

NO LONGER TRAPPED? PROMOTING ENTREPRENEURSHIP THROUGH CASH TRANSFERS TO ULTRA-POOR WOMEN IN NORTHERN KENYA

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We examine the short-to-medium-run impacts of the Rural Entrepreneur Access Program, a poverty graduation program that promotes entrepreneurship among ultra-poor women in arid and semi-arid northern Kenya, a context prone to poverty traps. The program relies on cash transfers (rather than asset transfers) in addition to business skills training, business mentoring, and savings. Participation in each of the program's three rounds 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, and asset accumulation, similar to more traditional poverty graduation programs that rely on asset transfers.

Key words: Cash transfers, entrepreneurship, field experiment, microenterprise, poverty graduation, ultra-poor.

JEL codes: C93, D13, J24, O12, O13, Q12.

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 can be potential engines of economic development by raising the 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, some policymakers are concerned that some of the poorest people, sometimes referred to as the ultra-poor, are constrained from establishing such businesses or from participating in

many popular approaches aimed at stimulating microenterprise formation. Yet results of recent evaluations of interventions meant to target individual constraints, particularly access to finance through microcredit, and access to human capital through microenterprise training programs, have been mixed (Banerjee, Karlan, and Zinman 2015; Karlan and Zinman 2011).

The apparent lack of success of these “one-constraint-at-a-time” approaches suggests that the ultra-poor might need a multifaceted “big push” that simultaneously addresses multiple constraints. One influential approach, pioneered by the non-governmental organization (NGO) Building Resources Across Communities (BRAC), is Challenging the Frontiers of Poverty Reduction – Targeting the Ultra-Poor (CFPR/TUP). During a limited period (two years), its participants benefit from a set of interventions, including initial consumption support and an asset transfer (such as livestock), together with savings services, skills training, and regular follow-up visits (Matin, Sulaiman, and Rabbani 2008; Goldberg and Salomon 2011). Several recent impact evaluations of such “graduation” programs provide support for this approach across a diverse set of developing countries. In Bangladesh the

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program enabled ultra-poor women to engage in microentrepreneurial activities resulting in a 38% increase in earnings that persisted two years after the program (Bandiera et al. 2016). Likewise, Banerjee et al.'s (2015) study across sites in six countries documents similar impacts on consumption, productive assets, income and revenue, and stability in impacts up to at least one year after the program.

While these results are promising, more research is necessary. Banerjee et al. (2015) find the weakest impacts in Peru, where the intervention sites are particularly remote, and Honduras, where negative impacts on assets are attributable to a disease outbreak among the in-kind asset (chickens). On one hand, this raises questions about the impacts of such programs in particularly remote settings, where program delivery is more challenging and market access might be more limited. On the other hand, it raises the question of whether cash transfers, which might reduce program costs and reduce the risks of working with live assets, could replicate these impacts. Concerns about external validity are also present in Bauchet, Morduch, and Ravi's (2015) study, which evaluates a similar intervention in Andhra Pradesh, India, and finds no net impact on consumption, income, or asset accumulation. The authors argue that these results reflect mistargeting of individuals with strong labor market opportunities who quickly opted out of the program. Furthermore, evidence suggests that recipients liquidated transferred assets 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). The REAP comprises an initial package of interventions, including a U.S. \$100 cash transfer to set up a microenterprise, business skills training, and business mentoring, which are followed, six months later, by an additional \$50 cash transfer (conditional on having an active enterprise), and training on the importance of savings as well as an introduction to savings groups.¹ This sequence of interventions targets ultra-poor women and intends to enable them to gain the assets and skills necessary to graduate from poverty, a motivation similar to the one behind

the CFPR/TUP (BRAC 2013). While we are able to evaluate the impact of this bundle of interventions, the design of the program prevents us from disentangling the impacts of its individual components, a shortcoming shared with much of the prior literature.

This program, while similar in spirit to other ultra-poor programs, also has a number of notable differences. First, contrary to most such programs, REAP relies entirely on the transfer of cash rather than of a physical asset. Although cash transfers provide flexibility that may provide beneficiaries with greater remunerative options, these transfers have played a minor role in these programs given concerns about possible misuse (e.g., for consumption or payment of existing debt). The purpose of cash transfers in ultra-poor programs is typically for consumption support, intended to prevent 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.

Second, the program focuses explicitly on enterprises, with the requirement that women form three-person groups to run the enterprise jointly. This may provide additional social support and accountability around the use of grant funds, but may also introduce additional challenges in jointly running a business.

Finally, and not necessarily less important, REAP targets women in the Arid and Semi-Arid Lands (ASALs) of east Africa, a regional economy based on one main activity (livestock) and with very limited market access. Studies of poverty dynamics in this area offer the most persuasive empirical evidence in support of the threshold-based poverty traps hypothesis (Kraay and McKenzie 2014). Hence, it provides a setting that is arguably more extreme than those that have been the subject of previous studies. In the context of this literature, REAP could be interpreted as a "cargo net" policy intervention (Barrett, Carter, and Ikegami 2008) designed to enable ultra-poor women to escape persistent poverty.

While the program differs from other ultra-poor programs in these important aspects, the findings are qualitatively similar. After one year, this program has a positive and statistically significant impact on income

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

(31%), savings (131%), asset accumulation (35%), and, less clearly, on livestock (12%). The primary channel for impact is through the setup of new petty trade enterprises, which deal primarily in food items, with time use data showing a corresponding reduction in leisure time and household activity. As several other studies document, there appears to be a weak impact on consumption and expenditure, which is particularly evident following the introduction and promotion of new savings mechanisms. This suggests that in the medium-run, asset accumulation and savings are absorbing the increase in income. In line with the relatively low cost of distributing cash, the program is highly cost-effective: in just over one year, the average increase in household income covers the cost of delivering the program. Hence, the evidence demonstrates that substituting significant cash transfers for in-kind asset transfers does not mute program impact, and that the graduation approach can be effective in relatively remote and challenging settings.

The program we evaluate is perhaps most similar to the Women's Income Generating Support (WINGS) program, studied by Blattman et al. (2016), which also focuses on enterprise development through cash transfers.² The authors concluded that the program led to an important increase in microenterprise ownership and income. This, in turn, suggests that, even with no consumption support and in a context of little accountability around the use of grant funds, recipients were remarkably compliant in directing the funds to enterprise formation (rather than immediate consumption, as feared). The authors also determined that the promotion of self-help groups led to a doubling of the reported earnings, attributing this to increased informal finance and economic cooperation. This suggests the need for deeper financial services (insurance, in the case of WINGS; savings, in the case of REAP) to reinforce such interventions. Taken together, these results suggest that cash transfers can be an effective way of promoting business development among ultra-poor women regardless of the program's set-up.³

² However, there were significant differences between the two programs: WINGS distributed \$150 per individual, instead of \$150 per group of three beneficiaries, and provided more business training but less mentoring than REAP. Further, WINGS also operated in a substantially different context: post-war Uganda, with substantially lower levels of baseline income and lower levels of business activity.

