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Poverty graduation with cash transfers: a randomized evaluation*

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Key words: Poverty graduation, Cash transfers, Entrepreneurship, Ultra-poor, Field experiment, Africa
JEL classification: C93, D13, J24, O12, O13, Q12

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Abstract

We examine the impact of the Rural Entrepreneur Access Program (REAP), a poverty graduation program that combines multiple interventions with the aim of promoting entrepreneurship among ultra-poor women. The program emphasizes cash transfers (rather than asset transfers) to ultra-poor women, in addition to business skills training, business mentoring and savings. Participation in each of three rounds of the program was randomly determined through a public lottery. In the short-to-medium-run we find that the program has a positive and significant impact on income, savings, asset accumulation, and food security that are similar to more traditional poverty graduation programs that rely on asset transfers.

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Microenterprises are the source of employment for more than half of the labor force in developing countries (de Mel, McKenzie, and Woodruff 2008; Gindling and Newhouse 2014), and are seen as potential engines of economic development by raising income of owners, creating a demand for labor and raising wages, and increasing market competition to generate lower prices for consumers (Bruhn 2011; World Bank 2012). Despite these potential benefits, many policymakers are concerned that some of the world's poorest people, sometimes known as the ultra-poor, are prevented from establishing such businesses or from participating in many popular approaches aimed at stimulating microenterprise formation.

Until very recently microfinance was advocated as a way to overcome financial market imperfections that limited the capacity of the poor to invest in profitable projects (Jolis and

Yunus 2003). However substantial recent evidence, using randomized control trials, points to the limited impact of microfinance on poverty alleviation, particularly for those in the lower tail of the income distribution, suggesting that alleviating credit constraints alone is not sufficient to reduce poverty through microenterprises (Banerjee, Karlan, and Zinman 2015; Karlan and Zinman 2011). This has prompted a shift in attention to other possible constraints, particularly entrepreneurial skills, knowledge and human capital, but results of evaluations of such interventions have been similarly mixed (e.g. Drexler, Fischer, and Schoar 2014; Bruhn, Karlan, and Schoar 2013; Valdivia 2015).

Concerns around limited access to the microenterprise sector among the ultra-poor, that reflect the apparent lack of success of these "one-constraint-at-a-time" approaches, suggested the need for interventions that provide the ultra-poor with a localized "big push" to graduate from poverty by simultaneously addressing the overlapping set of constraints that they face. One influential approach, pioneered by BRAC, is the Challenging the Frontiers of Poverty Reduction - Targeting the Ultra-Poor (CFPR/TUP). This approach is structured as a poverty graduation program: during a limited period (two years), its participants benefit from a set of interventions (initial consumption support and an asset transfer, together with savings services, skills training, and regular follow-up visits) with the expectation that, at the end of that period, participants would be able to participate in microfinance (Matin, Sulaiman, and Rabbani 2008; Goldberg and Salomon 2011).

Several recent impact evaluation studies provide promising support for this approach across a diverse set of developing countries. For instance, 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 38% increase in earnings, which persists up to two years after participants graduate from the program (Bandiera et al. 2013). In a recent multi-site randomized evaluation across six countries, Banerjee 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 find no net impact on consumption, income or asset accumulation. The authors suggest that this result reflects mistargeting of individuals with strong labor market opportunities who quickly selected out of the program, which suggests broader lessons around muted impacts of ultra-poor programs when the opportunity costs to participation are relatively high, and how directed asset transfers could misdirect economic activity. There is also evidence that the asset transfers were liquidated and used to pay down debt, another source of targeting risk.

This paper presents a randomized evaluation of the Rural Entrepreneur Access Project (REAP), a variation of the CFPR/TUP graduation approach, implemented in arid and semi-arid northern Kenya, a region where more than 80% of the population is estimated to be living below the national poverty line (Kenya National Bureau of Statistics and Society for International Development 2013). REAP comprises a baseline package of interventions, including a USD 100 cash transfer to set up a microenterprise, business skills training, and business mentoring, which are followed, six months later, by a USD 50 cash transfer conditional on having an active enterprise, and 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).

This program, while similar in spirit to ultra-poor programs that have been implemented elsewhere, also has a number of notable differences. First, contrary to most other ultra-poor programs, the program relies entirely on the transfer of cash rather than of a physical

asset as a way to increase beneficiaries' wealth. (e.g. Banerjee et al. 2015; Bandiera et al. 2013; Bauchet, Morduch, and Ravi 2015). Although cash transfers have the potential advantage of providing increased flexibility, by allowing beneficiaries to decide on the nature of their investment, they have played a minor role in these programs given concerns about their possible misuse (consumption, payment of existing debt). Cash, when delivered, is mostly conceptualized as consumption support, intended at preventing beneficiaries from "eating" their assets (sometimes literally, in the case of livestock transfers). This concern is potentially more important in the case of REAP given that there was no provision of initial consumption support. Our results show that the structure of the program (in particular, we suspect, the conditionality of the second grant and mentor input), seem enough to direct women toward an enterprise investment.

Second, the program is explicitly enterprise focused, with the requirement that women form three-person groups to jointly run the enterprise. This may provide additional social support and help the enterprises reach a viable scale, while providing additional accountability around the use of grant funds, but may also introduce additional costs in running a business that may, ultimately, be detrimental to its success. Finally, and not necessarily less important, REAP is implemented in the context of very limited market access, with an economy that is based on one activity (raising livestock) that is prone to frequent shocks due to drought. In this context, and in contrast to settings such as that studied by Bauchet, Morduch, and Ravi (2015), participants are unlikely to have even the prospect of other remunerative opportunities, suggesting lower risk of mistargeting of the intervention.

While the program differs from other ultra-poor programs in some respects, the findings are qualitatively similar. We find that, after one year, this program has a positive and statistically significant impact on income (34%), savings (131%) and asset accumulation (both consumer durables (29%) and productive assets (12.5%)), and food security (21.5%). The primary channel for impact is through the setup of new petty trade enterprises, with time use data showing a corresponding tightly estimated reduction in leisure time and household

activity. However we find a weak impact on monetary measures of consumption, and expenditure; if anything there is a small decrease in these variables in the medium-run (one year), at least at the upper percentiles of the wealth distribution, following the introduction and promotion of new savings groups. It is possible that in the medium-run, new asset accumulation and savings activities are absorbing the income increase. We also find the program to be highly cost-effective, with the average increase in household income covering the cost of delivering the program in just over one year.

