All together now: the impact of a multifaceted approach to poverty alleviation*

Vilas J. Gobin† Paulo Santos‡

This version: December 2015

Abstract

We examine the impact of the Rural Entrepreneur Access Program (REAP), an example of an approach that combines multiple interventions with the aim of overcoming the constraints facing the ultra-poor. This program was implemented in an extreme setting (arid areas of northern Kenya) and emphasizes cash transfers (rather than asset transfers) to ultra-poor women, in addition to business skills training, business mentoring and savings trainings. Due to over-recruitment, the program was rolled-out in three rounds, with allocation to each round being randomly determined through a public lottery. In the short-run we find that the program has a positive and significant impact on income, savings and asset accumulation and food security.

*We thank Russell Toth, Gaurav Datt, Hee-Seung Yang, Asadul Islam and Andreas Leibbrandt for comments on earlier drafts of this paper. The data used in this study were supplied by The BOMA Project. We thank staff of The BOMA Project especially Kathleen Colson, Ahmed Omar, Meshack Omarre, Fredrick Learapo, Sabdio Doti, Bernadette Njoroge, Nate Barker, and Alex Villec for their assistance in data collection as well as in facilitating the first author's stay in Kenya. All analyses, interpretations or conclusions based on these data are solely that of the authors. The BOMA Project disclaims responsibility for any such analyses, interpretations or conclusions.

†PhD Student, Economics, Monash University, Melbourne, Australia. Corresponding author (e-mail: vilas.gobin@monash.edu)

‡Senior Lecturer, Economics, Monash University, Melbourne, Australia.

1 Introduction

Microenterprises are the source of employment for more than half of the labor force in developing countries (de Mel, S., D., & Woodruff, 2008; Gindling & Newhouse, 2014), and are seen as engines of economic development by raising income of managers, creating a demand for labor (earning higher wages), and increasing market competition (that generates lower prices to consumers) (Bank, 2012; Bruhn, 2011). Despite these benefits, it has been realized that the world's poorest are often prevented from establishing such businesses or from participating in many popular approaches aimed at stimulating microenterprise formation.

For example, until very recently microfinance was advocated as a way to overcome financial market imperfections which limited the capacity of the poor to invest in profitable projects (Jolis & Yunus, 2003). Recent evidence points to the limited impact of microfinance on poverty alleviation suggesting that alleviating credit constraints alone is not sufficient to reduce poverty through microenterprises (e.g. Angelucci, Karlan, & Zinman, 2014; Banerjee, Duflo, Glennerster, & Kinnan, 2013; Banerjee, Karlan, & Zinman, 2015; Karlan & Zinman, 2010). This has prompted a shift in attention to other possible constraints, particularly entrepreneurial skills, knowledge and human capital. But the conclusions of evaluations of interventions designed to overcome these constraints have been mixed (e.g. Bruhn, Karlan, & Schoar, 2013; Drexler, Fischer, & Schoar, 2014; Valdivia, 2015).

The limited impact of this "one-constraint-at-a-time" approach, particularly among the ultra-poor, suggested the need for interventions that simultaneously address the overlapping set of constraints and provide the ultra-poor with a localized "big push" to graduate from poverty. One influential approach, pioneered by BRAC, is the Challenging the Frontiers of Poverty Reduction - Targeting the Ultra-Poor (CFPR/TUP), structured as a poverty graduation program: during a limited period (two years), its participants would benefit from a set of interventions (savings services, skills training,

and a physical asset transfer, together with regular follow-up visits and consumption support) with the expectation that, at the end of that period, participants would be able to participate in microfinance (Goldberg & Salomon, 2011; Matin, Sulaiman, & Rabbani, 2008).

There is limited understanding of the impact of this type of intervention on poverty alleviation given its relative novelty but the existing studies find that it does lead to sustained improvements in household welfare as a result of increased incomes from self-employment activities. A randomized evaluation of CFPR/TUP across 1409 communities in Bangladesh finds that the program enabled ultra-poor women to engage in microentrepreneurial activities resulting in a significant increase in earnings, which persists up to two years after participants graduate from the program (Bandiera et al., 2013).

Evidence also comes from some of the 20 countries where this approach has been replicated or adapted. In a recent multi-site randomized evaluation across six of these countries, Banerjee, Duflo, et al. (2015) find similar impacts to those reported for Bangladesh: consumption, productive assets, income and revenue are higher in the treatment group at the conclusion of the program and remain higher one year after graduation. However, these impacts are found to be weaker in two study sites (Honduras and Peru), naturally raising questions about the external validity of the results.

Concerns about external validity are also present in another study in Andhra Pradesh, India, where Bauchet, Morduch, and Ravi (2015) evaluate a similar intervention and finds no net impact on consumption, income or asset accumulation. The authors suggest that the muted impacts could be explained by a strong labor market which sharply increased the wages for unskilled labor, highlighting the need to consider factors such as the economic environment in which households make decisions when addressing the external validity of these evaluations.

This paper examines the impact of the Rural Entrepreneur Access Project (REAP), a variation of the CFPR/TUP graduation approach, implemented

in northern Kenya, a region where more than 80% of the population is estimated to be living below the national poverty line (of Statistics & for International Development, 2013). The Rural Entrepreneur Access Project comprises of an initial package of interventions, including business skills training, cash transfers to set up a microenterprise and business mentoring which were followed, six months later, by a focus on the importance of savings (training and introduction to savings groups). [1] This sequence of interventions is targeted at ultra-poor women and is designed to enable them to gain the assets and skills necessary to graduate from poverty, a motivation that is similar to the one behind the CFPR/TUP (MacMillan, 2013).

There are however important differences, chief among them being the fact that women are provided with a cash transfer instead of an asset transfer, as in other poverty graduation programs studied to date (e.g. Bandiera et al., 2013; Banerjee, Duflo, et al., 2015; Bauchet et al., 2015). Cash transfers are arguably more attractive in that they allow the participants more freedom in their choice of enterprise and are potentially less costly to implement. [2] The program also differs in two other important ways: there is no provision of consumption support (which could potentially aggravate the risk of the cash transfer being used for consumption) and, finally, beneficiaries are required to work as a group in running their enterprise. The primary objective of this paper is to determine if jointly relaxing financial and human capital constraints results in poverty alleviation through microentrepreneurial activities.

The remainder of the paper proceeds as follows. In section 2 we provide a detailed description of REAP before presenting, in section 3, the identifica-

[1] The program is implemented through a NGO, The BOMA Project. See `http://bomaproject.org/the-rural-entrepreneur-access-project/` for a complete description of REAP.

[2] The preference for an asset transfer is usually justified by fears that cash transfers could be immediately spent on consumption instead of invested in an enterprise. See Annan, Blattman, Green, Jamison, and Lehmann (2015) for empirical evidence against such belief.

tion strategy and the data used in this paper. We are able to take advantage of randomized roll-out of the program, which resulted from over-recruitment during the participant selection stage of the program, to obtain unbiased estimates of the program's impact on household welfare. Section 3 also includes tests of the assumptions underlying the identification strategy and a discussion of spillover and anticipation effects. The main results are presented in section 4. We find that, in the short-run, this program has a positive and statistically significant impact on income, savings and asset accumulation (both consumer durables and productive assets), and food security (as measured by the number of nights children went to bed hungry). However, we do not find an impact on consumption. We conclude in section 5.

2 Overview of the intervention

The Rural Entrepreneur Access Project was implemented in 14 locations in the southern and central parts of Marsabit County, in the Arid and Semi-Arid Lands (ASALs) of northern Kenya (see Figure 1), a region where more than 80% of the population are estimated to live below the national poverty line (of Statistics & for International Development, 2013). [3] The main livelihood option in these locations is pastoralism, with livestock serving both as a source of income and food for herders and their families. Pastoralism, however, is highly susceptible to weather and other shocks, and repeated droughts frequently have devastating impacts on households' livelihoods (Silvestri, Bryan, Ringler, Herrero, & Okoba, 2012), resulting in many households no longer being able to meet their basic needs due to the loss of herds. Such households are forced into begging, unskilled wage labor, different forms of petty trade,

[3] In 2005/06, the poverty line was estimated at Ksh 1,562 (PPP USD 77.07 at 2014 prices) per adult equivalent per month for rural households (of Statistics, 2007). In 2009 it was estimated that nationally, 45.2% of the population lived below the poverty line (of Statistics & for International Development, 2013).

and become reliant on food aid to meet their dietary needs. [4]

Opportunities to engage in non-pastoral activities are further restricted by the fact that communities in this region tend to be excluded from national development processes, have low population densities ($<5/km^2$), have limited access to markets or other infrastructure, and face financial and human capital constraints (Elliot & Fowler, 2012). By targeting the poorest women in these communities, REAP aims to provide the most vulnerable households with a pathway out of poverty by alleviating the financial and human capital constraints that they face.

2.1 Structure and timing of the program

The main aim of REAP is to graduate ultra-poor women from poverty, through a set of interventions that include the development of business plans and mentoring, grants and access to saving mechanisms. The sequence of these interventions is presented in Figure 2, and each intervention is briefly described below.

Participant Selection. Program eligibility is determined by local committees, formed specifically for targeting. [5] These committees were asked to identify women who were among the poorest of the poor in the community, with no other sources of income besides the business being formed, and who were also considered to be responsible and entrepreneurially minded, and were willing to run a business with two other women. [6] Trained business mentors ensured

[4]Little, McPeak, Barrett, and Kristjanson (2008) examine different proxies for poverty and welfare in northern Kenya. They identify poverty as being most prevalent among sedentary households that are no longer directly involved in pastoral production or are in the process of exiting pastoralism. They have little or no livestock and tend to be involved in unskilled wage labour and petty trade.

[5]The committees genrally comprise 10 persons, with equal representation of clans and ethnic groups in the community, and with at least half of them being women.