Overview of the Intervention

The REAP implementation occurred in fourteen locations in the southern and central parts of Marsabit County, in the ASALs of northern Kenya.⁴ In this region, more than 80% of the population live below the national poverty line (Kenya National Bureau of Statistics and Society for International Development 2013).⁵ 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 is highly susceptible to weather and other shocks, and repeated droughts frequently have devastating impacts on households' livelihoods (Silvestri et al. 2012). This has resulted in many households losing their ability to meet their basic needs due to the loss of herds, which is hard to recover from. A prominent body of literature provides significant evidence that a non-linear threshold governs asset (livestock) dynamics in this region. Below the threshold, livestock holding sharply converges toward a low-level steady state of almost no livestock holding (Lybbert et al. 2004; Barrett et al. 2006; Santos and Barrett 2011; Barrett and Santos 2014; Toth 2015). Households experiencing such a steady state must resort to begging, unskilled wage labor, various forms of petty trade, and become reliant on food aid to meet dietary needs.⁶ This raises important questions about what type of "cargo net" policies (if any) can help such destitute households escape persistent poverty.⁷

³ Finally, the recently evaluated GiveDirectly program (Haushofer and Shapiro 2016), an unconditional cash transfer targeted at poor households in Kenya that differs substantially from the poverty graduation approach, reaches similar conclusions: in the short-run, these transfers lead to increased investment in, and revenue from, livestock and small businesses, even in the absence of additional interventions such as training.

⁴ See figure A.1 in appendix A of the [supplementary material online](#) for a map displaying these locations.

⁵ In 2005/06, the poverty line was estimated at Kenya Shillings (Ksh) 1,562 Purchase Power Parity (PPP) \$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).

⁶ Little et al. (2008) examine different proxies for poverty and welfare in northern Kenya. These authors 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.

⁷ Barrett, Carter, and Ikegami (2008) distinguish between policies that increase the assets of the poor and allow them to escape poverty ("cargo nets") from more traditional "safety net"

Opportunities to engage in non-pastoral activities are further constrained by these communities' lack of attention by national development processes. Additionally, these areas have low population densities and limited access to markets or other infrastructure (Elliot and Fowler 2012). By targeting the poorest women in these communities, REAP aims to provide households with a pathway out of poverty through the alleviation of financial and human capital constraints, thereby enabling these women to obtain a sustainable livelihood without directly engaging in the livestock economy.

Structure and Timing of the Intervention

The REAP's main aim 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 savings. The sequence of these interventions appears in figure 1, and an elaboration of each intervention follows below.

Participant selection. The REAP established guidelines for the formation of local committees that determine eligibility for prospective participants.⁸ These committees identified women who were among the poorest in the community. The committees prioritized women with no other sources of income, and who demonstrated a likelihood of responsible entrepreneurship and willingness to run a business with two other women.⁹ Trained business mentors ensured that the local committees followed these criteria when selecting participants.¹⁰ After the participant selection and acceptance process, 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, business mentors met with beneficiaries to assist with the development of a business proposal. The meetings' purpose was to allow mentors to get a better understanding of the group members' abilities and previous business experience before going through the basics of setting up a business. On the day of program enrollment, all participants had to attend a short business skills training session that mentors delivered under the supervision of REAP field officers.¹¹ Over the course of the program, participants benefited from approximately seventeen hours of training.¹²

First grant and business mentoring. At the end of the business skills training session, REAP provided each group with a cash grant of \$100 (Purchase Power Parity [PPP] \$237.97 at 2014 prices) for the purpose of establishing their business, an amount equivalent to approximately 7.5 months of expenditure per capita.¹³ The REAP required each group to invest the entire grant in the business, but members had the flexibility to use the cash as they saw fit, including by modifying their initial business proposal.¹⁴

After the distribution of the initial grant, mentors regularly met with the business group (at least once a month) to monitor their progress and offer advice and training. The role of the mentor was to help start up the business (e.g., by providing information regarding where to source goods or market conditions). Additionally, the mentor was also responsible for assisting the group with record-keeping, and if necessary, in managing conflicts within the group. Over the course of the program, each business was expected to benefit from approximately thirty hours of mentoring.

¹¹ 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.

¹² 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.

¹³ From here on, all monetary values reported in the article 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), and \$40.35 (2014). These values are then converted to 2014 prices by multiplying the ratio of the 2014 U.S. Consumer Price Index (CPI) to the U.S. CPI for the relevant year.

¹⁴ The investment of the grant was ensured by the mentor, who met with the businesses soon after disbursement. Additionally, the conditionality of the second grant, as described below, likely created incentives for groups to invest the first grant in a business.

policies that seek to prevent these families from falling into the poverty trap in the first place.

⁸ 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.

⁹ In addition, and recognizing the importance of inter-ethnic rivalries in northern Kenya, selection committees were asked to select participants to ensure 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.

¹⁰ Mentors are employed at the location level. Mentors participated in a training of trainers program, which lasted for five days and occurred prior to the recruitment of participants. Each location comprises many sub-locations that are formed by smaller villages, known as manyattas.