Our results are similar to those presented in Blattman et al. (2015), in an analysis of a program that shares important similarities with REAP, as it also focuses on enterprise development through a cash transfer (USD 150), short business training, and ongoing supervision. They find that, over a similar evaluation horizon to ours, the program leads to an important increase of microenterprise ownership, mostly in petty trade, and income. They argue that, even with no consumption support and in a context of arguably little accountability around the use of grant funds, recipients are remarkably compliant in directing the funds to enterprise formation (rather than immediate consumption, as feared). A further treatment encouraging the formation of self-help groups, implemented in half of the treatment villages, led to a doubling of the reported earnings of those receiving the additional treatment, with most of the impact apparently due to increased informal finance and economic cooperation, a result that is suggestive of the additional importance of deeper financial services (insurance, in this case; savings, in the case of REAP) in buttressing such interventions.

The results presented in our article complement the recent evidence on ultra-poor interventions, while providing additional corroboration of external validity in a particularly remote and economically-challenging setting. In contrast to ultra-poor programs that focus on a relatively narrow set of enterprises, selected by the implementers of those programs, the REAP program provides a wider agency in how beneficiaries use the relatively small transfers that they receive. Despite this notable difference, the impact results match up

relatively well with recent ultra-poor programs on outcomes, with notable increases in income and assets and little impact on consumption in the initial stages of the program. The pathway of livelihood change is also quite clear, as underemployed women shift away from leisure and household activity and into remunerative petty trade. This suggests some robustness in the implementation of such programs, with room for experimentation in program design in future iterations, for example in using group-based approaches, transferring cash rather than an asset (which can greatly reduce implementation costs), or reducing costs by minimizing initial consumption support.

The remainder of the paper proceeds as follows. In the next section we provide a detailed description of REAP before presenting the identification strategy and the data used in this paper. We are able to take advantage of the 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. We next present results of tests of the assumptions underlying the identification strategy before discussing spillover and anticipation effects. This is followed by the presentation and discussion of the main results.

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 (Kenya National Bureau of Statistics and Society for International Development 2013). [2] 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 et al. 2012), resulting in many households no longer being able to meet their basic needs

due to the loss of herds from which it is hard to recover (Lybbert et al. 2004; Barrett and Santos 2014). Such households are forced into begging, unskilled wage labor, different forms of petty trade, and become reliant on food aid to meet their dietary needs. [3]

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, have limited access to markets or other infrastructure, and face financial and human capital constraints (Elliot and 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.

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. [4] These committees were asked to identify women who were among the poorest of the poor in the community, prioritizing those with no other sources of income besides the business to be formed, who were also considered to be responsible and entrepreneurially minded, and were willing to run a business with two other women. [5] Trained business mentors ensured that the local committees followed these criteria when selecting participants. [6] 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. On the day of program enrollment, all participants were required to attend a short business skills training session, delivered by mentors under the supervision of REAP field officers. [7] Over the course of the program participants benefited from approximately 17 hours of ongoing training. [8]

First Grant and Business Mentoring. At the end of the business skills training session business groups 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. [9] Once the groups received their grants they were free to invest the money, including by making changes in their initial business proposal.

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 exited the program, two years after its start, and over the course of the program each business was expected to benefit from approximately 30 hours of mentoring.

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.

Research design

In this section we provide details of the random allocation of participants to treatment and control groups. We also report on tests of the assumptions underlying the identification strategy and discuss spillover and anticipation effects.

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). [10] Assignment to each cycle was done randomly, through a public lottery that took place in each of the locations from which participants had been recruited, with one-third of the women enrolled in each funding cycle. [11] 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. All eligible women were interviewed at baseline (November 2012) and at two follow-up surveys, con-

ducted at six month intervals and timed to coincide with the beginning of each new funding cycle. [12]

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

Together, the sequential roll-out 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 identify the program impacts in a relatively straightforward way.

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 women across northern Kenya (Merttens et al. 2013). Average monthly expenditure per capita is approximately PPP USD 33.96, which is well below the national 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 in-

secure, with children going to bed hungry at least 2 times a month. Households also own 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 and Barrett 2001). [13] However, more than half 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.

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. We investigate three possible pathways for such influence: lower prices to consumers due to higher competition from new businesses; lower profits of non-REAP businesses due to increased competition from new businesses; and, easier access to loans, given higher savings.

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 impact of increased competition among businesses, may be 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. [14]

A different path through which businesses started by REAP participants may affect the welfare of non-participant households is through a reduction in income from petty trade.

We test for this possibility by examining the income from petty trade earned by participants. In Table 4 we report the average income from non-REAP petty trade for participants in groups *A*, *B* and *C* at baseline and endline. We find that income from petty trade decreases among those still waiting to join the program, i.e. group *C*, by approximately 8% but this decrease is not statistically significant. [15]

Another potential source of spillover effects might be easier access to loans. Although only REAP participants can actively participate in all saving groups' 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 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, respectively, 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). We check for differences in monthly income per capita, monthly expenditure per capita, monthly consumption per capita, savings per capita, TLU per capita, durable asset index, and the nights that a child has gone to bed hungry in the last week, our outcome variables, and find no statistically significant differences between groups *B* and *C*, as shown in Table 3. [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). This is not surprising given that we also find no statistically significant differences in how these two groups of participants allocate their time at midline (see panel A of Table E1 in Appendix E) or in the proportion of women that have ever taken a loan. [18]

More than 90% of loans taken by women in groups *B* and *C* are used to purchase food, with less than 2% of the loans used for investment in a business or livestock while the remainder are mainly used to pay for medical emergencies and school fees. The limited use of loans for investment in businesses can be attributed to the limited access to capital in this region: Osterloh and Barrett (2007), for example, show that the average size of loans available in similar locations in northern Kenya are often not sufficient to cover the cost of transport to sites where provisions can be purchased.

Although these investigations are not sufficient to definitely disprove the possibility of anticipation effects, together they point to their limited importance, if any.

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:

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

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 $X_i(t)$ 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). [19] 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 benefiting 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:

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

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 analyze, 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.