[6]In addition, and recognizing the importance of inter-ethnic rivalries in northern Kenya, selection committees were asked to select participants in order to lead to an equal representation from various clans and ethnic groups and appropriate representation of persons from the town centre and more distant villages. Finally, immediate relatives of any

that the local committees followed these criteria when selecting participants. [7] Once the participants were selected and accepted the invitation to participate in REAP, the business mentor proceeded to form business groups of three women.

Business Planning and Business Skills Training. In the month leading up to program enrollment, the business mentors met with beneficiaries to assist with the development of a business proposal. The mentor was expected to get a better understanding of the group members' abilities and previous business experience before going through the basics of setting up a business with the group. [8] On the day of program enrolment, all participants were required to attend a short business skills training session, delivered by mentors under the supervision of REAP field officers. [9]

First Grant and Business Mentoring. At the end of the business skills training session participants were provided with a cash grant of USD 100 (PPP USD 237.97 at 2014 prices) to be used to establish their business, an amount which is equivalent to approximately 7.5 months of expenditure per capita. [10] Once the groups received their grants they were free to invest the money, including by making changes in their initial business proposal.

BOMA Project staff were considered ineligible. More recently, participant selection procedures included a Participatory Wealth Ranking to identify the poorest, followed by a short interview, used to confirm eligibility.

[7] Mentors are employed at the location level. Each location comprises many sub-locations which are formed by smaller villages, known as manyattas.

[8] This early training session included: understanding the needs and size of the market, identification of the type of business that will be established and its location, identification of suppliers, decisions regarding how to spend the initial grant (and other inputs), allocation of responsibilities among group members, and, finally, decisions regarding how profits are to be used.

[9] Which covered the following topics: what is a business; how to make a profit; what to sell (or produce); how to attract customers; management of the business; record keeping; and the value of savings.

[10] All monetary values reported in the paper are in PPP terms at 2014 prices. We use the following PPP exchange rates to convert Kenya Shillings to USD PPP: 36.83 (2012), 38.38 (2013), 40.35 (2014). These values are then converted to 2014 prices by multiplying the ratio of the 2014 US Consumer Price Index (CPI) to the US CPI for the relevant year.

The distribution of the initial grants was followed by a period during which a mentor regularly met with the business group (at least once a month) to monitor its progress and offer advice and training. The role of the mentor was to help in the start-up of the business, through the provision of information (such as where to source goods and market conditions). Additionally, it was expected that, by providing ongoing training and support, the mentor would help the group with record keeping and, if needed, in managing conflicts within the group. Mentoring would last until groups formally exit the program, two years after its start.

Second Grant, Savings Training and Savings Group Formation. Six months after the start of the business, groups were eligible for a follow up grant of USD 50 (PPP USD 118.98) conditional on meeting the following criteria: two or more original members remained involved in the business; members held business assets collectively; and the business value (defined as the sum of cash on hand, business savings and credit outstanding, and business stock and assets) was equal to or greater than the value of the initial grant. Participants were also required to participate in a short training session on savings, designed to provide a basic understanding of the formation and operation of savings groups including their rules, record-keeping, and issuing of loans. These conditions were known by participants since the start of the program.

After the savings training and the second grant distribution, participants were encouraged to form a savings group (SG) or join existing ones. The decision to join a group was both non-compulsory and individual (i.e., it was not a business group decision). The savings group model introduced to participants during the training most closely resembled Village Savings and Loans Associations (VSLA), also known as Accumulating Savings and Credit Associations (ASCAs), described in Allen (2006). The groups are self-managed and allow members to save money and access loans which are paid back with interest.

3 Research design

3.1 Randomization of program assignment

In November 2012, the local selection committees across 14 locations in northern Kenya, identified 1755 women as being eligible for REAP. Due to lack of capacity to simultaneously enroll all participants, it was decided to split the eligible women into three groups to be successively enrolled over the next three funding cycles (March/April 2013, September/October 2013 or March/April 2014, hereafter groups A, B and C, respectively). [11] Assignment to each cycle was done randomly, through a public lottery that took place in each of the 14 locations from which participants had been recruited, with one-third of the women enrolled in each funding cycle. [12] A public lottery was used to ensure that the allocation to funding cycle was transparent and fair, and seen as such. The random assignment of the beneficiaries to each cycle, if not defied, should lead to balanced groups, statistically identical in all observable (and, by assumption, unobservable) characteristics. All eligible women were interviewed at baseline (November 2012) and at two follow-up surveys, conducted at six month intervals and timed to coincide with the beginning of each new funding cycle. [13]

None of the eligible participants declined to participate in the program, or was allowed to participate outside of the group to which they were randomly allocated. Survey attrition is very low in both follow-up rounds of survey. Less than 2% of women could not be reached for a follow-up interview in

[11] As a result, sample size was determined by the capacity of the program to reach participants. We conduct *ex post* power calculations to determine if there is sufficient power, given the predetermined sample size, to reliably estimate program impacts, and find that in most cases the minimum detectable effect size is as low as 15%. These calculations are available from the authors upon request.

[12] Initially 1755 women were selected, but 3 women were subsequently disqualified leading to 585 women being assigned to the first and second cycles and 582 women being assigned to the final funding cycle.

[13] Figure A1, in Appendix A, presents a timeline and sequence of activities for participants in the three funding cycles, including the timing of the surveys.

either the midline or endline surveys (see Table 1).

Together, the sequential rollout of the program, the randomized allocation to each cycle, the perfect compliance of observations to treatment and control groups, and the extremely low attrition rate, allow us to disentangle some of the program impacts in a relatively straightforward way.

3.2 Checking the integrity of randomized design

We test the assumption that baseline characteristics are uncorrelated with treatment status by comparing the distribution of the baseline characteristics of participants. We make several comparisons that take into account the changing composition of the treatment and control groups as the program is progressively rolled-out. The results are presented in Table 2.

In panel A, we present summary statistics (mean and standard deviations) of variables that may be impacted by the program (expenditure, income, savings, asset ownership) or that may mediate its impact (household size, previous business experience, education). The baseline characteristics of the participants (and their households) are similar to those of other ultra-poor households in other regions of northern Kenya, which suggests that the findings of this study may be generalizable to ultra-poor pastoralist women across northern Kenya (Merttens et al., 2013). Average monthly expenditure per capita is approximately PPP USD 33.96, which is well below the poverty line. Approximately 70% of this expenditure is on food. Households are relatively large and have approximately 3.8 children on average, with less than 50% of children enrolled in school. Many households are food insecure, with children going to bed hungry at least 2 times a month. Women also report owning very little livestock: less than one Tropical Livestock Unit (TLU) per capita, well below the self-sufficiency threshold for mobile pastoralists in East African ASALs (McPeak & Barrett, 2001). [14] However, more than half

[14]Tropical Livestock Unit (TLU) is a standardized unit, designed to measure the size of a mixed livestock herd: 1 TLU is equivalent to 1 head of cattle, 0.7 camels, 10 sheep/goats,

of the participants report having some form of business experience, typically petty trade or the selling of livestock and livestock products.

In panel B, we present the t-tests of the null hypothesis of equality of means at baseline. These results indicate that randomization was successful in creating groups of individuals that are observationally identical, and in only one case can we reject the null hypothesis at the conventional 5% level. This conclusion is reinforced by the results of a F-test of the joint effect of these variables on treatment status, reported in panel C.

3.3 Spillover effects and program anticipation

Given the geographical proximity of individuals in the treatment and control groups, it is possible that control households use and benefit from the products and services offered by the businesses established by the treated households. Given that more than 95% of the businesses that are established by the treated individuals are in petty trade (primarily of food items), the main benefit might be increased competition among businesses, with a consequent reduction in market prices. Although this reduction is not expected to be substantial given the large number of pre-existing businesses in each location, we are able to control for this general equilibrium effect through the inclusion of the number of pre-existing businesses as a control variable when estimating the effect of the program. [15]

Another potential source of spillover effects might be easier access to loans. Although only REAP participants can actively participate in all SG activities, loans can be (and typically, are) extended to other members of the community, so that they can deal with shocks and emergencies (usually, health, or school and food related expenditures). We capture information on borrowing from REAP SGs for all women, and therefore can control for this

or 2 donkeys.

[15] Overall, there were 1932 businesses before the program. The program funded 195 businesses (approximately 10% of the pre-existing number) in each funding cycle. See Table B1, in Appendix B, for further details.

effect when estimating the impact of the program.

Finally, bias could potentially arise from participants changing their behavior in anticipation of receiving the program. If true, then we would expect that the effect of such changes would differ between individuals that enroll in the program in the second and third funding cycles, given that one group would anticipate receiving funding six months sooner than the other. [16] If this intuition is correct, these differences would then be captured during the midline survey (when group B is expected to immediately receive the first grant while group C is still six months away from participating in the program). As shown in Table 3, we find no evidence of program anticipation effects. [17] We also collected information on income earned from other businesses (besides REAP businesses) in all rounds of data collection, which allows us to examine if anticipation of the program led to investment in a business that did not exist at baseline. We find no evidence of statistically significant differences in income from own business between groups B and C at midline (not reported).

4 Main results

The random assignment of treatment status allows us to obtain unbiased estimates of the impact of REAP, and its variance (that takes into account stratification) by estimating the following regression for each outcome of interest:

[16] Given that we are dealing with the ultra-poor it is difficult to conjecture how behavior would change in anticipation of this program. Individuals might try to observe other businesses and how they operate or business groups might meet to discuss what will happen when they are enrolled in the program, but both capital access and human capital constraints are likely to prevent them taking any action that would affect measured outcomes.