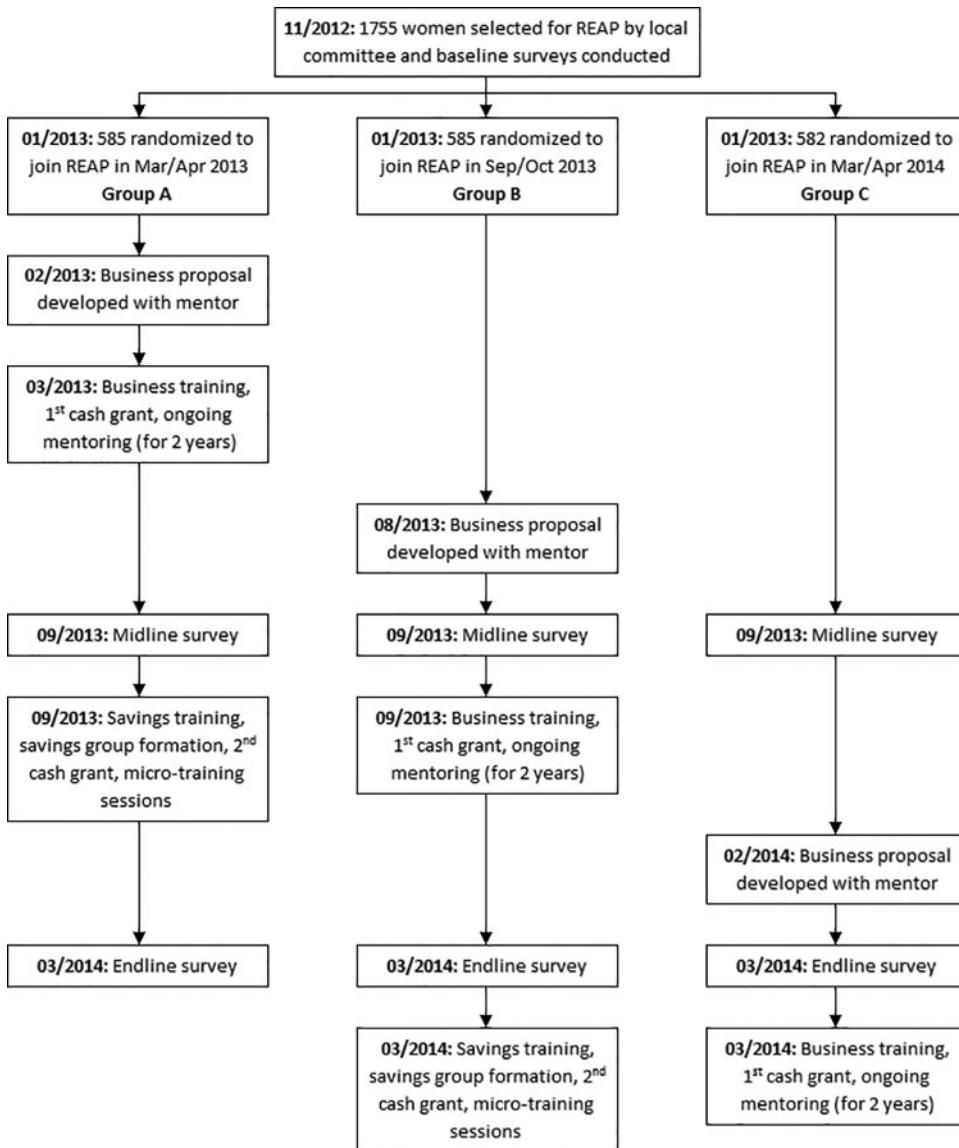


Figure 1. Description of the intervention, study sample, and experimental design

Second grant, savings training, and savings group formation. Six months after the start of the business, groups were eligible for an additional grant of \$50 (PPP \$118.98) conditional on meeting the following criteria: two or more original members remained involved in the business, the group held 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 aware of these conditions from the start of the program. Another requirement mandated that

participants attend a short training session on savings to develop a basic understanding of the formation and operation of savings groups, including their rules, record-keeping, and issuance of loans.

After the savings training and the distribution of the second grant, mentors encouraged participants to form a savings group (SG) or join existing ones. The decision to join a group was both voluntary and individual (i.e., it was not a business group decision). The SGs most closely resembled Village Savings and Loans Associations (VSLAs), 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 that are paid back with interest.

Research Design

Several factors allow us to identify the program impacts in a relatively straightforward way. These include 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 low attrition rate. 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 fourteen locations in northern Kenya identified 1,755 women eligible for REAP. Capacity constraints resulted in the division of eligible women into three equal-sized groups that successively enrolled in REAP over the next three funding cycles (March/April 2013, September/October 2013, or March/April 2014, hereafter referred to as groups A, B, and C, respectively).¹⁵ In order to ensure actual and apparent transparency and fairness, a random public lottery in each location determined assignment to each of the three funding cycles.¹⁶ None of the eligible participants declined to take part in the program, which did not permit them to participate outside of their randomly allocated group.

Eligible women had interviews at the baseline (November 2012) with an additional two follow-ups at six-month intervals, timed to occur before each new funding cycle (see figure 1). Survey attrition was very low in both follow-up survey rounds, with less than 4% of women not re-interviewing at the first follow-

up (midline) and less than 6% of women doing so at the second follow-up (endline; see table B.1 in appendix B of the online [supplementary appendix](#)). Taken together, less than 2% of women never re-interviewed and are lost. An analysis of the correlates of attrition, presented in table B.2 in the online [supplementary appendix](#), shows that although low, attrition at endline is correlated with assignment to group A, but it is not correlated with assignment to group B in any of the surveys. Additionally, no baseline characteristics significantly predict attrition at either midline or endline at the 5% significance level. These results indicate that, upon disbursement of both grants (as happens with group A at endline), respondents seemed to feel less motivated to answer the surveys, but this behavior did not manifest itself along observable dimensions of heterogeneity. The estimated impacts of REAP are largely robust to this attrition, as discussed below.

Checking the Integrity of the 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 throughout the program's progressive roll-out. We present the results in table 1.

In panel A, we present summary statistics (mean and standard deviations) of variables the program may impact (expenditure, income, savings, and asset ownership) or that may mediate the program's impact (household size, previous business experience, and 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 (see Mertens et al. 2013), suggesting that the findings of this study may be generalizable to ultra-poor women across northern Kenya. Average monthly expenditure per capita is approximately PPP \$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 insecure, with children going to bed hungry at least

¹⁵ The capacity of the BOMA Project to reach participants determined sample size. 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%.

¹⁶ The subsequent disqualification of three of the initial 1,755 women led to the assignment of 585 women to the first and second cycles and 582 women to the final funding cycle.

Table 1. Summary Statistics and Balance Checks for the Treatment and Control Groups

Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
	Monthly income per capita	Monthly expenditure per capita	Monthly food expenditure per capita	Monthly non-food expenditure per capita	Savings per capita	TLU per capita	Durable asset index	Meals per day	Nights hungry in past week	Proportion of children in school	Household Size	# children	Married	Years of education	Business experience	Benefiting from HSNP	Participating in CARE VSLA
Panel A: Means and standard deviations of variables at baseline																	
\bar{X}_A (standard deviation)	21.770 (22.365)	34.562 (36.672)	24.182 (28.739)	10.380 (18.069)	3.772 (8.314)	0.638 (0.723)	-0.234 (4.084)	1.941 (0.394)	0.549 (0.651)	0.435 (0.289)	5.778 (1.908)	3.875 (1.729)	0.800 (0.400)	0.328 (1.454)	0.576 (0.495)	0.106 (0.308)	0.089 (0.285)
Observations	585	585	585	585	585	585	585	581	578	585	585	585	585	585	585	585	585
\bar{X}_B (standard deviation)	22.319 (22.574)	34.480 (33.920)	23.862 (25.991)	10.617 (18.612)	3.920 (7.932)	0.640 (0.898)	0.113 (4.574)	1.950 (0.387)	0.576 (0.706)	0.442 (0.288)	5.692 (1.823)	3.737 (1.696)	0.831 (0.375)	0.470 (1.730)	0.562 (0.497)	0.103 (0.304)	0.106 (0.308)
Observations	585	585	585	585	585	585	585	585	579	579	585	585	585	585	585	585	585
\bar{X}_C (standard deviation)	22.449 (24.008)	32.825 (29.320)	22.494 (21.090)	10.331 (15.622)	5.123 (14.420)	0.684 (0.814)	0.124 (4.312)	1.933 (0.339)	0.576 (0.701)	0.412 (0.272)	5.596 (1.861)	3.711 (1.691)	0.773 (0.419)	0.414 (1.683)	0.538 (0.499)	0.113 (0.317)	0.108 (0.311)
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					F-Stat												
Control group	A				0.76												
B and C	B				1.18												
A	A				1.15												
						p-value											
						0.723											
						0.283											
						0.308											

