larger gains in school participation and basic mathematics skills than the CCT (due to greater re-enrollment of children who had previously dropped out). We provide evidence that the program increased parents' belief that education was a worthwhile investment, which may be a pathway for the results.

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1. Introduction

This paper evaluates the impact of a "labeled cash transfer" (LCT), as an alternative to conditional cash transfers for education. Ever since the pioneer PROGRESA program in Mexico in the late nineties, conditional cash transfers, or CCTs, are large in amount, targeted at poor households within a community, conditional on regular school attendance, and paid out to mothers. The program we evaluate features small transfers, targeted to poor communities (with all households eligible in those communities), and paid out to fathers.[2] The program is unconditional but retains an implicit endorsement of education through its school-based enrollment procedure. This program was designed and implemented on a (randomized) pilot basis by Morocco's Ministry of Education. Within the same experiment, conducted over 600 communities, we estimate the value added by typical CCT features, namely: (1) making the transfer explicitly conditional on regular attendance, (2) making payments to mothers instead of fathers, and (3) doing both at the same time.

A large body of rigorous evidence, based on CCT programs implemented around the world over the last 15 years, demonstrates their ability to affect households' investments in education and health (see Fizbein, Schady et al. (2009) for a review and Saavedra and Garcia (2012) for a recent meta-analysis). A potential drawback of CCTs as currently designed, however, is that two of their standard features, targeting (typically, individual level proxy-means testing) and conditionality, make them expensive to administer. These two features have been estimated to account for 60% of the administrative costs of PROGRESA (Caldes, Coady and Maluccio, 2006) 49% of the costs for RPS in Nicaragua, and 31% for PRAF in Honduras.

A further drawback of both targeting and conditionality is that they have the potential to lead to the exclusion of the people that policymakers would most like to aid. In Indonesia, Alatas et al. (2012a) find that a proxy-means test mimicking the government's standard practice incorrectly excluded 52% of truly poor households (based on their consumption level) from the list of beneficiaries for a large cash assistance program, while it incorrectly included 18% of non-poor households. In Malawi, under a program whose ultimate goal was to improve

[2] In our context, paying out to father is much less constraining than paying out to mothers and is seen as the "normal" way to proceed.

female adolescent health, girls who dropped out of school and lost their cash transfer eligibility transitioned into marriage and childbearing faster than comparable girls sampled for unconditional transfers (Baird et al., 2011). Furthermore, conditionality can reduce the effectiveness of transfer programs by discouraging some households to even apply for them.

Yet, both targeting and conditionality play important roles in existing CCT programs. Transfers are in part redistributive, and it would not be feasible within the budgets of developing countries to provide all citizens with unconditional transfers worth 20% of a poor household's consumption (to take the example of PROGRESA). Targeting is therefore critical. Regarding conditionality, several recent studies have shown that the incentives that conditionality (or at least perceived conditionality) give to parents may have an additional impact on educational investments, beyond the pure income effect that comes about from unrestricted cash transfers. De Brauw and Hoddinott (2011) exploit the fact that PROGRESA, due to administrative issues, made unconditional transfers to a set of beneficiaries to compare educational outcomes of both groups. They find no effect of conditionality on the likelihood that children attend primary school, but a significant difference among those making the transition from primary to secondary school. Barrera-Osorio et al. (2011) find that making transfers conditional on secondary school graduation significantly improves educational achievement. Baird et al. (2011) run an experiment to compare a CCT to a UCT (Unconditional Cash Transfer) in Malawi between 2007 and 2009. They find that conditioning cash transfers on school attendance increases the effectiveness of the program at keeping adolescent girls in school, but, as mentioned above, decreases its effectiveness at averting teen pregnancy and marriage. Also in an experiment, Akresh et al. (2013) compare a UCT to a CCT conditional on enrollment in Burkina Faso. They argue that CCTs lead to larger impacts than UCTs among girls, and initially out-of-school children, though not for boys and children already enrolled.[3]

Given this tension between, on the one hand, the administrative and human costs of targeting and incentives, and, on the other hand, the fact that they do play a role given the scale of

[3] Conditionality has also been shown to matter for health behavior outcomes. Attanasio, Oppedisano and Vera-Hernández (2013) estimate that, in the Colombian program *Familias en Acción*, children would receive 86% less preventive care visits if the program was not conditional on these visits.

existing CCTs, a natural question is whether it is possible to retain at least some of the human capital benefits of CCTs through a much more limited program. Under the standard economic theory underlying CCTs, conditionality provides economic incentives for households, but those should only have bite if the programs are sufficiently large that the households stand to lose something if they do not comply. At the same time, there is evidence that even small conditional transfers have positive effects on human capital investment (see Banerjee et al. (2010) on incentives for vaccinations, and Filmer and Schady (2008) on the impact of a small CCT in Cambodia). This suggests that economic incentives may not be the only factor at play in CCT. In other words, a "nudge" may be sufficient to significantly increase human capital investment, while CCTs as currently designed provide a big shove. By offering a small cash transfer and tying it loosely to the goal of education, a government may be able to make the importance of education salient and increase the demand for it even without formal incentives. A small cash transfer would not need to be targeted at the household level, since the budgetary implications of inclusion errors (giving it to less poor people) would not be large, and if the explicit incentives are replaced by an implicit endorsement, this removes the need for monitoring.

We evaluate such a program in Morocco, and test the added benefits of attaching more strings to it (conditionality and gender of the recipient), keeping the main features (small size and community targeting only) constant. We were contacted by the government of Morocco who wanted our help in conducting an evaluation of a new CCT program, *Tayssir*, aimed at increasing the rural primary school completion rate, which stood below 60% as of 2008. They had in mind a small transfer to households with children aged 6-15, conditional on enrollment and attendance, paid out to fathers, and targeted at the community level (meaning all households with eligible children in targeted communities could receive the transfer). The transfer amount increased with age/grade but remained modest: the average annual transfer per household equaled about 5% of their annual expenditures, compared to 20% in the PROGRESA program. We proposed to add two components to the planned evaluation: compare it to an unconditional component, and compare it to a more standard version where transfers are given to mothers. The Ministry of Education (the *Ministère de l'Education Nationale*, or MEN), which was administering the program, was very keen that even an unconditional form of the program should be framed as an education intervention. Thus, even for children who were not enrolled in school, the enrollment for Tayssir was done through schools, by headmasters.

Over 320 school sectors (with at least two communities each) were randomly assigned to either a control group or one of four variants of the program: LCT to fathers, LCT to mothers, CCT to fathers and CCT to mothers. Using objective measures of school participation (collected through surprise school visits by the research team) for over 44,000 children, and detailed survey data for over 4,000 households, we find large impacts of cash transfers on school participation under all versions of the program, with larger impacts for the LCTs. Over two years, the LCTs reduced the dropout rate by around 70% among those enrolled at baseline; increased re-entry by 85% among those who had dropped out before the baseline; and cut the share of never-schooled by 43%. The LCTs had modestly positive, though insignificant, impacts on math scores. While the CCTs also had a large positive effect on school participation, explicitly conditioning transfers on attendance significantly *decreased* their impact in the context of this program. In particular, relative to LCTs, CCTs lowered the impact on re-enrollment of children who had dropped out, perhaps because conditionality discouraged some households (or some teachers) from enrolling weaker children in the program. Correspondingly, CCTs also had a significantly lower impact than LCTs on math scores (CCTs had no impact whatsoever, with negative point estimates). We find very little difference in impacts between transfers made to mothers and those made to fathers.

Note that the comparison between LCTs and CCTs tells us little about the question that other papers in the literature have addressed, namely how an unconditional and unlabeled cash transfer program would compare to a CCT. Instead, we study a program where transfers are not conditioned on school participation but school enrollment is strongly encouraged. Indeed, because registration for both LCTs and CCTs was done by school headmasters on the school compound, one reason behind the large impacts of LCTs seems to be that they increased the salience of education as much as CCTs. By the end of the second year, parents' beliefs about the returns to education had increased in all groups, and so had their beliefs about the quality of the local school, even though neither of these two dimensions was affected by the cash transfers. This is consistent with parents interpreting the introduction of a pro-education government program, whether it formally requires regular school participation or not, as a signal that education is important. In line with this, in all groups, there was a large reduction in dropouts reported due to "child not wanting to attend school" and to "poor school quality."

Our results also bring attention to the fact that complex government programs may not always be understood easily, and therefore some of the expected benefits of imposing rules (e.g. conditionality) can be lost in implementation. We took care in our data collection to elicit beliefs from teachers and parents regarding the rules governing the cash transfer program in their community. While teachers had a relatively good understanding of the program in their specific community, among parents we see only minor differences between CCT and LCT communities, in both years 1 and 2, in how the programs were perceived. In the first year, in both groups about 50% of the parents thought the transfers were conditional on attendance. This means that half of the parents in the LCT group wrongly believed the transfers were conditional on attendance, and half of the parents in the CCT group did not know they were. We thus cannot reject that parents in either group had no idea and just guessed when asked about conditionality. By the second year over 80% of parents in the LCT communities had understood that the program was unconditional, but most parents in the CCT communities also perceived transfers as unconditional, most likely because absence rates are low in Morocco, and few children saw their transfers docked. Thus the gap in perceived conditionality between LCT and CCT, while significant statistically at the end of year 2, was less than 5 percentage points. This could explain why we see little impact of adding conditionality above and beyond labeling. Importantly, however, the fact that school participation impacts stayed large for both LCT and CCT programs in year 2, when a great majority of parents believed transfers were *not* conditional on attendance, implies that the confusion regarding the rules is *not* the reason behind the success of the LCT.

Overall, our results suggest that cash transfer programs may work in part by changing how parents perceive education. Of course, much larger transfers may have even larger effects on education, particularly if they are conditional and stringent (as the previous studies looking at the impact of conditionality have found). But just changing perceptions seems to be getting a long part of the way. This is consistent with the recent literature showing that the perceived returns to education are an important determinant of the demand for education, but in developing countries, information about these returns is often imperfect (Jensen, 2010; Jensen, 2012; Kaufmann, 2012; Nguyen, 2008).

To summarize, the LCT program was as effective at increasing education as traditional CCTs have been in other contexts, and cost much less. First, the transfers were small: the average household transfer represented around 5% of household consumption, compared to a range of 6% to 27% for existing CCTs in middle-income countries.[4] What's more, despite the small transfer amounts, the ratio of administrative costs to transfers was favorable to the Tayssir program: 10% compared to a range of 10% to 50% in other programs for which costs are documented.[5] Overall we estimate that the cost per extra year of education in the Tayssir program was at least 40% cheaper than it was in the PROGRESA program.

2. Background and Experimental Design

2.1. Background

Morocco is a lower middle income country, with a GDP per capita estimated at $3,000 in 2011 ($5,100 in PPP terms). Education levels in the general population are still relatively low, with only about 56% of the population literate. As of 2006, the Ministry of Education estimated that while over 87% of rural children started primary school, 40% dropped out before completing the full 6 years of primary education.

How much children learn may be limited, even if they are in school. Morocco ranked 59 out of 69 countries in the math scale for eighth-graders of the TIMMS international tests, and 64 out of 70 on the science scale. This may be due to relatively poor schooling infrastructure in rural areas, and to relatively low motivation levels among teachers, who may resent being posted in remote locations.

[4] World Bank Report (2009). The one exception is Pakistan, which has a transfer program for adolescent girls only amounting to 3% of household consumption.

[5] Authors' calculations based on available information on average administrative costs over the pilot period for PROGRESA (Mexico), PRAF II(Honduras) and RPS (Nicaragua) (Caldes, Coady and Maluccio, 2006). Tayssir cost-transfer ratio is reported for a shorter period than the rest of the programs. For example, Progresa reached a cumulative cost-transfer of 10% after 4 years of implementation and 2,600,00 beneficiaries by the end of the period. During the first two years of Progresa pilot, the cost-transfer ratio represented 1.22 and 0.28 (vs 0.11 and 0.08 for Tayssir).

Despite this, “Mincerian” estimates of the returns to schooling appear large even among rural households. We present some evidence on this (based on our baseline survey data) in Appendix Table A1. Primary school completion for either the male or the female head of the household is correlated with 20% higher consumption at the household level, and these effects are additive. Of course, part or all of these correlations could be driven by selection effects.

2.2. Experimental Design

Tayssir was targeted at the geographic level. The pilot took place in the five poorest regions of Morocco (out of sixteen administrative regions), and within those regions, in the poorest rural municipalities (administrative units called “communes” in Morocco) with high dropout rates at the primary school level.[6] A total of 320 rural primary school sectors (close to 65% of all school sectors in the selected regions) were sampled for the study in those municipalities. Each rural school sector has a well-identified catchment area validated by the *Delegation de l’Education Nationale*, the provincial-level authority for education policy. A school sector includes a “main” primary school unit and several “satellite” school units (four on average). Satellite units fall under the authority of the headmaster of the main unit, and sometimes offer only lower grade classes.

Figure A1 summarizes the experimental design. Out of the 320 school sectors in the study, 260 were randomly selected to participate in the Tayssir pilot program. These school sectors constitute the treatment group. The other 60 sectors in the sample were selected to constitute the comparison group.[7] The 260 school sectors in the treatment group were subdivided randomly into four subgroups, with a two-by-two design: conditional on attendance or simply labeled as designed to facilitate educational investments (“Tayssir” means facilitation in Arabic); and father-beneficiary vs. mother-beneficiary. The groups were not even in size:

[6]The regions are Marrakech-Tensift-Al Haouz, Meknès-Tafilalet, l'Oriental, Souss-Massa-Draa and Tadla-Azilal.

[7] The randomization was stratified by region, school size, dropout rate and by whether the government was planning to make improvements to school infrastructure within the two-year time frame of the evaluation.

while the father vs. mother split was 50%-50%, the conditional vs. labeled only split was 69%-31%.[8]

Two school sectors (one in the control group and one in the treatment group) had to be dropped after the randomization because floods rendered them completely inaccessible to the research team during baseline, leaving us with a final sample of 318 school sectors. Thus, we ultimately have 59 schools in the control group, 40 school sectors in the LCT-to-fathers group, 40 school sectors in the LCT-to-mothers group, 90 schools sectors in the CCT-to-fathers group and 89 school sectors in the traditional-style CCT-to-mothers group.

School sectors participating in the pilot program were selected such that they would be relatively far from each other, which limited the risk that parents transferred their children from control to treatment schools or from CCT to LCT schools, as well as other forms of externalities.[9]

Table 1 presents summary statistics for schools in the control sample (column 1), differences between the control group and the LCT-to-fathers group (column 2), as well as differences between the LCT-to-fathers group and the three variants with added components (columns 3-5).[10] Schools in the sample are relatively small, with an average enrollment in grades 1 to 5 of only 77 pupils. Over 60% of classes are taught in multi-grade groups. Only 42% of the students are girls, suggesting that girls are less likely to be enrolled than boys. Schools are

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[8]The reason why there was more CCT than LCT schools is that, in an attempt to estimate the intensity with which conditionality needs to be monitored if it ends up mattering, each of the two CCT groups was randomly subdivided in three more subgroups of equal size. In one group, teachers were in charge of recording absences in a register that was then passed on to the central Tayssir administration determining payment amounts ("light monitoring"). In the second group, the same system was used, but to encourage accurate reporting, teachers were informed that their registers would be audited through unannounced school visits by school inspectors ("moderate monitoring"). In the third group, in addition to the teachers filling registers, biometric machines were installed in the classrooms to record child attendance daily through fingerprint recognition ("full monitoring"). In practice, neither the moderate nor the full monitoring arms could be implemented. School inspectors were reluctant to perform audits and the biometric machines proved too fragile or error-prone to be reliable. As a result, the "light monitoring" system was used to enforce conditionality in all schools in the CCT groups and we therefore lump all three subgroups for the analysis.

[9] The median distance between any two school sectors in the regions of study is six kilometers, which is quite large considering that 99.5% of children in our sample report walking to school. The median distance between any two school sectors in the experimental sample is even larger by design.

[10] This table follows the same format as the main regression tables below. As explained in more details in section 4, column 2 presents estimates on the differential characteristics for schools sampled in the *LCT-to-fathers* group compared to control group schools. Columns 3 to 5 present estimates on the differential effect of the three other treatment groups compared to the *LCT-to-fathers* group, along with the total effects p-values for test of equality between LCT and CCT, and mothers versus fathers.

quite poor, with only 19% of the classrooms equipped with electricity and just about half equipped with latrines/toilets. Overall, the control and LCT-to-fathers groups appear relatively well balanced with respect to observable characteristics: one out of 12 of differences estimated are significant at the 10% level. There are, however, some differences between the CCT and LCT groups, and some differences between father and mother groups. In the analysis, we control for the two baseline school characteristics that are imbalanced at baseline (remoteness and electricity) as well as student characteristics (age and gender). The control variables do not affect the results.

2.3. The Tayssir Cash Transfer Program

The Tayssir program consisted of cash payments made to parents of primary school age children (6 to 15). The cash allowance was increasing with age, starting from 60 Moroccan dirhams (MAD) per month (~$8 in 2008 USD) per child old enough for grades 1 or 2 (6-7 years old), to 80 MAD per month (~10 USD) per child old enough for grades 3 and 4 (8-9 years old), to 100 MAD per month (~13 USD) per child old enough for grades 5 and 6 (10-11 years old). Thus for young children the cash allowance for a year (10 school months) was up to 600 MAD, and for the older children it was up to 1,000 MAD. This compares favorably to (very modest) yearly schooling expenditures, reported at 180 MAD on average per child in primary school in our control group, suggesting that the transfers were ample enough to compensate for at least the direct costs of schooling. But the transfers are very small compared to most CCTs: the monthly transfer for a child in grade 3 to 4, for example, represents 2.7% (3%) of the mean (median) monthly household consumption level in our sample (and still only 6.3% of the monthly consumption level of households at the bottom 5th percentile). The transfer that the average household was eligible for represented 5% of the average monthly consumption. In contrast, the range for traditional CCTs is between 6% and 27% of mean monthly household consumption (World Bank, 2009). In PROGRESA, the average transfer for grade 3 to grade 6 was $14 and the total monthly average transfer received by households was $43, which corresponds to 20% of household consumption.[11]

[11] Transfer reported in Coady (2000) for 1997-2000 period, expressed in 2008 USD.

Parents had to formally enroll each of their children into the program. Headmasters, who had been trained through group-specific province-level meetings just before the start of the academic year, were responsible for disseminating information to parents of school-age children about the program and its rules, and for enrolling them. For all groups, unconditional and conditional, this enrollment took place at the primary school, and required the presence of the designated beneficiary (the father or the mother, depending on which experimental group the school sector was in). In both years, the open enrollment period started at the beginning of the school year (early September) and lasted for approximately three months. Children who had been enrolled into the program in year 1 were automatically re-enrolled in year 2 provided the school headmaster forwarded their names to the provincial authorities.

In the LCT groups, the transfer was fixed and not conditional on attendance or continued enrollment, but parents still had to enroll their child in the Tayssir program yearly in order to receive the money. While in the original design enrolling in school at the beginning of the year was not a condition for enrolling in Tayssir, in practice the two turned out to be linked: enrollment in the Tayssir program was done at school by the headmaster, and *de facto* children were systematically registered and enrolled in a grade by the headmaster at the same time they were registered for Tayssir (if not yet enrolled). (School registration is free in rural areas of Morocco). The fact that Tayssir enrollment took place at the school, even when continued school enrollment was not required to receive the transfers, is an important feature, because drawing applicants into that environment served to link the program to education. Indeed, it made it very clear and salient to households that the transfers were coming from and overseen by the Ministry of Education, and were part of an effort to promote education. The flyers that schools were given to advertise the program showed schoolchildren sitting at their school desk and studying. This is why we call this a *Labeled Cash Transfer* (LCT).

In the CCT groups, the transfer was formally conditional on enrollment and regular attendance. The rule was that the allowance for a given month and a given child would be cancelled if the child missed school more than four times over that month. Absences from school caused by the teacher's absence were excluded from this count. Headmasters, teachers and school committees received guidelines from the Ministry of Education on how to monitor and record attendance and how to submit reports every two months to the provincial-level program manager at the Ministry. The reports included, for each month, the total number of absences for each child enrolled in the program. These reports were then digitized by the provincial-level program managers, and shared, through an integrated information system, with the central management team at Ministry of Education. The central management team determined whether the conditionality had been respected and estimated the amounts that each household should receive for any given month. This process was time-consuming and created important delays, especially early on, as described below.

Headmasters were instructed to enroll only mothers or only fathers, depending on which variant of the program the school was in. There was however an exception policy: households with a written authorization from the *Moqadem* (the local representative of the Moroccan administration) could enroll another adult in the household. Exceptions were typically granted when the sampled recipient did not live at home (for example, if the father worked in the city and came home only a few times a year, the mother was allowed to enroll instead). Overall, as we discuss below, compliance with the gender assignment was above 80%.

The cash transfers were disbursed to the assigned recipients (upon presentation of a national ID card) at the local post office. Areas that did not have a post office (about a third of the sample) received the visit of a "mobile cashier" in charge of distributing the transfers. On average, the cost of a round trip to the nearest pick-up point was around 20 MAD or 8% of the average transfer. However, if they wanted to save on transportation costs, recipients could wait and withdraw all their transfers at once.

Overall, program take-up is very high: 97% of households in our household sample had at least one child enrolled in Tayssir by the end of year 2, and the take-up rate at the household level was almost identical across all four treatment groups. Households had on average two children enrolled in the program. This is much higher than the take-up of a CCT program in Indonesia, for which poor households had to register by showing up on a specific registration day: Alatas et al. (2012b) find that only 61% of the very poorest households (those guaranteed eligibility) signed up (and the sign-up rate is lower among all income groups). The take-up rate in our household sample may be an overestimate of the overall take-up rate, however, since our household sample excludes households with no prior contact with the local school (given our sampling strategy, discussed in section 3.2). Our household sample also over-represents households living relatively close to the school. To the extent that headmasters played an important role in contacting households they knew or who lived nearby, take-up in our sample is an upper bound of overall take-up. Contrasting the administrative records on Tayssir enrollment at the municipality level with the (very noisy) data on total number of households in a given municipality as reported by the local chief (the Moqadem) confirms the take-up rate was quite high, however, with the ratio between the two at 88% on average (with a very large standard deviation, however).

Three payments were made to enrolled households over the course of the first year. Due to delays in setting up the system for collecting and managing school attendance data, the Ministry of Education decided in December 2008 that the first transfer, corresponding to the first two months (September-October 2008), would be given to all households enrolled in the program without conditionality. For the conditional groups, the next two transfers in year 1 were conditional on attendance.[12] In year 2, five transfers were made to households, and each transfer covered a two-month period, as per the program protocol. For the conditional groups, all those payments were conditional on attendance. To maintain comparability, each payment was made simultaneously to conditional and unconditional groups. Across groups, households qualified for just around 3,000 MAD (~ 350 USD) on average in total transfers through the first 18 months of the pilot.

3. Data

To estimate the impacts of the Tayssir Program, we collected detailed data on schooling achievement in two school units (the main school unit and one randomly chosen satellite unit) for each of the 318 sectors included in the study.

[12] See Figure A2 for the timeline of the program implementation. The first transfer took place in late January and early February, 2009. The second transfer took place in late May/early June 2009, and it covered four months, November 2008 to February 2009. The third and last payment for year 1 covered the rest of the school year, and took place late August 2009/early September 2009.