[17] We are interested in the following outcome variables: monthly expenditure per capita, monthly income per capita, savings per capita, TLU per capita, durable asset index (see Appendix C for details of how this index was constructed), and the nights that a child has gone to bed hungry in the last week.

$$Y_i(t) = \theta + \beta T_{ij}(t) + \delta Y_i(0) + \tau M_i + \varphi X_i + \epsilon_i \qquad t, j = \{1, 2\} \qquad (1)$$

where $Y_i(t)$ is the outcome of interest for household i, at time t (=1 if midline, and =2 if endline), $Y_i(0)$ is the baseline value of the outcome variable for household i, M_i is a set of sub-location dummy variables, and X_i is a matrix of control variables (including a dummy variable to indicate if an individual has ever borrowed from a REAP SG, the number of REAP businesses in an individual's sub-location and the number of non-REAP businesses in an individual's location).[18] Finally $T_{i\bullet}$ is treatment status of individual i.

Given the structure of the program, we can consider two sets of interventions: business training, a cash grant of USD 100, and mentoring, which are introduced first, and that we label as ($T_{\bullet 1}$) and are followed by savings training, an additional cash grant of USD 50 and continued mentoring, that we label as ($T_{\bullet 2}$). Simplifying notation, by dropping the i-th individual subscript, it is clear from the description of the program (and from figure 2) that we can observe T_1 at both midline and endline ($T_1(1)$ and $T_1(2)$), and the joint effect of the two sets of interventions at the endline ($T_1(1) + T_2(2)$).

To estimate the impact of T_1 at $t = 1$ we use the data collected during the midline survey to compare group A to a combined control group formed by those benefitting from the program in the second and third cycles (i.e. groups B and C). We refer to this impact as $\beta(T_1(1))$. We can similarly estimate the impact of T_1 on group B at $t = 2$ by using the endline data to compare group B to control group C. We refer to this impact as $\beta(T_1(2))$. We can then use these two estimates of impacts to test the hypothesis that the impact of T_1 is constant throughout the period:

$$H_0 : \beta(T_1(1)) = \beta(T_1(2)) \qquad (2)$$

[18] Stratification took place at the sub-location level (77 sub-locations).

Failure to reject (2) would suggest that the impact of this subset of interventions is stable, providing further support to our assumption that there were no adverse effects from late entry into treatment (due, for example, to increased market competition).

It is important to notice that failure to reject (2) is not enough to plausibly identify the impact of T_2 in isolation given that, at the end of $t = 1$, beneficiaries of T_1 will potentially be different from the same individuals at $t = 0$ both in ways that are easy to control (asset ownership, for example) and in ways that are not easy to observe (experience in managing a business as part of a group, for example). Hence, without further assumptions regarding how such variables influence the outcomes we analyse, we can only identify the effect of T_2 conditional on previously benefiting from T_1. To do that, we use the endline data to estimate the combined impact of T_1 and T_2 at $t = 2$, $[\beta(T_1(1) + T_2(2))]$, by comparing group A with control group C.

4.1 The impact of the initial cash grant and business training

Table 4, panel A, provides the estimates of the impact of T_1 in both periods. Asterisks denote statistical significance based on the unadjusted p-values but we also adjust p-values (reported in brackets) to account for multiplicity. Because we estimate the impacts of REAP on several outcomes, some outcomes may display significance even if no effect exists since we have increased the probability of type 1 errors by testing multiple simultaneous hypotheses at set p-values. [19] Several methods exist to adjust p-values for multiple-inference and in this study we implement the step-up method to control for the false discovery rate (FDR) as proposed by Benjamini and Hochberg (1995). Using the procedure outlined by Anderson (2008) we are able to obtain adjusted p-values or q-values, which should be interpreted as the smallest significance

[19] By performing six independent tests, the probability of a type 1 error is no longer 0.05 but instead 0.265.

level at which the null hypothesis is rejected

After accounting for the possibility of simultaneous inference (by adjusting p-values), and searching for consistent impacts across all periods, we can only conclude that, after six months of benefiting from REAP, beneficiaries have higher income per capita. These changes are economically significant in both periods, and they represent an improvement of 45.4% over the control group mean (or 0.260 SDs) at $t = 1$ and 32.6% over the control group mean (or 0.236 SDs) at $t = 2$.

However, and somewhat surprisingly, these changes do not seem to translate into changes in monthly expenditure per capita which, although positive, are much less precisely estimated. This is especially true during $t = 2$, when we can reject the equality between increases in income and expenditure (p-value=0.048). [20]

One explanation for this discrepancy is that additional income is being allocated to asset accumulation rather than consumption. Our data offers some support to this explanation, in particular for $t = 2$, during which we observe increases in savings and assets (both livestock and other assets) and a reduction in the number of nights a child has gone to bed hungry. Despite this apparent difference in the impact of T_1 between periods, with the effects being generally more positive in the second period, we can never reject the null hypothesis of equality of impact across periods (equation 2). [21]

Limiting our discussion to the changes identified in $t = 2$, we can conclude that, as with income per capita, changes in wealth (savings and assets) are economically important: per capita savings are 37.5% higher among compared beneficiaries (or 0.220 SDs), while durable asset ownership is higher by 26.1% (or 0.111 SDs). Finally, livestock ownership is also significantly higher in the second period (at the 10% level) with participants in the treatment

[20] However, we cannot reject this equality during $t = 1$ at the usual levels of significance (p-value=0.119).

[21] Depending on outcome, the q-values are between 0.330 and 0.537. Specific results are available from the authors on request.

group owning 15.7% (or 0.128 SDs) more livestock per capita compared to the control group. We discuss the possible reasons for the differences across periods in section 4.3, after the analysis of the one year impact of the program, to which we now turn.

4.2 The one year impact of REAP

Table 4, panel B provides estimates of the combined impact of T_1 and T_2 (i.e. $\hat{\beta}(T_1(1) + T_2(2))$, after one year of participation in REAP. These estimates are in line with the ones presented in panel A, (i.e. the impact of T_1), with treated participants reporting significantly higher income per capita, savings per capita, and asset ownership. After one year of participation in REAP, income per capita is 34.0% (0.246 SDs) higher compared to the control group mean and savings per capita is 131.4 % (0.769 SDs) higher compared to the control group mean, with both increases statistically significant at the 5% level of significance.

As before, we find that the increase in household income does not translate to an increase in expenditure, which in fact decreases by 6.1% (0.061 SDs), although this decrease is not statistically significant. We find a similar impact on livestock and durable asset ownership at one year compared to six months, with both outcomes increasing as a result of REAP. The impact of REAP on the durable asset index represents a 28.6% (0.122 SDs) increase over the control group mean, and the impact on livestock represents a 12.5% (0.102 SDs) increase over the control group mean. However, only the increase in the durable asset index is statistically significant (at the 10% level). The estimates in Table 4 also reveal that participation in REAP results in a decrease in the instances in which a child is reported as going to bed hungry in the past week, a decrease that is statistically significant at the 10% level and represents a 21.5% (0.141 SDs) decrease compared to the control group mean.

Since T_2 is never implemented in isolation, we can only estimate its im-

pact conditional on the implementation of T_1. As argued above, treated individuals may have changed in ways that are different to control individuals (experience in managing a business as part of a group, for example), making the impact of the second set of interventions unidentifiable without further assumptions.

We find that T_2 has a positive and statistically significant impact on savings per capita, with participants saving 106.7% more compared to the control group mean (Table 5). This impact is expected since one of the interventions in T_2 provides training on savings and helps participants to establish savings groups. We do not find any significant impacts on other outcomes of interest after adjusting for FDR.

4.3 Discussion

Income. The Rural Entrepreneur Access Project significantly increased the income earned by participants in the short-term (i.e., 6 months and 1 year after participation in the program). The obvious mechanism through which the program may have led to this outcome is the formation of new micro-enterprises. One important question is whether such new enterprises crowd-out existing sources of income.

The results presented in Table 6 directly address this question by disaggregating income changes by source. The first conclusion is that the overall increase in income is being driven by changes in income from non-agricultural trade, which includes income from the REAP micro-enterprise. The increase in income from non-agricultural trade is statistically significant at the 5% level of significance and this effect persists for up to one year after being enrolled in REAP. The second conclusion is that increased activity does not crowd out other sources of income, suggesting that the program is bringing idle resources into productive activities.

However, the results point to the importance of seasonality in the evaluation of this program. The endline survey was collected during the dry

season which happened to follow a wet season of below average rainfall. This resulted in critical levels of food insecurity in the region which led to the increased distribution of relief food and other humanitarian aid, which may have crowded out usual consumption expenditure resulting in lower incomes for business owners, as reported in Table 6 (recall that over 95% of businesses are involved in petty trade of primarily food items).[22]

The dry conditions may have also forced households with smaller livestock herds to turn to other sources of food to meet their dietary needs since milk production would have been lower. Such households tend to be poorer on average and are seen as less creditworthy. To avoid extending credit to persons that may be unable to repay their debts, businesses in this region are known to decrease the amount of stock they carry during droughts. We find evidence of the employment of this strategy among REAP participants, with the ratio of the value of stock held by the business to the total value of the business decreasing from 0.336 in the first period to 0.209 in the second period, when the endline survey was collected. [23] By carrying less stock businesses may feel less inclined to sell goods on credit, but they also reduce the amount of income they can earn. [24]

Savings. As mentioned in section 2, after six months of participation in the program participants receive training on savings, including on the functioning of Savings Groups . After this training, more than 95% of participants join a SG, a decision that is both voluntary and individual (while at baseline only 10% were members of pre-existing SGs). It is therefore not surprising that

[22]See `http://reliefweb.int/sites/reliefweb.int/files/resources/` for an assessment of the conditions in Marsabit County in January 2014, one month before the endline survey was conducted.

[23]We find that the total value of the business (i.e. the sum of cash on hand, business savings and credit outstanding, and business stock and assets) is significantly higher at $t = 2$ (for both sets of participants) compared to the business value at $t = 1$ (PPP USD 374.61 and PPP USD 451.55 for the six month and one year groups at $t = 2$, respectively, compared to PPP USD 305.50 for the six month group at $t = 1$)

[24]The increase in income from non-agricultural trade is significantly lower in $t = 2$ for both treatment groups compared to $t = 1$.

after one year of participation in REAP, participants have saved more per capita.