Note: All monetary values are reported in 2014 USD. PPP terms. Superscript ^a indicates this regression includes monthly food expenditure per capita and excludes monthly non-food expenditure per capita. Asterisks *, **, and *** stand for significance at the 10%, 5%, and 1% levels, respectively.

two times per month. Households also own very little livestock: less than one Tropical Livestock Unit (TLU) per capita, which is well below the self-sufficiency threshold for mobile pastoralists in East African ASALs (McPeak and Barrett 2001).¹⁷ Hence, these households appear to be overwhelmingly drawn from the cohorts that have fallen out of direct participation in the livestock economy (Little et al. 2008), with asset holdings below the poverty trap threshold (Barrett et al. 2006; Toth 2015). In line with this, we find that more than half of the participants report having some business experience, typically petty trade or selling 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 statistically balanced: in only one case can we reject the null hypothesis of balance at the conventional 5% level. The results of an *F*-test of the joint effect of these variables on treatment status, as reported in panel C, reinforce this conclusion.

Spillover Effects and Program Anticipation

The geographical proximity of individuals in the treatment and control groups may lead to control households benefiting from the products and services of the businesses established under REAP.¹⁸ Such spillovers could potentially bias our estimated treatment effects. We investigate three possible pathways for such influence: lower prices to consumers and lower profits of non-REAP businesses (both due to increased competition from new businesses), and easier access to loans (due to the increase in savings groups).

Given that more than 95% of the businesses established under REAP are in petty trade (primarily food items), the main impact of increased competition among businesses may be a consequent reduction in market prices of food. We do not have data on the market price for foodstuffs these businesses usually sell, hence we cannot directly test

whether this is the case. However, we have information about the number of REAP businesses in an individual's manyatta at baseline, and include it as a control variable when estimating the impact of the program as a proxy for the effect of competition on prices.¹⁹

The increased competition from new businesses established under REAP may also affect the welfare of non-participant households by reducing any income they may earn from pre-existing petty trade businesses. The only proxy for this increase in competition in our data is the number of individuals per business at location level for which we have data at the time of the baseline. We can estimate the evolution of this measure of market size if we are willing to assume that the only meaningful source of change in business numbers is the program and that there is no important change in population size. Both assumptions seem reasonable given the context and the short time horizon. We can then use this new variable to examine the effect of changes in market size on income from petty trade from pre-existing businesses. We present these estimates in table 2.²⁰ There is no evidence of an effect of changes in market size on the income from petty trade earned by control group participants.²¹ These results are consistent with recent findings from a market-level experiment in Kenya that leverages a cluster randomization design to show that business training that increases the income of treated women does not lead to adverse spillovers on non-treated women in the same markets (McKenzie and Puerto 2017).

These tests suggesting an absence of spillovers assume that the spillover effect is linear in our measure of market size. However, one might be concerned that the effect that changes in market size have on income from petty trade differs depending on how much income households earn from petty trade or by the size of the location, for example. We investigate these two non-linearities

¹⁷ Tropical Livestock Unit (TLU) is a standardized unit, designed to measure the size of a mixed livestock herd: one TLU is equivalent to one head of cattle, 0.7 camels, ten sheep/goats, or two donkeys.

¹⁸ We would also expect such benefits to extend to the wider community, but a lack of data prohibits us from examining the benefits of REAP to households outside of our study sample.

¹⁹ Overall, there were 1,932 businesses before the program. The program funded 195 businesses (approximately 10% of the pre-existing number) in each funding cycle. See table C.1, in appendix C of the [supplementary material online](#), for further details.

²⁰ Note that at baseline there are no statistically significant differences in per capita income from non-REAP petty trade between groups A, B, and C.

²¹ Restricting the sample to only those households that were engaged in petty trade at baseline does not change this conclusion (see table C.2 in appendix C of the [supplementary material online](#)).

Table 2. Spillover Effect of REAP on Income from Non-REAP Petty Trade

	(1) Monthly income from non-REAP petty trade per capita
<i>Population per business</i>	0.047 (0.037)
<i>Midline</i>	1.189** (0.588)
<i>Endline</i>	0.631 (0.661)
Observations	2851
R-squared	0.029

Note: The sample is restricted to participants in groups B (baseline and midline) and C (all surveys) who received funding in Sept./Oct. 2013 and March/April 2014, respectively. The dependent variable is *monthly income from non-REAP petty trade per capita*. *Population per business* is derived by dividing the number of individuals in a location by the number of businesses in that location (using figures reported in table C.1 of the online supplementary appendix). Additional controls include location fixed effects. Robust standard errors, clustered at the sub-location level, are shown in parentheses. All monetary values are reported in 2014 USD, PPP terms. Asterisks *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

separately by including interaction terms in our original specification (see tables C.3 and C.4 in appendix C of the online supplementary appendix). In both cases, the interaction terms are not statistically significant.

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) available to other members of the community so that they can deal with shocks and emergencies (usually health-, school-, and food-related expenditures). We do not have baseline information on borrowing from REAP SGs, but we do find that at midline, 7% of participants in the control group borrowed from a REAP SG, and this value increases to 20% at endline. This change may result in our estimated treatment effects being downward biased if, for example, the control group is now consuming more than they would otherwise in the absence of REAP SGs.

Finally, bias could arise from participants changing their behavior in anticipation of receiving the program.²² It is unclear in which

²² Given that we are dealing with ultra-poor women, 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

direction such an anticipation effect may bias our results. For example, if participants delay investments in anticipation of receiving the grants, then estimated treatment effects would be upward-biased. On the other hand, treatment effects may be downward-biased if participants awaiting the grant are more willing to invest given the certainty of receiving the grant, which may act as a form of insurance (Bianchi and Bobba 2013).