Four types of data were collected. (1) We measured school participation through school visits spread over the two years of the program, for all students enrolled in the study schools at the beginning of “year 0” (the academic year 2007-2008). We call this the “school sample” and it comprises over 47,000 students. (2) We conducted a comprehensive survey at both baseline and endline with close to 4,400 households – we call this the “household sample.” (3) We administered a basic arithmetic test (ASER test) to one child per household during the endline household survey; and (4) We conducted “awareness” surveys at and around schools to measure teachers’ and households’ understanding of the program. Figure A2 summarizes the timeline of the data collection and we provide below the details for each of these datasets.

3.1. School Participation

Through school visits, the research team (which had no relationship with the Tayssir team or the Ministry of Education and was blind to the assignment to the different groups) collected data on school participation. We conducted a total of seven visits per school. The first visit was announced, and conducted at baseline, in June 2008, just before the end of the pre-program school year (we call this “year 0”). During that first visit, we copied school registers for all grades 1 to 5. This register data provides the universe of children that were enrolled in school at the beginning of year 0, and whether they had dropped out or where still enrolled by the end of year 0 (June 2008, when we conducted our baseline). This constitutes our “school sample.” Appendix Table A2 provides summary statistics at the child level for this school sample, broken down by treatment group. The second visit was also announced, and conducted at the beginning of the first program year. Two additional (unannounced) visits were conducted during the first year of the program (in March/April and May 2009). The fifth visit was announced, and conducted at the beginning of the second academic year. Two unannounced visits were conducted later on that year (in February and April 2010).

During each visit, we updated the schooling status of all children in the initial lists, recording who had dropped out of school and when, which grade each pupil was in (if still attending regularly), whether the teacher was present in class, and, whether the pupil was present. Names of newly enrolled students were also recorded. To analyze the impact of the program on school participation and dropout, we use data from all seven visits. However, to analyze the impact of the program on attendance, we use only data from the four surprise visits.

Attrition in this dataset (shown in Appendix Table A2) is very low since we did not need to individually track each child in the sample to obtain their schooling status, but instead relied on whether the child was found in the classroom on the day of the visit, and if not, checked registers and interviewed teachers and other students/siblings to determine whether the child had dropped out. We consider a student as a *dropout* if he or she was absent from school on the surprise visit, and was considered as dropped out by the teachers and other students. We consider a student as *attending school* if he was present on the visit day, or absent but listed on the register as enrolled for that month and having attended school at least some time in the previous 30 days.[13]

3.2. Household Surveys

For each school unit, eight households were sampled for a baseline survey (administered in June 2008, before Tayssir was announced and before school sectors had been randomly assigned to either treatment or control) and an endline survey (administered in June 2010). The sampling frame used to select these households was the following. Enumerators visited each school (again, these were two per school sectors, the main unit and one satellite unit) in spring 2008, and used the 2007/2008 school register, as well as the registers of the previous three academic years, to draw two lists: (1) the list of all households in the school's vicinity that had at least one child enrolled in school, and (2) the list of households with no child currently enrolled in school but at least one child of school-age who had enrolled at some point but dropped out within the previous three years. A total of six households were randomly selected from list 1, and two households were randomly selected from list 2, using a random number generator spreadsheet. This sampling method means that our sampling frame does not include households who never enrolled any school-age children in school, but

[13] School attendance registers were very well kept and updated. Teachers are supposed to update the list of enrolled students every month (when they have to write the names of all currently active students on a new page) and to record their presence on a daily basis. Teachers typically do not copy the name of students that they consider as dropouts when they move on to a new page (i.e. a new month). The fact that we find a very high attendance rate of 95% (objectively measured through surprise spot checks) for those officially enrolled (on the register for that visit's month), while at the same time observing a high dropout rate, confirms that teachers truthfully report the *de facto* dropouts as dropouts.

such households appear very rare. (We attempted to get lists of such households from the Moqadem, but they could rarely come up with any household fitting that description, which is why systematically enrolling a few such households in the study at each location was not possible.)

Overall, a total of 5,032 households were sampled. Of them, 4832 (96%) could be interviewed at baseline. Of those interviewed at baseline, 91% were interviewed at endline. An additional 111 households that were sampled but not surveyed at baseline were found and surveyed at endline. Table A3 presents analyses of attrition at both baseline and endline. Attrition was more pronounced in the control group than Tayssir groups. To check whether this differential attrition yields imbalance in household characteristics, Table 2 presents summary statistics by group for the final, post-attrition endline sample of 4,385 households. The groups appear relatively well balanced with respect to observable characteristics. Fewer than 10% of all possible pair-wise comparisons yield differences that are significant at the 10% level. There appears to be some differences in baseline schooling rates, however. In the control group, 7 percent of children 6-15 had never enrolled and 14% had enrolled but dropped out, with the remainder (79%) enrolled. The share out of school at baseline for the LCT-to-fathers group is significantly lower, with 3.2 percentage points fewer never-enrolled and 2.7 percentage points fewer dropouts. Schooling rates for the other treatment groups fall somewhere in between the control and the LCT-to-fathers group. In all analyses below we condition on baseline schooling status so these baseline differences do not drive our results.

Households in the sample are relatively large, with an average of 6.8 members across all groups, including 3.1 children under 16 years old and 2.4 children in the 6-15 age group, the target group for Tayssir. Literacy rates are quite low, with only 23% of household heads knowing how to read and write. Financial access is also very low, with only 3% of households holding a bank account.

3.3. ASER Arithmetic Tests

As part of the endline survey administered to study households, one child between six and 12 years old at baseline was randomly selected to take a short arithmetic test based on the ASER test developed by Pratham.[14] This test does not evaluate children for age- or-grade specific competency. Instead, it tests the ability of children to perform basic arithmetic, such as recognizing one-digit or two-digit number, performing a subtraction, and performing a division. Of the 4,682 children sampled, only about 3,316 (71%) were available for the arithmetic test during the endline survey. Table A4 presents analysis of attrition, which was equally high across all five groups. Observable household characteristics for children who took the test are overall balanced.

3.4. Program Awareness Surveys

In order to estimate how much communities knew about Tayssir and its rules by the end of the first program year, a survey on "program awareness" was conducted in 387 schools in April 2009. The survey included only a few questions such as: "Have you heard of a program called Tayssir?"; "Have you been receiving transfers from the government related to your children?"; "Do you know what the transfers depend on?"; etc. The survey was administered to teachers (for each school, we attempted to survey the headmaster or deputy headmaster, as well as one grade 4 Arabic language teacher) as well as parents (for each school, we attempted to survey two households from the household sample).

A similar awareness survey was administered at the end of the second year (May/June 2010) to headmasters and teachers in all schools. We also included a module on Tayssir in the endline survey administered to study households.

4. Empirical Strategy and Results

4.1. Empirical Strategy

The random assignment of cash transfers, their conditionality and their designated beneficiary across school sectors means that, in expectation, students in the control and various treatment groups have, conditional on baseline schooling status, comparable background characteristics and abilities. Thus, they likely would have, on average, comparable outcomes in the absence of any cash transfer program. By comparing outcomes between the LCT-to-fathers group and the control group, we can thus estimate the effect of

[14] See information on ASER at http://www.pratham.org/M-19-3-ASER.aspx

the small unconditional cash transfer program we are testing. By comparing outcomes across treatment groups, we can estimate the relative importance of the various program components – conditioning on attendance and beneficiary's gender. The sample size was large enough that we are able to detect even small differences in impact across groups.

We estimate the effect of being assigned to each of the treatment groups, on the outcomes of interest, using the following specification:

$$Y_{i,j} = \alpha + \beta_1 * TAYSSIR_j + \beta_2 * LCT_{mother\,j} + \beta_3 * CCT_{father\,j} + \beta_4 * CCT_{mother\,j} + X'_{i,j}\gamma + \varepsilon_{i,j}$$

where:

$Y_{i,j}$ is the outcome for student *i* in school *j*

$TAYSSIR_j$ is a dummy equal to 1 if school *j* is selected for TAYSSIR in any form (i.e., in any of the cash transfer groups)

$LCT_{mother\,j}$ is a dummy equal to 1 if school *j* is in the LCT-to-mothers group

$CCT_{father\,j}$ is a dummy equal to 1 if school *j* is in the CCT-to-fathers group

$CCT_{mother\,j}$ is a dummy equal to 1 if school *j* is in the CCT-to-mothers group

$X_{i,j}$ is a vector of strata dummies, school-level controls (access to electricity and remoteness) and child-level controls (age, gender, schooling status and grade at baseline)

In this equation, $\widehat{\beta_1}$ estimates the effect of unconditional but labeled cash transfers paid to the father of primary school-age children, and therefore the impact of the version of the program that has minimal strings attached (since having the father pick up the money would be the natural default in Morocco). $\widehat{\beta_2}$ captures the differential (compared to LCT-to-fathers) effect of designating the mother as transfer recipient (while maintaining the lack of conditionality on attendance); $\widehat{\beta_3}$ estimates the differential effect of making transfers conditional on attendance (while keeping the father as transfer recipient) and, lastly, $\widehat{\beta_4}$ is the estimate of both making transfers conditional and paying them to the mother. Strata dummies take account of stratification variables used in the randomization. We adjust the standard errors for clustering at the school sector level. Finally, because our sampling procedure at the household level oversampled households with dropout children, we use sampling weights in all analyses using the household survey data.[15]

Most tables we present estimates of equation (1) with the same format. Each row corresponds to a given dependent variable. Column 1 presents the mean of that variable in the control group (with its standard deviation in bracket underneath). Columns 2-5 present the β coefficient estimates and standard errors (in parentheses) from equation (1). Columns 7 and 8 present the p-values for the hypotheses that CCT has no differential impact compared to LCT and that transfers to fathers have no differential impacts compared to transfers to mothers.[16] We only present results that include controls for the key school and child characteristics mentioned above ($X_{i,j}$), but results remain essentially unchanged when we omit those controls.

4.2 Compliance with, and understanding of, the experimental design

To interpret the results, it is important to check that the experimental design was actually implemented as planned. Table 3 presents summary statistics on program implementation in the four Tayssir groups.

Enrollment in the Tayssir program was high. In the LCT-to-fathers group, 97% of the households in our survey had at least one child enrolled, and 73% of the children ages 6-15 at baseline were enrolled. There is no systematic pattern by gender or by conditionality: at the child level, enrollment was a little higher in the LCT-to-mothers group and in the CCT-to-mothers group than in the other two. Compliance with the gender assignment was very high: it was close to 89% on average in schools where mothers had been designated as recipients, and around 80% in schools where fathers had been. This lower compliance rate for fathers is primarily due to the fact that men in rural Morocco sometimes out-migrate for work for part of the year. Overall, though, fathers were over 75% more likely to be Tayssir recipients in the

[15] Our final household sample includes 17% of households with dropout children, while those households represent only 9% of the population.

[16] Note that the test in column 8 weights the impact of gender on CCTs three times as much as the impact of gender for LCTs, since in our experiment the CCT group was three times larger than the LCT group.

father groups than in the mother groups; therefore our study is powered to detect even small impacts of the designated gender of the recipient.[17]

Compliance by the Tayssir staff with the transfer rules was high as well. Administrative data shows that, after the first transfer that all households got unconditionally, all subsequent transfers made to parents in the CCT groups were a function of attendance records, while none of the transfers in the LCT groups were. As a result, households in the LCT groups received more money over the lifetime of the program (though the difference is not very large, given that overall compliance with the conditionality was extremely high in the CCT groups).

Among local communities, conditionality appears to have been poorly understood, however. In Table 4, we present data on understanding of the program in both years. While teachers were quite well informed on the exact amounts of the transfers for various age groups, there is at most a 20 percentage point difference in the beliefs that transfers are conditional on attendance between teachers in the CCT and those in the LCT groups (Panel A). While this difference is highly significant, it is quite far from the 100 percentage point difference we would have expected under perfect understanding. Over the course of the program, understanding improved among teachers. By the end of year 2, close to 75% of teachers in CCT schools believed transfers were conditional on attendance, against only 40% in LCT schools.

Our measure of understanding of parents is, unfortunately, not perfect (it is very difficult to ask parents neutral (non-leading) questions about their understanding of the rules, and be sure that they have actually understood the questions), but the data we have suggests that parents were confused. There was no apparent difference in beliefs about the conditionality between CCT and LCT groups at the end of year 1 (Panel C), with just about 50% of parents in all groups thinking that the transfers were conditional on attendance (so parents may just have been taking a guess when answering the survey). By the end of year 2, confusion had cleared

[17] One could be concerned that the money, while handed to the mothers, was directly appropriated by the father. To test this, Table 3 also checks whether the designated recipient picked up the cash transfer alone. We find that 33% of designated mothers picked up the transfer alone (compared to 70% of designated fathers). 14% of designated mothers were accompanied by their husband when they picked up the transfers, and 40% were accompanied by another household member.

in the LCT communities, with over 80% of parents knowing the transfers were not conditional. [18] But the dominant belief in the CCT groups was also that transfer amounts were *not* conditional on attendance. This could be because, as we will see, school attendance happens to be very high in Morocco, conditional on enrollment. Most households in the CCT groups therefore ended up getting the full transfers, and had no experience of what would happen if the children were absent a lot. What's more, as shown in Figure A2, government delays meant that transfers arrived in lumps of different sizes (from 2 to 4 months worth) with a delay of at least 3 months – making it difficult for parents to infer the rules by themselves.

The relatively poor understanding of the CCT rules among intended beneficiaries is an important outcome in and of itself. Indeed, at the beginning of each school year, a real effort was made to try to make communities (who were the ones in charge of enrolling parents) understand the rules of the program. Each school director received instructions and handouts explaining the rules specific to their school sector. If, despite this, parents only have a dim sense of what the program rules are and the extent to which they're enforced, the role conditionality plays in providing incentives is necessarily blunted. This relates to a recent paper by Kaufmann et al. (2012): studying a CCT program in Brazil in which conditionality is strictly enforced, they find that child attendance increases once households get formal warnings that their child's absenteeism threatens their standing in the program, and increases even more after the households start being punished. This highlights the role of perceptions in the role that incentives can play in CCT. This is an important point since timely enforcement of conditionality, and therefore their proper understanding, is likely to be difficult to achieve in many settings.[19]

[18] In the LCT group, program officers visited individual households at the end of year 1 to re-iterate that they only needed to enroll their children in Tayssir at the school to get the transfer every month.

[19] Evaluations of cash transfer programs so far have not systematically collected data on program comprehension, so comparing the level of understanding in our setting with that in others is difficult. In particular, Akresh et al. (2013) do not report perception of conditionality by parents in their program. Baird et al. (2011) look at the perception of the conditionality among adolescent girls receiving a UCT by conducting qualitative interviews. They report a good understanding of the program rules (i.e. of the fact that no condition is required to receive the transfer), but they also provide evidence that girls in the UCT arm had friends in the CCT arm and knew the school attendance of these friends was monitored, putting the UCT in the broader context of an education program.

4.3 Results: Impacts on School Participation

Table 5 shows the main results on school participation. We present the results obtained from two separate sources: the household surveys (Panel A) and the school visits (Panel B), finding very consistent results across the two sources.

Starting with the household sample, the first row of the table shows the main result: the impact of the program on school participation at the end of year 2 among all primary-school aged children in the household sample, irrespective of status at baseline (but controlling for schooling status at baseline). School participation is a dummy equal to 1 if the child was reported as having attended school at least once in the last month of program year 2. The effect is very large. We find that school participation is 7.3 percentage points higher in the LCT-to-fathers group than in the control group. This corresponds to a decrease in non-participation of around 30 percent. It is much larger than the impact of the first CCT, PROGRESA, at the primary level, at least in part because attrition from primary school is a larger problem to start with in Morocco than in Mexico. The effect is similar regardless of the gender of the recipient (father/mother) but 2 percentage points *higher* (significantly so) under the LCT than under the CCT program.

The next rows provide a breakdown of the school participation effect by baseline school participation status. We find that both the dropout and the re-enrollment margins are affected. In the household sample, the dropout rate diminishes by 75% under the Tayssir program, no matter how it is implemented (a drop of 7.5 percentage points, off of a base rate of 10% in the control group). In the much larger school sample (Panel B), the results are very similar: dropout declines from 7.6% in the control group to about 2.5% in all the Tayssir groups. The consistency between the self-reported participation data in the household survey and the school sample results (which are based on direct observations in classrooms during spot checks) is important and implies that parental reports of child participation were truthful.

The household data also shows that re-entry almost doubles in the LCT groups (from 14.7% in the control group to 27.2% in the LCT-to-fathers group). In the CCT group, the effect is still large, but significantly smaller. The re-entry difference is the source of the greater overall impact on school participation of the LCT compared to the CCT.

Since CCT is conditional on attendance, while LCT is not, it is important to check the impact on attendance. The results of surprise attendance checks are presented in row 4 of Panel B (for the school sample). Note that attendance conditional on enrollment is a selected outcome, since the program affects dropout, and this would bias us against finding positive impact on attendance. Attendance of enrolled children is very high overall during the periods covered by our unannounced spot checks (February, March and May). The mean attendance rate of 95.5% in the control group corresponds to an average of 1.1 days of absence per month, well below the threshold of four absences imposed on students in the CCT arm. Attendance in the LCT group is, if anything, higher than in the control group, though not significantly so. The LCT impact on school participation that we found in the household survey data thus translates into effective participation in school, and it is definitely not the case that parents enrolled their children in school just to get enrolled with Tayssir and did not bother to send them to school very regularly afterwards.

If children spent more time in school, what did school participation crowd out? We collected hour-level time-use data for the day preceding the endline survey for every child aged 6-15 at baseline. Table 6 presents results from this data, restricting the sample to the 25% of households interviewed before the summer school break started.[20] (We don't present the four versions of the Tayssir program separately, as the sample size is too small to detect small differences between them, but we find no systematic patterns.) Looking first on the extensive margin of school participation, we find a large impact of the Tayssir transfers, with children of program households over 50% more likely to have attended school the day before the survey. This is a much larger effect than that observed in Table 5, and suggests that the program has much more bite in the very last weeks of school before the summer break – a period during which both pupils and teacher attendance appears much spottier than the rest of the year.

Correspondingly, we see a large increase in the time children spent in school-related activities in the day before the survey (this includes the time spent in school as well as time doing homework and participating in extracurricular activities organized by the school). In

[20] The initial plan was to interview all households before the school break, but the start of the endline survey was delayed due to logistical constraints. We have the same proportion of households surveyed before the school break in all groups.

Tayssir groups, children spent about an extra hour and a half on average in school-related activities in the day preceding the survey compared to 2.5 hours spent by children in the control group. Overall, the magnitude of the time use results in Table 6, when compared to those in Table 5, suggests an important intensive margin effect in addition to the extensive margin effect: children in the Tayssir groups spend more time studying and more time physically at the school, as well as more time traveling to and from school, *conditional* on being enrolled. This extra time spent on learning did not come at the expense of time spent on chores, but in a small part at the expense of household farming or business activities and in a larger part at the expense of what we call leisure: play and social activities. This suggests that children had time to spare invest in education and thus, in this environment, the barrier to schooling may have had more to do with lack of interest than with severe constraints.

4.4Results: Impacts on Basic Math Skills

Few studies of conditional cash transfers have measured learning outcome among school-age children, but when they have, they found no effects, despite increases in participation (Behrman et al., 2005; Filmer and Schady, 2009). This is line with many other studies that have been effective at increasing school participation but have found little impact on learning (see Glewwe and Kremer, 2006, and Glewwe et al., 2012, for reviews), which raises some questions on the value of promoting school participation without some improvements in school quality. To be able to test the underlying premise behind the Ministry of Education's plan for a cash transfer program, we collected a simple measure of learning achievement, the ASER arithmetic test, that could be administered at home, and thus does not suffer from sample selection due to differential school participation rates across groups. Table 7 shows the impact on performance on the test, which as administered to one randomly selected child per household during the endline survey. Panel A show the results question by question for all children, as well as results on a standardized measure of achievement on the test, and Panel B presents the standardized measures by gender, school type and baseline enrollment status. There is a modest positive impact of LCT-to-fathers on standardized test scores (0.075, which rescaled amounts to 0.075/0.694=0.11 or 11% of a standard deviation in the control group), which is not quite significant in the overall sample (although it is larger and significant for students enrolled at baseline and those from satellite school units).

Interestingly however, here again we can rule out equality of the LCT and the CCT impacts: the CCT had significantly *smaller* impacts than the LCT. In fact, even a small positive effect of the CCT program (over the control group) can be ruled out. The difference between CCT and LCT is significant at 5%. This is consistent with the participation results, and suggests that LCTs are, if anything, more effective than the CCT in this context.

4.5Results: Who did the program affect most?

Akresh et al. (2013), who compare a purely unconditional cash transfer and a CCT program in Burkina Faso, fond insignificant differences on average between the programs, but argued that the UCT had smaller effects than the CCT on more "marginal" children: girls, out-of-school children, and children of lower ability. To investigate this question in our context, Table 8 shows the main impact of LCT-to-fathers and the effect of all the other versions of the program for these different subgroups (and Panel B of Table 7 presents the subgroup results on learning).

Possibly because we consider a *labeled* unconditional cash transfer program rather than a pure UCT, our results differ from those found by Akresh et al. (2013). First, as mentioned earlier, the impact of the Tayssir LCT on re-enrollment rate for children who had dropped out is significantly *larger* than the impact of the CCT. Second, although girls have a lower education level than boys (67% of girls aged 6-15 were in school at the end of year 2, against 80% of boys), the LCT does not have a smaller effect on girls than boys. In fact, if anything it appears that girls are driving the larger impact of LCT than CCT on re-enrollment: for girls initially dropped out school, the increase in re-enrollment in the LCT-to-fathers group is 12 percentage points and is significant, in the LCT-to-mothers it is 12.8 percentage points and significant, but it is zero in the CCT-to-fathers groups and only 4 percentage points) in the CCT-to-mothers group. The difference between LCT and CCT for these girls initially out of school is significant at 1%.

In the last two columns of Panel A, Table 8, we break down the children in the household sample based on their predicted probability of school participation. This predicted probability is constructed using coefficient estimates of enrollment status on school-level, household-level and child-level characteristics in the control group (these coefficient estimates are shown in Table A5). Not surprisingly, we find that all the program effects on school participation are concentrated among those with a predicted likelihood of school participation below the median. And for those, the effect of the CCT is significantly smaller (3.6 percentage points, or 23% lower) than that of the LCT. This result, while important in itself, also confirms that despite poor understanding by parents of the specific rules of the programs, it is *not* the case that the LCT and CCT programs were completely equivalent in practice – if they were, we would not see any difference in impacts.

5 Mechanisms

The main findings so far are that the Tayssir program, which provided small transfers to parents to help with the education of their children, had a large impact on school participation, both through reducing dropout and through encouraging re-enrollment. Further, attendance is very high for all children who are enrolled, so this increase in enrollment translated into real gains in schooling, although we do not find large impacts on learning. The second important finding is that there is essentially no difference between transfers to fathers and mothers (and there was very good enforcement of the gender of the recipient). The third finding is that the LCT has a significantly larger impact on school participation (mostly through higher re-enrollment). In this section we provide some additional evidence to shed light on the mechanisms behind these results.