The six month impact of REAP

Table 5, 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. [20] 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 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). [21]

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 a negligible (and statistically insignificant)

decrease in consumption, an increase 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). [22]

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 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 after the analysis of the one year impact of the program, to which we now turn.

The one year impact of REAP

Table 5, 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 or consumption, which in fact decrease by 6.1% (0.061 SDs) and 5.5% (0.074 SDs) respectively, although these decreases are 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 5 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 impact 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 6). 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.

Discussion

Income. The Rural Entrepreneur Access Project significantly increased the income earned by participants in the short-to-medium-run (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 7 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

microenterprise (recall that more than 95% of groups invest in petty trade businesses). 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 business activity does not crowd out other sources of income, suggesting that the program is bringing idle resources into productive activities. When we examine how participants allocate their time resources at $t = 2$ we find that those that have benefited from REAP are spending approximately 6% of their day on REAP related activities on average, and to achieve this increased activity they have decreased the average time spent on leisure and household activities, as well as other productive activities (see Table E2 in Appendix E). [23]

It should be noted that the increase in income from non-agricultural trade is significantly lower in $t = 2$ for both treatment groups compared to $t = 1$. This result points to the importance of seasonality in the evaluation of this program, with the fluctuation in income from $t = 1$ to $t = 2$ likely due to seasonality in production in the region. This is supported by the fact 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 $t = 1$), despite significantly lower incomes from non-agricultural trade at $t = 2$.

Expenditure and consumption. We do not find any significant impacts of REAP on expenditure or consumption but in Table 6 we show that the effect on these outcomes are lower after one year of participation compared to after six months. Recall that one of the roles of mentors is to promote practices that would led to successful businesses. Also recall that after six months of participation in REAP more than 95% of participants join savings groups where they are required to deposit savings on a monthly basis. These two factors are likely to result in the observed dip in consumption and expenditure after one year in REAP as

participants may choose to divert additional income to savings and their businesses instead of additional consumption.

Savings. As previously mentioned, 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 after one year of participation in REAP, participants have saved more per capita.

What might be surprising is that we also find that 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 8) we see that after six months REAP participants are saving more at home compared to the control group.

Livestock and other assets. 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 Kenya National Bureau of Statistics (2007). In Table 9 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 or consumption 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, as expected, given the earlier findings on expenditure and consumption.

Impact Heterogeneity. We next consider the evidence for differentiated impacts of REAP across the distribution of outcomes. In Table 10 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. [24] 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 10, and these effects are increasing with the quantile of the distribution. [25] This is true for both 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-to-medium-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 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 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. [26]

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 variables such as savings or investment in livestock or other durables. The similarity in the patterns exhibited in Table 10 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 distri-

bution of income and of other outcome variables. In Table 11 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, some 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 to previous studies, namely Banerjee 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 et al. (2015) and Bandiera et al. (2013). [27] The estimate of the impact of the program on savings (131.4% increase) is also similar to those estimated by Banerjee 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 et al. (2015). They find that this variable improves by 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). However, as with the other ultra-poor poverty graduation programs, our findings are more conservative compared to those of Blattman et al. (2015), which may be explained both by the post-war setting that they study and the possibility of anticipation effects, that cannot be ruled out in their study.

Turning to the cost-benefit analysis of this program, it is estimated that the cost for one additional woman to be enrolled in REAP in 2015 for two years was approximately USD 300 or PPP USD 713.91 at 2014 prices. This figure is well below the direct costs of the six programs evaluated by Banerjee et al. (2015) as well as the program evaluated by Blattman et al. (2015). Assuming that this was the cost to implement the program in 2013, and ignoring discounting and inflation, the gains in income (which we estimate to be the average of the one year and six month impacts) would have to persist for one additional month for the gains in income to cover the cost of the program.

Conclusion

In this paper we study a multifaceted approach to poverty alleviation that is being increasingly recognized for its ability to set ultra-poor households on a sustainable pathway out of extreme poverty (Banerjee et al. 2015; Bandiera et al. 2013).

We show that a variation of the BRAC approach, the Rural Entrepreneur Access Project (REAP), that provides disadvantaged women with capital, skills, and ongoing mentorship in business and savings, but that excludes consumption support, replaces asset transfers with cash transfers, and targets groups instead of individuals, has enabled beneficiaries to run microenterprises that led to improved household incomes. These short-to-medium-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 and assets). The pathway of change is quite clear, with a tightly-estimated shift of time use from leisure and household activity into non-farm enterprise activity, with 95% of enterprises involved in petty trade of consumer goods.

The estimates of the impact of this program are, largely, in line with other evaluations of similar programs (Banerjee et al. 2015; Bandiera et al. 2013), and with a similar intervention examined by Blattman et al. (2015), with a relatively similar evaluation horizon. And, although the existing data do not allow us to examine the sustainability of the impacts once

participants stop receiving support, the similarity in results between our analysis and prior ultra-poor trials raises a plausible prospect that these impacts should be stable over time. [28] A simple cost-benefit analysis shows that if this were true, the program would cover costs within a reasonable time horizon.

We are also able to demonstrate the potential for this approach to be applied in a different, arguably more extreme context to those already studied. The Rural Entrepreneur Access Project was implemented in some difficult to work in locations, with very low average population densities, highly variable weather conditions, low infrastructure, and limited access to markets. Yet, women were able to make use of the capital and skills delivered through REAP to establish and run successful enterprises. This consistency of results provides important support for the robustness of the poverty graduation approach and further corroborates the external validity of the other studies, while suggesting further opportunities for experimentation in the design and implementation of such programs.

Notes

[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]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 (Kenya National Bureau of Statistics 2007). In 2009 it was estimated that nationally, 45.2% of the population lived below the poverty line (Kenya National Bureau of Statistics and Society for International Development 2013).

[3]Little et al. (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. These households have little or no livestock and tend to be involved in unskilled wage labor and petty trade.

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

[5]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 center and more distant villages. Finally, immediate relatives of any 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.

[6] Mentors are employed at the location level and, prior to the recruitment of participants, participated in a training of trainers program which lasted for five days. Each location comprises many sub-locations which are formed by smaller villages, known as manyattas.

[7]These two sessions took approximately four hours to complete and covered the following content: accounting, financial planning, product ideas, marketing, pricing and costing, inventory management, customer service, business investment and growth strategies, employee management, savings, and debt.