The design of the study does not allow for a straightforward way to test for anticipation effects. However, if anticipation results in either behavior, then we would expect to find differences 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. If this intuition is correct, then these differences would be ascertainable during the midline survey (when group B would immediately receive the first grant, while group C would still be six months away from participating in the program). We check for differences in our outcome variables: *monthly income per capita*, *monthly expenditure per capita*, *monthly consumption per capita*, *savings per capita*, *TLU per capita*, *durable asset index*, and the *number of nights that a child has gone to bed hungry in the last week*.²³ We see no statistically significant differences between groups B and C, as shown in columns (1) to (7) of table 3.

Additionally we find no statistically significant differences in income earned from non-REAP businesses (table 3, column [8]), which suggests that participants awaiting the grant did not alter their pre-existing business practices. We also do not find any statistically significant differences in how these two groups of participants allocate their time (see table F.1 in appendix F of the online supplementary material) or in the proportion of women that have ever taken a loan at midline.²⁴ Less than 2% of the women in groups B and C who took loans used them for investment in a business or livestock. The limited use of loans

both lack of access to capital and human capital constraints are likely to prevent them from taking any action that would affect measured outcomes.

²³ We define these outcome variables in appendix D and appendix E of the supplementary material online.

²⁴ Approximately 24.2% (24.3%) of group B (C) participants have ever taken a loan from banks, Microfinance institutions (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.

Table 3. Testing for Anticipation Effects

Variable:	(1) Monthly income per capita	(2) Monthly expenditure per capita	(3) Monthly consumption per capita	(4) Savings per capita	(5) TLU per capita	(6) Durable asset index	(7) Nights that child has gone to bed hungry in past week	(8) Monthly income from non-REAP petty trade per capita
= 1 if in group B	2.744 (2.824)	-2.082 (2.666)	-1.238 (2.283)	0.610 (0.653)	-0.072 (0.096)	-0.125 (0.407)	-0.084 (0.054)	0.304 (0.438)
Observations	1133	1133	1106	1133	1133	1133	1077	1133
R-squared	0.068	0.293	0.161	0.062	0.260	0.257	0.183	0.066
Group C mean	23.437	51.703	49.828	3.178	1.119	2.086	0.565	1.551

Note: Groups B and C refer to REAP participants who received funding in Sept./Oct. 2013 and March/April 2014, respectively. Estimates are from Ordinary least squares (OLS) regressions using midline data collected prior to Group B participants receiving the first grant. The sample is restricted to participants in groups B and C and the reference category in the regressions is group C. Regressions include controls for the number of REAP businesses in a manyatta at baseline, location fixed effects, and baseline levels of the outcome variable (with the exception of *monthly consumption per capita* for which baseline levels are not available). Robust standard errors, clustered at the sub-location level, are shown in parentheses. All monetary values are reported in 2014 USD, PPP terms. Asterisks *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

for business investment is attributable to the limited access to capital in this region as Osterloh and Barrett (2007) demonstrate in similar locations in northern Kenya. Though not definitive for disproving anticipation effects, these results suggest that anticipation effects should be of limited importance, if any.

Main Results

The random assignment of treatment status allows us to obtain unbiased estimates of REAP’s impact by estimating the following regression for each outcome of interest:

$$\begin{aligned}
 (1) \quad Y_i(t) &= \theta + \beta T_{ij}(t) + \delta Y_i(0) + \tau M_i \\
 &\quad + \varphi X_i(0) + \varepsilon_{ij} \\
 &= \{1, 2\}, t = \{midline, endline\}
 \end{aligned}$$

where $Y_i(t)$ is the outcome of interest for household i at time t , $Y_i(0)$ is the baseline value of the outcome variable for household i , M_i is a set of location dummy variables, and $X_i(0)$ is the number of REAP businesses in an individual’s manyatta at baseline.²⁵ Finally, T_{ij} is the treatment status of individual i . Given the structure of the program, we can consider two sets of interventions, indexed by j : business training, a cash grant of \$100, and mentoring that the program introduces first and that we label T_{i1} , followed by savings training, an additional cash grant of \$50, and continued mentoring, which we label T_{i2} .

Simplifying the notation by dropping the i th individual subscript, it is clear from the description of the program (and from figure 1) that we can observe T_1 at both midline and endline ($T_1(midline)$ and $T_1(endline)$), and the joint effect of the two sets of interventions at endline ($T_1 + T_2(endline)$). To estimate the impact of T_1 at midline, we compare group A to a combined control group comprised of those benefiting from the program in the second and third cycles (i.e., groups B and C). We can similarly estimate the impact of T_1 on group B at endline by comparing group B with the control group C. We can use these two estimates to test the hypothesis

²⁵ When we exclude the number of REAP businesses in an individual’s manyatta at baseline from this model, we find no difference in the statistical significance or size of the estimated impacts of the program.

that the impact of T_1 is constant throughout the period

$$(2) \quad H_0 : \beta[T_1(\text{midline})] = \beta[T_1(\text{endline})].$$

Failure to reject equation (2) would suggest that the impact of this subset of interventions is stable. This, in turn, provides further support to our assumption that there were no adverse effects from late entry into treatment (due, e.g., to increased market competition).

It is important to note that failure to reject equation (2) is not enough to plausibly identify the impact of T_2 in isolation given that, at midline, beneficiaries of T_1 will potentially be different from the same individuals at baseline, both in ways that are easy to control (e.g., asset ownership) and in ways that are not easy to observe (e.g., experience in managing a business as part of a group). Therefore, without further assumptions regarding how such variables influence the outcomes we analyze, we are limited in our ability to identify the effect of T_2 conditional on previously benefiting from T_1 .

The Six-Month Impact of REAP

Panels A and B of table 4 provide the estimates of the impact of T_1 at midline and endline, respectively. We cluster standard errors (presented in parentheses) at the sub-location level, as this is the level at which stratification occurred.²⁶ We also present (within square brackets) adjusted p -values or q -values that account for testing the program's impact on several outcomes.²⁷ We obtained the q -values by implementing the step-up method to control for the false discovery rate (FDR) as Benjamini and Hochberg (1995) proposed.²⁸

After accounting for the possibility of multiple inference (by adjusting p -values), and searching for consistent impacts across both periods, we see that beneficiaries have higher *monthly income per capita* after six months of benefiting from REAP (column [1]). These

²⁶ As previously noted, locations are comprised of sub-locations that are made up of smaller villages known as manyatas. There are sub-locations.

²⁷ Because we estimate the impacts of REAP on several outcomes, we increase the probability of type 1 errors by testing multiple simultaneous hypotheses at set p -values. For example, by performing seven independent tests, the probability of a type 1 error is no longer 0.05, but rather 0.302. The adjusted p -values should be interpreted as the smallest significance level at which the null hypothesis is rejected.