5.1 Making Education Salient

Figure 1 shows the dropout rates by cause in the control group, and how they were affected by the program (we only show all the Tayssir groups together for brevity, but there was no significant difference across any of the groups). In the control group, the three main reasons for dropping out of school are accessibility of the school ("school is too far"), financial reasons, and the fact that the child did not like school ("child's choice"). Tayssir was not designed to affect distance to school and, not surprisingly, did not reduce dropout rates due to distance. In contrast, it reduced the incidence of dropouts due to financial difficulties, though this effect is not quite significant at conventional levels (the p-value is 0.123). Interestingly, Tayssir had an even larger impact on dropouts due to children simply not wanting to be in school. Also, dropouts due to the belief that school is of poor quality were also considerably reduced by Tayssir. This is surprising, since Tayssir was not accompanied by improvements in school quality and did not include any transfers to schools, therefore leaving school infrastructure quality unchanged.[21] If anything, the increase in school participation in those schools may have lowered quality, to the extent that class size matters.[22]

One conjecture is that the program, which gave teachers the crucial role of enrolling households, was perceived as an implicit endorsement of the local schools by the Ministry of Education. Table 9 provides further evidence for this. Parents in schools sampled for the Tayssir program, irrespective of which variant of the program they are in, rank the school quality significantly higher.

Parents may also have interpreted the introduction of a program sponsored by the Ministry of Education as a positive signal about the value of education more generally. Consistent with this, the evidence in Table 9 shows that parental beliefs regarding the returns to education dramatically increased, especially for girls. For girls, the cash transfer programs led to very large positive changes in the perceived returns to education.[23] In the control group, parents point estimate of the returns to primary school for girls is actually negative. There is a large increase in the Tayssir group, and it becomes positive. The perceived returns to secondary school are more than twice as large in the Tayssir group as in the control group. This is driven by changes on the extensive margin – parents in the Tayssir group believe the likelihood of getting employed is higher with primary or junior high school education than parents in the control group. For boys, the effect is small at primary school, and large but not significant at the secondary school level.

Did Tayssir make parents over-optimistic about education? As mentioned earlier, Appendix Table A1 reports estimate of the "Mincerian" returns to education in our sample. The

[21] Within the two-year time frame of the Tayssir pilot, there were improvements in school infrastructure through an emergency plan put in place by the Ministry of Education, but, as explained in Section 2.2, we were able to stratify by whether a school was scheduled to receive infrastructure support when randomly assigning school sectors to experimental arms.
[22] We can also rule out the possibility that the Tayssir program increased teacher effort or motivation. Overall, we find no program effect on teacher absenteeism. Teachers miss about 10% of school days in control schools, which corresponds to an average of 2.5 days in a given month. Teacher attendance was unaffected by the introduction of Tayssir in any form.
[23] We observe this increase in perceived returns among both types of households in our sample (those sampled from the list of enrolled children and those sampled from the list of recent dropouts). The increases are similar in all versions of the programs, so we pool here for precision.

increase in household consumption when a female has completed primary education is actually much larger than what even household in the Tayssir groups estimate (parents underestimate the returns to primary education), though this may in part due to selection bias, of course.

Several studies (Jensen, 2010, 2012; Nguyen, 2008) have shown that parents respond to interventions that increase the perceived returns to education by increasing participation and effort in school. Although the Tayssir program was not focused on persuading parents of the returns to education, the impact on the perceived value of education was actually larger in our intervention than in those ones and, as in those, we find an increase in school enrollment.

5.2 Is a nudge all that is needed?

To the extent that conditionality had any impact, it was a negative one: the LCT impacts on overall school participation and learning were slightly stronger than the impacts of the CCT. As mentioned in the introduction, this result differs from those of previous studies, which tend to find positive impacts of conditionality, at least for some subgroups (see Baird et al. (2011) and Akresh et al. (2013) for two experiments). This is likely because, while the transfers were not conditional on attendance, Tayssir was quite explicitly framed as an education program: headmasters were conducting the enrollment into the program, and the enrollment took place in schools. Thus, while *unlabeled* unconditional transfers may be less effective at increasing school participation than transfers tied to education, and while strict enforcement of conditionality seems to have additional impact on attendance (Kaufmann et al., 2012), unconditional but labeled transfers such as the one piloted in Morocco may well provide the nudge that is sufficient to convince parents to send their children to school. While we have not experimented with larger transfers or with finer targeting, it seems that a small transfer targeted only through at the community level was sufficient to achieve a large impact. Thus, the big shove that is provided by the CCT may not be necessary to substantially raise school participation.

In the Moroccan context at least, the nudge has a number of advantages over the shove. First, it is substantially cheaper, both because the transfers per child are smaller and because the administrative costs are lower. If one considers that transfers are not costs (only the deadweight loss of raising the funds for them is a cost), the point is even stronger, because the administrative costs of Tayssir are a fraction of those of the traditional CCT. Table 10 presents a cost-effectiveness analysis of the program. The overall cost of the LCT was $99 per child per year (in 2008 US dollars, $89 in transfers and $10 (7%) in administrative costs). Compared to the three earlier CCT programs presented in Table 10, Tayssir LCT has the smallest cost-transfer ratio, even relative to PRAF II in Honduras which had mainly a geographical targeting and small transfers (of 4% of household consumption). The cost-effectiveness comparison also favors Tayssir: for the Tayssir LCT program, $1,000 led to an increase of 0.24 years of education at the primary level -- in other words, the cost of an extra year of education is $4,228 in the Tayssir LCT program. For PROGRESA, the cost of an extra year of primary education induced by the CCT is at least 70% higher.[24]

The second advantage of the nudge over the shove in our context is that the LCT had actually larger impacts on enrollment and days spent in school than the CCT. This comes from the marginal children – those with a lower propensity to be in school absent any transfers. One likely explanation for this result is that, for people who understand it, the conditionality on attendance may be discouraging: someone who feels like they will not manage to have less than four absences a month may either not enroll or give up under a CCT, but continue under the LCT. Parents in our study context seemed relatively confused about the rules governing the CCT, but this effect could also have come about through the teachers themselves. Indeed, teachers were much more likely to have understood the conditionality, and it is possible that in conditional schools they did not bother to actively seek and enroll into Tayssir the parents of students whom they feel would not regularly attend. Since pupil absenteeism is not a big problem, the incentives based on attendance may thus have discouraged students to enroll without having much bite for those enrolled anyway, making the LCT a better alternative in this context.

Finally, while we did not explicitly compare different ways of targeting households, the very large take-up of Tayssir points to a very important advantage of the geographical targeting

[24] Coady (2000) estimates the cost of an extra year of primary education, for Progresa, at 55,000 pesos (in 2000 pesos), which is equivalent to around $7,300 (in 2008 USD). Dhaliwal et al (2013) estimate an even higher cost per year of education for Progresa.

used in this study. Indeed, in Indonesia, Alatas et al (2012b) find that in a proxy-means tested program where eligible households must sign-up on their own to enroll and receive benefits, many poor eligible households do not actually sign-up. By removing any ambiguity on eligibility, and putting the responsibility of enrolling households on the teachers, the geographical targeting in the Tayssir program was able to eliminate this problem to a large extent (although as we just discussed not all *children* enrolled, and some vulnerable children may have been left out in the CCT).

6 Conclusion

Through a large-scale randomized experiment conducted jointly with the Moroccan Government, we show that a cash transfer labeled for education and made to households of primary school age children in rural areas had a very large impact on school participation – despite the fact that the transfer was not conditional on attendance, was given to fathers rather than mothers, and was relatively small – enough to cover the direct costs of education but very small relative to most earlier CCTs as a share of household consumption, even for the poorest households in our sample. These strong results are due in part to an endorsement effect: parents update upwards their beliefs about the value of education when a large pro-education government program enters their community. The cash transfer was labeled for education purposes, since it was coming from the Ministry of Education, and enrollment for the program was administered by school headmasters. In this context, adding formal conditions on attendance tends to decrease the overall impact on participation and learning, and targeting the program to mothers makes no difference.

In a context where pupil absenteeism (conditional on teacher's presence) is negligible for most of the school year, this labeled unconditional cash transfer (LCT) is more cost effective than the standard CCT, both because it requires transfers of lower amounts (which may not be counted as costs anyway) and, more importantly, because the administrative costs are much lower. Even in our setting, the administrative costs are reduced by more than one fourth in the LCT version compared to the CCT, and the conditionality slightly lowers the effect and worsens the targeting. We note that our context is not unique: in Burkina Faso, Akresh et al. (2013) find similarly low rates of absenteeism among enrolled students. In

Kenya, Duflo et al. (2012) also find very low rates of absenteeism among lower grade students conditional on teacher's presence.

A key question is whether LCT impacts would persist in the long run. To the extent the impacts are due to an increased estimate of the returns to education, long-run impacts will be hampered if the program leads parents to temporarily overestimate those returns. Overoptimistic parents should revert back to their previous levels of investment once they realize that their child's education has not delivered what they had hoped it would. In our survey data, however, parents appear to still underestimate the returns to education, even after the introduction of the program, suggesting that this disappointment effect will be unlikely.

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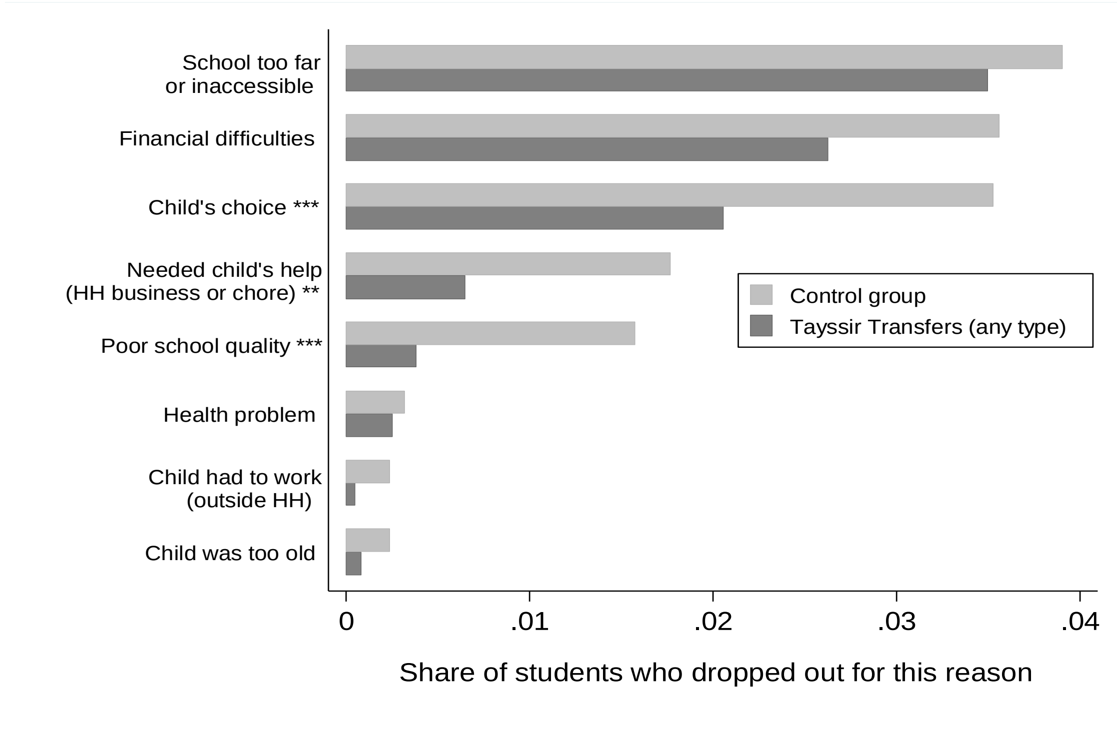
Figure 1. Effect of Tayssir Program on Dropouts, by Cause



Notes: Data source: Household survey collected from study households; unit of observation: child; average of 2.5 children per household. Standard errors are clustered at the school-sector level. ***, **,* indicate significance at 1, 5 and 10%. The p-value for the difference in "financial difficulties" between Tayssir groups and the control group is 0.123.

Table 1. School Level Characteristics at Baseline: Balance Check

	Mean in Control Group	*Difference between* LCT to Fathers *and* Control	Difference between [....] and LCT to Fathers			*N*	P-value for CCT different from LCT	P-value for Mother different from Father
			LCT to Mothers	CCTs to Fathers	CCTs to Mothers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Total Enrollment	77.402	-2.826	5.094	-1.818	0.938	627	0.398	0.345
	[57.468]	(5.504)	(6.258)	(5.036)	(5.028)			
Share of grades that are taught in multigrade classes	0.611	-0.012	0.005	0.02	-0.035	627	0.761	0.197
	[0.399]	(0.05)	(0.057)	(0.047)	(0.047)			
Average number of sections per grade	1.305	-0.006	0.062	0.002	-0.006	627	0.313	0.685
	[0.344]	(0.048)	(0.057)	(0.045)	(0.046)			
Average age	9.636	0.094	-0.089	0.005	-0.111	610	0.885	0.03**
	[0.589]	(0.074)	(0.086)	(0.063)	(0.059)*			
Share of Female Students	0.422	0.007	0.014	0.01	0.015	612	0.663	0.468
	[0.111]	(0.018)	(0.02)	(0.016)	(0.016)			
Students-Teacher Ratio	21.698	-0.137	0.315	-0.432	-0.603	612	0.36	0.983
	[9.566]	(1.149)	(1.3)	(1.087)	(1.057)			
Teachers Presence Rate during baseline surprise visit	0.794	0.109	-0.065	-0.072	-0.072	600	0.283	0.601
	[0.379]	(0.05)**	(0.06)	(0.046)	(0.049)			
Students Presence Rate during baseline surprise visit†	0.926	0.026	-0.032	-0.027	-0.037	491	0.144	0.201
	[0.141]	(0.02)	(0.016)**	(0.018)	(0.017)**			
Distance to main road (in km)	9.127	2.328	-0.722	-3.665	-3.432	613	0.055*	0.968
	[12.446]	(2.793)	(3.117)	(2.413)	(2.551)			
School inaccessible during winter	0.425	-0.011	-0.023	-0.11	-0.174	587	0.012**	0.27
	[0.497]	(0.092)	(0.095)	(0.081)	(0.082)**			
School has electricity	0.188	0.131	-0.044	-0.102	0.045	601	0.875	0.033**
	[0.392]	(0.068)*	(0.074)	(0.06)*	(0.066)			
School has toilets	0.495	0.085	0.013	-0.036	-0.079	611	0.141	0.549
	[0.502]	(0.066)	(0.075)	(0.058)	(0.059)			
Distance to the post office (in km)	24.765	-4.703	3.541	0.113	1.008	611	0.608	0.424
	[27.239]	(3.989)	(4.025)	(2.679)	(3.016)			
Number of school units	117	80	78	176	176	628		
Number of school sectors	59	40	40	90	89	318		

Notes: Data source: Preliminary school survey and baseline school survey. Unit of observation: School unit.
Column 1: Standard deviations presented in brackets. Columns 2-5: coefficients and standard errors (in parentheses) from an OLS regression of the school characteristic on treatment dummies, controlling for strata dummies. Standard errors are clustered at the school-sector level. ***, **, * indicate significance at 1, 5 and 10%.
† Conditional on teacher presence.

Table 2. Study households: Balance Check

	Mean in Control Group	Difference between LCT to Fathers and Control	Difference between [….] and LCT to Fathers				P-value for CCT different from LCT	P-value for Mother different from Father
			LCT to Mothers	CCTs to Fathers	CCTs to Mothers	N		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Head of HH is Male	0.97	0.009	-0.001	-0.001	-0.007	4385	0.539	0.39
	[0.17]	(0.011)	(0.01)	(0.009)	(0.009)			
Age of Head of HH	46.171	-1.259	1.324	1.369	0.562	4309	0.4	0.69
	[9.751]	(0.614)**	(0.65)**	(0.531)**	(0.522)			
# of HH members	6.764	-0.021	0.006	-0.02	0.021	4385	0.971	0.712
	[2.057]	(0.142)	(0.157)	(0.134)	(0.135)			
# of children 6-15	2.394	-0.05	-0.025	0.033	-0.001	4385	0.497	0.445
	[0.98]	(0.071)	(0.074)	(0.069)	(0.07)			
% of children 6-15 never enrolled in school at baseline (year 0)	0.07	-0.032	0.018	0.021	0.008	4369	0.394	0.597
	[0.163]	(0.011)***	(0.011)	(0.01)**	(0.009)			
% of children 6-15 who were dropped out at baseline (year 0)	0.141	-0.027	0.009	0.03	0	4369	0.261	0.073*
	[0.239]	(0.016)*	(0.017)	(0.015)**	(0.014)			
HH Head reads and writes	0.234	0.035	-0.067	-0.002	-0.032	4318	0.364	0.025**
	[0.424]	(0.03)	(0.031)**	(0.027)	(0.027)			
HH Head has at least some education	0.281	0.018	-0.05	0.015	-0.021	4303	0.25	0.026**
	[0.45]	(0.033)	(0.032)	(0.027)	(0.027)			
Monthly Per capita consumption (MAD)	448.979	7.726	-11.233	-6.962	-4.625	4279	0.985	0.864
	[196.751]	(18.202)	(20.95)	(15.938)	(17.177)			
Owns agricultural land	0.636	0.004	0.024	0.023	-0.025	4277	0.63	0.283
	[0.481]	(0.043)	(0.045)	(0.038)	(0.038)			
Owns a cellphone	0.614	0.132	-0.065	-0.08	-0.081	4348	0.021**	0.325
	[0.487]	(0.035)***	(0.033)*	(0.029)***	(0.027)***			
Owns a television	0.714	0.041	-0.046	-0.059	-0.027	4348	0.417	0.725
	[0.452]	(0.048)	(0.045)	(0.038)	(0.038)			
Owns a bank account	0.03	0.012	0.001	-0.006	-0.007	4347	0.447	0.991
	[0.17]	(0.015)	(0.016)	(0.014)	(0.015)			
HH has electricity	0.545	0.071	-0.037	-0.087	0.004	4385	0.551	0.175
	[0.498]	(0.069)	(0.067)	(0.058)	(0.058)			
Number of households	790	567	574	1227	1227	4385		

Notes: Data source: Baseline household survey. Sample: Random subset of around 7 households per school unit, including only households also surveyed at endline (see Table A2 for attrition analysis). Sampling weights are used since households with dropout children were over-sampled.
Unit of observation: Household. Column 1: Standard deviations presented in brackets. Columns 2-5: coefficients and standard errors (in parentheses) from an OLS regression of the left-hand side variable on treatment dummies, controlling for strata dummies. Standard errors are clustered at the school-sector level. ***, **, * indicate significance at 1, 5 and 10%.

Table 3. Take-up and Compliance with Study Design

	Mean in LCT to Fathers	Compared to LCT to Fathers, differential effect of…			*N*	P-value for CCT different from LCT	P-value for Mother different from Father
		LCT to Mothers	CCTs to Fathers	CCTs to Mothers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HH enrolled in program	0.967	0.009	0.002	0.004	3707	0.834	0.574
	[0.178]	(0.012)	(0.01)	(0.01)			
% of children age 6-15 enrolled in program	0.734	0.039	0.022	0.005	3707	0.585	0.979
	[0.268]	(0.015)**	(0.013)*	(0.014)			
Female Head is transfer recipient	0.14	0.757	-0.024	0.771	3707	0.826	0***
	[0.347]	(0.027)***	(0.026)	(0.024)***			
Mother usually goes alone to pickup Tayssir transfer	0.06	0.321	0.025	0.283	3690	0.782	0***
	[0.238]	(0.03)***	(0.023)	(0.031)***			
Father usually goes alone to pickup Tayssir transfer	0.712	-0.655	0.021	-0.683	3690	0.969	0***
	[0.453]	(0.032)***	(0.03)	(0.028)***			
Mother and father usually go together to pickup Tayssir transfer	0.02	0.12	-0.023	0.114	3690	0.264	0***
	[0.139]	(0.019)***	(0.011)**	(0.017)***			
Mother usually goes with other people to pickup Tayssir transfer	0.069	0.271	-0.026	0.347	3690	0.451	0***
	[0.253]	(0.032)***	(0.021)	(0.032)***			
Cost of a round trip to the nearest pick-up point (MAD)	21.149	0.565	-0.999	1.833	3586	0.93	0.111
	[25.42]	(2.598)	(2.102)	(2.216)			
# of payments received (source: Tayssir admin. data)	6.562	-0.102	-0.051	-0.117	3477	0.649	0.294
	[1.387]	(0.105)	(0.081)	(0.097)			
Amount for which the HH was eligible (source: Tayssir admin. data)	3048.059	-154.24	-105.767	-266.203	3470	0.104	0.009***
	[1486.965]	(111.593)	(99.414)	(102.962)**			
Sum of payments cashed out as share of monthly expenditures at baseline	1.118	-0.01	-0.024	-0.101	3367	0.136	0.11
	[0.726]	(0.067)	(0.053)	(0.055)*			
Number of months (out of 16 total) in which at least one child in the HH had more than 4 absences (source: Tayssir admin. data)			1.025	0.641			0.11

Notes: Data sources: Endline Household survey and Tayssir Admninistrative database. Unit of observation: Household. Sampling weights are used since households with dropout children were over-sampled.

Column 1: Standard deviations presented in brackets. Columns 2-5: coefficients and Standard errors (in parentheses) from an OLS regression of the left-hand side variable on treatment dummies, controlling for strata dummies, and variables specified below. Standard errors are clustered at the school-sector level. ***, **, * indicate significance at 1, 5 and 10%.

School-level controls: access to electricity and remoteness. Household-level controls: share of children enrolled in school at baseline and household owns a cellphone.

Table 4. Program understanding

	Mean in LCT groups	Differential effect of UCT	N
	(1)	(2)	(3)
Panel A: Program understanding among Teachers at the end of Year 1			
Thinks the transfers are conditional on attendance	0.535	0.168	457
	[0.501]	(0.051)***	
If thinks transfers are conditional: knows precise rule (<5 absences)	0.737	0.181	292
	[0.443]	(0.063)***	
Knows exact amount of transfer for compliant 4th-grade child	0.852	0.021	457
	[0.356]	(0.035)	
Could not be surveyed	0.123	0.07	542
	[0.33]	(0.04)*	
Panel B: Program understanding among Teachers at the end of Year 2			
Thinks the transfers are conditional on attendance	0.399	0.37	690
	[0.491]	(0.046)***	
If thinks transfers are conditional: knows precise rule	0.747	0.095	453
	[0.437]	(0.056)*	
Knows exact amount of transfer for compliant 4th-grade child	0.877	0.023	659
	[0.329]	(0.027)	
Could not be surveyed	0.084	0.026	767
	[0.277]	(0.019)	
Panel C: Program understanding among Parents at the end of Year 1			
Ever heard of the program	0.942	-0.036	664
	[0.234]	(0.029)	
	0.296	0.037	620
Thinks the transfers are conditional on something but doesn't know what	[0.458]	(0.043)	
Thinks the transfers are conditional on attendance	0.49	-0.011	620
	[0.501]	(0.052)	
If thinks transfers are conditional: knows precise rule	0.313	0.105	315
	[0.466]	(0.07)	
Could not be surveyed	0.068	-0.007	702
	[0.252]	(0.025)	
Panel D: Program understanding among Parents at the end of Year 2			
Ever heard of the program	0.995	-0.008	3707
	[0.07]	(0.003)**	
Thinks the transfers depend on something but does not know what	0.068	0.016	3654
	[0.251]	(0.009)*	
Thinks the transfers depend on attendance	0.115	0.031	3654
	[0.319]	(0.018)*	
If thinks transfers depend on attendance: knows precise rule	0.7	0.007	481
	[0.46]	(0.053)	

Notes: Data sources: Panels A-C: Knowledge surveys administered to a subset of school teachers (including school directors) and households. Panel D: Endline survey administered to all households sampled for the study.
Weights are included in Panel C to get a sample representative of households surveyed at baseline. Sampling weights are used in Panel D since households with dropout children were over-sampled.
Column 1: Standard deviations presented in brackets. Column 2: coefficients and Standard errors (in parentheses) from an OLS regression of the left-hand side variable on a dummy for the Conditional Treatment dummy, controlling for strata dummies and variables specified below. Standard errors are clustered at the school-sector level. ***, **, * indicate significance at 1, 5 and 10%.
Panels A and B controls include: respondent gender, respondent status (teacher or headmaster) and school-level controls for access to electricity and remoteness. Panel C controls include: school-level controls for access to electricity and remoteness, and household level controls for share of children enrolled in school at baseline and household owns a cellphone.