What might be surprising is that we also find that after six months in REAP, and before the training on savings, participants have also saved more per capita. This points to a shift in savings behavior that takes place even before the formal introduction of savings groups. If we look more closely at the savings mechanisms used by women (Table 7) we see that after six months REAP participants are saving more at home compared to the control group.

Asset Ownership. Average livestock ownership among both the treatment and control groups has increased from baseline (0.669 TLU per capita) to midline (1.070 TLU per capita) to endline (1.405 TLU per capita), and, given the economic and social importance of livestock among participants, one would expect some of the increased income from entrepreneurial activities to be invested in the acquisition of livestock. We do find increased livestock ownership among REAP participants, which is in line with our expectations. By providing participants with an alternative source of income, REAP enables households to increase their herd size which is essential for pastoralist households to escape the poverty trap and to be able to recover from shocks that can push them back into poverty (Little et al., 2008), providing further evidence of how REAP can lead to sustained increases in well-being and graduate participants from ultra-poverty. Treated households also invest more in durable assets such as blankets, mosquito nets and latrines, which improve the living conditions of their households.

Graduation from poverty. The main aim of this program is to graduate participants from poverty, which we equate with being above the Kenya rural poverty line as reported by the of Statistics (2007). In Table 8 we provide estimates of the impact of REAP on the probability of being non-poor at six months and one year after the start of the program, when poverty lines are defined in terms of income or expenditure.

We find that beneficiaries are more likely to have incomes above the poverty line both after six months and one year of participation in REAP, and these effects are statistically significant at the 1% level. At $t = 1$ ($t = 2$) we find that T_1 increases the probability that beneficiaries are above the poverty line by 12.6% (6.6%), an effect that represents a 74.3% (39.6%) increase over the control group probability of being above the poverty line. The effects are similar at one year, with beneficiaries being 12.9% more likely to have incomes above the poverty line (a 77.0 % increase over the control group). When looking at the impact on the probability that a beneficiary has expenditure above the poverty line we find a slight increase in the treated group at $t = 1$ and a slight decrease at $t = 2$. However, none of these impacts are statistically significant at conventional levels.

Impact Heterogeneity. We next consider the evidence for differentiated impacts of REAP across the distribution of outcomes. In Table 9 we present quantile regression estimates at the 10th, 25th, 50th, 75th, and 90th percentiles of the distribution of outcomes, at six months (panels A and B) and one year (panel C). In Figure 3 we graph the quantile regression estimates for each of the 99 percentiles of the distribution of outcomes, again distinguishing for the duration of participation in the program (six months vs. one year) and the two periods of data collection. [25]Taken together, these results suggest several conclusions.

The first is that the effects on income are positive and statistically significant at each of the five quantiles reported in Table 9, and these effects are increasing with the quantile of the distribution. [26] This is true for both

[25]Quantile regressions were estimated with the user-written command -qreg2- which allows for standard errors that are robust to intra-cluster correlation (**?**). We were unable to reliably estimate quantile regressions for the outcome "number of nights that a child has gone to bed hungry in the past week", as this variable does not have a well-behaved density. We were also unable to estimate quantile regressions on savings per capita at $t = 1$ for the following percentiles: 0.02, 0.03, 0.04, 0.07, 0.08, 0.09, 0.11, 0.12, 0.13, 0.16, 0.19.

[26]The one exception is the 6 month effect (at $t = 2$) for the 50th percentile, which is not statistically significant at conventional levels.

time periods and irrespective of the length of participation in the program. Hence, it seems possible to conclude that REAP was particularly effective, in terms of increases in income and in the short-run, for those who were better-off (relatively speaking, as we are still talking of extremely poor populations): the effect of the program estimated at the 90th percentile is almost four times the effect at the 10th percentile. If the motivation of the poverty graduation approach is to include the ultra-poor, we can then conclude that this approach may take longer (or require modifications) for those who are at the bottom of the distribution.

The second conclusion is that we also observe more pronounced effects among individuals in the upper quantiles of the other outcome distributions. These patterns are clearly illustrated in Figure 3 where we see larger treatment effects for those in the upper quantiles of the savings, livestock and durable asset distributions, particularly when these effects are measured at $t = 2$.

The third conclusion is that the timing of measurement of the impact of the program ($t = 1$ vs. $t = 2$) seems to matter more in terms of shaping the effect of the program than the length of exposure to the program (six months vs. one year), which likely reflects the importance of seasonality in the context we study. The exception to this conclusion is, clearly, savings for which we find evidence suggesting that the lack of access to savings institutions (or lack of awareness about their functioning) may have prevented individuals from keeping liquid savings. When these constraints are removed (through the promotion of savings groups) we find significant treatment effects across the entire distribution and not just the upper quantiles. [27]

Finally, we would expect that those individuals with higher incomes (who gain most from REAP, in terms of income) would also be the ones who would show higher effects of participating in the program in terms of other vari-

[27] Note that before the introduction of savings groups, we only observe significant effects on savings in the upper quantiles (75th and 90th at $t = 1$, and 90th at $t = 2$) of the savings distribution.

ables such as savings or investment in livestock or other durables. The similarity in the patterns exhibited in Table 9 and Figure 3 could be thought to suggest some support to that expectation. To determine if this is true, we check whether individuals occupy similar quantile positions in the conditional distribution of income and of other outcome variables. In Table 10 we present the proportion of individuals who are in the 90th percentile of different combinations of outcome variables. It turns out that, for most pairs of outcome variables, less than 25% of individuals are in similar places in the distribution of outcomes. This result suggests that beneficiaries may employ different strategies, with some choosing to invest more in productive assets such as livestock, with others opting for durable assets or liquid savings and others choosing to consume. Such fundamental heterogeneity is reminiscent of the distinction between subsistence and transformative entrepreneurship (Schoar, 2009) but we leave a deeper analysis of these differences for future research.

Comparison of our findings to other studies. Finally, it seems also important to notice that our estimates of the impact of this program are of a similar order of magnitude of previous studies, namely Banerjee, Duflo, et al. (2015) and Bandiera et al. (2013). After one year, we find a 34% increase in income compared to the control group, similar to the increases in income that can be estimated from the results presented in Banerjee, Duflo, et al. (2015) and Bandiera et al. (2013).[28] The estimate of the impact of the program on savings (131.4% increase) is also similar to those estimated by Banerjee, Duflo, et al. (2015) who report a 155.5% increase after two years and 95.7% increase after three years. Our indicator of food security (number of nights that child has gone to bed hungry in the past week) is most similar to the variable "everyone in the household gets enough food everyday" reported on by Banerjee, Duflo, et al. (2015). They find that this variable improves by

[28]Banerjee, Duflo, et al. (2015) find an average increase of 25.7% (22.8%) across four sources of income after two years (three years), and Bandiera et al. (2013) find a 38% increase in income after four years.

10% (20%) after two years (three years) and we find a similar result, with our indicator improving by 21.5% after one year. Overall we find that REAP increases the probability of being above the poverty line by 12.9% which is similar to the 11% shift in women out of extreme poverty estimated by Bandiera et al. (2013).

5 Conclusion

In this paper we study a multifaceted approach to poverty alleviation that is being increasingly recognised for its ability to set ultra-poor households on a sustainable pathway out of extreme poverty (Bandiera et al., 2013; Banerjee, Duflo, et al., 2015). By providing these households with capital and skills, the Rural Entrepreneur Access Project has empowered disadvantaged women, enabling them to successfully run microenterprises that have led to improved household incomes. These short-run impacts are economically significant and allow women to meet current household needs (through increased incomes) and plan for future shocks (through the accumulation of liquid savings).

We show that a variation of the BRAC approach, that excludes consumption support, replaces asset transfers with cash transfers, and targets groups instead of individuals, improves the well-being of participants, at least in the short-run. The estimates of the impact of this program are, largely, in line with other evaluations of similar programs (Bandiera et al., 2013; Banerjee, Duflo, et al., 2015). And, although the existing data does not allow us to examine the sustainability of the impacts once participants stop receiving support, the similarity in results between our analysis and others, makes us hopeful that we should expect these impacts to improve or at worst, remain the same over time.[29] Whether that hope is validated has to be left for future

[29]Banerjee, Duflo, et al. (2015) examine two year and three year impacts and find no evidence of mean reversion of the impacts. Bandiera et al. (2013) look at two year and four year impacts and find more pronounced effects across many outcomes after four years compared to after two years.

research.

We are also able to demonstrate the potential for this approach to be applied in a different, more extreme context to those already studied. The Rural Entrepreneur Access Project was implemented in some of the most difficult to work in locations, with low population densities, insecurity, extreme weather conditions, low infrastructure, and limited access to markets being just some of the challenges faced by the population. Yet, women were able to make use of the capital and skills delivered through REAP to establish and run successful enterprises.

References

Allen, H. (2006). Village Savings and Loans Associations: sustainable and cost-effective rural finance. *Small Enterprise Development*, *17*(1), 61–68.

Anderson, M. L. (2008). Multiple inference and gender differences in the effects of early intervention: A reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects. *Journal of the American Statistical Association*, *103*(484), 1481–1495.

Angelucci, M., Karlan, D., & Zinman, J. (2014). *Microcredit impacts: evidence from a randomized microcredit program placement experiment by Compartamos Banco (Working Paper 19827).* National Bureau of Economic Research.

Annan, J., Blattman, C., Green, E. P., Jamison, J., & Lehmann, C. (2015). *The returns to microenterprise development among the ultra-poor: A field experiment in post-war Uganda (forthcoming).* American Economic Journal: Applied Economics.

Bandiera, O., Burgess, R., Das, N. C., Gulesci, S., Rasul, I., & Sulaiman, M. (2013). *Can basic entrepreneurship transform the economic lives of the poor? (No. 7386).* IZA Discussion Papers.