²⁸ We make use of the procedure outlined by Anderson (2008) to obtain the q -values.

changes are economically significant in both periods, and represent an improvement of 44.6% over the control group mean (or 0.255 Standard deviation (SDs)) at midline, and 34.7% over the control group mean (or 0.250 SDs) at endline.

Somewhat surprisingly, these changes do not seem to translate into changes in *monthly expenditure per capita* or *monthly consumption per capita* (reported in columns [2] and [3]), which, although positive, are much less precisely estimated. This is especially true during endline, when we can reject the equality between increases in income and expenditure (p -value = 0.087) and between income and consumption (p -value = 0.014), although it is also clear at midline when comparing increases in income and consumption (p -value = 0.008).

One explanation for this discrepancy is the allocation of additional income to asset accumulation. Our data offer some support to this explanation, in particular at endline when we observe increases in savings (column [4]) and durable assets (column [6]) that, like the changes in income, are economically significant (34.6% or 0.203SDs, and 26.4% or 0.112 SDs above the respective control group mean). The effects on livestock (column [5]) and on the number of nights a child has gone to bed hungry (column [7]) are less precisely estimated, and marginally insignificant after accounting for multiple inference. Despite this apparent difference in the impact of T_1 between periods, with the effects being apparently more positive in the second period, we can never reject the null hypothesis of equality of impact across periods (equation [2]).²⁹

The One-Year Impact of REAP

Table 4, panel C, provides estimates of the combined impact of T_1 and T_2 after one year of participation in REAP (i.e., $(T_1 + T_2(\text{endline}))$). These estimates align with those in panels A and B (i.e., the impact of T_1), with treated participants reporting significantly higher income per capita, savings per capita, and durable asset ownership. After one year of participation in REAP, income per capita is 30.8% (0.222 SDs) higher compared to the control group mean, and savings per capita is 131.4% (0.769 SDs)

²⁹ Depending on outcome, the q -values are between 0.588 and 0.680 (see table 4 panel D).

Table 4. The Impacts of REAP on Household Outcomes

Outcome:	(1) Monthly income per capita	(2) Monthly expenditure per capita	(3) Monthly consumption per capita	(4) Savings per capita	(5) TLU per capita	(6) Durable asset index	(7) Nights that child has gone to bed hungry
Panel A: The impacts at six months measured at midline							
<i>Treatment</i>	11.084*** (2.639)	5.668 (3.933)	2.773 (2.335)	1.077 (0.714)	-0.011 (0.044)	0.526 (0.392)	-0.070 (0.060)
<i>Outcome at baseline</i>	[0.001]	[0.291]	[0.291]	[0.291]	[0.809]	[0.105]	[0.291]
<i>REAP businesses in manyatta</i>	0.146** (0.066)	0.257*** (0.061)		0.068*** (0.024)	0.417*** (0.107)	0.503*** (0.051)	0.008 (0.093)
Observations	-0.498* (0.272)	-0.120 (0.545)	-0.224 (0.310)	-0.055 (0.068)	-0.014 (0.017)	0.126** (0.053)	-0.005 (0.008)
R-squared	1682	1682	1646	1682	1682	1682	1597
Control group mean	0.067	0.198	0.142	0.078	0.270	0.231	0.189
	24.849	50.805	49.225	3.430	1.075	2.050	0.514
Panel B: The impacts at six months measured at endline							
<i>Treatment</i>	8.744** (3.209)	2.446 (3.963)	0.038 (2.134)	1.536** (0.607)	0.180 (0.099)	0.751* (0.401)	-0.153 (0.083)
<i>Outcome at baseline</i>	[0.049]	[0.629]	[0.986]	[0.049]	[0.105]	[0.089]	[0.105]
<i>REAP businesses in manyatta</i>	0.111 (0.090)	0.286*** (0.083)		0.058 (0.042)	0.352*** (0.143)	0.509*** (0.068)	0.014 (0.093)
Observations	0.728 (0.506)	0.091 (0.383)	-0.232 (0.258)	-0.088 (0.074)	-0.010 (0.018)	0.128** (0.060)	0.003 (0.008)
R-squared	1117	1117	1117	1117	1117	1117	1089
Control group mean	0.054	0.033	0.216	0.029	0.129	0.274	0.033
	25.232	57.394	46.245	4.440	1.303	2.843	0.789
Panel C: The impacts at one year measured at endline							
<i>Treatment</i>	7.769*** (2.067)	-1.565 (3.444)	-2.195 (2.202)	5.836*** (0.765)	0.158 (0.086)	0.994** (0.372)	-0.108 (0.087)
<i>Outcome at baseline</i>	[0.001]	[0.651]	[0.376]	[0.001]	[0.121]	[0.021]	[0.304]
<i>REAP businesses in manyatta</i>	0.159*** (0.060)	0.291*** (0.074)		0.040 (0.069)	0.763*** (0.262)	0.571*** (0.065)	0.327*** (0.098)
Observations	0.033 (0.394)	0.313 (0.356)	-0.010 (0.339)	-0.121* (0.072)	-0.009 (0.013)	0.157** (0.075)	-0.008 (0.010)
R-squared	1095	1095	1095	1095	1095	1095	1068
Control group mean	0.072	0.054	0.166	0.097	0.188	0.275	0.034
	25.232	57.394	46.245	4.440	1.303	2.843	0.789

Continued

Table 4. continued

Outcome:	(1) Monthly income per capita	(2) Monthly expenditure per capita	(3) Monthly consumption per capita	(4) Savings per capita	(5) TLU per capita	(6) Durable asset index	(7) Nights that child has gone to bed hungry
Panel D: P-values and adjusted p-values (in brackets) from Wald tests of the equivalence of treatment effects							
6 month (midline) = 6 month (endline)	0.583 [0.680]	0.580 [0.680]	0.428 [0.680]	0.604 [0.680]	0.084 [0.588]	0.680 [0.680]	0.428 [0.680]
6 month (midline) = 1 year (endline)	0.369 [0.431]	0.172 [0.272]	0.066 [0.154]	0.000 [0.001]	0.063 [0.154]	0.194 [0.272]	0.722 [0.722]
6 month (endline) = 1 year (endline)	0.737 [0.830]	0.304 [0.710]	0.273 [0.710]	0.000 [0.001]	0.830 [0.830]	0.531 [0.804]	0.574 [0.804]

Note: In Panel A, the treatment group is group A and the control group is groups B and C combined. In Panel B, the treatment group is group B and control group is group C. Groups A, B, and C refer to REAP participants who received funding in March/April 2013, Sept/Oct. 2013, and March/April 2014, respectively. Regressions include controls for the number of REAP businesses in a manyatta at baseline, location fixed effects and baseline levels of the outcome variable (with the exception of *monthly consumption per capita* for which baseline levels are not available). Robust standard errors, clustered at the sub-location 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. Asterisks *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively (based on adjusted *p*-values).

higher compared to the control group mean, with both increases being statistically significant at the 1% level. Additionally, durable asset ownership is 35.0% (0.149 SDs) higher compared to the control group mean, a difference that is significant at the 5% level.