Table 5. Effect on School Participation

	Mean in Control Group	Impact of LCT to Fathers	Difference between [….] and LCT to Fathers			N	P-value for CCT different from LCT	P-value for Mother different from Father
			LCT to Mothers	CCTs to Fathers	CCTs to Mothers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Household sample								
Attending school by end of year 2, among those 6-15 at baseline	0.737	0.073	0.004	-0.019	-0.02	11074	0.011**	0.948
	[0.44]	(0.016)***	(0.014)	(0.012)	(0.013)			
Dropped out by end of year 2, among those enrolled in grades 1-4 at baseline	0.1	-0.075	-0.005	0.014	0.004	5998	0.013**	0.114
	[0.3]	(0.011)***	(0.008)	(0.007)**	(0.007)			
Attending school by end of year 2 if had dropped out at any time before baseline	0.147	0.125	-0.007	-0.063	-0.047	1264	0.063*	0.629
	[0.355]	(0.04)***	(0.047)	(0.04)	(0.039)			
Never Enrolled in school by end of year 2, among those 6-15 in year 0	0.035	-0.011	0.003	0.012	0.000	11072	0.227	0.086*
	[0.185]	(0.008)	(0.006)	(0.006)**	(0.005)			
Panel B: School sample								
Dropped out by end of year 2, among those enrolled in grades 1-4 at baseline	0.076	-0.051	0.006	0.004	-0.002	35755	0.54	0.497
	[0.265]	(0.01)***	(0.006)	(0.005)	(0.005)			
Dropped out during year 1, among those enrolled in grades 1-4 at baseline†	0.029	-0.017	0.001	0.000	-0.004	35755	0.339	0.277
	[0.168]	(0.007)**	(0.004)	(0.003)	(0.003)			
Dropped out during year 2, among those enrolled in grades 1-4 at baseline†	0.048	-0.036	0.005	0.004	0.002	35215	0.976	0.904
	[0.214]	(0.005)***	(0.004)	(0.003)	(0.003)			
Attendance rate during surprise school visits, among those enrolled	0.955	0.007	0.002	0.007	0.007	86694	0.125	0.918
	[0.206]	(0.01)	(0.009)	(0.007)	(0.007)			
Completed primary school, among those enrolled in grade 5 at baseline	0.644	0.079	-0.029	-0.025	-0.041	6680	0.408	0.46
	[0.479]	(0.032)**	(0.036)	(0.035)	(0.031)			

Notes: Data source: Panel A: Household survey collected from study households; unit of observation: child; average of 2.5 children per household; sampling weights are used since households with dropout children were over-sampled. Panel B: School visits data; unit of observation: child (rows 1,2,3, 5 of Panel B) and child-day (row 4 of Panel B).

Each row presents the results of a separate regression. Column 1: Standard deviations presented in brackets. Columns 2-5: coefficients and standard errors (in parentheses) from a LPM regression of the left-hand side variable on treatment dummies, controlling for strata dummies and variables specified below. Standard errors are clustered at the school-sector level. ***, **, * indicate significance at 1, 5 and 10%.

Panel A: School-level controls include: access to electricity and remoteness. Household controls include: share of children enrolled in school at baseline and household owns a cellphone. Individual controls include: age, gender and schooling status at baseline (end of year 0).

Panel B: Individual controls include: age, gender, schooling status and grade the child attended at the end of year 0; school-level controls include access to electricity and remoteness. The regression on attendance also control for the day of the visit.

† Dropout during year X include dropouts in the summer between school year X-1 and year X, as well as dropouts in the course of year X.

Table 6. Daily Time use

	Mean in Control Group	Effect of Tayssir (any type of treatment group)	N
	(1)	(2)	(3)
Spent at least some time in school	0.36	0.19	1227
	[0.48]	(0.05)***	
Dep. Var: Minutes spent doing […] during the day before survey, children 6-15 at baseline			
Any type of schooling activity	140.94	80.69	1227
	[178]	(16.68)***	
Including:			
Time spent in school	90.83	50.86	1227
	[126.71]	(11.73)***	
Time spent doing homeworks	31.25	13.42	1227
	[66.76]	(7.54)*	
Time to go and to come back from school	18.75	16.66	1227
	[32.59]	(3.9)***	
Household chores	97.19	-10.54	1227
	[148.67]	(9.13)	
Working on HH business/farm/outside	69.45	-26.82	1227
	[149.83]	(13.01)**	
Social activities/leisure[a]	307.69	-57.29	1227
	[190.26]	(16.94)***	
Personal time (eating, sleeping, dressing…)	749.96	7.8	1227
	[96.04]	(8.09)	
Other activities (not doing anything, walking (not to school)…)	58	-3.71	1227
	[81.72]	(8.37)	

Notes: Data source: Endline household survey. Unit of observation: Child. Sample is restricted to 554 households interviewed before the summer school break started (June 15, 2010).
Column 1: Standard deviations presented in brackets. Column 2: coefficients and Standard errors (in parentheses) from an OLS regression of the left-hand side variable on Tayssir dummy, controlling for strata dummies and variables specified below. Standard errors are clustered at the school-sector level. ***, **, * indicate significance at 1, 5 and 10%.
Child controls include: age, gender, schooling status and grade in year 0 (if any) and day of the week the survey was administered. All regressions also include household- and school-level controls as in Table 5 Panel A. Sampling weights are used since households with dropout children were over-sampled.
[a] This category consists of 7 sub-activities pre-specified in the survey under the header "leisure/social activities": social and religious activities; social celebrations; playing with other children; visiting family or neighbors; playing sports; watching TV; using the internet or playing video games; and playing at home.

Table 7. Impacts on basic math skills: Results of ASER Arithmetic test

	Mean in Control Group	Impact of LCT to Fathers	Difference between [….] and LCT to Fathers			*N*	P-value for CCT different from LCT	P-value for Mother different from Father
			LCT to Mothers	CCTs to Fathers	CCTs to Mothers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. All								
Can recognize one-digit numbers	0.967	0.008	0.01	-0.009	-0.013	3316	0.004***	0.986
	[0.178]	(0.01)	(0.008)	(0.009)	(0.008)*			
Can recognize two-digits numbers	0.91	0.033	0.006	-0.028	-0.022	3316	0.004***	0.517
	[0.287]	(0.015)**	(0.015)	(0.015)*	(0.013)*			
Knows how to substract	0.466	0.05	-0.029	-0.034	-0.041	3316	0.417	0.586
	[0.499]	(0.047)	(0.049)	(0.041)	(0.041)			
Knows how to divide	0.346	0.019	-0.01	0.003	-0.034	3316	0.689	0.238
	[0.476]	(0.04)	(0.042)	(0.036)	(0.036)			
Summary index	0	0.075	0	-0.051	-0.076	3316	0.044**	0.593
	[0.694]	(0.052)	(0.05)	(0.046)	(0.043)*			
Panel B. Summary Index, by subgroups								
Boys	0	0.081	-0.017	-0.052	-0.04	1722	0.348	0.942
	[0.685]	(0.06)	(0.065)	(0.055)	(0.056)			
Girls	0	0.079	-0.002	-0.078	-0.115	1594	0.028**	0.587
	[0.706]	(0.072)	(0.072)	(0.063)	(0.061)*			
Main school unit	0	0.029	0.016	-0.052	-0.029	1706	0.208	0.605
	[0.698]	(0.068)	(0.062)	(0.058)	(0.055)			
Satellite school unit	0	0.136	-0.047	-0.051	-0.124	1610	0.162	0.127
	[0.691]	(0.072)*	(0.075)	(0.072)	(0.067)*			
Enrolled in school at baseline (end of year 0)	0	0.097	0.004	-0.059	-0.071	2950	0.043**	0.838
	[0.686]	(0.056)*	(0.052)	(0.048)	(0.045)			
Out of school at baseline (end of year 0)	0	0.073	-0.17	-0.143	-0.18	366	0.544	0.423
	[0.694]	(0.151)	(0.178)	(0.145)	(0.15)			

Notes: Data sources: ASER test administered to (at most one) child aged 6-12 at baseline per household during endline household survey visit. Sampling weights are used since households with dropout children were over-sampled. Unit of observation: Child.
Column 1: Standard deviations presented in brackets. Columns 2-5: coefficients and Standard errors (in parentheses) from an OLS regression of the left-hand side variable on treatment dummies, controlling for strata dummies and variables specified below. Standard errors are clustered at the school-sector level. ***, **, * indicate significance at 1, 5 and 10%. Controls included: child age and gender, dummies for child schooling status by June 2008, school was in session at the time of the survey, and same school-level and household-level controls as in Table 5 Panel A.

Table 8. School Participation by subgroups

	Mean in Control Group	Impact of LCT to Fathers	Difference between [....] and LCT to Fathers			*N*	P-value for CCT different from LCT	P-value for Mother different from Father
			LCT to Mothers	CCTs to Fathers	CCTs to Mothers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Attending school by end of year 2, among those 6-15 at baseline (Household sample)								
Boys	0.802	0.067	0.000	-0.011	-0.014	4713	0.279	0.821
	[0.399]	(0.021)***	(0.02)	(0.016)	(0.016)			
Girls	0.67	0.082	0.018	-0.03	-0.017	4522	0.01**	0.187
	[0.47]	(0.02)***	(0.02)	(0.018)	(0.018)			
Main school unit	0.75	0.065	0.01	-0.022	-0.026	5632	0.009***	0.971
	[0.433]	(0.019)***	(0.018)	(0.016)	(0.016)			
Satellite school unit	0.723	0.087	0.004	-0.016	-0.018	5442	0.082*	0.962
	[0.448]	(0.022)***	(0.018)	(0.016)	(0.016)			
Predicted probability of school participation below median †	0.614	0.107	0.009	-0.028	-0.027	5536	0.012**	0.728
	[0.487]	(0.023)***	(0.021)	(0.018)	(0.019)			
Predicted probability of school participation above median †	0.867	0.033	0.007	-0.006	-0.01	5538	0.152	0.903
	[0.34]	(0.016)**	(0.013)	(0.012)	(0.012)			
Panel B: Dropped out by end of year 2, among those enrolled in grades 1-4 at baseline (Household sample)								
Boys	0.083	-0.068	0.01	0.021	0.011	3231	0.083*	0.675
	[0.277]	(0.015)***	(0.011)	(0.01)**	(0.009)			
Girls	0.12	-0.091	-0.019	0.008	-0.004	2765	0.071*	0.033**
	[0.326]	(0.013)***	(0.01)*	(0.01)	(0.01)			
Main school unit	0.082	-0.067	0.002	0.02	0.013	3070	0.025**	0.618
	[0.275]	(0.014)***	(0.011)	(0.01)**	(0.009)			
Satellite school unit	0.117	-0.081	-0.021	0.008	-0.008	2928	0.209	0.038**
	[0.322]	(0.02)***	(0.013)	(0.012)	(0.012)			
Panel C: Attending school by end of year 2 if had dropped out at any time before baseline (Household sample)								
Boys	0.122	0.148	-0.013	0.077	0.041	449	0.188	0.476
	[0.33]	(0.092)	(0.088)	(0.086)	(0.082)			
Girls	0.161	0.12	0.008	-0.119	-0.08	815	0.001***	0.29
	[0.369]	(0.041)***	(0.054)	(0.043)***	(0.046)*			
Main school unit	0.173	0.118	-0.034	-0.066	-0.071	639	0.257	0.792
	[0.38]	(0.065)*	(0.069)	(0.064)	(0.065)			
Satellite school unit	0.121	0.153	0.014	-0.064	-0.046	625	0.11	0.581
	[0.327]	(0.053)***	(0.073)	(0.054)	(0.056)			
Panel D: Attendance rate during surprise school visits, among those enrolled (School sample)								
Boys	0.953	0.007	0.005	0.006	0.007	48616	0.384	0.587
	[0.211]	(0.01)	(0.009)	(0.008)	(0.008)			
Girls	0.958	0.006	-0.002	0.01	0.008	38078	0.021**	0.631
	[0.2]	(0.01)	(0.009)	(0.007)	(0.008)			
Main school unit	0.958	0.001	0.000	0.007	0.01	56262	0.122	0.744
	[0.201]	(0.011)	(0.011)	(0.009)	(0.009)			
Satellite school unit	0.951	0.021	0.001	0.006	0.003	30432	0.423	0.698
	[0.216]	(0.01)**	(0.009)	(0.007)	(0.008)			
Panel E: Minutes spent doing any type of schooling activity during the day before survey, children 6-15 at baseline (Household sample)								
Boys	164.93	165.91	-61.85	-49.85	-53.4	609	0.924	0.377
	[186.74]	(51.42)***	(38.98)	(49.7)	(52.13)			
Girls	114.31	-23.4	79.46	125.95	63.41	618	0.147	0.575
	[164.41]	(34.05)	(33.09)**	(36.96)***	(40.44)			
Main school unit	146.38	81.66	21.37	19.91	22.53	647	0.92	0.862
	[184.46]	(50.28)	(36.73)	(40.86)	(53.54)			
Satellite school unit	133.8	102.84	84.35	85.54	-19.8	580	0.958	0.342
	[169.65]	(30.34)***	(26.29)***	(34.56)**	(39.77)			

Notes: Panel A, B and C: same as Table 5 panel A. Panel D: same as Table 5 panel B. Panel E: same as Table 6.
***, **, * indicate significance at 1, 5 and 10%.
† Predicted probability computed using an OLS regression of endline enrollment on a set of baseline characteristics among the control group. See Table A5 in Annex.

Table 9. Mechanisms: Beliefs about Education

	Mean in Control Group	Effect of Tayssir (any type of treatment group)	N
	(1)	(2)	(3)
At least one parent from the HH is a member of the School Board, PTA or other School Association	0.042	0.009	4026
	[0.201]	(0.009)	
School quality index†	2.569	0.118	4250
	[0.67]	(0.043)***	
Parents expected returns to education:			
<u>*Overall returns:*</u> ***All Households***			
Increase in income for **girls** who complete primary school	-7.654	17.417	4417
	[181.436]	(7.06)**	
Additional increase in income for **girls** who complete junior high school	48.186	51.068	4383
	[325.395]	(16.869)***	
Increase in income for **boys** who complete primary school	91.043	30.141	4171
	[585.382]	(27.873)	
Additional increase in income for **boy**s who complete junior high school	198.72	62.985	3933
	[740.308]	(37.26)*	
<u>*Extensive margin: Probability ‡ of being employed, once adult, for…*</u>			
A **girl** who did not complete primary school	0.013	-0.003	4454
	[0.066]	(0.003)	
A **girl** who completed primary school	0.012	0.002	4454
	[0.063]	(0.003)	
A **girl** who completed junior high school	0.024	0.012	4435
	[0.098]	(0.006)**	
A **boy** who did not complete primary school	0.231	0.000	4423
	[0.188]	(0.008)	
A **boy** who completed primary school	0.244	-0.007	4389
	[0.185]	(0.008)	
A **boy** who completed junior high school	0.26	0.01	4317
	[0.21]	(0.01)	
<u>*Intensive margin: If employed, income in MAD, once adult, for…*</u>			
A **girl** who did not complete primary school	1177.552	-184.203	165
	[738.175]	(211.119)	
A **girl** who completed primary school	1101.088	-54.268	202
	[546.858]	(168.973)	
A **girl** who completed junior high school	1342.461	7.472	402
	[607.484]	(121.741)	
A **boy** who did not complete primary school	1285.782	-0.347	3192
	[581.403]	(37.579)	
A **boy** who completed primary school	1343.744	25.326	3248
	[608.022]	(37.561)	
A **boy** who completed junior high school	1507.117	60.186	3056
	[637.848]	(36.048)*	

Notes: Data source: Endline household survey. Sampling weights are used since households with dropout children were over-sampled. Unit of observation: Household. Column 1: Standard deviations presented in brackets. Columns 2: coefficients and Standard errors (in parentheses) from an OLS regression of the left-hand side variable on treatment dummy, controlling for strata dummies and variables below specified. Standard errors are clustered at the school-sector level. ***, **, * indicate significance at 1, 5 and 10%. All regressions include same household- and school-level controls as in Table 5 Panel A.
† Average across 3 school quality indicators. Coded so that 4 reflects highly satisfied, 3 satisfied, 2 disatisfied and 1 highly disatisfied. The 3 indicators are: infrastructure quality, headmaster availability and teacher quality.

‡ Respondents were not asked for a probability between 0 and 1. They were asked to choose between five categories (no chance, few chances, 50% chance, lots of chances, and certain chance). We impute probabilities of 0, 0.25, 0.5, 0.75 and 1 to these categories, respectively.

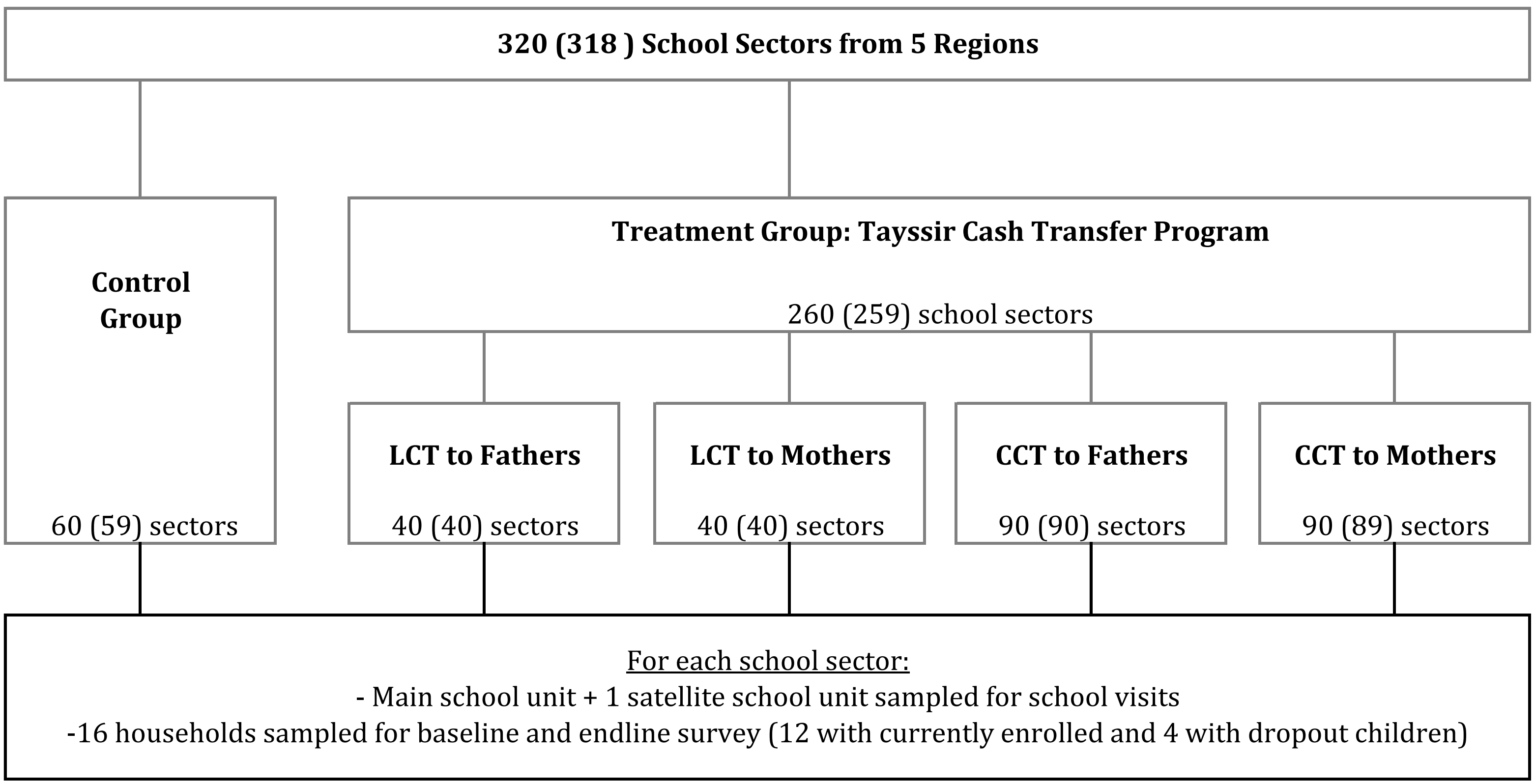
Table 10. Program Costs and Cost-effectiveness analysis

	TAYSSIR		PROGRESA/ OPORTUNIDADES	PRAF	RPS
	CCT	LCT	Mexico	Honduras	Nicaragua
Ratio administrative costs-transfers(a)					
Year 1	0.158	0.119	1.223	0.664	2.107
Year 2	0.11052	0.082	0.280	0.226	0.405
Year3	-	-	0.082	0.163	0.331
Year 4	-	-	0.049	-	-
Cumulative	0.133	0.100	0.106	0.325	0.489
Average	0.134	0.101	0.409	0.351	0.948
Household coverage by the end of the studied period	52,000		2,600,000	47,800	10,000
Cost-effectiveness (in 2008 USD) (b)					
Administrative cost per child per year	13	10			
Transfer cost per child per year	85	89			
Total cost per child per year	98	99			
Cost per extra year of education (in 2008 USD) (c)	4,043	4,228	7,300		
Extra years of education per USD 1000 spent	0.247	0.237			

Notes: (a) Sources: Tayssir: own calculations based on admin data and estimates provided by the program. Progresa, PRAF and RPS cost-transfer ratio: Cortes, Coady and Maluccio (2006), excluding impact evaluation costs. (b) Average per child per year over the 2-year pilot period of Tayssir. Source: own calculations. (c) Tayssir: Computed as present value of total cost divided by present value of extra years of education, over the 2-year studied period. Progresa: estimate from Coady (2000).

APPENDIX

Figure A1: Experimental Design



Notes: Sample size X (Y) indicates the initial (realized) sample size. The realized sample size is slightly smaller than the initial sample size due to 2 school sectors that couldn't be reached at baseline due to floods.
CCT stands for Conditional Cash Transfer. The condition was "no more than 4 absences in the month". LCT stands for Labeled (unconditional) Cash Transfer. See section 2.3 of paper for details on the amounts of the transfers.