Banerjee, A. V., Duflo, E., Glennerster, R., & Kinnan, C. (2013). *The miracle of microfinance? evidence from a randomized evaluation (Working Paper 13-09)*. Cambridge, MA: MIT, Department of Economics.

Banerjee, A. V., Duflo, E., Goldberg, N., Karlan, D., Osei, R., Parient, ., W., & Udry, C. (2015). A multifaceted program causes lasting progress for the very poor: Evidence from six countries. *Science*, *348*(6236), 1260799.

Banerjee, A. V., Karlan, D., & Zinman, J. (2015). Six randomized evaluations of microcredit: introduction and further steps. *American Economic Journal: Applied Economics*, *7*(1), 1–21.

Bank, W. (2012). (2012). *World Development Report: Jobs*.

Bauchet, J., Morduch, J., & Ravi, S. (2015). Failure vs displacement: Why an innovative anti-poverty program showed no net impact in South India. . *Journal of Development Economics*, *116*, 1–16.

Benjamini, Y., & Hochberg, Y. (1995). Controlling the false discovery rate: a practical and powerful approach to multiple testing. *Journal of the Royal Statistical Society, Series B*, *57*, 289–300.

Booysen, F., Berg, V. D., S., B., R., M., V., M., & Rand, G. D. (2008). Using an asset index to assess trends in poverty in seven Sub-Saharan African countries. *World Development*, *36*(6), 1113–1130.

Bruhn, M. (2011). License to sell: the effect of business registration reform on entrepreneurial activity in Mexico. *The Review of Economics and Statistics*, *93*(1), 382–386.

Bruhn, M., Karlan, D., & Schoar, A. (2013). *The impact of consulting services on small and mediumenterprises: evidence from a randomized trial in Mexico (Research Working Paper Series 6508)*. Washington, DC: World Bank.

de Mel, S., M., D., & Woodruff, C. (2008). Returns to capital in microenterprises: evidence from a field experiment. *Q. J. Econ. 123*, *123*, 1329–1372.

Drexler, A., Fischer, G., & Schoar, A. (2014). Keeping it simple: financial literacy and rules of thumb. *American Economic Journal: Applied Economics*, *6*(2), 1–31.

Elliot, H., & Fowler, B. (2012). *Markets and poverty in northern Kenya: Towards a financial graduation model*. Nairobi, Kenya: Financial Sector Deepening.

Filmer, D., & Pritchett, L. H. (2001). Estimating wealth effects without expenditure dataor tears: An application to educational enrollments in states of India. *Demography*, *38*(1), 115–132.

Gindling, T. H., & Newhouse, D. (2014). Self-employment in the developing world. *World Development*, *56*, 313–331.

Goldberg, N., & Salomon, A. (2011). *Ultra Poor Graduation Pilots: Spanning the gap between charity and microfinance (Commisioned Workshop Paper)*. Valladolid, Spain: 2011 Global Microcredit Summit.

Howe, L. D., Hargreaves, J. R., & Huttly, S. R. (2008). Issues in the construction of wealth indices for the measurement of socio-economic position in low-income countries. *Emerging Themes in Epidemiology*, *5*(3).

Jolis, A., & Yunus, M. (2003). *Banker to the poor: micro-lending and the battle against world poverty*. New York: PublicAffairs.

Karlan, D., & Zinman, J. (2010). Expanding microenterprise credit access: Using randomized supply decisions to estimate the impacts. *Review of Financial Studies*, *23*(1), 433–464.

Little, P. D., McPeak, J., Barrett, C. B., & Kristjanson, P. (2008). Challenging orthodoxies: understanding poverty in pastoral areas of East Africa. *Development and Change*, *39*(4), 587–611.

MacMillan, S. (2013). *An end in sight for extreme poverty. scaling up BRAC's graduation model for the ultra-poor (Briefing Note 1: Ending Extreme Poverty*. New York, NY: BRAC USA. Retrieved from `http://tup.brac.net/images/BRAC\_Briefing\_Document\_on\_TUP.pdf`

Matin, I., Sulaiman, M., & Rabbani, M. (2008). *Crafting a graduation path-*

way for the ultra poor: Lessons and evidence from a BRAC programme (Working Paper 109). Dhaka, Bangladesh: BRAC Research and Evaluation Division.

McPeak, J. G., & Barrett, C. (2001). Differential risk exposure and stochastic poverty traps among East African pastoralists. *American Journal of Agricultural Economics*, *83*(3), 674–679.

Merttens, F., Hurrell, A., Marzi, M., Attah, R., Farhat, M., Kardan, A., & MacAuslan, I. (2013). *Kenya hunger safety net programme monitoring and evaluation component: Impact evaluation final report 2009 to 2012.* Oxford, United Kingdom: Oxford Policy Management.

of Statistics, K. N. B. (2007). Basic report on well-being: Based on kenya integrated household budget survey 2005 "06.

of Statistics, K. N. B., & for International Development, S. (2013). Exploring kenya's inequality: Pulling apart or pooling together?

Schoar, A. (2009). *The divide between subsistence and transformational entrepreneurship.* (MIT, Cambridge, MA)

Silvestri, S., Bryan, E., Ringler, C., Herrero, M., & Okoba, B. (2012). Climate change perception and adaptation of agro-pastoral communities in Kenya. *Regional Environmental Change*, *12*(4), 791–802.

Valdivia, M. (2015). Business training plus for female entrepreneurship? Short and medium-term experimental evidence from Peru. *Journal of Development Economics*, *113*, 33–51.

Warui, H. M., & Kshatriya, M. (2009). Implications of community based management of woody vegetation around sedentarised pastoral areas in the arid northern Kenya. *Field Actions Science Reports*, *3.*. Retrieved from `http://factsreports.revues.org/279`

Figures and tables

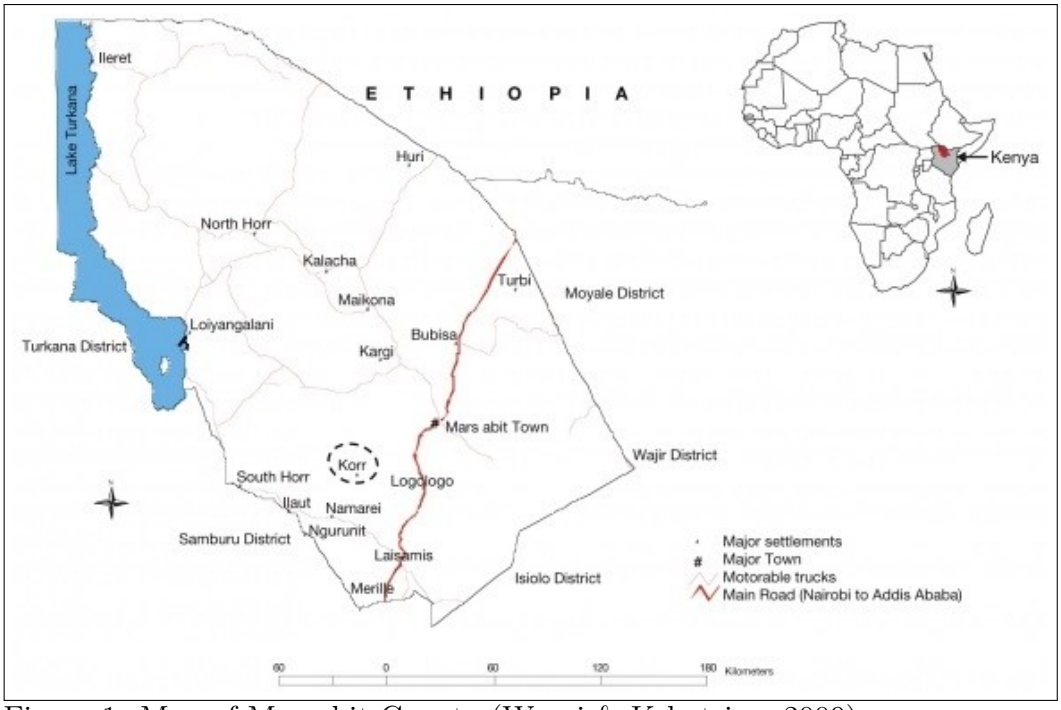

Figure 1: Map of Marsabit County (Warui & Kshatriya, 2009).

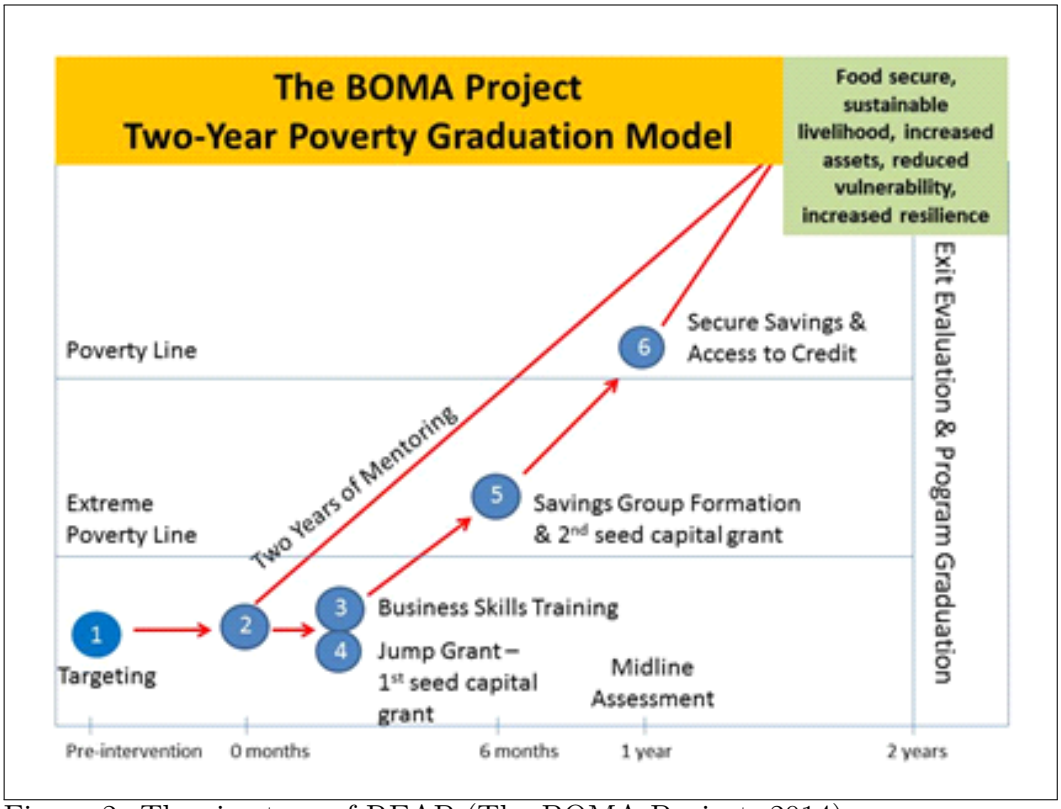

Figure 2: The six steps of REAP (The BOMA Project, 2014).