[8]This included a half day training on savings that took place six months after the business training, and nine one-hour training sessions that took place during savings group meetings.

[9]From hereon in, all monetary values reported in the paper are in PPP terms at 2014 prices unless otherwise stated. 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.

[10]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%.

[11]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.

[12]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.

[13]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, or 2 donkeys.

[14]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.

[15]Participants in groups *A* and *B*, however, experience large (and in the case of group *A*, statistically significant) gains in income from their non-REAP petty trade businesses, suggesting that they are able to transfer the training, experience and income gained from participating in REAP to their own individually-owned businesses.

[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]These outcome variables are defined in Appendix C and Appendix D.

[18]Approximately 24.2% (24.3%) of group *B* (*C*) participants have ever taken a loan from banks, MFIs, moneylenders, savings and self-help groups, or family. More than 36% of participants in group *A* have accessed loans and this is statistically significantly higher compared to groups *B* and *C*.

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

[20]By performing seven independent tests, the probability of a type 1 error is no longer 0.05 but instead 0.302.

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

[22]Depending on outcome, the q-values are between 0.387 and 0.537. Specific results are available from the authors on request.

[23]The income generated from other productive activities has not declined in either treatment group at $t = 2$.

[24]Quantile regressions were estimated with the user-written command -qreg2- which allows for standard errors that are robust to intra-cluster correlation (Parente and Santos Silva 2016). 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.

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

[26]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.

[27]Banerjee 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.

[28]Banerjee 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.

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Figures

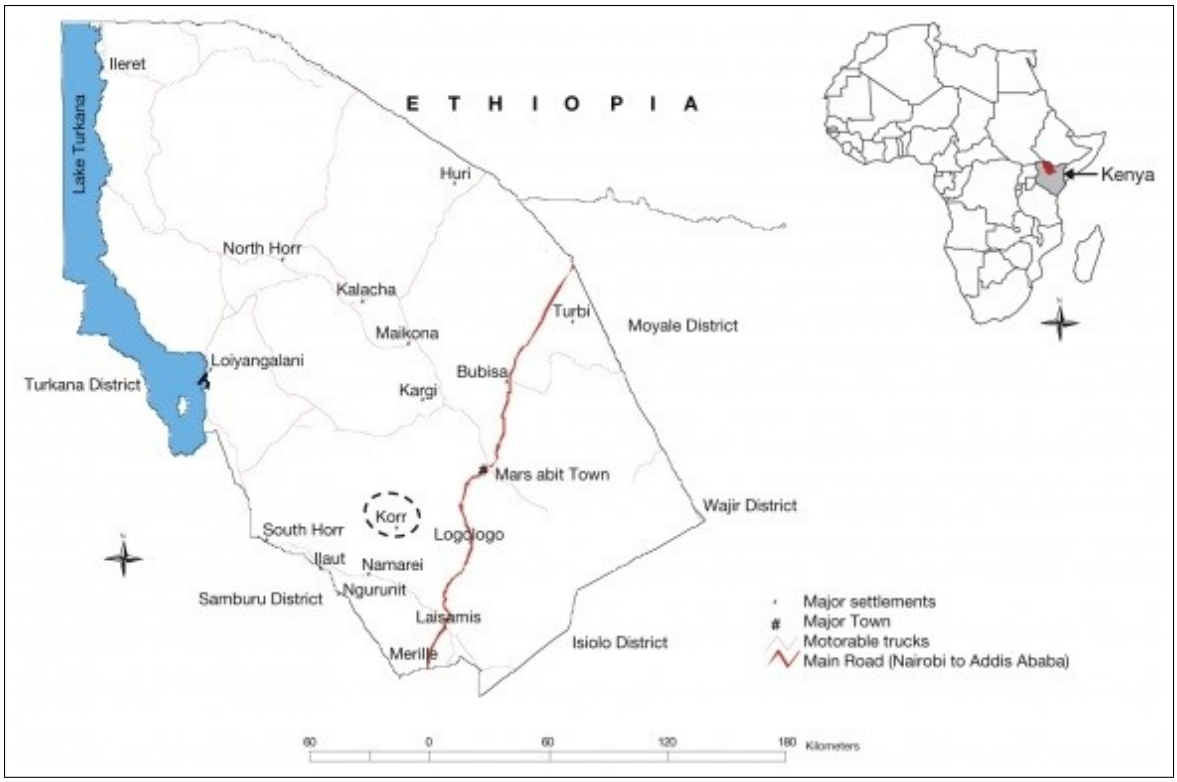

Figure 1. Map of Marsabit County (Warui and Kshatriya 2009).