Figure A2: Timeline

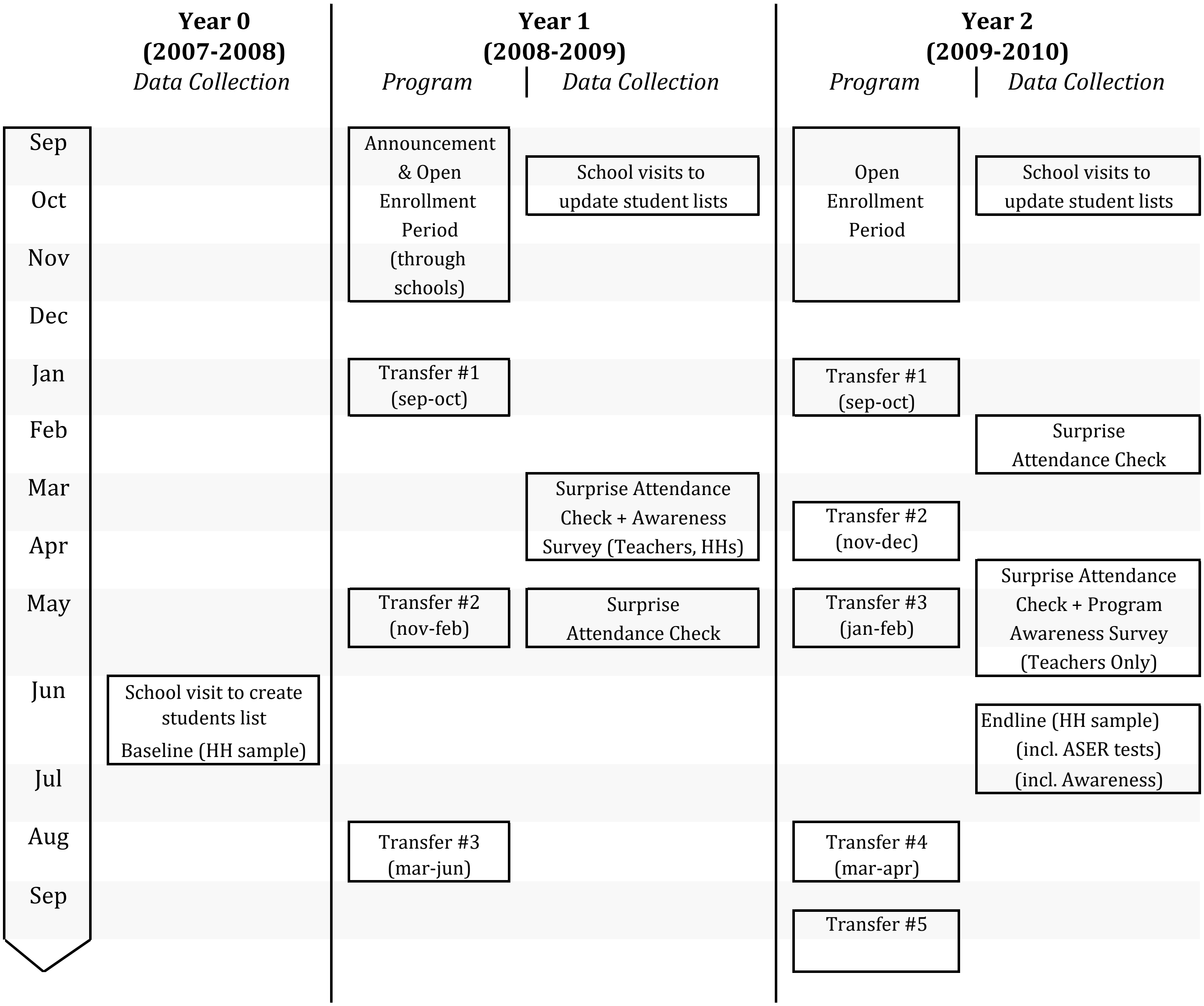


Table A1: Mincerian estimates of returns to education

	Mean education variable	OLS regressions Dep. Variable: baseline monthly consumption (in MAD)		
Male head of Household has at least some general education	0.1801	60.859		
		(10.537)***		
Female head of Household has at least some general education	0.0416	32.375		
		(17.508)*		
Number of other members who have at least some general education	0.4321	11.632		
		(4.078)***		
Male head of Household has completed primary school	0.0375		96.227	
			(28.676)***	
Female head of Household has completed primary school	0.0072		97.135	
			(46.689)**	
Number of other members who have completed primary school	0.1105		29.837	
			(8.771)***	
Male head of household can read and write	0.2382			54.340
				(8.504)***
Female head of household can read and write	0.0394			51.542
				(19.283)***
Number of other members who can read and write	0.4115			14.779
				(3.961)***
Number of Observation		4225	4225	4260
R-squared		0.07	0.06	0.07
Mean monthly consumption if all education variables are at zero		440.030	442.225	432.099

Notes: Data source: Baseline household survey. Unit of observation: Household. Sampling weights are used since households with dropout children were over-sampled. We exclude education at Koranic schools from general education.
Column 1: Mean explanatory variables. Columns 2-4: coefficients and Standard errors (in parentheses) from an OLS regression of the dependent variable on the education variables. Standard errors are clustered at the school-sector level. ***, **, * indicate significance at 1, 5 and 10%. Top 1% of consumption variable trimmed.

Table A2: Balance Check for School Sample

	Mean in Control Group	Impact of LCT to Fathers	Difference between [....] and LCT to Fathers			*N*	P-value for CCT different from LCT	P-value for Mother different from Father
			LCT to Mothers	CCTs to Fathers	CCTs to Mothers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>Panel A. Students Level Characteristics at Baseline</u>								
Age at baseline	9.742	0.12	-0.123	-0.064	-0.15	47255	0.295	0.026**
	[2.115]	(0.063)*	(0.071)*	(0.065)	(0.065)**			
Female	0.426	-0.003	0.019	0.013	0.02	47255	0.384	0.164
	[0.495]	(0.013)	(0.014)	(0.011)	(0.012)*			
Information on dropout by year 2 missing	0.081	-0.037	0.031	0.021	0.036	47255	0.204	0.074*
	[0.272]	(0.015)**	(0.016)*	(0.012)*	(0.015)**			
<u>Panel B. Attrition to other schools. (Dep. Var: Moved to another school by the end of year 2, among those enrolled in grades 1-4 at the end of year 0)</u>								
All	0.036	-0.001	0.007	-0.001	0.001	38753	0.353	0.337
	[0.186]	(0.005)	(0.007)	(0.005)	(0.005)			
Boys	0.037	-0.001	0.006	-0.005	-0.004	21642	0.108	0.508
	[0.188]	(0.007)	(0.008)	(0.006)	(0.006)			
Girls	0.035	-0.001	0.008	0.004	0.006	17111	0.845	0.322
	[0.183]	(0.006)	(0.007)	(0.005)	(0.005)			
Main school unit	0.039	-0.007	0.012	0.004	0.005	24989	0.671	0.257
	[0.194]	(0.007)	(0.008)	(0.006)	(0.006)			
Satellite school unit	0.03	0.007	0.000	-0.011	-0.006	13764	0.107	0.535
	[0.171]	(0.007)	(0.01)	(0.006)*	(0.007)			

Notes: Data source: School visits data. Unit of observation: Child.
Each row presents the results of a separate regression. Column 1: Standard deviations presented in brackets. Columns 2-5: coefficients and Standard errors (in parentheses) from a LPM regression of the left-hand side variable on treatment dummies, controling for strata dummies. Columns 7-9 show p-values testing that the outcome in each treatment arms are significantly different from those in the control group. Standard errors are clustered at the school-sector level. ***, **, * indicate significance at 1, 5 and 10%.

Table A3. Attrition in Household Sample

	Mean in Control Group	Impact of LCT to Fathers	Difference between [....] and LCT to Fathers: LCT to Mothers	Difference between [....] and LCT to Fathers: CCTs to Fathers	Difference between [....] and LCT to Fathers: CCTs to Mothers	*N*	P-value for CCT different from LCT	P-value for Mother different from Father
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>Panel A. Attrition from Household Sample</u>								
Not surveyed at baseline (as share of HHs sampled at baseline)	0.038	0.000	-0.004	0.007	-0.003	5032	0.81	0.58
	[0.192]	(0.026)	(0.028)	(0.024)	(0.025)			
Not surveyed at endline (as share of HHs surveyed at baseline)	0.123	-0.036	-0.007	0.005	-0.001	4832	0.653	0.609
	[0.328]	(0.02)*	(0.023)	(0.018)	(0.018)			
Reasons for attrition at endline:								
HH permanently migrated	0.044	-0.028	0.012	0.019	0.022	4832	0.038**	0.407
	[0.204]	(0.01)***	(0.013)	(0.009)**	(0.009)***			
HH temporarily migrated	0.028	-0.004	0.002	0.005	0.001	4832	0.728	0.675
	[0.164]	(0.008)	(0.008)	(0.007)	(0.007)			
Refusal	0.007	-0.002	0.003	-0.002	0.002	4832	0.588	0.208
	[0.083]	(0.005)	(0.005)	(0.004)	(0.004)			
HH merged with other study HH	0.004	0.002	0.002	-0.002	-0.003	4832	0.121	0.991
	[0.063]	(0.004)	(0.005)	(0.004)	(0.003)			
HH unknown	0.012	0.003	-0.008	-0.001	-0.006	4832	0.913	0.133
	[0.109]	(0.007)	(0.006)	(0.005)	(0.005)			
HH location could not be reach due to weather (e.g. flood)	0.025	-0.003	-0.02	-0.017	-0.019	4832	0.242	0.241
	[0.156]	(0.016)	(0.014)	(0.013)	(0.013)			
Other reason	0.003	-0.003	0.001	0.003	0.002	4832	0.227	0.793
	[0.057]	(0.002)	(0.002)	(0.002)	(0.002)			
<u>Panel B. Attrition from endline ASER Arithmetic Test</u>								
Total number of children tested	600	415	423	921	957	3316		
Not surveyed at endline (as share of HHs surveyed at baseline)†	0.305	-0.009	0.018	0.002	-0.03	4682	0.307	0.455
	[0.461]	(0.038)	(0.039)	(0.034)	(0.035)			
Reasons for attrition:								
HH not surveyed at endline	0.122	-0.036	-0.006	0.003	-0.001	4682	0.749	0.67
	[0.327]	(0.021)*	(0.023)	(0.018)	(0.018)			
Sampled child not at home on the day of the survey	0.127	0.037	-0.009	-0.017	-0.031	4682	0.262	0.469
	[0.333]	(0.028)	(0.032)	(0.026)	(0.025)			
Child or parents refused	0.029	-0.018	0.016	0.017	0.000	4682	0.896	0.178
	[0.169]	(0.008)**	(0.008)*	(0.007)**	(0.006)			
Child migrated	0.007	-0.006	0.006	0.007	0.005	4682	0.252	0.967
	[0.083]	(0.005)	(0.004)	(0.004)*	(0.004)			
Other reason	0.02	0.013	0.012	-0.008	-0.002	4682	0.19	0.233
	[0.14]	(0.012)	(0.014)	(0.013)	(0.011)			

Notes: Data source: Baseline and Endline household survey.
Column 1: Standard deviations presented in brackets. Columns 2-5: coefficients and Standard errors (in parentheses) from an OLS regression of the left-hand side variable on treatment dummies, controling for strata dummies. Standard errors are clustered at the school-sector level. ***, **, * indicate significance at 1, 5 and 10%.
† Only child aged 6-12 at baseline were surveyed, so we excluded the 150 households without any 6-12 child at baseline from the ASER sample

Table A4. ASER tests sample: Balance check

	Mean in Control Group	Impact of LCT to Fathers	Difference between [....] and LCT to Fathers			N	P-value for CCT different from LCT	P-value for Mother different from Father
			LCT to Mothers	CCTs to Fathers	CCTs to Mothers			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Balance check: HH Characteristics (ASER sample)								
Head of HH is Male	0.967	0.016	-0.007	-0.003	-0.012	3316	0.478	0.161
	[0.178]	(0.012)	(0.01)	(0.009)	(0.009)			
Age of Head of HH	45.728	-1.354	1.436	1.321	1.014	3272	0.258	0.591
	[9.295]	(0.62)**	(0.645)**	(0.54)**	(0.498)**			
# of HH members	6.815	-0.042	0.045	0.017	-0.032	3316	0.742	0.81
	[2.086]	(0.152)	(0.162)	(0.14)	(0.142)			
# of children (under 16)	3.251	0.045	-0.134	-0.063	-0.135	3316	0.596	0.116
	[1.274]	(0.102)	(0.105)	(0.096)	(0.094)			
% of children 6-15 enrolled in school at baseline	0.797	0.066	-0.024	-0.039	-0.015	3304	0.192	0.394
	[0.269]	(0.02)***	(0.02)	(0.018)**	(0.017)			
HH Head reads and writes	0.233	0.042	-0.053	-0.014	-0.035	3269	0.908	0.126
	[0.423]	(0.033)	(0.034)	(0.029)	(0.029)			
HH Head completed primary school	0.042	0.001	-0.012	-0.015	-0.009	3249	0.505	0.932
	[0.2]	(0.017)	(0.016)	(0.013)	(0.014)			
HH Head has at least some education	0.286	0.026	-0.04	0.006	-0.031	3258	0.726	0.06*
	[0.452]	(0.035)	(0.036)	(0.028)	(0.029)			
Per capita consumption (MAD)	466.714	15.091	-2.391	-9.724	-17.437	3296	0.445	0.633
	[276.734]	(25.759)	(30.988)	(23.022)	(23.509)			
Owns a cellphone	0.628	0.124	-0.074	-0.083	-0.09	3290	0.03**	0.202
	[0.484]	(0.038)***	(0.037)**	(0.028)***	(0.026)***			
Owns a television	0.715	0.033	-0.037	-0.024	-0.008	3289	0.919	0.983
	[0.452]	(0.051)	(0.049)	(0.039)	(0.038)			
Owns a radio	0.638	0.025	-0.038	-0.018	-0.069	3289	0.235	0.029**
	[0.481]	(0.036)	(0.035)	(0.027)	(0.03)**			
Main occupation: Farming	0.627	0.018	-0.01	0.024	-0.023	3233	0.847	0.181
	[0.484]	(0.049)	(0.052)	(0.043)	(0.044)			
Owns a bank account	0.032	0.016	0	-0.008	-0.011	3288	0.396	0.883
	[0.176]	(0.019)	(0.019)	(0.017)	(0.018)			
HH has electricity	0.548	0.056	-0.013	-0.054	0.03	3316	0.891	0.16
	[0.498]	(0.072)	(0.07)	(0.062)	(0.06)			
Panel B. Balance check: Children Characteristics (ASER sample)								
Age in 2008	9.454	0.033	-0.098	-0.033	-0.045	3316	0.889	0.577
	[1.701]	(0.124)	(0.131)	(0.101)	(0.106)			
Female	0.454	0.021	0.025	0.022	-0.002	3316	0.909	0.643
	[0.498]	(0.031)	(0.036)	(0.028)	(0.028)			
Enrolled in primary school in 2008	0.877	0.056	-0.035	-0.033	-0.016	3240	0.554	0.935
	[0.329]	(0.018)***	(0.019)*	(0.017)*	(0.015)			
Ever enrolled in primary school in 2008	0.928	0.043	-0.025	-0.005	-0.002	3241	0.311	0.5
	[0.259]	(0.014)***	(0.016)	(0.012)	(0.011)			

Notes: Data source: Baseline household survey. Column 1: Standard deviations presented in brackets. Columns 2-5: coefficients and Standard errors (in parentheses) from an OLS regression of the left-hand side variable on treatment dummies, controling for strata dummies. Standard errors are clustered at the school-sector level. ***, **, * indicate significance at 1, 5 and 10%.

Table A5: Determinants of school participation in the control group

	Attending school by the end of year 2
Individual characteristics	
Female	-0.130
	(0.012)***
Age at baseline	-0.024
	(0.004)***
Household characteristics	
Household head is a male	-0.067
	(0.043)
Age of Household head	-0.001
	(0.001)**
Age of Household head spouse	-0.001
	(0.000)
Household head speaks Amazygh	0.008
	(0.017)
Household head can read and write	0.019
	(0.010)*
Household head spouse can read and write	0.014
	(0.017)
Perceived School quality at baseline (index)†	0.019
	(0.009)**
Agreed to the statement: "Children are loosing their time in school"	-0.020
	(0.014)
Number of household members	0.008
	(0.003)***
Number of children in the household	-0.032
	(0.006)***
Number of female in the household	0.014
	(0.006)**
Number of rooms in the house	0.001
	(0.003)
House is mainly made of stone	-0.000
	(0.012)
Household owns a TV	0.013
	(0.013)
Household owns a cellphone	0.013
	(0.010)
Household owns agricultural land	-0.013
	(0.010)
Household owns a fridge	0.003
	(0.013)
Someone in the household has a bank account	0.001
	(0.024)
Household house has electricity	0.017
	(0.012)
Household monthly per capita consumption	0.003
	(0.001)**
School characteristics	
Satellite school	-0.001
	(0.012)
School in the village has electricity	0.017
	(0.012)
School in the village inaccessible during winter	-0.034
	(0.013)***
School in the village has toilet	-0.013
	(0.013)
Observations	9203
R-squared	0.09
Mean dependent variable	0.818

Notes: Data source: Household surveys. Sampling weights are used since households with dropout children were over-sampled. Unit of observation: Child.
The regression also control for strata dummies. Standard errors are clustered at the school-sector level. ***, **, * indicate significance at 1, 5 and 10%.
† Average across 3 school quality indicators. Coded so that 4 reflects highly satisfied, 3 satisfied, 2 disatisfied and 1 highly disatisfied. The 3 indicators are: infrastructure quality, headmaster availability and teacher quality.

CHAPTER 3

Estimating the Impact of Microcredit on Those Who Take It Up: Evidence from a Randomized Experiment in Morocco†

*By Bruno Crépon, Florencia Devoto, Esther Duflo, and William Parienté **

Abstract

We report results from a randomized evaluation of a microcredit program introduced in rural areas of Morocco in 2006. Thirteen percent of the households in treatment villages took a loan, and none in control villages did. Among households identified as more likely to borrow, microcredit access led to a significant rise in investment in assets used for self-employment activities, and an increase in profit, but also to a reduction in income from casual labor. Overall there was no gain in income or consumption. We find suggestive evidence that these results are mainly driven by effects on borrowers, rather than by externalities.

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† Go to http://dx.doi.org/10.1257/app.20130535 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

1. Introduction

Several recent randomized evaluations in different countries and contexts have found that granting communities access to microcredit has positive impacts on investment in self-employed activities, but no significant impact on overall consumption—or on overall income, when that is measured (Attanasio et al. 2011; Augsburg et al. 2013; Banerjee et al. 2013; Angelucci, Karlan, and Zinman 2013; Desai, Johnson, and Tarozzi 2013). A plausible interpretation of these findings is that the small businesses that the households gaining access to microcredit invest in have low marginal product of capital. Consistent with this hypothesis, these studies often find no significant impact of microcredit access on business profits or income from self-employment activities on average, although several do find an impact on profits for preexisting businesses or for businesses at the top end of the distribution of profits (Angelucci, Karlan, and Zinman 2013; Banerjee et al. 2013). Since the marginal business funded by a microfinance loan is often more likely to be female-operated, this interpretation (that the impact of microcredit on overall profits is low because it mainly funds unprofitable businesses) is also consistent with the cash-drop literature that finds that while the marginal productivity of capital appears to be large for male-run small businesses, it is much lower for those run by women (de Mel, McKenzie, and Woodruff 2008). One remaining question about this interpretation, however, is that while the impact on average self-employment profit is statistically insignificant in all existing studies, the point estimates are generally positive. Moreover, in most studies, the differential take-up of microcredit between treatment and control groups is generally low, either because interest in microcredit in treatment areas is low or because there is also some take-up in the control group (due either to leakage or entry of competitors into the control area). This implies that the insignificantly positive point estimates would translate into large (though still insignificant, obviously) instrumental estimates of the impact of microcredit (as opposed to microcredit access) on the average business profit. Could it be that the effect on those who take up microcredit is actually large, although perhaps imprecisely estimated? The studies where microcredit access is randomized at the area level, however, generally focus on reporting reduced-form estimates and do not use area-level access as an instrument for microcredit. There are good reasons to believe that microcredit availability impacts not only on clients, but also on nonclients through a variety of channels: equilibrium effects via changes in wages or in competition, impacts on behavior of the mere possibility to borrow in the future, etc. Thus, the exclusion restriction—that the instrument only affects the outcome through its impact on microcredit borrowing—is likely to be violated, and studies that randomize at the area level (rightly) avoid using area-level microcredit access as an instrument. On the other hand, in order to maximize power in the face of low demand, most of these studies use as the study sample a convenience sample, which surveys people who are eligible and likely to borrow based on observables (for example, demographic characteristics or prior expression of interest). The results are thus reduced-form estimates on a specific population. Furthermore, (with the exception of Desai, Johnson, and Tarozzi 2013) identification comes from increased microcredit access in treatment areas (rather than no access versus some access), and we are thus not capturing the effect driven by those who want microcredit the most (who may borrow both in control and treatment areas). In this paper, we present results from a randomized evaluation of microcredit in rural areas of Morocco. The study has three features that make it a good complement to existing papers. First, it takes place in an area where there is absolutely no other microcredit penetration, before or after the introduction of the product, and for the duration of the study. We are thus capturing the impact on the most interested households in villages (although those are still marginal villages for our partner, since they were chosen to be at the periphery of their planned zones of operation). Second, we designed and implemented a sampling strategy that would give us sufficient power to estimate the impact on borrowers, and also to capture impacts representative at the village level. Finally, we propose a strategy to test for externalities on nonborrowers, and to estimate direct effect on borrowers.