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 2.7% (0.027 SDs) and 4.7% (0.064 SDs), respectively, although these decreases are not statistically significant. As before, the impact on livestock, although positive (an increase of 12.1% [0.099 SDs] over the control group mean), is not statistically significant at conventional levels once we account for multiple inference (*q*-value = 0.121).

Robustness Tests

We check the robustness of our results to attrition, to different specifications of equation (1), and to the effect of outliers. Detailed results of these tests are presented in tables G.1, G.2, and G.3 in appendix G of the [supplementary appendix](#) online.

Attrition, although low (recall that less than 2% of women are never re-interviewed), correlates with assignment to group A at endline. We check for the robustness of our results to attrition by estimating different types of attrition bounds. We consider bounds that either impute missing observations using observations for those participants who did not attrit, or bounds that trim observations from the treatment arm that suffers from less attrition.³⁰ The results are largely robust to attrition, though impacts on livestock and assets show more sensitivity to the degree of conservatism in selecting bounds (see appendix G in the [supplementary online material](#)).

In appendix G we also present results from other regression specifications. We re-estimate equation (1) using difference-in-difference (DiD) estimates to check that our results are not being driven by any observable or unobservable baseline imbalance between our treatment and control group. We also re-estimate equation (1) with no control variables to check that our results are not being driven by these variables. In both cases

³⁰ These bounds are described in detail in appendix G of the [supplementary material online](#).

Table 5. The Impacts of REAP on Sources of Income

Variable:	(1) Monthly total income per capita	(2) Monthly income from livestock per capita	(3) Monthly income from other agriculture per capita	(4) Monthly income from non-agritrade per capita	(5) Monthly income from labor per capita	(6) Monthly income from transfers per capita
Panel A: The impacts at six months measured at midline						
<i>Treatment</i>	11.084*** (2.639)	1.146 (1.960) [0.765]	-0.003 (0.076) [0.970]	10.041*** (1.732) [0.001]	-0.268 (0.216) [0.545]	0.084 (0.165) [0.765]
Observations	1682	1682	1682	1682	1682	1682
R-squared	0.067	0.050	0.036	0.195	0.059	0.086
Control group mean	24.849	19.817	0.117	3.031	1.205	0.678
Panel B: The impacts at six months measured at endline						
<i>Treatment</i>	8.744** (3.208)	3.938 (2.563) [0.323]	0.108 (0.110) [0.550]	4.458*** (0.633) [0.001]	0.334 (1.239) [0.821]	-0.064 (0.283) [0.821]
Observations	1117	1117	1117	1117	1117	1117
R-squared	0.054	0.066	0.042	0.092	0.023	0.036
Control group mean	25.232	19.811	0.172	1.534	2.911	0.803
Panel C: The impacts at one year measured at endline						
<i>Treatment</i>	7.769*** (2.067)	1.177 (1.780) [0.745]	0.039 (0.141) [0.781]	5.683*** (0.668) [0.001]	0.971 (0.879) [0.683]	-0.132 (0.248) [0.745]
Observations	1095	1095	1095	1095	1095	1095
R-squared	0.072	0.073	0.031	0.115	0.029	0.036
Control group mean	25.232	19.811	0.172	1.534	2.911	0.803
Panel D: p-values and adjusted p-values (in brackets) from Wald tests of the equivalence of treatment effects						
6 month (midline) = 6 month (endline)		0.372	0.380	0.005	0.637	0.613
		[0.634]	[0.634]	[0.025]	[0.637]	[0.637]
6 month (midline) = 1 year (endline)		0.990	0.807	0.025	0.163	0.434
		[0.990]	[0.990]	[0.125]	[0.408]	[0.724]
6 month (endline) = 1 year (endline)		0.274	0.575	0.049	0.596	0.769
		[0.685]	[0.745]	[0.245]	[0.745]	[0.769]

Note: In Panel A, the treatment group is group A and control group is groups B and C combined. In Panel B, the treatment group is group B and control group is group C. In Panel C, the treatment group is group A and control group is group C. Groups A, B, and C refer to REAP participants who received funding in March/April 2013, Sept./Oct. 2013, and March/April 2014, respectively. Column (1) repeats the values from table 4, column (1). Regressions include controls for the number of REAP businesses in a manyatta at baseline, location fixed effects and baseline levels of the outcome variable. Robust standard errors, clustered at the sub-location 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. Asterisks *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively (based on adjusted *p*-values).

our previous conclusions are robust to these alternative specifications.

Finally, we test for the robustness of our results to outliers by replacing outcomes above the 99th percentile with values at the 99th percentile. We find that our previous conclusions are robust to outliers.

Discussion

Income. The REAP significantly increased the income that participants earned in the

short-to-medium-run (i.e., six months and one year after participation in the program). Microenterprise formation clearly seems to be the mechanism that led to this positive outcome.

The results appearing in table 5, where we disaggregate income changes by source, support this conclusion. The data show that the overall increase in income is the result of changes in income from non-agricultural trade (column [4]), which includes income from the REAP microenterprise, measured

as the profits that the participant earned from the business. The increase in income from non-agricultural trade is statistically significant at the 1% level of significance, and this effect persists for up to one year after REAP enrollment. When we examine how participants allocate their time at endline, we find that those who benefited from REAP spend an average of approximately 6% of their day on REAP-related activities. To achieve this increased activity, participants decreased the average time spent on leisure, household activities, and other productive activities in roughly equal amounts (see table F.2 in appendix F of the [supplementary online material](#)).³¹

The six-month impact of the program in terms of income from non-agricultural trade is significantly lower at endline compared to midline. There are two potentially complementary explanations for this difference: competition and shocks.³² As noted in our previous discussion of spillover effects, we find no evidence of a relationship between changes in our estimate of market size (that reflects increased competition) and income from non-REAP businesses (mostly non-agricultural trade) in our control group (see [table 2](#)). Given that both REAP and non-REAP businesses operate in the same sector, we discount the importance of competition in explaining the apparent differences in income between midline and endline.