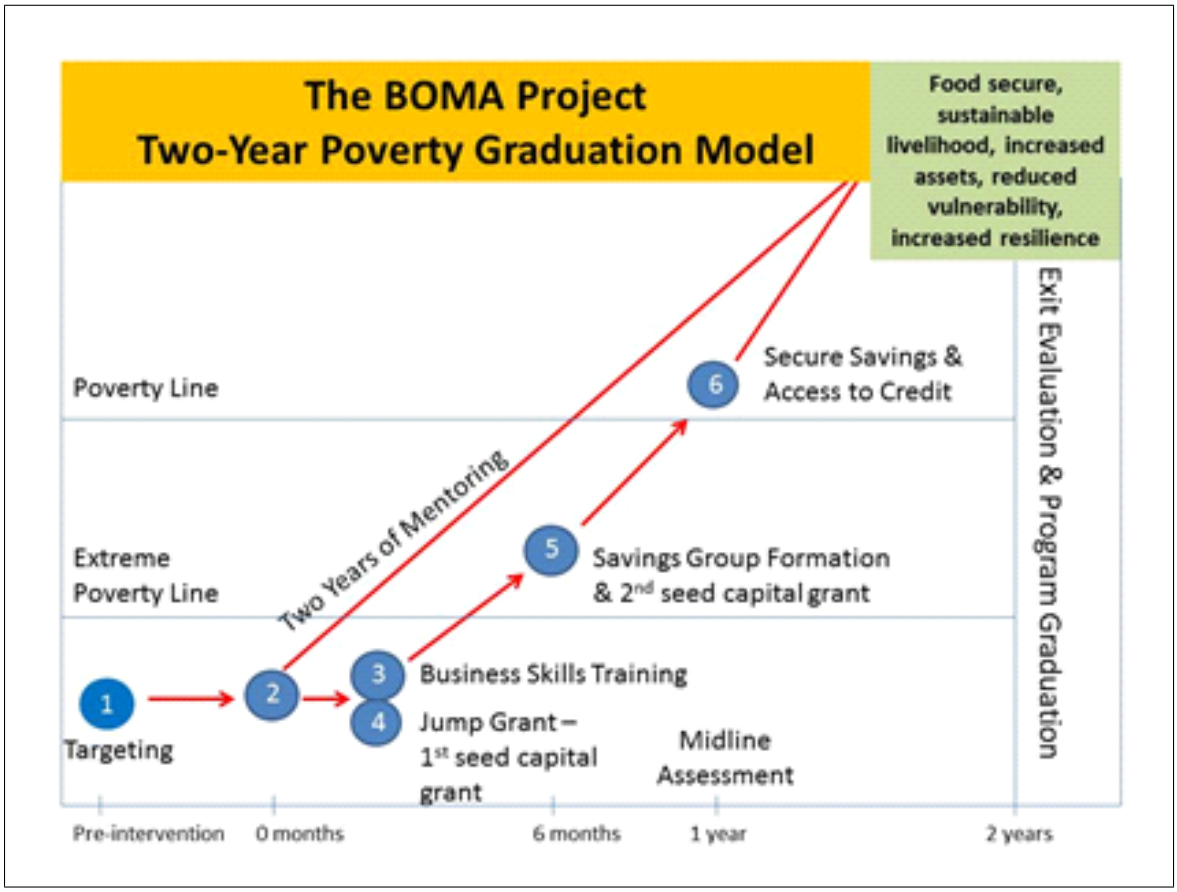

Figure 2. The six steps of REAP (The BOMA Project, 2014).

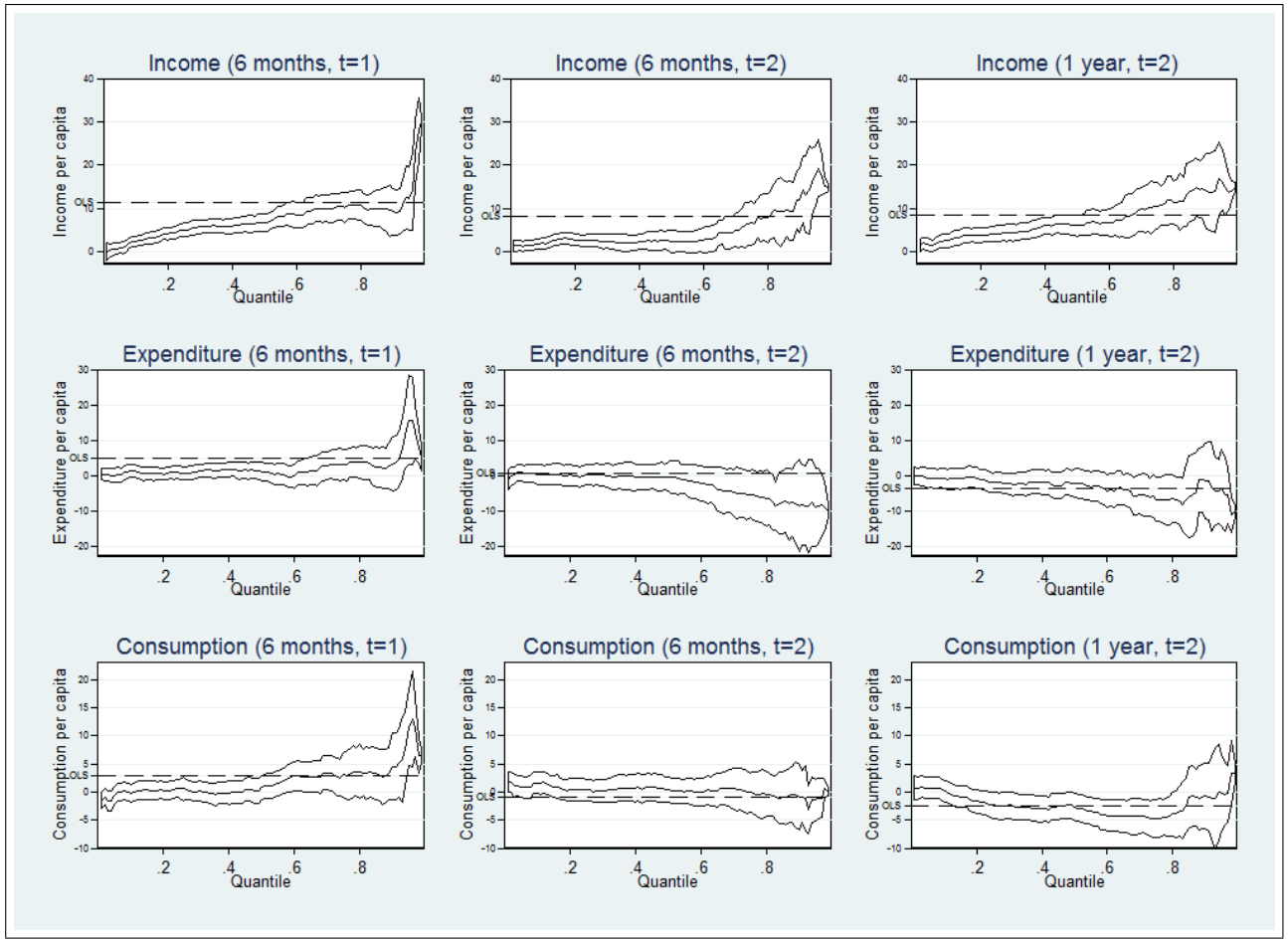

Figure 3. The quantile treatment effects of REAP (income, expenditure and consumption).