Existing strategies to estimate spillovers, which use two-step randomization (e.g. Crépon et al. 2013) are not feasible for this question, first because excluding a subset of potential clients once an office is open would be difficult, and second because part of the potential impact of microcredit on nonparticipants would only affect those eligible to be clients. We thus propose a simple strategy, based on the different probabilities to borrow found by the households that were surveyed, and build this strategy explicitly into the sample design. The evaluation was implemented in 162 villages, divided into 81 pairs of similar villages. The pairs were chosen at the periphery of the zone where Al Amana, our partner microfinance institution (MFI), was planning to start their operations. We randomly selected one village in each pair, and Al Amana started working in that village only. In a pilot phase, we collected extensive data on a sample of 1,300 house holds in 7 pairs of villages (7 treatment, 7 control), before introduction of microcredit. Several months after the program was introduced in the pilot villages, we estimated a model of credit demand in those villages and selected a small number of variables that were correlated with higher take-up. For all the remaining villages, before Al Amana started their operation, we conducted a short survey (which included the variables correlated to higher take-up) on 100 randomly selected households. We then calculated for each household a propensity score to borrow based on our model. We interviewed at baseline and endline (two years after rollout) all the households in the top quartile of the score (in treatment and control group), plus five households randomly selected from the rest of the village. In addition, at endline, we added a third group that had an even higher propensity to borrow, by reestimating the take-up equation in the whole sample, and using the initial census (available for all households) to construct a new score. In total, our sample includes 4,465 households at baseline, 92 percent of which were successfully interviewed at endline (an unusually low attrition rate), and 1,433 new households that were added at endline. Our sample thus has three categories of households classified ex ante in terms of their probability to borrow. We take advantage of the heterogeneity in the propensity to borrow in our sample to test the existence of potential externalities from borrowers to nonborrowers. We evaluate the effect of the treatment on households who have a high propensity to borrow and those who have a low probability to borrow. Finding no effect on low-propensity households would indicate the absence of externalities or other effects of microcredit availability on nonborrowers. Since low-propensity households come from both villages with low microcredit take-up (where almost everyone has a low propensity to borrow) and villages with higher take-up, our estimates on this specific population are likely to capture spillovers from borrowers and anticipation effect (impact from the mere fact that microcredit is available). For most outcomes we fail to reject that microcredit has no effect on the low-propensity sample. Motivated by this evidence, we use a treatment as an instrument for borrowing, the last step of our analysis. For consistency with the other papers on microcredit, we first report a complete set of reduced-form estimates on the households in the top quartile of ex ante propensity to borrow, as well as on households that were added at endline. Even in this sample, we find fairly low take-up of microcredit (17 percent in treatment and 0 in control). Households in treatment villages invest significantly more in self-employment activities, particularly agriculture and animal husbandry, which are dominant ones (74 percent of the sample engages in either of these activities). We find a significant increase in total self-employment profit, on average, but the effect appears to be very heterogeneous. In particular, the effect on profits is significantly positive at the higher quantiles of profitability (as in other studies) but significantly negative at the lower quantiles. The moderate increase in self-employment income is offset by a decrease in employment income, which comes from a drop in labor supplied outside the farm or household business. Overall, income increases (insignificantly) and consumption declines slightly (again, this is insignificant). Finally, similarly to other studies, we find a significant decline in nonessential expenditures (expenditures on festivals), but no change in any of the other "social outcomes" often meant to be affected by microcredit. We then present, for our key variables, estimates of the impact of making microcredit available in a village on the population as a whole. We do this by using our entire endline sample and applying the sampling probability in order to appropriately weight the observations. The bottom line is similar. Not surprisingly, take-up of microcredit is even smaller in this sample: 13 percent. Yet, the relatively small difference between the average household and one determined to be "high probability" underscores how difficult it is to predict who will take up microcredit. Correspondingly, the impact on most variables of interest is also smaller. However, even at the population level, we find that microcredit access significantly increases sales and expenditures in the business (however there is now a negative and insignificant effect on profits). We also find significant declines in labor supplied outside the home and salary income, and an insignificant decrease in consumption. As we mentioned, our test of externality fails to reject the hypothesis of no externality, on every variable considered individually except for two (labor supply outside the home and income). Of course, a caveat could be the lack of statistical power. We nevertheless move on to present an instrumental variable estimate of the impact of microcredit, using a dummy for being in a treatment village as an instrument for take-up. This essentially scales up our previous estimates, and gives us a sense of what the relatively modest reduced-form impact at the village level (or for likely borrowers) implies for those who actually borrow. On average, the point estimate suggests roughly a 50 percent increase in asset holding, a doubling of sales, and a more than doubling of profits. Labor outside the home declines by about 50 percent both in terms of earnings and hours supplied. Back-of-the-envelope calculations suggest that our profit estimates imply an average return to microcredit capital in terms of business profit of around 140 percent, not taking into account interest payments. Given this appealing figure, why aren't more people taking out loans? One possible reason is that, according to our estimates, the impacts of credit on profits are very heterogeneous. We present counterfactual distributions for profits among compliers based on Imbens and Rubin (1997): 25 percent of the compliers in the treatment groups have negative profits, while almost no one in the control group does. Given this risk level, it is plausible that individuals do not fully know what kind of returns to expect and are therefore hesitant to borrow. Another possibility is that profits do not capture welfare improvement. We observe no change in total income and consumption and a drop in hours worked outside the home. We do not observe a significant increase in labor supply in the household business, but the confidence interval does not rule out a relatively large increase, and it is plausible that labor in the business was not adequately measured, or that the hours spent taking care of a larger business are more stressful for the households. (Otherwise, it would suggest that the entire increase in total income due to microcredit is spent on leisure, which seems somewhat implausible given that households do not work very many hours to start with.) Overall, our study confirms the key finding from other research: even in an environment with very little access to credit, the aggregate impact of microcredit on the population at large is fairly limited, at least in the short term. This holds true even for those who are most predisposed to borrow. We can reject that household consumption increased by more than 10 percent monthly among those who take up a loan. But our study reveals that, at least in this context, these lackluster impacts appear to result from the combination of several offsetting factors. First, the take-up is low, even in these rural areas of Morocco where there is essentially no formal credit alternative. Second, among those who take up, there are proportionally large average impacts on self-employment investments, sales, and profits although there also appears to be great heterogeneity in these effects. Third, in the Moroccan context, those gains are offset by correspondingly large declines in employment income, stemming from substantial decline in labor supplied outside the household. Thus, some households choose to take advantage of microcredit to change, in pretty significant ways, the way their lives are organized. But even these borrowers do not appear to choose microcredit as a means to increase their standard of living, at least in the relatively short run.

2. Context and Evaluation

2.1 Al Amana's rural credit Program

With about 307,000 active clients and a portfolio of 1,944 million Moroccan dirhams or MAD (US$235 million) as of December 2012, Al Amana is the largest microfinance institution in Morocco. Since the start of its activities in 2000, Al Amana expanded from urban areas, into peri-urban and then to rural areas. Between 2006 and 2007, Al Amana opened around 60 new branches in nondensely populated areas. Each branch has a well-defined catchment area served by credit agents permanently assigned to the branch.[1]

The main product Al Amana offers in rural areas is a group liability loan. Groups are formed by three to four members who agree to mutually guarantee the reimbursement of their loans. Loan amounts range from 1,000 to 15,000 MAD (US$124 to US$1,855) per member. It can take 3 to 18 months to reimburse loans, through payments made weekly, twice a month, or monthly. For animal husbandry activities, a two-month grace period is granted. Interest rates on rural loans ranged between 12.5 percent and 14.5 percent at the time of the study (i.e. between 2006 and 2009).

To be eligible for a group liability loan, the applicant must be between 18 and 70 years old, hold a national ID card, have a residency certificate, and have been running an economic activity other than nonlivestock agriculture for at least 12 months. Unlike most MFIs worldwide, Al Amana does not restrict its loans to women exclusively, but it does generally require that credit agents have at least 35 percent of women among their clients. However, this requirement was first removed among the branches participating in the study and then among all branches. From March 2008, individual loans for housing and nonagricultural businesses were also introduced in rural areas. These loans were larger (up to 48,000 MAD, or about US$6,000), had an additional set of requirements, and were targeted at clients that could provide some sort of collateral. During our period of focus, households almost only took out group liability loans, so this study is primarily an evaluation of that product.

[1] A map is established and approved by Al Amana headquarters before the branch is opened, specifying the exact area, and therefore villages, that are eligible to be served by the branch. An intervention area can consist of one to six rural communities, and several villages belong to a community.

2.2 Experimental Design and Data collection

The design of our study tracked the expansion of Al Amana into nondensely populated areas between 2006 and 2007. Before each branch was opened, data was collected from at least six villages located on the periphery of the intervention areas— villages that could either have been included or excluded in the branch's catchment area. Villages that were close to a rural population center or along a route to other areas served by the branch were excluded, as this would have disrupted Al Amana's development. A very small number of villages where other MFIs were present (around 2 percent) were also excluded. Selected villages were then matched in pairs based on observable characteristics (number of households, accessibility to the center of the community, existing infrastructure, type of activities carried out by the households, type of agriculture activities). On average, two pairs per branch were kept for the evaluation. In each pair, one village was randomly assigned to treatment, and the other to control. In total, 81 pairs belonging to 47 branches were included in the evaluation.

Between 2006 and 2007, Al Amana opened new branches in six phases. These branches were opened throughout rural Morocco.[2] For the purposes of our evaluation, we divided this expansion into four periods, and conducted the baseline survey in four waves of field operations between April 2006 and December 2007. Our sampling strategy followed a novel approach to maximize the evaluation's power to detect both direct and population-level effects of microfinance access. Specifically, we selected two samples of households: one containing those with the highest probability to become clients of the microfinance institution and one containing a random selection of households from the rest of the population. Using the first sample increases the probability to detect an effect on those who are the most likely to become clients, if there is one. Using both samples together, with appropriate weights, allows us to measure the effect on the whole population of offering access to microfinance services.

To this end, in each of the 14 villages of the first wave, we sampled 100 households to whom we administered a full baseline survey. In villages of fewer than 100 households, we surveyed them all. This wave took place in April–May 2006, six months before the scheduled launch of

[2] Our sample is spread throughout rural areas of the entire country. Opened branches, 47 in total, are located in 27 provinces belonging to 11 regions (out of a total of 16 regions in the country) and cover all main dialects spoken in the country. Figure B1 in the online Appendix shows the spatial distribution of Al Amana branches participating of the study.

the second wave. We used data from this survey and administrative data on credit take-up in treatment villages over the first six months (reported weekly by credit agents) to estimate a model to predict the likeliness to borrow for each household. We present the result of this model in Appendix Table A1.

Based on this model, we designed a short survey instrument including the key variables predicting a higher likelihood to borrow.[3] For each of the subsequent waves, we started by administering this short survey to a random sample of 100 households in each village (or all the households if the village had fewer than 120). We entered survey data on computers on site, and an Excel macro selected the top quartile of households predicted to be the most likely to borrow on the basis of the model, as well as five additional households from the rest of the population. We administered the full baseline survey to this sample. The baseline survey included questions on assets, investment, and production in agriculture, animal husbandry, nonagricultural self-employment activities, labor supply of all household members (hours and sectors), as well as a detailed consumption survey. Since microcredit aims to have broad impacts on behavior and wellbeing, we also included questions on education, health, and women's decision-making power in the households.

After the baseline survey was completed in each wave, one treatment and one control village were randomly assigned within each pair. In treatment villages, credit agents started to promote microcredit and to provide loans immediately after the baseline survey.[4] They visited villages once a week and performed various promotional activities: door-to-door campaigns, meetings with current and potential clients, contact with village associations, cooperatives, and women's centers, etc.

[3] The variables collected in this short survey were the following: household size, number of members older than 18, number of self-employment activities, number of members with trading or services or handicraft as main activity, gets a pension, distance to souk (in km), does trading as self-employment activity, has a fiber mat, has a radio, owns land, rents land, does crop-sharing, number of olive and argan trees, bought agriculture productive assets over the past 12 months, uses sickle, uses rake (in agriculture), number of cows bought over the past 12 months, phone expenses over the past month (in MAD), clothes expenses over the past month (in MAD), had an outstanding formal loan over the past 12 months, would be ready to form a four-person group and guarantee a loan mutually, amount that would be able to reimburse monthly (in MAD), would take out a loan of 3,000 MAD to be repaid in nine monthly installments of 400 MAD.

[4] By the time of the baseline survey, branches were fully operational and were conducting business in the center of their catchment areas (within a 5 km radius of the branch location). Once the baseline survey was completed, credit agents started to cover the whole branch catchment area, with the only exception of control villages.

Two years after the start of each wave of the Al Amana intervention, we conducted an endline household survey, based on the same instrument, in the same 81 pairs of villages (May 2008–January 2010), and 4,465 households interviewed at baseline were sampled for endline.[5] Of them, 92 percent (4,118 households) were found and interviewed again. To maximize power, an additional 1,433 households (also predicted to have a high probability to borrow based on the credit model and the data from the short-form survey) were sampled at endline. To select these additional households, we reestimated the model to predict the likelihood to borrow for each household using administrative data on who borrowed by the time of the endline survey (i.e. over the two years of the evaluation time frame), matched with data collected with the short-form survey before the rollout of microcredit (and, hence, not affected by the rollout), updated the dependent variables including clients over the two-year period, and reestimated the coefficients of the model. This allowed us to much better identify likely borrowers.[6] Thus, the endline household survey was conducted, in total, with 5,551 households.[7]

2.3 Potential Threat to Experiment integrity

The experimental design was generally well respected, and we observe essentially no entry of Al Amana (or any other MFI, as it turns out) in the control group.[8] Villagers did not travel to other branches to get loans either.

[5] In wave 1 villages, we kept for the analysis 25 percent of households with a high probability to borrow, plus five households chosen randomly.

[6] Note that the sample is still selected using a linear combination of variables collected at baseline (the same in treatment and control villages) and is therefore not endogenous to the treatment.

[7] Out of the 5,551, to remove obvious outliers without risking cherry-picking, we trimmed 0.5 percent of observations using the following mechanical rule: for each of the main continuous variables of our analysis (total loan amount, Al Amana loan amount, other MFI loan amount, other formal loan amount, utility company loan amount, informal loan amounts, total assets, productive assets of each of the three self-employment activities, total production, production of each of the three self-employment activities, total expenses, expenses of each of the three self-employment activities, income from employment activities, and monthly household consumption), we computed the ratio of the value of the variable and the ninetieth percentile of the variable distribution. We then computed the maximum ratio over all the variables for each household and we trimmed 0.5 percent of households with the highest ratios. Analysis is thus conducted over 5,424 observations instead of the original 5,551, and no further trimming is done in the data.

[8] A few of the originally selected pairs of treatment and control villages were removed from the sample early on—before data collection—because it turned out that the treatment and control villages were served by another Al Amana branch. A few more were removed because Al Amana decided not to operate in their area at all. Implementation was done effectively and according to plan in the rest of the sample.

Attrition was not a major concern in the experiment since 92 percent of the households in the baseline were found at endline. (Attrition is slightly higher in the treatment group at 8.6 percent, compared to 6.8 percent in control; see Table 1, panel B.) Tables B3 and B4 in the online Appendix compare attrition in the treatment and the control groups, and examine the characteristics of the attritors compared to nonattritors. Table B3 focuses on attrition of the baseline sample, while Table B4 uses the short-form survey to examine attrition in the full endline sample (including households that were not included at baseline). Attritors belong to smaller households with younger household heads, and are less likely to have a self-employment activity. We then look at whether attritors' characteristics differ between the treatment and control groups (panel C of Tables B3 and B4). We find only two characteristics that differs for attritors in treatment villages (they are relatively more likely to run a self-employment activity and less likely to borrow from other formal institutions).

Next, we examine balance between treatment and control. Table 1 provides means in the control group and the treatment-control difference for the variables collected in the baseline survey of 4,465 households. In Table B1 and B2, we reproduce the same analysis for the whole sample of 5,898 households and for the 4,934 households with high probability to borrow.

Unfortunately, there are some differences between the treatment and control groups, more than would be expected by pure chance (although we know that the randomization was well done, since it was carried out in our office, by computer). Jointly, these baseline characteristics are different in the treatment and control groups. At baseline, households in treatment villages had, on average, a slightly larger access to financial services, but not larger loans. They had higher probability to be engaged in livestock activity in treatment villages, and, hence, larger assets, and lower probability to run a nonfarm business. As a result of these imbalances, we include individual-level control variables in our analysis, and present a robustness check without such control variables in the Appendix. Our results are not sensitive to control variables.

3. Reduced-Form Results

For consistency with the other papers in the literature, we first report a set of reduced-from results on the sample of likely borrowers (the top quartile of households selected to be most likely to borrow). We then turn to population-level estimates, and estimates of the impact of the treatment on the treated.

3.1 Specification

We estimate the following reduced-form specification:

$$(1) \qquad y_{pij} = \alpha + \beta\, \mathbf{T}_{pi} + \mathbf{X}_{pij}\, \delta + \Sigma_{m=1}^{p} \gamma_m\, \mathbf{1}\,(p = m) + \omega_{ij}\,,$$

where p denotes the village pair, i the village, and j the household. $\boldsymbol{T}_{pi}$ is a dummy for the introduction of microcredit in village i, and y_{pij} is an outcome for household j in village i in pair p. $\mathbf{X}_{pij}$ is a vector of control variables.[9] The regression includes the 81 pair dummies represented by $\Sigma_{m=1}^{p} \gamma_m\, \mathbf{1}\,(p = m)$. Standard errors are clustered at the village level.

Equation (1) is estimated on two different samples. The first is the sample of households more likely to become clients of the microfinance institution (see Section 3.2). In Section 4.1, we also present estimation results obtained using the whole sample, using sampling weights to obtain results representative of the whole village population. As we evaluate the effect of microcredit on a large number of outcomes, we account for multiple hypothesis testing. Each table of results we present focuses on a specific family of outcomes for which we produce (in the last column) an index (which is the average of the z-scores of each outcome within the family). Furthermore, we report both the standard p-value and the p-value adjusted for multiple hypotheses testing across all the indexes.[10] For a reduced set of outcome variables (and still for the sample of likely borrowers), we also consider the corresponding quantile regressions. To perform the regression, we follow Chamberlain (1994) and simply compute the desired quantiles of the considered outcome variable in each village and then implement minimum distance estimation, explaining the different estimated quantiles by the treatment variable and pair dummy variables. We consider quantiles 10, 25, 50, 75, and 90 percent.

[9] The basic set of covariates for most of our regression includes the number of household members, number of adults, head age, does animal husbandry, does other nonagricultural activity, had an outstanding loan over the past 12 months, household spouse responded the survey, and other household member (excluding the head) responded the survey. Since part of the sample includes households that were only included at endline, we do not have baseline information for them. In regressions, we enter a dummy variable identifying them and set to zero the other covariates. We present in online Appendix Table B7 regression results in which no covariates are introduced and a table in which an extended set is considered.

[10] We adjust p-values following Hochberg (1988) in order to control the familywise error rate (FWER).

3.2 Access to credit

Table 2 presents the results on credit access and borrowing. As in previous studies (Banerjee et al. 2013; Karlan and Zinman 2010), we find that households tend to underreport borrowing: administrative data suggest that 17 percent of households in this sample borrow in the treatment villages (and none in the control villages), while in survey data only 11 percent of households admit to borrowing.

The administrative data is more reliable in this context, and this is what we will use for the first stage in our instrumental variable regressions below. Access to any other form of formal credit is very limited. In the control villages, 2 percent of households report borrowing from another MFI, 2 percent from another bank, and 2 percent from any other formal source. Only 6 percent report borrowing from informal sources though this may be underestimated to the extent that households do not like to admit to borrowing (as it is frowned upon by Islam), or to the extent that informal loans between villagers are recorded as gifts. The only common source of loans is the utility companies: 16 percent of households in control villages borrow from a utility company to finance their electricity or water and sanitation installation. The pattern is very similar in treatment villages, except that households report 1pp more borrowing from other formal sources (there may be some confusion between these other sources and Al Amana, partially accounting for the underreporting of Al Amana loans). Therefore, microfinance was introduced by Al Amana in our treatment villages in a context where households had very limited alternative access to finance. This is a unique feature that sets our study apart from most other impact evaluations of access to microfinance.

Turning to loan amounts, households in treatment villages report additional outstanding loans of 795 MAD (US$96), on average, from Al Amana over the 12 months prior the survey.[11] There are also small but significant increases in reported amounts borrowed from both other formal credit sources and the utility companies, as well as a small insignificant substitution with informal loans, which might be related to confusion between various types of loans, as previously mentioned. In total, average outstanding loan amount increases by 1,206 MAD and

[11] Average outstanding loans of 975 MAD (795 + 180) represent 2.7 percent of average household annual consumption in the control group. If we consider loan amounts declared by actual borrowers in our survey, this share increases to 24 percent of annual consumption.

repayment per month increases by 33 MAD, as reported by households in treatment villages. Online Appendix Table B5 uses administrative data to provide some characteristics of the loans disbursed by Al Amana in treatment villages. According to this administrative data, clients in treatment villages borrowed, on average, 10,571 MAD. This compares to outstanding loan amounts of 8,863 MAD as declared in our survey data.[12] Thus, households underreport borrowing both on the extensive and the intensive margins. In terms of other loan characteristics, clients most often form groups of four people who act as mutual guarantors and reimburse their loans in 12 or 18 monthly installments. The average client household took up a loan 5.7 months after microcredit was made available in the village and 50 percent of them took a second loan by the end of the two-year evaluation timeframe. Most of loans were taken within the first six months (67.9 percent). When applying for microcredit, most of clients (68 percent) declared to be planning to use the loan in animal husbandry activities, mainly cattle and sheep rising, 26.4 percent in trade-related businesses, and the remaining 5.5 percent in other nonagricultural businesses, such as services and handicraft. It is not surprising that no client declared an intent to allocate loans to other agricultural activities (crops and fruit trees), as Al Amana did not lend for such activities.

3.3 Income Levels and Composition, and Labor Allocation

Table 3 shows the impact of the introduction of microcredit on self-employment activities. Eighty-three percent of the households in the control group have some form of self-employment activity—the dominant forms being animal husbandry and agriculture—whereas only 14.7 percent of households have a nonfarm business (see online Appendix Table B6).

The results of Table 3 suggest that the introduction of microcredit leads to a significant expansion of the existing self-employment activities in agriculture and animal husbandry, but does not help start new activities. We even find a small nonsignificant reduction in self-employment of 1.5 percentage points for the households in treated villages.

Access to microfinance has a positive effect on assets: the estimated impact is 1,448 MAD. We do not find any effect of microcredit on investments over the last 12 months, probably because most additional investments caused by the new access to microfinance took place in

[12] This amount can be directly deduced from information in Table 2 as (795 + 180)/(0.09 + 0.02) = 8863.

the first year of the intervention (since most loans were disbursed in the first 6 months), thus more than 12 months before the endline.

Figure 1 shows that quantile treatment effects on asset accumulation are positive at almost all quantiles. Assets of self-employment activities mainly consist of animals (cows or goats) owned by the households. Additional results reported in Table B6 show that the impact on the stock of assets mainly comes from livestock activities. This building up of assets could correspond to business investment strategy (the assets representing unrealized profits), or to a self-insurance mechanism (the assets are in-kind savings), or to a combination of the two.

One other important result in Table 3 is that, summed across all types of activities, there is a significant expansion in self-employment activities (which comes from existing activity since there is no impact on the extensive margin): revenues, expenditures, and profit all significantly increase. Profit, defined as the difference between revenues and expenses, increases by 2,005 MAD, a substantial amount compared to the average profit in the control group, 9,056 MAD. Figure 1 presents the results of quantile regressions. It shows that quantile treatment effects are significantly negative for the lowest quantile (0.10), nonsignificant at the median, and significantly positive for the quantiles 75 and 90. The finding that the increase in self-employment activity is concentrated at the highest quartile echoes Banerjee et al. (2013) and Angelucci, Karlan, and Zinman (2013). Negative profits at the low end of the distribution might be partially due to long-term investments misclassified as current expenses. These quantile treatment effects are only reduced forms: they do not necessarily mean that the impact of getting credit itself has the same heterogeneity (since there may be externalities, and we do not know where the compliers lie in the distribution of outcomes). We return to this question in Section 4.3.

Table 4 shows the impact of microcredit on different sources of income. The major result in this table is that the increase in self-employment profit is offset by a significant decrease in employment income.

Note that, despite the fact that 83 percent of households have a self-employment activity, employment income accounts for as much as 56.9 percent of household income while self-employment activities account for only 32.7 percent. Most (90 percent) of employment income comes from casual (day) labor and very little from stable salaried work (10 percent). The effect of access to microfinance is quite substantial, −1,050 MAD, a reduction of 6.7 percent compared to the control group mean. As a result of the reduction in wage earnings, the net increase of employment and self-employment income taken together is small and insignificant. Thus, it appears that, in this context, microfinance access leads to a change in the mix of activities, but no income growth overall.

Table 5 reports on the effect of the introduction of microcredit on the time worked by household members aged 6 to 65 over the past 7 days, for various age ranges.

Column 1 shows that there is an insignificant reduction in the total amount of hours of labor supplied, and columns 2–4 show there is substitution between the different types of activities. Considering all members together, we find a significant reduction in work outside the home of 2.8 hours, or 8.3 percent of the control group mean. Time spent on self-employment activities increases, but not significantly so. Overall, hours of work decline in every age group, although the reduction is significant only for the youth (16 to 20) and the elderly (51 to 65).