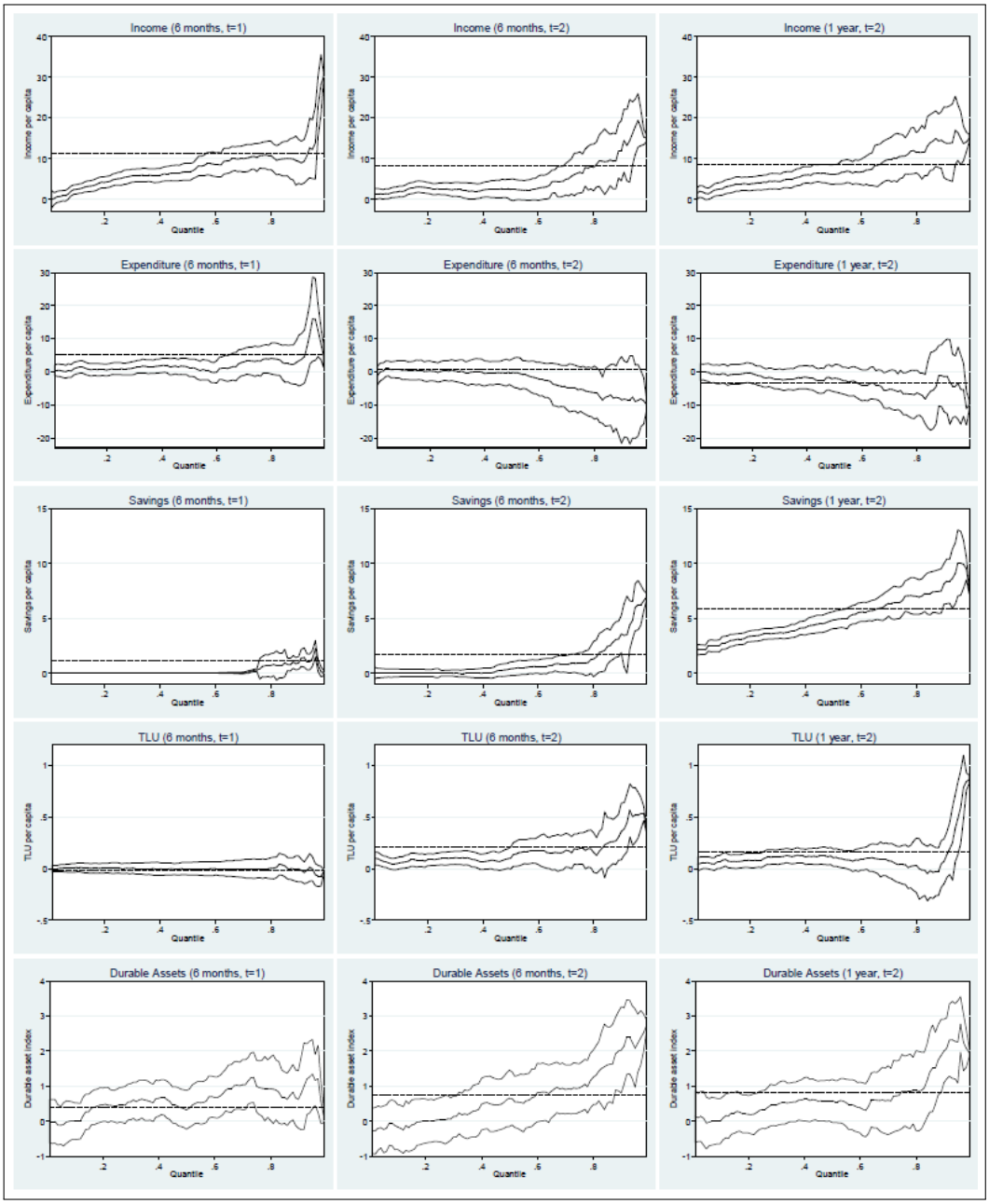

Figure 3: The quantile treatment effects of REAP.

Table 1: Sample sizes (individuals and businesses)

	Group A		Group B		Group C	
	# Women	# Businesses	# Women	# Businesses	# Women	# Businesses
Baseline (Nov 2012)	585 (100%)	195 (100%)	585 (100%)	195 (100%)	582 (100%)	194 (100%)
Midline (Sep 2013)	549 (93.8%)	186 (95.4%)	565 (96.6%)	193 (99.0%)	565 (97.1%)	193 (99.5%)
Endline (Apr 2014)	534 (91.3%)	189 (96.9%)	556 (95%)	192 (98.5%)	561 (96.4%)	190 (97.9%)

Table 2: Summary statistics and balance checks for the treatment and control groups

Variable:	Monthly income per capita	Monthly expenditure per capita	Monthly food expenditure per capita	Monthly non-food expenditure per capita	Total savings per capita	TLU per capita	Durable asset index	Meals per day	Nights that child has gone to bed hungry in past week	Proportion of children in school	Household Size	# children	Married	Years of education	Business Experience	Benefitting from HSNP	Participating in CARE VSLA
Panel A: Means and standard errors of variables at baseline.																	
$\bar{X}_A$	21.770	34.562	24.182	10.380	3.772	0.683	-0.234	1.941	0.549	0.435	5.778	3.875	0.800	0.328	0.576	0.106	0.089
(standard error)	(0.925)	(1.516)	(1.188)	(0.747)	(0.344)	(0.030)	(0.169)	(0.016)	(0.027)	(0.012)	(0.079)	(0.071)	(0.017)	(0.060)	(0.020)	(0.013)	(0.012)
Observations	585	585	585	585	585	585	585	581	578	583	585	585	585	585	585	585	585
$\bar{X}_B$	22.319	34.480	23.862	10.617	3.920	0.640	0.113	1.950	0.576	0.442	5.692	3.737	0.831	0.470	0.562	0.103	0.106
(standard error)	(0.933)	(1.402)	(1.075)	(0.770)	(0.328)	(0.037)	(0.189)	(0.016)	(0.029)	(0.012)	(0.075)	(0.070)	(0.016)	(0.072)	(0.021)	(0.013)	(0.013)
Observations	585	585	585	585	585	585	585	585	579	579	585	585	585	585	585	585	585
$\bar{X}_C$	22.449	32.825	22.494	10.331	5.123	0.684	0.124	1.933	0.576	0.412	5.596	3.711	0.773	0.414	0.538	0.113	0.108
(standard error)	(0.995)	(1.215)	(0.874)	(0.648)	(0.598)	(0.034)	(0.179)	(0.014)	(0.029)	(0.011)	(0.077)	(0.070)	(0.017)	(0.070)	(0.021)	(0.013)	(0.013)
Observations	582	582	582	582	582	582	582	580	572	579	582	582	582	582	582	582	582
Panel B: t test comparison of means of baseline characteristics.																	
$H_0 : \bar{X}_A = \bar{X}_{B+C}$ (p-values)																	
	0.593	0.610	0.466	0.916	0.123	0.540	0.099*	0.994	0.430	0.588	0.163	0.083*	0.919	0.145	0.302	0.899	0.220
$H_0 : \bar{X}_B = \bar{X}_C$ (p-values)																	
	0.927	0.373	0.323	0.776	0.078*	0.379	0.967	0.426	0.991	0.075*	0.373	0.798	0.014**	0.575	0.411	0.551	0.901
$H_0 : \bar{X}_A = \bar{X}_C$ (p-values)																	
	0.617	0.372	0.253	0.961	0.051*	0.307	0.146	0.711	0.496	0.171	0.100	0.102	0.264	0.351	0.192	0.685	0.268

Panel C: F-test from regression of treatment on variables above.[a]

Treatment group	Control group	F-Stat	p-value
A	B and C	0.76	0.723
B	C	1.18	0.283
A	C	1.15	0.308

Note: All monetary values are reported in 2014 USD, PPP terms. [a]Monthly food and non-food expenditure per capita are excluded from this regression.*, ** and ** stand for significant at the 10%, 5% and 1% level of significance, respectively.

Table 3: Testing for anticipation effects

Variable:	Monthly income per capita	Monthly expenditure per capita	Total savings per capita	TLU per capita	Durable asset index	Nights that child has gone to bed hungry in past week
Panel A: Means and standard errors of outcome variables for participants in groups B and C.						
$\bar{X}_B$	26.263	49.906	3.683	1.031	2.014	0.463
(standard error)	(2.198)	(1.949)	(0.408)	(0.052)	(0.265)	(0.058)
Observations	566	566	566	566	566	546
$\bar{X}_C$	23.437	51.703	3.178	1.119	2.086	0.565
(standard error)	(1.354)	(2.191)	(0.544)	(0.057)	(0.272)	(0.044)
Observations	567	567	567	567	567	545
Panel B: t test comparison of means of outcome variables for participants in groups B and C.						
$H_0 : \bar{X}_B = \bar{X}_C$ (p-values)						
	0.274	0.540	0.458	0.254	0.848	0.163

Panel C: F-test from regression of treatment on variables above.