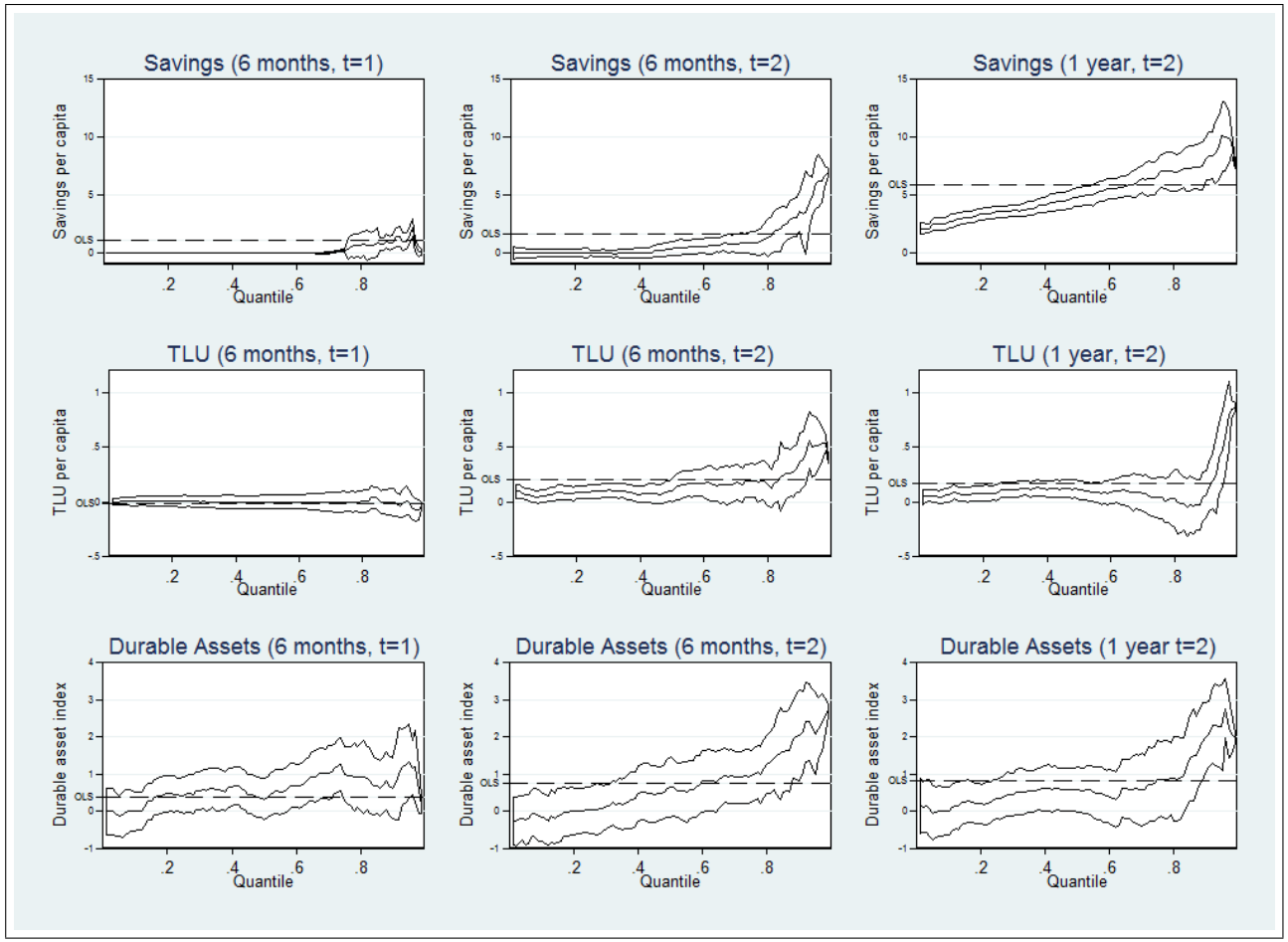

Figure 3 (Cont.). The quantile treatment effects of REAP (savings, TLU and durable assets).

Tables

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	Monthly consumption 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	47.418	3.683	1.031	2.014	0.463
(standard error)	(2.198)	(1.949)	(1.463)	(0.408)	(0.052)	(0.265)	(0.058)
Observations	566	566	566	566	566	566	546
$\bar{X}_C$	23.437	51.703	48.685	3.178	1.119	2.086	0.565
(standard error)	(1.354)	(2.191)	(1.546)	(0.544)	(0.057)	(0.272)	(0.044)
Observations	567	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	0.552	0.458	0.254	0.848	0.163

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

F-Stat	p-value
0.98	0.447

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

Table 4. Income from non-REAP petty trade at baseline and endline

	Overall	Group *A*	Group *B*	Group *C*
$\bar{X}_{baseline}$	0.767	0.443	0.887	0.957
(standard error)	(0.122)	(0.094)	(0.153)	(0.311)
Observations	1651	534	556	561
$\bar{X}_{endline}$	1.257	1.355	1.542	0.880
(standard error)	(0.182)	(0.367)	(0.382)	(0.150)
Observations	1651	534	556	561
$H_0 : \bar{X}_{baseline} = \bar{X}_{endline}$ (p-values)	0.026**	0.016**	0.112	0.824

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

Table 5. The impacts of REAP on household outcomes

Outcome:	Monthly income per capita		Monthly expenditure per capita		Monthly consumption per capita		Total savings per capita		TLU per capita		Durable asset index		Nights that child has gone to bed hungry	
	$t=1$	$t=2$	$t=1$	$t=2$	$t=1$	$t=2$	$t=1$	$t=2$	$t=1$	$t=2$	$t=1$	$t=2$	$t=1$	$t=2$
Panel A: Estimates of the impact of $T_1(t)$														
$\hat{\beta}(T_1(t))$	11.276***	8.238***	5.079	0.819	2.802	-0.822	1.095	1.666**	-0.016	0.205**	0.379	0.743*	-0.103*	-0.184**
	(2.822)	(2.790)	(3.417)	(4.015)	(2.533)	(2.089)	(0.573)	(0.739)	(0.060)	(0.100)	(0.333)	(0.399)	(0.059)	(0.080)
	[0.001]	[0.024]	[0.241]	[0.839]	[0.314]	[0.810]	[0.194]	[0.058]	[0.795]	[0.074]	[0.314]	[0.089]	[0.194]	[0.058]
Observations	1682	1117	1682	1117	1682	1117	1682	1117	1682	1117	1682	1117	1597	1089
R-squared	0.107	0.110	0.280	0.102	0.222	0.284	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	48.052	46.245	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		-2.542		5.832***		0.163		0.814**		-0.170**
		(2.232)		(3.453)		(2.582)		(0.789)		(0.107)		(0.368)		(0.084)
		[0.001]		[0.326]		[0.326]		[0.001]		[0.178]		[0.049]		[0.049]
Observations		1095		1095		1095		1095		1095		1095		1068
R-squared		0.127		0.126		0.252		0.148		0.272		0.424		0.103
Control group mean		25.232		57.394		46.245		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, sub-location fixed effects and baseline levels of the outcome variable (with the exception of monthly consumption for which baseline levels are not available). 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 6. 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]	0.465
Monthly expenditure per capita	5.079 (3.417)	-3.509 (3.453)	-8.588** [0.048]	0.112
Monthly consumption per capita	2.802 (2.533)	-2.542 (2.582)	-5.345** [0.030]	0.104
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]	0.164
Durable asset index	0.379 (0.333)	0.814** (0.368)	0.435 [0.207]	0.290
Nights that child has gone to bed hungry in past week	-0.103* (0.059)	-0.170** (0.084)	-0.067 [0.490]	0.490