The reduction in labor supplied outside the home is consistent with the results on employment income (Table 4). The relatively small increase in time spent on self-employment activities despite increased investment may be due to the fact that investments in agriculture and animal husbandry may not need to be coupled with a proportional increase in labor input. Still, this is a remarkable fact: the average quantity of labor (24 hours per week) supplied per adult household member seems relatively low, suggesting that members may have the opportunity to increase their efforts by a large margin (provided that we measure time allocation correctly). This would suggest that households take the opportunity of access to credit to invest in less labor-intensive occupations and increase their leisure time.

3.4 Consumption

Table 6 reports the estimated effects of the introduction of microcredit on household consumption (expenditure and consumption of home production are both included). The table shows the effect on total consumption at the household level (column 1), and by type of consumption expenditures: durables, nondurables, food, health, etc. (columns 2 to 8).

Consistent with the lack of effect of overall income, we find a small, negative, and insignificant point estimate on consumption (46 MAD per month). This absence of effect on consumption is confirmed by quantile treatment effect presented in Figure 1, which shows no effect at any quantile.

Turning to the composition of consumption, we do not find the increase in durable consumption that other papers have reported, but this may be due to the fact that the survey was administered more than 12 months after most people got the loans. Consistent with all the other papers, we find a statistically significant reduction in nonessential expenditures (in this case, festivals, rather than other temptation goods).

3.5 Education and Female Empowerment

The impact of microfinance is supposed to go beyond the expansion of business activity and consumption levels. Indirect effects, such as the empowerment of women and improvements in the health status and education levels of children, are often considered potential impacts of microfinance.

We did not see any shift in the composition of household consumption that would support this hypothesis. Table 7 looks at other “empowerment” outcomes, namely, education and female empowerment. We find no impact on education, despite the reduction in outside labor among teenagers (other randomized controlled trials have found different effects, some finding positive and others negative impacts).

Since the majority of borrowers of our sample are men, the expected effect on female empowerment is less clear cut than for standard microfinance programs, which tend to focus on women. Nevertheless, we do examine the impacts on female empowerment using several proxies. The first is the number of income-generating activities managed by a female household member (column 5). In remote rural areas, such activities are usually managed by male members (1.5 activities, on average, compared to 0.39 for women). We also use a series of qualitative indicators to describe female empowerment such as the capacity of women to make decisions, and their mobility inside and outside the villages. We construct a summary index of these qualitative variables (column 3) as they are part of the same “family” of outcomes. We find no evidence of the effect of microfinance on any of these variables or on the index.

These results are in line with the fact that only a small proportion of women borrow in remote rural areas and that additional borrowing for men is unlikely to change the bargaining power of women within the household. They are also consistent with the results from all the other microfinance evaluations except for Angelucci, Karlan, and Zinman (2013), which find improvements in female empowerment in Mexico.

4. Estimation of Externalities and Instrumental Variable Estimates

Section 3 presented reduced-form estimates of the impacts of access to microcredit on the specific population of households that were ex ante the most likely to become clients of Al Amana. We were also interested in two other questions: measuring impacts on the population as a whole, and disentangling direct effects on those who choose to borrow from indirect effects on others, such as general equilibrium effects due to changes in prices, or changes in behavior stemming from the possibility to borrow in the future. We now exploit our experimental design to get at both questions.

4.1 Impact of Access to Microcredit over the Whole Population of Selected Villages

Measuring the impact of access to credit on the village population is straightforward given our design: we just reestimate the same set of regressions, but using the whole sample, and weighting appropriately using the sampling weights, so that the estimates are now representative at the village level. Those results are of course representative of the marginal villages selected to be in our experiment (and not of the entire catchment area of Al Amana branch).

Table 8 presents the results for some key outcome variables. Panel A simply reproduces the results presented in Section 3 for the population of households likely to become clients of Al Amana (those who were in the top quartile of the propensity score). Panel B presents intention-to-treat estimates on the same outcomes but over the whole population selected for the endline survey (the households in the top quartile plus the five randomly selected), weighted by the inverse of the probability to be selected in that population.

Not surprisingly, take-up of microcredit is even smaller in this sample (13 per-cent), although the relatively small (though statistically significant) difference with the "high-probability" sample underscores how difficult it is to predict who will take up microcredit. Correspondingly, the impact on most variables of interest is also smaller. However, even at the population level, still we find that microcredit access significantly increases sales and expenditures in the business. We also find significant declines in labor supplied outside the home and in salary income, and an insignificant decrease in consumption. There is now a negative and insignificant impact on profits: combined with the estimate on likely borrowers and the quantile regressions, which did show significant negative treatment effects at the lowest quantiles, this suggests that those who are least likely to borrow are those with the most negative treatment effect on profit.

4.2 Externalities

Prima facie, results in the previous section are not suggestive of strong externalities. We evaluate the effect of the treatment on the samples of households with high and low propensity to borrow. Finding no effect on the households who are predicted not to borrow is an indication that the no effect on nonborrowers (in the form of externalities and anticipation effects). In practice, we estimate the treatment effect separately for those with the highest 30 percent and lowest 30 percent probability to borrow, and omit the middle group.

To implement this test, we first reestimate the propensity to borrow based on actual endline behavior. By using actual borrowing behavior as measured by the endline survey, instead of using the model based on only pilot phase 1, we increase the predictive power of the model. This is done by estimating a logit regression for the decision to become a client of Al Amana, using the set of baseline variables obtained from the initial short survey (which we collected at baseline well before the intervention took place, and which we have for the entire population) and village dummies. This model is estimated on the whole set of households in treatment villages that were interviewed at endline. The results are presented in online Appendix Table B8. Several characteristics are individually significant in the regression, and they are also strongly significant taken together. The predicted probability to borrow ranges from almost 0 to 0.80. It has an interquartile range of 20 percentage points, and a 37 percentage point difference between quantiles of order 90 percent and 10 percent. This allows us to identify reasonably well the heterogeneity related to the propensity to borrow.

Panel C of Table 8 presents estimation results of the main equation with the two interaction terms (high and low propensity sample).[13] Column 1 presents the results on the probability to borrow. Households in the high probability sample are 36 percentage points more likely to have taken a loan from Al Amana than their control counterparts. In the low-probability sample, the difference between treatment and control households is statistically different than 0 but very small (less than 2 percentage points). A caveat of our analysis is that a significant part of the low-probability sample comes from villages where there is very little or no access to credit. Thus, the estimates on the low-probability sample capture the effect of credit availability in areas where microcredit was offered but where there is no demand and a combination of credit availability and spillover (from borrowers to non borrowers) effects in villages where some households took loans.

Columns 2 to 9 present the results for the key outcome variables individually. For most outcomes, estimated values for the coefficient associated to the interaction between treatment and the low-probability sample are insignificant and generally fairly small.

An interesting exception to the finding that externalities do not seem to be important arises from the variables on time worked by households outside the home and the income derived from it: there we see highly significant negative impacts on hours worked outside even among low-probability households. This is surprising, as prima facie we might have expected the externalities to run in the other direction (if those who borrow free up opportunities, leading to more jobs or increases in wages). It could be that the ability to borrow (and thus to smooth out shocks if needed) reduces the need for income diversification.

[13] This equation is run without weights, to leverage to the maximum extent the power given to us by our design, which made sure we had enough people in the sample with relatively high probability to borrow. Under the null, OLS is BLUE and the regressions should not be weighted. With weights, we still reject the hypothesis of no externalities, but the results are noisier.

4.3 Local Average Treatment Effect

Motivated by the finding that externalities (except for labor supply) do not seem to be very important, we present suggestive estimates of the impact of microcredit take-up on outcomes, using a dummy for residing in a treatment village as an instrument for borrowing. This amounts to rescaling the reduced-form estimates by dividing them by 0.17. Given how noisy the evidence on externality is, this is at best tentative; still, it is useful to get an order of magnitude of what the reduced-form evidence would entail.

The equation we estimate is

$$y_{pij} = a + b\,\mathbf{C}_{pij} + \mathbf{X}_{pij}\,c + \Sigma_{m=1}^{p}\gamma_m\,\mathbf{1}\,(\boldsymbol{p} = \boldsymbol{m}) + u_{ij}, \qquad (2)$$

where $\mathbf{C}_{pij}$ is a dummy variable corresponding to being a client of Al Amana. This equation is estimated using the treatment village dummy variable as an instrumental variable for $\mathbf{C}_{pij}$, and for comparison by OLS. The IV strategy is valid only if the assumption of no externalities is correct.

Table 9, panel B reports the IV estimates for the main outcome variables selected in Table 8. We present the means for compliers at the bottom of the table, as well as the control group means.[14]

The IV estimates imply that, if the entire effect can indeed be attributed to borrowers, the changes induced by Al Amana are large for those who do take up, although the orders of magnitude remain plausible. Assets (column 1) increase by 64 percent, and production (column 2) increases by 153 percent compared to the compliers' mean. Similarly, expenses increase by 147 percent (column 3) and profits by 168 percent (column 4). The reduction in weekly hours worked in employment activities and the derived income (columns 7 and 5) are also sizable, and both represent a substantial share of compliers' mean (wage earnings decrease from 18,530 MAD to 12,249 MAD; hours of work decrease from 42 to 24 hours per week). If we assume that the impact on profits is entirely driven by borrowers, this suggests large average returns to microcredit loans. In Table 3, we found that impact of the treatment dummy on profits is

[14] The complier mean in the control group is calculated as $E(y(0) \mid c) = [E(y \mid Z = 0) - E(y \mid Z = 1, T = 0) \times (1 - P(T = 1))] / P(T = 1)$, where Z indicates treatment assignment, T indicates being a microcredit client and $P(T = 1)$ the proportion of clients in $Z = 1$.

2,005 MAD for the second year of the experiment (the profits are measured over the previous year). During that year, the average amount borrowed in the treatment group was 834 MAD (with an average maturity of 16 months).[15] If we do not value any increase in hours worked, this suggests an average financial return to microcredit capital of 2.4, well above the microcredit interest rate. While this number is large, it is in line with prior estimates based on capital drop (de Mel, McKenzie, and Woodruff 2008), or for credit to larger firms (Banerjee et al. 2013).

The impacts on consumption are small and relatively precise: we can reject with 95 percent confidence that microcredit take-up increases consumption by more than 10 percent.

To assess the extent of heterogeneity in the treatment effect, we first estimate, under the maintained assumptions of no externality, the cumulative distribution of potential outcomes (with and without treatment) for the compliers. The distribution F_1 of potential outcome when benefiting from the treatment is simply the cumulative distribution over the clients. Following (Imbens and Rubin 1997), the counterfactual cumulative distribution F_0 of potential outcome, when not benefiting from the treatment for the compliers/clients, is given by[16]

$$F_0 (y \mid C) = (F(y \mid T = 0) - F(y \mid T = 1, C = 0)(1 - P(C))/P(C).$$

Figure 2 presents the results.[17] There are some interesting findings. First, while the distribution among compliers in the treatment group stochastically dominates that in the control for asset accumulation, and there is visibly no impact on consumption, the two curves are clearly

[15] This figure is the product of 9,500 MAD borrowed by people who borrowed, multiplied by 16.7 percent (the share of clients) and by 52.5 percent (the share of clients who are borrowing in the second year). See Table B5 in the online Appendix, where we estimate these figures on a subsample of clients who could be matched into the Al Amana administrative database.

[16] We estimate the underlying cumulative distribution functions as step function with a large number of small intervals. Although the corresponding estimated function is asymptotically positive and increasing, a problem documented by (Imbens and Rubin 1997) is that the estimated function can fail to be either positive or increasing, and they propose a method to constrain the CDF to be nonnegative and increasing. Following them, we start the estimation procedure with the first interval by applying the formula for unconstrained estimation and retaining either the estimated value if is positive, or zero otherwise. We then estimate the CDF recursively for all the other intervals by applying for each interval the formula for unconstrained estimation and retaining either the estimated value if greater than or equal to the estimated value in the preceding interval, or else the estimated value in the preceding interval. Finally, we rescale all estimates so that the cumulative distribution function reaches 1 on the last interval.

[17] Note that we do not present confidence intervals, which would likely be wide, given that the first stage is not very large.

different for profits: in the treatment groups, compliers have both more instances of low (negative) profits and high profits. Indeed, among the compliers in the control group, it seems that very few people have negative profit (the estimated CDF is very close to 0), while about 25 percent of compliers in the treatment group have negative profits. The two curves cross for a value of profits roughly equal to zero. On the other hand, the compliers with the top 40 percent of profits have higher profits in the treatment groups than in the control group.

Turning to income from employment activities, Figure 2 shows that impact of being a client of Al Amana also appears to be far from homogeneous on the population of compliers. As can be seen on the graph, there is no effect above the quantile of order 60 percent; all effects are concentrated at the bottom of the distribution. In particular, 45 percent of the compliers who are clients do not supply any labor outside their own activity, compared to only 30 percent for the nonclients. Similarly, a higher proportion of compliers rely less on day labor income in the treatment than in the control for low values (below 15,000 MAD) of the variables. This suggests that the negative impact of credit on work supplied outside the home is driven primarily by households that do not rely heavily on casual labor in the first place.

Last, Table 9, panel A, presents the results of the OLS control variable regression estimates obtained from a regression of our key outcomes on a dummy variable for being a client of Al Amana on the subsample of households in treatment villages. The differences of these estimates with the LATE estimates are sizable both in magnitude and sign. This underscores the problems associated with identification of causal effect of microcredit.

4.4 Robustness checks

In this section, we briefly report on robustness checks. We experimented with changes in the list of control variables and different ways to compute standard errors. Results are presented in online Appendix Table B7. The first panel considers simple regressions just including the set of strata dummy variables, and the second panel reproduces our previous results, including a set of control variables listed in Table 2. This panel also provides standard errors computed assuming clustered residuals, as well as standard errors without clusters. The last panel provides results obtained by adding to the previous set of control variables an extended set involving, among others, the dependent variable at baseline, as well as other variables listed in the footnote of Table B7. As can be seen from the table, results are very robust. We obtain the same order of magnitude for all estimated coefficients, as well as for standard errors. Expanding the list of control variables does not lead to any gain in precision. Finally, the clustered and unclustered errors in panel B are quite similar, suggesting that, in this case, clustering did not have a large impact on our standard errors.

5. Conclusion

In this paper, we measure the impact of access to microfinance in remote rural areas in Morocco, where during the span of the intervention there was no access to credit outside that provided by our partner, Al Amana.

We identified pairs of villages at the periphery of the catchment area of new branches, and randomly selected one village in each pair for treatment. We surveyed both households that were identified ex ante as having relatively higher probability to borrow, as well as randomly selected households in the village: the objective of this sampling strategy was to be able to estimate both direct impact and possible externalities on non borrowers.

On average, take-up of microfinance is only 13 percent in the population and 17 percent in our "higher probability" sample (and 0 in the control group). Consistent with other evaluations of microfinance programs, we find that households that have access to microcredit expand their self-employment activity (primarily agriculture or animal husbandry, in this context), and their profits increase. Our estimates seem to suggest that these effects are driven by those who actually borrow, implying that the modest reduced-form estimates actually come from fairly large average impacts (we estimate average returns to capital of close to 140 percent before repayment of interest) combined with a low take-up.

This presents a puzzle: if the returns are really that high, why are people not borrowing in larger numbers? And why are half of the clients apparently dropping out after a year? We see two plausible explanations. The first is that although microfinance is associated with large average increases in profits, the utility gain may not be as large as these estimate suggest: running one's own business may be stressful (as Karlan and Zinman 2010 find in the Philippines). We may also not capture increase in labor in the household's own business, which may be difficult for survey respondents to remember.

The second possible explanation is the substantial heterogeneity in how profitable microfinance investments are. Although noisy, both the reduced-form quantile regressions and the IV estimates of the changes in the distribution of profit for the outcomes suggest that for a substantial minority of households (about 25 percent of those who take up microcredit), the impact on profit may actually be negative. This large dispersion may explain the fairly low take-up of microfinance: households may recognize the unpredictable rate of return, and be risk averse.

Another key finding is that despite significant increase in self-employment income (at least among the population that is most likely to borrow), we see no net impact of microcredit access on total labor income or on consumption. This result is similar to what other evaluations of microcredit programs find. In our context, this appears to be driven by a loss in income from wage labor, which is large enough to offset the gain in self-employment income, and is directly related to a substantial decline in labor supply outside the home by those who take up microcredit. What is surprising is that this does not appear to be driven by time constraints: the increase in labor supply on self-employment activities is small and insignificant, although the confidence intervals does not allow us to rule out an increase in hours spent.

There are two plausible channels for this set of results. The first is that access to microcredit allows households to invest in agriculture and animal husbandry and increase their profit. Leisure being a normal good, the income effect leads them to reduce their labor supplied, particularly outside the home. Anecdotal evidence suggests that there is a strong disutility associated with day labor, giving credence to this explanation. A second possible channel is that our results reflect a shift in the way households cope with risk. Access to credit enables households to purchase lumpy assets, such as livestock, which are typically used for self-insurance (Deaton 1991; Rosenzweig and Wolpin 1993). This increased form of insurance can be a substitute of other ex ante risk-management strategies such as income diversification through day labor, which are also taking place in the absence of formal insurance markets (Kochar 1999; Rose 2001). Regardless, microcredit appears to be a powerful financial instrument for the poor, but not one that fuels an exit from poverty through better self-employment investment, at least in the medium run (two years after the introduction of the program). We are currently following up with the households, now that a much longer time period has elapsed, to check if the investment in business assets paid off in the longer run.

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Figure 1. Quantile regressions (ITT)