F-Stat	p-value
0.99	0.432

Note: All monetary values are reported in 2014 USD, PPP terms.

Table 4: The short-term impacts of REAP on household outcomes

Outcome:	Monthly income		Monthly expenditure		Total savings		TLU		Durable asset index		Nights that child has gone to bed hungry	
	$t=1$	$t=2$	$t=1$	$t=2$	$t=1$	$t=2$	$t=1$	$t=2$	$t=1$	$t=2$	$t=1$	$t=2$
Panel A: Estimates of the impact of $T_1(t)$												
$\hat{\beta}(T_1(t))$	11.276***	8.238***	5.079	0.819	1.095	1.666**	-0.016	0.205**	0.379	0.743*	-0.112*	-0.184**
	(2.822)	(2.790)	(3.417)	(4.015)	(0.573)	(0.739)	(0.060)	(0.100)	(0.333)	(0.399)	(0.061)	(0.080)
	[0.001]	[0.018]	[0.207]	[0.838]	[0.130]	[0.050]	[0.795]	[0.063]	[0.306]	[0.076]	[0.130]	[0.050]
Observations	1682	1117	1682	1117	1682	1117	1682	1117	1682	1117	1597	1089
R-squared	0.107	0.110	0.280	0.102	0.116	0.105	0.331	0.224	0.318	0.408	0.260	0.089
Control group mean	24.849	25.232	50.805	57.394	3.430	4.440	1.075	1.303	2.050	2.843	0.514	0.789
Panel B: Estimates of the impact of $T_1(1)+T_2(2)$.												
$\hat{\beta}(T_1(1)+T_2(2))$		8.589***		-3.509		5.832***		0.163		0.814**		-0.170**
		(2.232)		(3.453)		(0.789)		(0.107)		(0.368)		(0.084)
		[0.001]		[0.310]		[0.001]		[0.153]		[0.054]		[0.065]
Observations		1095		1095		1095		1095		1095		1068
R-squared		0.127		0.126		0.148		0.272		0.424		0.103
Control group mean		25.232		57.394		4.440		1.303		2.843		0.789

Note: Regressions include controls for loans taken from REAP savings groups, number of REAP businesses in a manyatta, non-REAP businesses in a location and sub-location fixed effects. Robust standard errors, clustered at the business group level, are shown in parentheses, while q-values, using the Benjamini-Hochberg step-up method, are shown in brackets. All monetary values are reported in 2014 USD, PPP terms. *, ** and ** stand for significant at the 10%, 5% and 1% level of significance, respectively (based on unadjusted p-values).

Table 5: The estimated impact of T_2 conditional on the participant receiving T_1

	$\hat{\beta}(T_1(1))$	$\hat{\beta}(T_1(1)+T_2(2))$	$\hat{\beta}(T_1(1)+T_2(2))$ $-\hat{\beta}(T_1(1))$	q-value for all 6 hypotheses
Monthly income per capita	11.276*** (2.822)	8.589*** (2.232)	-2.687 [0.398]	0.478
Monthly expenditure per capita	5.079 (3.417)	-3.509 (3.453)	-8.588** [0.048]	0.144
Total savings per capita	1.095* (0.573)	5.832*** (0.789)	4.737*** [0.000]	0.001***
TLU per capita	-0.016 (0.060)	0.163 (0.107)	0.179* [0.094]	0.188
Durable asset index	0.379 (0.333)	0.814** (0.368)	0.435 [0.207]	0.311
Nights that child has gone to bed hungry in past week	-0.112* (0.061)	-0.170** (0.084)	-0.058 [0.554]	0.554

Note: In columns (1) and (2) robust standard errors reported in parentheses. In column (3) p-values from a Wald test of the null hypothesis $H_0 : \hat{\beta}(T_1(1) = \hat{\beta}(T_1(1)+T_2(2))$ are reported in squared brackets. In column (4) we estimate q-values based on the p-values reported in column (3).
*, ** and ** stand for significant at the 10%, 5% and 1% level of significance, respectively (based on unadjusted p-values).

Table 6: The impact of REAP on various sources of income

Variable:	Monthly total income per capita	Monthly income from livestock per capita	Monthly income from other agriculture per capita	Monthly income from non-agri trade per capita	Monthly income from labor per capita	Monthly income from transfers per capita
Panel A: The impact at six months measured at $t = 1$						
$\hat{\beta}(T_1(1))$	11.276***	1.368	-0.013	9.863***	-0.085	0.005
	(2.822)	(2.462)	(0.069)	(0.976)	(0.301)	(0.223)
Observations	1682	1682	1682	1682	1682	1682
R-squared	0.107	0.091	0.123	0.288	0.162	0.108
Control group mean	24.849	19.817	0.117	3.031	1.205	0.678
Panel B: The impact at six months measured at $t = 2$						
$\hat{\beta}(T_1(2))$	8.238***	3.649	0.109	4.480***	0.192	-0.156
	(2.790)	(2.432)	(0.102)	(0.589)	(0.951)	(0.261)
Observations	1117	1117	1117	1117	1117	1117
R-squared	0.110	0.118	0.135	0.144	0.073	0.104
Control group mean	25.232	19.811	0.172	1.534	2.911	0.803
Panel C: The impact at one year measured at $t = 2$						
$\hat{\beta}(T_1(1) + T_2(2))$	8.589***	1.602	0.077	5.853***	0.987	-0.062
	(2.232)	(1.810)	(0.099)	(0.772)	(1.093)	(0.277)
Observations	1095	1095	1095	1095	1095	1095
R-squared	0.127	0.121	0.172	0.174	0.128	0.100
Control group mean	25.232	19.811	0.172	1.534	2.911	0.803

Note: Regressions include sub-location fixed effects, in addition to control variables for 1) loans taken from REAP savings groups, and 2) the number of REAP businesses in a manyatta and 3) non-REAP businesses in a location. Robust standard errors in parentheses. Standard errors are clustered at the business group level. All monetary values are reported in 2014 USD, PPP terms. *, ** and ** stand for significant at the 10%, 5% and 1% level of significance, respectively (based on unadjusted p-values).

Table 7: The impact of REAP on savings held using various mechanisms.

Variable:	Personal savings percapita	Non-BOMA savings group savings per capita	BOMA savings group savings per capita
Panel A: The impact at six months measured at $t = 1$			
$\hat{\beta}(T_1(1))$	1.038*	0.055	-
	(0.568)	(0.174)	-
Observations	1682	1682	-
R-squared	0.119	0.098	-
Control group mean	3.030	0.400	0
q-value	0.136	0.750	-
Panel B: The impact at six months measured at $t = 2$			
$\hat{\beta}(T_1(2))$	1.624***	-0.010	-
	(0.552)	(0.170)	-
Observations	1117	1117	-
R-squared	0.107	0.124	-
Control group mean	4.082	0.357	0
q-value	0.006***	0.952	-
Panel C: The impact at one year measured at $t = 2$			
$\hat{\beta}(T_1(1) + T_2(2))$	1.450***	0.246	4.141***
	(0.544)	(0.303)	(0.131)
Observations	1095	1095	1095
R-squared	0.087	0.063	0.605
Control group mean	4.082	0.357	0
q-value	0.012**	0.417	0.001***

Note: Regressions include sub-location fixed effects, in addition to control variables for 1) loans taken from REAP savings groups, and 2) the number of REAP businesses in a manyatta and 3) non-REAP businesses in a location. Robust standard errors in parentheses. Standard errors are clustered at the business group level. All monetary values are reported in 2014 USD, PPP terms. Personal savings includes savings kept at home and savings kept at a formal financial institution including mobile service providers. *, ** and ** stand for significant at the 10%, 5% and 1% level of significance, respectively (based on unadjusted p-values).

Table 8: The impact of REAP on the probability of living above the poverty line.

Variable:	Proportion of households with monthly income per adult equivalent above the poverty line		Proportion of households with monthly expenditure per adult equivalent above the poverty line	
Panel A: Estimates of the six month impact at $t = 1$ and $t = 2$.				
	$t = 1$	$t = 2$	$t = 1$	$t = 2$
$\hat{\beta}(T_1(t))$	0.126***	0.066***	0.034	-0.003
	(0.024)	(0.025)	(0.027)	(0.032)
Observations	1682	1117	1682	1117
R-squared	0.179	0.092	0.335	0.113
Control group mean	0.169	0.168	0.500	0.581
Panel B: $H_0 : \hat{\beta}(T_1(1)) = \hat{\beta}(T_1(2))$				
	0.082*		0.365	
Panel C: Estimates of the one year impact.				
		$t = 2$		$t = 2$
$\hat{\beta}(T_1(1) + T_2(2))$		0.129***		-0.017
		(0.027)		(0.032)
Observations		1095		1095
R-squared		0.123		0.102
Control group mean		0.168		0.581

Note: Estimates from a linear probability model. Regressions include sub-location fixed effects, in addition to control variables for 1) loans taken from REAP savings groups, and 2) the number of REAP businesses in a manyatta and 3) non-REAP businesses in a location. Robust standard errors in parentheses. Standard errors are clustered at the business group level.

The Kenya rural poverty line used is as defined by the of Statistics (2007) which, after conversion, is estimated to be 77.069 USD per month and per adult equivalent in PPP 2014 terms.

*, ** and ** stand for significant at the 10%, 5% and 1% level of significance, respectively (based on unadjusted p-values).