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 7. 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 controls for loans taken from REAP savings groups, number of REAP businesses in a manyatta, non-REAP businesses in a location, sub-location fixed effects and baseline levels of the outcome variable. Robust standard errors, clustered at the business group level, are shown in parentheses. 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 8. 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 controls for loans taken from REAP savings groups, number of REAP businesses in a manyatta, non-REAP businesses in a location, sub-location fixed effects and baseline levels of the outcome variable. Robust standard errors, clustered at the business group level, are shown in parentheses. 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 9. 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		Proportion of households with monthly consumption per adult equivalent above the poverty line	
Panel A: Estimates of the six month impact at $t = 1$ and $t = 2$.						
	$t = 1$	$t = 2$	$t = 1$	$t = 2$	$t = 1$	$t = 2$
$\hat{\beta}(T_1(t))$	0.126***	0.066***	0.034	-0.003	0.009	-0.016
	(0.024)	(0.025)	(0.027)	(0.032)	(0.026)	(0.030)
Observations	1682	1117	1682	1117	1682	1117
R-squared	0.179	0.092	0.335	0.113	0.343	0.257
Control group mean	0.169	0.168	0.500	0.581	0.591	0.526
Panel B: $H_0 : \hat{\beta}(T_1(1)) = \hat{\beta}(T_1(2))$						
	0.082*		0.365		0.524	
Panel C: Estimates of the one year impact.						
		$t = 2$		$t = 2$		$t = 2$
$\hat{\beta}(T_1(1) + T_2(2))$		0.129***		-0.017		-0.049
		(0.027)		(0.032)		(0.032)
Observations		1095		1095		1095
R-squared		0.123		0.102		0.264
Control group mean		0.168		0.581		0.526

Note: Estimates from a linear probability model. Regressions include controls for loans taken from REAP savings groups, number of REAP businesses in a manyatta, non-REAP businesses in a location, sub-location fixed effects and baseline levels of the outcome variable (with the exception of monthly consumption for which baseline levels are not available). Robust standard errors, clustered at the business group level, are shown in parentheses.
The Kenya rural poverty line used is as defined by the Kenya National Bureau 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 10. 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)
Monthly consumption per capita	2.802	-0.183	0.560	0.811	2.963	4.564
	(2.533)	(0.967)	(0.954)	(1.378)	(2.255)	(3.647)
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)
Monthly consumption per capita	-0.822	1.593	0.364	0.480	–0.095	-1.009
	(2.089)	(1.172)	(1.128)	(1.368)	(2.122)	(3.545)
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)
Monthly consumption per capita	-2.542	0.118	-2.329*	-2.698*	-4.660**	-0.723
	(2.582)	(1.148)	(1.398)	(1.467)	(2.043)	(3.545)
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 controls for loans taken from REAP savings groups, number of REAP businesses in a manyatta, non-REAP businesses in a location, sub-location fixed effects and baseline levels of the outcome variable (with the exception of monthly consumption for which baseline levels are not available). Robust standard errors, clustered at the business group level, are shown in parentheses. 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 11. Overlap between individuals in the 90th percentile of the outcome distribution

Outcome	Monthly income per capita	Monthly expenditure per capita	Monthly consumption 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	0.225	0.148	0.231	0.183
Monthly expenditure per capita		1	0.249	0.195	0.166	0.195
Monthly consumption per capita			1	0.254	0.225	0.154
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	0.107	0.169	0.214	0.250
Monthly expenditure per capita		1	0.295	0.143	0.134	0.205
Monthly consumption per capita			1	0.188	0.089	0.179
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	0.145	0.182	0.109	0.282
Monthly expenditure per capita		1	0.300	0.155	0.127	0.191
Monthly consumption per capita			1	0.291	0.136	0.173
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