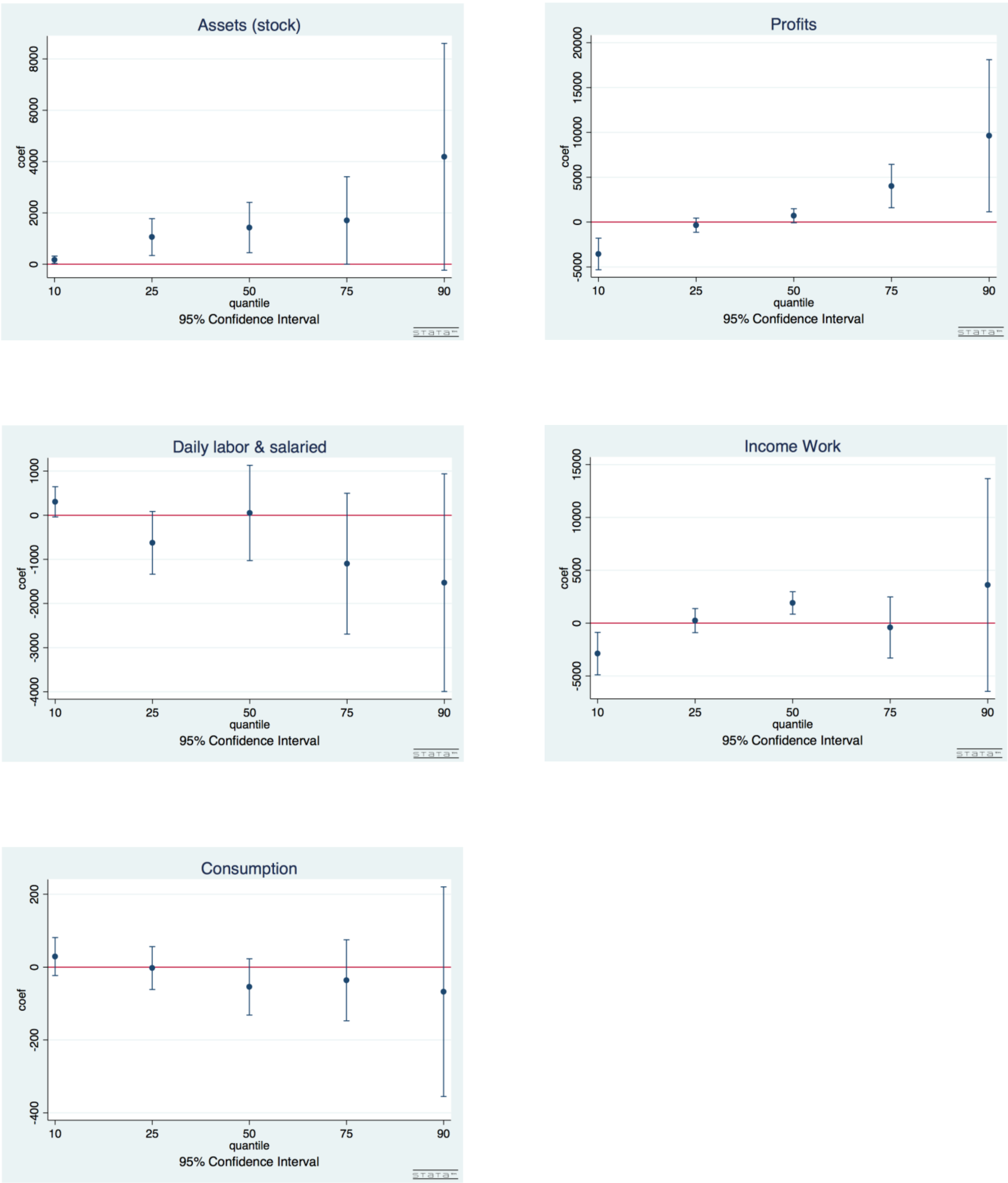

Figure 2. Cumulative distribution of potential outcomes for compliers

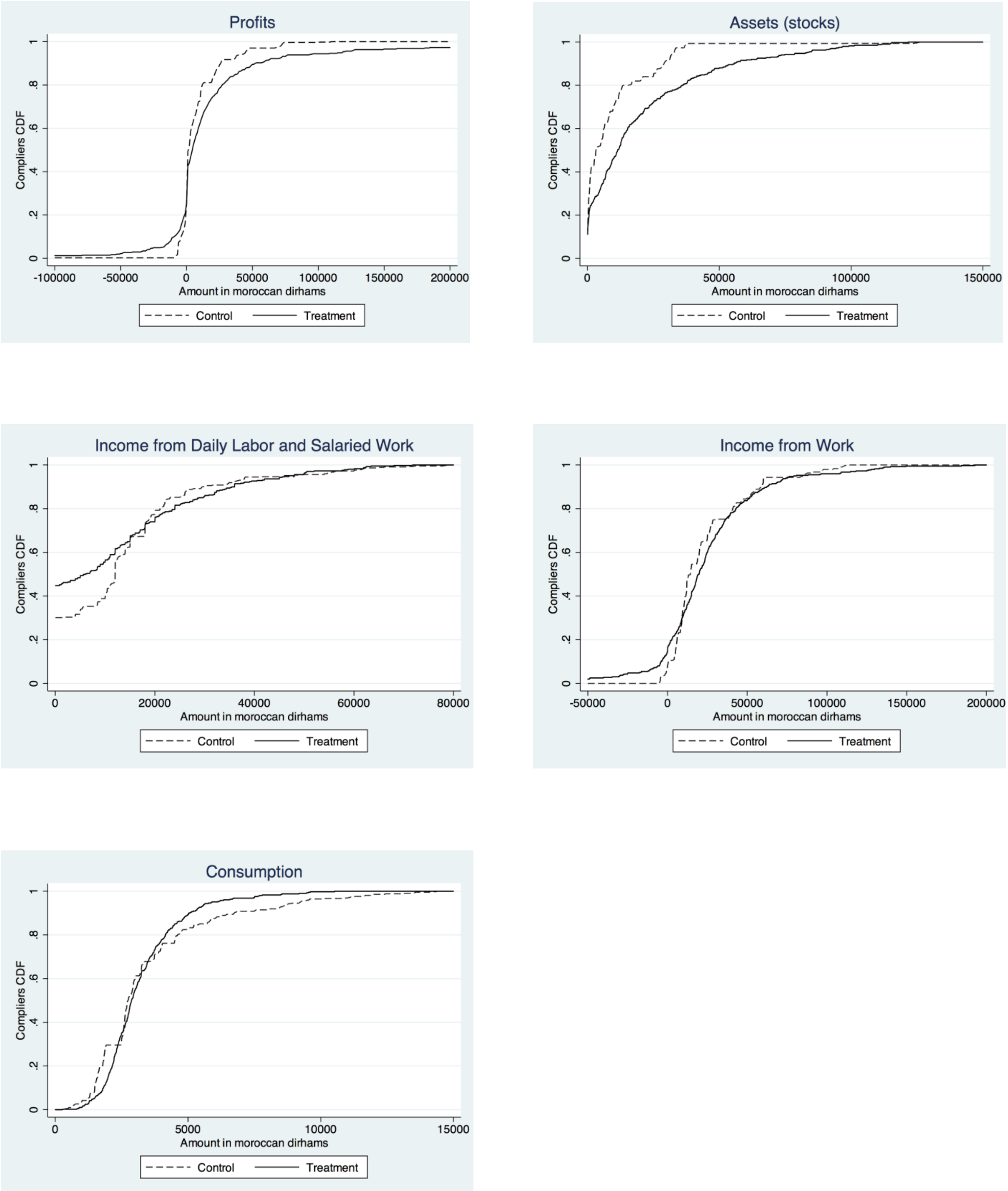

Table 1. Summary Statistics

	Obs	Control Group			Treatment - Control		
		Obs	Mean	St. Dev.	Coeff.		*p-value*
Panel A. Baseline Household Sample							
Household composition							
# members	4,465	2,266	5.14	2.70	0.04		*0.583*
# adults (>=16 years old)	4,465	2,266	3.45	1.99	0.03		*0.564*
# children (<16 years old)	4,465	2,266	1.68	1.65	0.01		*0.859*
Male head	4,465	2,266	0.935	0.246	0.001		*0.813*
Head age	4,465	2,266	48	16	1	**	*0.012*
Head with no education	4,465	2,266	0.615	0.487	-0.013		*0.353*
Access to credit:							
Loan from Al Amana	4,465	2,266	0.007	0.084	-0.003		*0.425*
Loan from other formal institution	4,465	2,266	0.060	0.238	0.030	**	*0.023*
Informal loan	4,465	2,266	0.068	0.251	0.023	***	*0.006*
Electricity or water connection loan	4,465	2,266	0.156	0.363	0.013		*0.523*
Amount borrowed from (in MAD):							
Al Amana	4,465	2,266	34	460	-13		*0.534*
Other formal institution	4,465	2,266	355	2,340	92		*0.188*
Informal loan	4,465	2,266	248	2,248	-8		*0.880*
Electricity or water entities	4,465	2,266	528	1,370	22		*0.758*
Self-employment activities							
# activities	4,465	2,266	1.6	1.2	0.0		*0.435*
Farms	4,465	2,266	0.599	0.490	0.017		*0.321*
Investment	4,465	2,266	13	72	0		*0.775*
Sales	4,465	2,266	9,335	36,981	-392		*0.665*
Expenses	4,465	2,266	3,369	8,428	266		*0.241*
Savings	4,465	2,266	1,271	3,505	-77		*0.433*
Employment	4,465	2,266	22	95	-1		*0.477*
Self-employment	4,465	2,266	61	102	5		*0.122*
Does animal husbandry	4,465	2,266	0.533	0.499	0.042	**	*0.027*
Investment	4,465	2,266	397	1,912	67		*0.2*
Sales	4,465	2,266	3,444	8,831	339		*0.184*
Expenses	4,465	2,266	4,111	10,897	386		*0.206*
Savings	4,465	2,266	10,249	17,032	1,066	*	*0.050*
Employment	4,465	2,266	7	49	-1		*0.272*
Self-employment	4,465	2,266	111	158	7		*0.215*
Runs a non-farm business	4,465	2,266	0.217	0.412	-0.034	**	*0.011*
# activities managed by women	4,465	2,266	0.218	0.585	0.004		*0.750*
Share of HH activities managed by women	4,465	2,266	0.160	0.367	0.007		*0.466*
Distance to souk	4,125	2,077	20.1	25.2	0.2		*0.87*
Has income from:							
Self-employment activity	4,465	2,266	0.780	0.414	-0.016		*0.163*
Day labor/salaried	4,465	2,266	0.580	0.494	-0.016		*0.194*
Risks:							
Lost more than 50 percent of the harvest	4,125	2,077	0.106	0.308	0.004		*0.642*
Lost more than 50 percent of the livestock	4,125	2,077	0.030	0.172	0.003		*0.606*
Lost any livestock over the past 12 months	4,465	2,266	0.189	0.392	0.029	**	*0.012*
HH member illness, death and/or house sinister	4,465	2,266	0.218	0.413	0.013		*0.168*
Consumption							
Consumption (in MAD)	4,465	2,266	2,272	1,349	28		*0.440*
Non-durables consumption (in MAD)	4,465	2,266	2,227	1,295	20		*0.559*
Durables consumption (in MAD)	4,465	2,266	45	236	8		*0.231*
HH is poor	4,465	2,266	0.247	0.431	0.002		*0.858*
Panel B. Attrition							
Not surveyed at endline	4,465	2,266	0.068	0.252	0.018	**	*0.018*

Notes: Data source: Baseline household survey. Unit of observation: household. Panel A & B: sample includes all households surveyed at baseline. ***, **, * indicate significance at 1, 5 and 10%.

Table 2. Credit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Al Amana - Admin data	Al Amana - Survey data	Other MFI	Other Formal	Utility company	Informal	Total	Loan repayment	Index of dependent variables
Panel A. Credit access†									
Treated village	0.167	0.090	-0.006	0.007	0.017	-0.003	0.076		0.129
	(0.012)***	(0.010)***	(0.004)	(0.003)**	(0.017)	(0.007)	(0.017)***		(0.017)***
Observations	4,934	4,934	4,934	4,934	4,934	4,934	4,934		4,934
Control mean	0.000	0.022	0.023	0.016	0.157	0.059	0.247		0.000
Hochberg-corrected p-value									0.000
Panel B. Loan amounts (in MAD) ††									
Treated village		795	-13	356	180	-112	1,206	33	
		(103)***	(34)	(181)*	(89)**	(169)	(290)***	(13)**	
Observations		4,934	4,934	4,934	4,934	4,934	4,934	4,934	
Control mean		180	124	519	566	493	1,882	42	

Notes: Data source: Column1: Al Amana administrative data. Columns 2-9: Endline household survey. Observation unit: household. Sample includes households with high probability-to-borrow score surveyed at endline, after trimming 0.5% of observations (3,525 who got both a full baseline and endline household survey administered, plus an additional 1,409 households who got only the full endline survey administered). (see Section 3 for an explanation of sample strategy). Coefficients and standard errors (in parentheses) from an OLS regression of the variable on a treated village dummy, controlling for strata dummies (paired villages) and variables specified below. Standard errors are clustered at the village level. ***, **, * indicate significance at 1, 5 and 10%. Controls include: number of household members, number of adults, head age, does animal husbandry, does other non-agricultural activity, had an outstanding loan over the past 12 months, HH spouse responded the survey, and other HH member (excluding the HH head) responded the survey.
† Column 1-8: dummy variable equal to 1 if the households had an outstanding loan over the 12 months prior to the survey. †† Sum of outstanding loans (in MAD) over the 12 months prior to the survey.

Column 9: the dependent variable consists of an index of z-scores of the outcome variables in columns 2-8 (including both credit access and loan amounts) following Kling, Liebman, and Katz (2007). P-values for this regression are reported using Hochberg's correction method.

Table 3. Self-employment activities: revenues, assets and profits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Assets	Sales and home consumption	Expenses	*Of which:* Investment	Profit	Has a self-employment activity	Index of dependent variables
Treated village	1,448	6,061	4,057	-224	2,005	-0.015	0.029
	(658)**	(2,167)***	(1,721)**	(223)	(1,210)*	(0.010)	(0.015)**
Observations	4,934	4,934	4,934	4,934	4,934	4,934	4,934
Control mean	15,984	30,450	21,394	1,529	9,056	0.832	0.000
Hochberg-corrected p-value							0.233

Notes: Data source: Endline household survey. Observation unit: household. Coefficients and standard errors (in parentheses) from an OLS regression of the variable on a treated village dummy, controlling for strata dummies (paired villages) and variables specified below. Standard errors are clustered at the village level. ***, **, * indicate significance at 1, 5 and 10%. Same controls as in Table 2.

Definitions:

(1) Sum of assets owned in the three activities, including the stock of livestock.

(2) Total Production = sum of agricultural, livestock and non-agricultural business production over the 12 months prior to the survey. Production includes both sales and self-consumption. Agricultural production also includes stock.

(3) Sum of labor, inputs, rent and investment in all three activities, purchased over the 12 months prior to the survey.

(4) Sum of productive assets purchased over the 12 months prior to the survey. Animal husbandry assets include the purchases of livestock.

(5) Profit =(2)-(3)

(6) Variable equals 1 if the HH ran a self-employment activity over the 12 months prior to the survey.

(7) The dependent variable consists of an index of z-scores of the outcome variables in columns 1-6 following Kling, Liebman, and Katz (2007). P-values for this regression are reported using Hochberg's correction method.

Table 4. Income

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
		HH income, over the past 12 months, from:					
Panel A. Income (in MAD)	Total	Self-employment, daily labor & salaried	Self-employment activities	Day labor & salaried	Household asset sales	Other	Index of dependent variables
Treated village	447	954	2,005	-1,050	-679	171	0.000
	(1,342)	(1,267)	(1,210)*	(478)**	(262)**	(233)	(0.017)
Observations	4,934	4,934	4,934	4,934	4,934	4,934	4,934
Control mean	27,669	24,804	9,056	15,748	709	2,157	0.000
Hochberg-corrected p-value							0.981

Notes: Data source: Endline household survey. Observation unit: household. Coefficients and standard errors (in parentheses) from an OLS regression of the variable on a treated village dummy, controlling for strata dummies (paired villages) and variables specified below. Standard errors are clustered at the village level. ***, **, * indicate significance at 1, 5 and 10%. Same controls as in Table 2.

Definitions:

(3): income equals total profit from the self-employment activity.

(7): the dependent variable consists of an index of z-scores of the outcome variables in columns 1-6 following Kling, Liebman, and Katz (2007). P-values for this regression are reported using Hochberg's correction method.

Table 5. Time worked by HH members

	(1)	(2)	(3)	(4)	(5)	(6)
	Hours worked by household members over the past 7 days†				# of HH members	Index of dependent variables
	Total	*of which:*				
		self-employment activities	*outside activities*	*chores*		
Household members 6-65 years old						
Treated village	-3.3	1.1	-2.8	-1.6		
	(2.5)	(1.5)	(1.1)***	(1.0)*		
Control mean	143.1	46.9	33.8	62.3	5.2	
Household members 6-15 years old						
Treated village	-0.5	0.5	0.2	-1.3		
	(0.7)	(0.4)	(0.3)	(0.4)***		
Control mean	19.2	6.3	3.4	9.4	1.4	
Household members 16-20 years old						
Treated village	-1.4	-0.2	-1.3	0.1		
	(0.8)*	(0.4)	(0.4)***	(0.4)		
Control mean	21.6	6.6	5.5	9.6	0.8	
Household members 21-50 years old						
Treated village	-0.5	1.1	-1.5	0.0		
	(1.5)	(0.8)	(0.8)**	(0.6)		
Control mean	84.4	26.3	21.9	36.3	2.5	
Household members 51-65 years old						
Treated village	-1.2	-0.5	-0.3	-0.4		
	(0.6)**	(0.3)	(0.3)	(0.3)		
Control mean	18.2	8.1	3.1	7.0	0.6	
Observations	4,918	4,918	4,918	4,918	4,918	
Index						
Treated village						-0.017
						(0.010)*
Observations						4,918
Control mean						0.000
Hochberg-corrected p-value						0.320

Notes: Data source: Endline household survey. Observation unit: household. Coefficients and standard errors (in parentheses) from an OLS regression of the variable on a treated village dummy, controlling for strata dummies (paired villages) and variables specified below. Standard errors are clustered at the village level. ***, **, * indicate significance at 1, 5 and 10%. Same controls as in Table 2.

† Sum of hours worked by household members over the past 7 days in self-employment, outside activities and housework. Households were asked at endline survey about the # of hours worked by each HH member over the past 7 days.

(6) The dependent variable consists of an index of z-scores of the outcome variables in all panels of columns 1-4 following Kling, Liebman, and Katz (2007). P-values for this regression are reported using Hochberg's correction method.

Table 6. Consumption

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Monthly household consumption (in MAD) in:								Index of dependent variables
	Total	Durables	Non-durables	Food	Health	Education	Temptation & entertainment	Festivals & celebrations	
Treated village	-46	18	-63	3	3	-1	-6	-39	-0.015
	(47)	(16)	(44)	(23)	(5)	(1)	(6)	(12)***	(0.015)
Observations	4,924	4,924	4,924	4,924	4,924	4,924	4,924	4,924	4,924
Control mean	3,057	64	2,993	1,784	46	24	298	425	0.000
Hochberg-corrected p-value									0.938

Notes: Data source: Endline household survey. Observation unit: household. Coefficients and standard errors (in parentheses) from an OLS regression of the variable on a treated village dummy, controlling for strata dummies (paired villages) and variables specified below. Standard errors are clustered at the village level. ***, **, * indicate significance at 1, 5 and 10%. Same controls as in Table 2.

Definitions:

(1)-(8): Monthly household expenditures, including food self-consumption.

(9): the dependent variable consists of an index of z-scores of the outcome variables in columns 1-8 following Kling, Liebman, and Katz (2007). P-values for this regression are reported using Hochberg's correction method.

Table 7. Social effects

	(1) Share of kids aged 6-15 in school	(2) Share of teenagers (aged 16-20) in school	(3) Index of women independence†	(4) Share of household with self-employment activities managed by women	(5) Number of self-employment activities managed by women	(6) Index of dependent variables
Treated village	0.004	-0.004	0.169	-0.014	-0.02	-0.007
	(0.008)	(0.006)	(0.205)	(0.009)	(0.01)	(0.012)
Observations	4,934	4,934	4,934	4,934	4,934	4,934
Control mean	0.453	0.088	-0.069	0.248	0.39	0.000
Hochberg-corrected p-value						>0.999

Notes: Data source: Endline household survey. Observation unit: household. Coefficients and standard errors (in parentheses) from an OLS regression of the variable on a treated village dummy, controlling for strata dummies (paired villages) and variables specified below. Standard errors are clustered at the village level. ***, **, * indicate significance at 1, 5 and 10%. Same controls as in Table 2.

† Effect on the sum of 14 standardized measures (measures include: at least one woman in the household has currently an own activity, decides by herself on activity assets, buys activity assets herself, decides by herself on activity inputs, buys inputs herself, decides what to produce, commercializes production, decides by herself on commercialization, makes sales herself, had an own activity in the past 5 years, is allowed to go to the market by herself, is allowed to take public transportation by herself, is allowed to visit family by herself, is allowed to visit friends by herself). Each measure is coded so that 1 reflects independence and 0 reflects lack of independence.

(6): the dependent variable consists of an index of z-scores of the outcome variables in columns 1-5 following Kling, Liebman, and Katz (2007). P-values for this regression are reported using Hochberg's correction method.

Table 8: Externalities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Client Al Amana - Admin data	Assets (stock)	Sales and home consumption	Expenses	Profit	Income from day labor/ salaried	Weekly hours worked by HH members aged 16-65		Monthly HH consumption (in MAD)
							self-employment	outside	
Panel A: Borrowers									
Treated village	0.167	1,448	6,061	4,057	2,005	-1050	0.6	-3.0	-46
	(0.012)***	(658)**	(2,167)***	(1,721)**	(1,210)*	(478)**	(1.3)	(1.0)***	(47)
Observations	4,934	4,934	4,934	4,934	4,934	4,934	4,918	4,918	4,924
Control mean	0.000	15,984	30,450	21,394	9,056	15,748	40.6	30.4	3,057
Panel B: All sample weighted									
Treated village	0.132	1,003	3,710	4,186	-476	-1234	0.5	-2.0	-44
	(0.011)***	(705)	(1,942)*	(1,334)***	(1,252)	(558)**	(1.1)	(1.1)*	(36)
Observations	5,524	5,524	5,524	5,524	5,524	5,524	5,508	5,508	5,513
Control mean	0.000	15,493	26,376	17,263	9,113	15,911	39	30.0	2,927
Panel C: Top and bottom 30% unweighted									
Treated village X High Predicted Propensity to Borrow	0.363	1,033	15,774	10,171	5,603	-2,113	2.9	-7.0	-93
	(0.011)***	(1,296)	(4,154)***	(3,555)***	(2,452)**	(692)***	(2.3)	(1.8)***	(94)
Treated village X Low Predicted Propensity to Borrow	0.015	1,612	647	1,013	-366	-2,453	-1.4	-6.2	82
	(0.003)***	(1,132)	(2,701)	(1,737)	(1,734)	(795)***	(1.3)	(1.6)***	(62)
Observations	3,315	3,315	3,315	3,315	3,315	3,315	3,303	3,303	3,307
Control mean	0.000	17,611	31,667	22,343	9,325	16,119	40.0	31.9	3,063
Control mean, hight PTB	0.000	21,692	37,988	27,073	10,915	15,652	45.8	32.4	3,253
Control mean, low PTB	0.000	13,691	25,595	17,798	7,796	16,567	34.4	31.4	2,881
p-value: T X Low PTB = T X High PTB	0.000	0.734	0.002	0.022	0.049	0.746	0.106	0.727	0.113

Notes: Data source: Endline household survey. Observation unit: household. Panel A: sample includes households with high probability-to-borrow score. Panel B: sample includes both households with high probability-to-borrow score and households picked at random. Observations are weighted by the inverse probability of being sampled. Panel C: sample includes both households with high probability-to-borrow score and households picked at random, but only those in the top 30% and in the bottom 30% of the predicted propensity to borrow (PTB) distribution. All panels include sample after 0.5% trimming of observations. Panel A & B: coefficients and standard errors (in parentheses) from an OLS regression of the variable on a treated village dummy, controlling for strata dummies (paired villages) and variables specified below. Panel C: coefficients and standard errors (in parentheses) from an OLS regression of the variable on a treated village dummy interacted with a dummy equal to 1 if HH predicted propensity to borrow is in the 0-30th percentile of the PTB distribution (Low Predicted PTB), on a treated village dummy interacted with a dummy equal to 1 if HH predicted PTB is in the 70-100th percentile of the PTB distribution (High Predicted PTB) and on a dummy equal to 1 if HH predicted PTB is in the 0-30th percentile of the PTB distribution (not shown), controlling for strata dummies (paired villages) and variables specified below. All panels: standard errors are clustered at the village level. ***, **, * indicate significance at 1, 5 and 10%. Same controls as in Table 2.

Table 9: The impact of borrowing

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Assets (stock)	Sales + home consumption	Expenses	Profit	Has a self-employment activity	Income from day labor/salaried	Weekly hours worked by HH members aged 16-65		Monthly HH consumption (in MAD)
							self-employment	outside	
Panel A: OLS									
	4,682	19,800	11,934	7,866	0.019	-1,263	6.6	-3.1	482
	(1,870)**	(7,758)**	(5,580)**	(4,122)*	(0.016)	(1,138)	(3.0)**	(3.1)	(192)**
	2,448	2,448	2,448	2,448	2,448	2,448	2,440	2,440	2,444
	16,524	31,182	21,574	9,608	0.816	15,127	39.2	27.8	2,947
Panel B: IV									
Client	8,663	36,253	24,263	11,989	-0.091	-6,281	3.6	-18.2	-274
	(4,008)**	(12,494)***	(9,944)**	(7,204)*	(0.060)	(2,866)**	(8.0)	(5.8)***	(278)
Observations	4,934	4,934	4,934	4,934	4,934	4,934	4,918	4,918	4,924
Control mean	15,984	30,450	21,394	9,056	0.832	15,748	40.6	30.4	3,057
Control complier mean†	13,568	23,703	16,551	7,152	0.900	18,530	43.5	42.1	3,421

Note : Data source: Endline household survey. Observation unit: household. Panel A: Sample includes households with high probability-to-borrow score in treated villages. Coefficients and standard errors (in parentheses) from an OLS regression of the variable on a client dummy, controlling for strata dummies (paired villages) and variables specified below. Client is a dummy variable equal to 1 if the household has borrowed from Al Amana. Panel B: Sample includes households with high probability-to-borrow score in treated and control villages. Coefficients and standard errors (in parentheses) from an instrumental variable regression of the variable on the variable *client* , controlling for strata dummies (paired villages) and variables specified below. *Client* is a dummy variable equal to 1 if the household has borrowed from Al Amana and is instrumented with *treated village* , a dummy equal to 1 if the household lives in a treatment village. Standard errors are clustered at the village level. ***, **, * indicate significance at 1, 5 and 10%. Same controls as in Table 2.

† The complier mean in the control group is calculated as E(Y0|C)=[E(Y|Z=0)-E(Y|Z=1,T=0)*(1-P(T=1))]/P(T=1), where Z indicates treatment assignment, T indicates being a microcredit client and P(T=1) the proportion of clients in Z=1

APPENDIX

Table A1. Propensity to borrow

Propensity to borrow, all households interviewed at **baseline in wave 1** treatment villages	Coef.
Does more than 3 self-employment activities	2.365
	(0.734)***
Does trading as self-employment activity	0.846
	(0.501)*
Share # of members with trading, services or handicraft as main activity to # of members	3.125
	(1.756)*
Owns land	-1.588
	(0.443)***
Rents land	-1.992
	(0.575)***
Have not bought agriculture productive assets over the past 12 months	-1.048
	(0.476)**
Uses sickle & rake (in agriculture)	-0.979
	(0.338)***
ln(# of olive and argan trees)	0.518
	(0.096)***
# of cows bought over the past 12 months	-2.010
	(1.020)**
Gets a pension	2.021
	(0.539)***
Has a radio	1.066
	(0.403)***
Has a fiber mat	1.574
	(0.650)**
Phone expenses over the past month (in MAD)	-0.019
	(0.006)***
Clothes expenses over the past month (in MAD)	0.001
	(0.001)*
Had an outstanding formal loan over the past 12 months	0.869
	(0.330)***
ln(amount that would be able to reimburse monthly (in MAD))	0.250
	(0.109)**
Would be ready to form a 4-person group and guarantee a loan mutually	0.570
	(0.321)*
Would uptake a loan of 3,000 MAD to be repaid in 9 monthly installments of 400 MAD	0.593
	(0.338)*
Observations	665
Mean dependent variable	0.104
Pseudo R2	0.280
Number of villages	7

Notes: Data source: Mini survey. Unit of observation: household. Sample includes all households surveyed at baseline in phase 1 pilot treatment villages (i.e. wave 1). Coefficients and standard errors (in parenthesis) from a logit regression of the variable client on variables specified in the table. Client is a dummy variable equal to 1 if the household had taken up a microcredit within the first 6 months of the intervention. ***, **, * indicate significance at 1, 5 and 10%.