Table 9: The quantile treatment effects of REAP

Outcome	OLS Estimates	10th percentile	25th percentile	50th percentile	75th percentile	90th percentile
Panel A: Treatment effects at six months (at $t = 1$)						
Monthly income per capita	11.276***	2.446***	5.041***	6.753***	10.266***	9.292***
	(2.822)	(0.690)	(0.933)	(1.264)	(1.997)	(3.274)
Monthly expenditure per capita	5.079	1.357	0.882	1.195	3.222	3.307
	(3.417)	(1.130)	(1.093)	(1.586)	(2.638)	(4.605)
Total savings per capita	1.095*	0.000	0.000	-0.000	0.265***	0.919***
	(0.573)	(0.000)	(0.000)	(0.000)	(0.074)	(0.353)
TLU per capita	-0.016	0.011	0.007	0.000	0.000	-0.001
	(0.060)	(0.026)	(0.030)	(0.037)	(0.056)	(0.076)
Durable asset index	0.379	-0.000	0.428	0.322	0.998**	0.622
	(0.333)	(0.317)	(0.294)	(0.334)	(0.440)	(0.498)
Panel B: Treatment effects at six months (at $t = 2$)						
Monthly income per capita	8.238***	1.931**	2.560***	2.393	6.610**	11.889***
	(2.790)	(0.795)	(0.852)	(1.605)	(2.966)	(4.331)
Monthly expenditure per capita	0.819	0.437	0.347	-0.259	-5.305	-8.576
	(4.015)	(1.732)	(1.825)	(2.377)	(4.339)	(8.033)
Total savings per capita	1.666**	-0.000	-0.008	0.384	0.897	3.552***
	(0.739)	(0.216)	(0.151)	(0.378)	(0.577)	(1.018)
TLU per capita	0.205**	0.055	0.097***	0.102	0.195**	0.359**
	(0.100)	(0.036)	(0.037)	(0.065)	(0.093)	(0.151)
Durable asset index	0.743*	-0.167	0.036	0.650	0.944**	2.033***
	(0.399)	(0.389)	(0.371)	(0.494)	(0.418)	(0.746)
Panel C: Treatment effects at one year (at $t = 2$)						
Monthly income per capita	8.589***	2.611***	4.210***	6.080***	10.558***	13.985***
	(2.232)	(0.985)	(1.113)	(1.485)	(3.316)	(5.318)
Monthly expenditure per capita	-3.509	-0.715	-1.960	-1.988	-6.689*	-1.503
	(3.453)	(1.720)	(1.669)	(2.496)	(3.564)	(6.110)
Total savings per capita	5.832***	2.744***	3.566***	4.826***	6.636***	8.139***
	(0.789)	(0.349)	(0.294)	(0.473)	(0.820)	(1.134)
TLU per capita	0.163	0.081*	0.097**	0.106**	0.037	0.070
	(0.107)	(0.042)	(0.044)	(0.050)	(0.111)	(0.131)
Durable asset index	0.814**	-0.000	0.330	0.548	0.851	2.005***
	(0.368)	(0.390)	(0.327)	(0.371)	(0.644)	(0.549)

Note: Regressions include sub-location fixed effects, in addition to control variables for 1) loans taken from REAP savings groups, and 2) the number of REAP businesses in a manyatta and 3) non-REAP businesses in a location. Robust standard errors in parentheses. Standard errors are clustered at the business group level. All monetary values are reported in 2014 USD, PPP terms.

*, ** and ** stand for significant at the 10%, 5% and 1% level of significance, respectively.

Table 10: Overlap between individuals in the 90th percentile of the outcome distribution

Outcome	Monthly income per capita	Monthly expenditure per capita	Total savings per capita	TLU per capita	Durable asset index
Panel A: Overlap at six months (at $t = 1$)					
Monthly income per capita	1	0.254	0.148	0.231	0.183
Monthly expenditure per capita		1	0.195	0.166	0.195
Total savings per capita			1	0.112	0.225
TLU per capita				1	0.101
Durable asset index					1
Panel B: Overlap at six months (at $t = 2$)					
Monthly income per capita	1	0.286	0.169	0.214	0.250
Monthly expenditure per capita		1	0.143	0.134	0.205
Total savings per capita			1	0.125	0.232
TLU per capita				1	0.134
Durable asset index					1
Panel C: Overlap at one year (at $t = 2$)					
Monthly income per capita	1	0.236	0.182	0.109	0.282
Monthly expenditure per capita		1	0.155	0.127	0.191
Total savings per capita			1	0.109	0.236
TLU per capita				1	0.118
Durable asset index					1

Note: Figures represent the proportion of overlap between individuals in the 90th percentile of the two corresponding outcome distributions.

Appendix A Timeline of activities

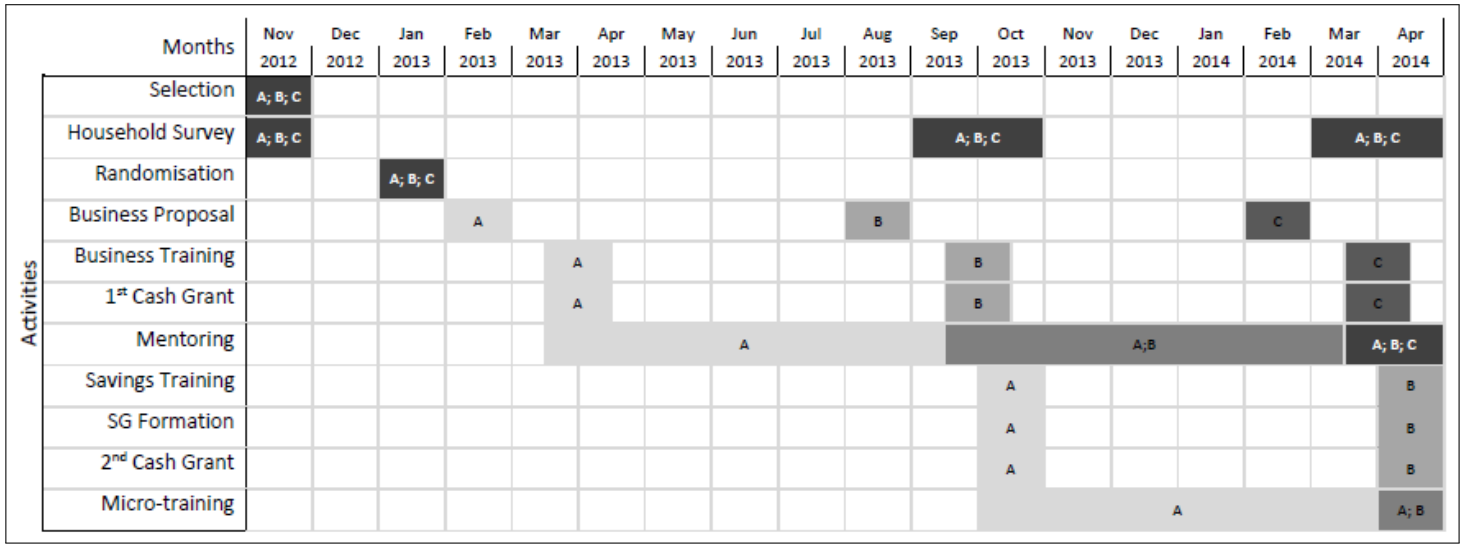

Figure A1: Timeline of data collection and program activities, by assignment to funding cycle. We label the beneficiaries who entered the program in the first cycle as group *A*, and subsequent groups as *B* and *C*.

Appendix B Pre-existing businesses

Table B1: Population and number of businesses by location.

Location	Population[a]	Pre-existent businesses	Businesses formed between March 2013 and April 2014[b]
1	13012	241	60
2	8357	159	30
3	7000	146	30
4	7800	227	30
5	4078	99	30
6	3300	70	30
7	10238	167	60
8	8935	131	60
9	4226	87	30
10	11220	289	60
11	3076	27	30
12	4065	56	30
13	8030	89	60
14	11223	144	44

Note: Information on the number of businesses was provided by the BOMA Project.

[a]Population numbers are based on the 2009 Kenya Census.

[b]One-third of new REAP businesses were formed in each round of funding with the exception of location 14 where 14 businesses were formed in March/April 2014 and 15 businesses in each of the two previous rounds.

Appendix C Durable asset index

Filmer and Pritchett (2001) were among the first to suggest the use of principle component analysis (PCA) to aggregate several asset ownership variables into a single dimension. Principle component analysis was seen as a more methodologically sound way of assigning weights to the variables that comprise an index compared to other methods, such as simple summation or the use of asset values. However, the use of PCA for this purpose has come

under criticism since one of the assumptions underlying PCA is that variables are continuous and normally distributed which is violated when discrete variables are included in the analysis (Howe, Hargreaves, & Huttly, 2008). Multiple correspondence analysis (MCA) has been suggested as an alternative approach that is analogous to PCA but is better suited for use with discrete data (Booysen et al., 2008).

We make use of the approach suggested by Booysen et al. (2008) to create an asset index including information on the ownership of 11 durable assets that were determined in all survey rounds. The assets include: 1) blanket, 2) flask, 3) kitchen, 4) lamp, 5) latrine, 6) mattress, 7) mobile phone, 8) mosquito net, 9) nylon sheet, 10) slasher, and, 11) spade. Using the *–mca–* command in Stata 13 we find that the first dimension accounts for 47% of the inertia.[30] We use the coordinates reported for the first dimension to generate weights for every asset included in the index. These weights are reported in Table C1.

[30]Inertia is Pearson's chi-squared divided by sample size and is analogous to variance reported on in PCA.

Table C1: Variables and MCA weights used in asset index.

Asset	Category	Weight
Owns a blanket	No	-0.543
	Yes	0.296
Owns a flask	No	-0.883
	Yes	1.395
Owns a kitchen	No	-0.372
	Yes	2.905
Owns a lamp	No	-0.483
	Yes	3.994
Owns a latrine	No	-0.165
	Yes	5.612
Owns a mattress	No	-0.534
	Yes	4.662
Owns a mobile phone	No	-0.354
	Yes	4.343
Owns a mosquito net	No	-1.234
	Yes	0.600
Owns a nylon sheet	No	-0.452
	Yes	0.175
Owns a slasher	No	-0.336
	Yes	0.032
Owns a spade	No	-0.322
	Yes	2.540