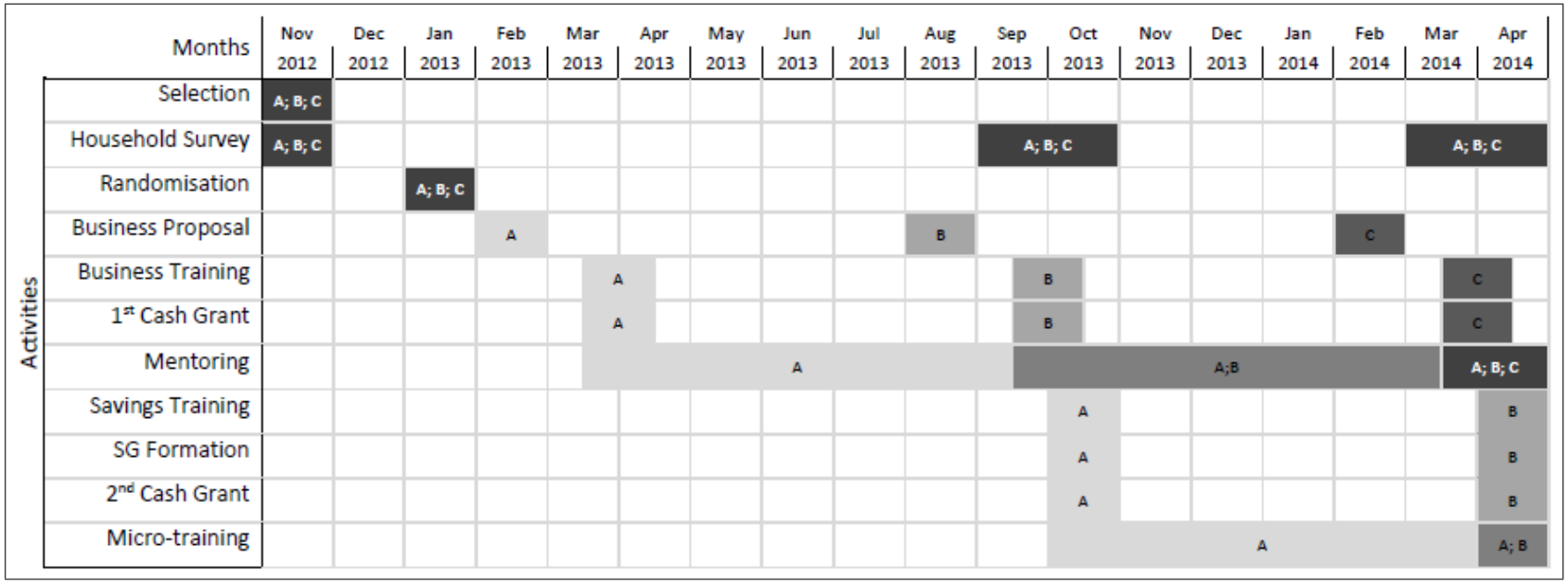

Figure A1. Timeline of data collection and program activities, by assignment to funding cycle. We label the beneficiaries who entered the program in the first cycle as group *A*, and subsequent groups as *B* and *C*.

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

Outcome variables

Table C1. Description of outcome variables included in analysis.

Variable	Description
Monthly income per capita	Income from 1) REAP business, 2) non-REAP business, 3) livestock and livestock products, 4) firewood and charcoal, 5) water, 6) crops, 7) salaried and casual labour, 8) pension, transfers and remittances.
Monthly expenditure per capita	Expenditure on 1) food, 2) clothes, 3) school, 4) health, 5) household items, 6) household repairs, 7) livestock, 8) travel, 9) cosmetics, 10) beads, 11) ceremonies.
Monthly consumption per capita	Monetary value of consumption of food and fuel.
Total savings per capita	Savings held 1) at home, 2) with formal financial institutions and mobile service providers, 3) with non-BOMA savings groups, 4) with BOMA savings groups.
TLU per capita	1 TLU is equivalent to 1 head of cattle, 0.7 camels, 10 sheep/goats, or 2 donkeys.
Durable asset index	As defined in Appendix D
Nights that child has gone to bed hungry in past week	The number of nights in the past week that any child in the household is reported as going to bed hungry.

Appendix D

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, and 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.[29] 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 D1.

Table D1. 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

Appendix E
Use of time

Table E1. Use of time at midline.

Variable:	Proportion of last day in leisure	Proportion of last day in household activities	Proportion of last day in REAP business activities	Proportion of last day in non-REAP business activities	Proportion of last day in other productive activities	Proportion of last day in social activities	Proportion of last day in other activities
Panel A: Means and standard errors of outcome variables for participants in groups A, B and C.							
$\bar{X}_A$	0.449	0.348	0.075	0.003	0.114	0.009	0.003
(standard error)	(0.003)	(0.001)	(0.004)	(0.001)	(0.004)	(0.001)	(0.001)
Observations	538	538	538	538	538	538	538
$\bar{X}_B$	0.482	0.382	0	0.011	0.113	0.008	0.004
(standard error)	(0.004)	(0.004)	(0)	(0.002)	(0.004)	(0.002)	(0.001)
Observations	525	525	525	525	525	525	525
$\bar{X}_C$	0.480	0.384	0	0.009	0.117	0.007	0.003
(standard error)	(0.004)	(0.004)	(0)	(0.001)	(0.004)	(0.001)	(0.001)
Observations	535	535	535	535	535	535	535
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)							
	0.786	0.779	.	0.251	0.543	0.802	0.553
Panel C: t test comparison of means of outcome variables for participants in groups A and C.							
$H_0 : \bar{X}_A = \bar{X}_C$ (p-values)							
	0.000***	0.000***	0.000***	0.000***	0.598	0.224	0.749
Panel D: t test comparison of means of outcome variables for participants in groups A and B.							
$H_0 : \bar{X}_A = \bar{X}_B$ (p-values)							
	0.000***	0.000***	0.000***	0.000***	0.928	0.367	0.790

Note: Eighty-four use of time observations are missing for midline survey respondents. *, ** and ** stand for significant at the 10%, 5% and 1% level of significance, respectively.

Table E2. Use of time at endline.

Variable:	Proportion of last day in leisure	Proportion of last day in household activities	Proportion of last day in REAP business activities	Proportion of last day in non-REAP business activities	Proportion of last day in other productive activities	Proportion of last day in social activities	Proportion of last day in other activities
Panel A: Means and standard errors of outcome variables for participants in groups A, B and C.							
$\bar{X}_A$	0.416	0.374	0.058	0.004	0.123	0.002	0.023
(standard error)	(0.003)	(0.004)	(0.003)	(0.001)	(0.004)	(0.001)	(0.003)
Observations	533	533	533	533	533	533	533
$\bar{X}_B$	0.415	0.381	0.056	0.003	0.131	0.002	0.011
(standard error)	(0.003)	(0.004)	(0.003)	(0.001)	(0.003)	(0.001)	(0.002)
Observations	551	551	551	551	551	551	551
$\bar{X}_C$	0.432	0.399	0	0.007	0.145	0.002	0.015
(standard error)	(0.004)	(0.004)	(0)	(0.001)	(0.004)	(0.001)	(0.002)
Observations	560	560	560	560	560	560	560
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)							
	0.000***	0.002***	0.000***	0.033**	0.008***	0.770	0.121
Panel C: t test comparison of means of outcome variables for participants in groups A and C.							
$H_0 : \bar{X}_A = \bar{X}_C$ (p-values)							
	0.001***	0.000***	0.000***	0.121	0.000***	0.716	0.020**
Panel D: t test comparison of means of outcome variables for participants in groups A and B.							
$H_0 : \bar{X}_A = \bar{X}_B$ (p-values)							
	0.812	0.169	0.646	0.681	0.123	0.934	0.000***

Note: Seven use of time observations are missing for endline survey respondents. *, ** and ** stand for significant at the 10%, 5% and 1% level of significance, respectively.