Turning to shocks, we note that our follow-up surveys occurred during the long dry season of 2013 (midline) and the short dry season of 2014 (endline). The short dry season of 2014 followed a short 2013 rainfall season, with levels of rainfall below average.³³ In the ASALs of east Africa, rainfall shocks such as this lead to less pasture and reductions in

income from pastoralism, which is the main economic activity in the area. This naturally reduces demand. Although this coincidence is not enough to dismiss alternative explanations, it is likely that it played a role in the differences we observe.³⁴

Expenditure and consumption. Increases in income due to REAP do not translate into increases in wellbeing as measurable through consumption or expenditure. The program seems to lead to a short-term increase and a medium-term reduction in consumption and expenditure, but these effects are never precisely estimated (q -values between 0.291 and 0.986, depending on the outcome, the treatment, and the time horizon). This result is perhaps surprising given the low level of initial consumption, but it is not unique, as we discuss below. It also warrants consideration in the context of the program's overall functioning and the emphasis placed on savings, as well as investment decisions in durable assets and livestock, which we discuss next.

Savings, livestock, and other assets. After the training on savings (including the functioning of savings groups) that occurs after six months of participation in the program, more than 95% of participants join a savings group, 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 higher savings per capita.

What might be surprising is that before this training, participants also saved more per capita, suggesting a shift in savings behavior that occurs even before the formal introduction of savings groups. If we look more closely at the savings mechanisms these women use ([table 6](#)), we see that after six months (measured at endline), REAP participants are saving more at home (column [1]) compared to the control group.

Given the economic and social importance of livestock among participants, one would expect an investment of some of the

³¹ It is possible that other labor, either from children or other members of the household, is being mobilized to compensate for the reduction in women's time in some non-REAP activities. We do not have data on other household members time use that would allow us to test for this possibility. However, if child labor is increasing as a result of the new businesses, this does not result in a decrease in school enrollment.

³² A third possible explanation, learning, also seems not to be in effect here given that income from non-agricultural trade at endline is lower than at midline.

³³ The climate in the study site is characterized by two wet and two dry seasons. From January to March there is a short dry spell that is followed by long rains from April to June. From July to October there is a long dry period and this is followed by short rains in November and December. See <http://reliefweb.int/sites/reliefweb.int/files/resources/Marsabit-January-2014.pdf> for an assessment of the conditions in Marsabit County in January 2014, one month before the endline survey was conducted.

³⁴ Despite this shock, there is evidence of continued capacity of these businesses to operate: only two businesses (formed during the first funding cycle) may have disappeared (see table B.1 of the [supplementary material online](#)) and 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 endline (for both sets of participants) compared to the business value at midline (PPP \$374.61 and PPP \$451.55 for group B and group A at endline, respectively, compared to PPP \$305.50 for group A at midline).

Table 6. The Impacts of REAP on Savings Mechanism

Variable:	(1) <i>Personal savings per capita</i>	(2) <i>Per capita savings in non-REAP savings group</i>	(3) <i>Per capita savings in REAP savings group</i>
Panel A: The impact at six months measured at midline			
<i>Treatment</i>	1.014 (0.611) [0.202]	0.054 (0.226) [0.813]	– – –
Observations	1682	1682	–
R-squared	0.085	0.054	–
Control group mean	3.030	0.400	0
Panel B: The impact at six months measured at endline			
<i>Treatment</i>	1.540*** (0.534) [0.010]	–0.034 (0.211) [0.871]	– – –
Observations	1117	1117	–
R-squared	0.026	0.033	–
Control group mean	4.082	0.357	0
Panel C: The impact at one year measured at endline			
<i>Treatment</i>	1.445** (0.558) [0.018]	0.207 (0.333) [0.536]	4.170*** (0.326) [0.001]
Observations	1095	1095	1095
R-squared	0.028	0.015	0.556
Control group mean	4.082	0.357	0
Panel D: p-values and adjusted p-values (in brackets) from Wald tests of the equivalence of treatment effects			
6 month (midline) = 6 month (endline)	0.501 [0.770]	0.770 [0.770]	
6 month (midline) = 1 year (endline)	0.560 [0.690]	0.690 [0.690]	
6 month (endline) = 1 year (endline)	0.888 [0.888]	0.390 [0.780]	

Note: In Panel A, the treatment group is group A and control group is groups B and C combined. In Panel B, the treatment group is group B and control group is group C. In Panel C, the treatment group is group A and control group is group C. Groups A, B, and C refer to REAP participants who received funding in March/April 2013, Sept./Oct. 2013, and March/April 2014, respectively. Regressions include controls for the number of REAP businesses in a man-yatta at baseline, location fixed effects and baseline levels of the outcome variable. Robust standard errors, clustered at the sub-location 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. Personal savings includes savings kept at home and savings kept at a formal financial institution, including mobile service providers. Asterisks *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively (based on adjusted *p*-values).

increased income from entrepreneurial activities in the acquisition of livestock. We find increased livestock ownership among REAP participants, which aligns with these expectations; however, the estimates are not statistically significant once we account for multiple inference.³⁵ Treated households also invest more in durable assets such as blankets and mobile phones, which may improve their living conditions.³⁶

³⁵ See table H.1 in appendix H of the [supplementary material online](#) for a breakdown of the impact of REAP on components of TLU.

³⁶ See table I.1 in appendix I of the [supplementary material online](#) for a breakdown of the impact of REAP on components of the *durable asset index*.

Graduation from poverty. The central aim of this program is to graduate participants from poverty, which we equate with being above the Kenyan rural poverty line as reported by the [Kenya National Bureau of Statistics \(2007\)](#). In table 7 we provide estimates of REAP's impact on the probability of being non-poor at six months and one year after the start of the program, when the defining characteristics of poverty lines are 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 (column [1]). At midline (endline) we find that T_1 increases

Table 7. The Impacts of REAP on the Probability of Living above the Poverty Line

Variable:	(1) Probability that income per adult equivalent is above the poverty line	(2) Probability that expenditure per adult equivalent is above the poverty line	(3) Probability that consumption per adult equivalent is above the poverty line
Panel A: The impact at six months measured at midline			
<i>Treatment</i>	0.138*** (0.034)	0.033 (0.032)	0.037 (0.025)
Observations	1682	1682	1646
R-squared	0.124	0.244	0.270
Control group mean	0.169	0.500	0.606
Panel B: The impact at six months measured at midline			
<i>Treatment</i>	0.065** (0.029)	-0.001 (0.042)	0.012 (0.030)
Observations	1117	1117	1117
R-squared	0.043	0.026	0.193
Control group mean	0.168	0.581	0.526
Panel C: The impact at one year measured at endline			
<i>Treatment</i>	0.132*** (0.025)	0.017 (0.038)	-0.013 (0.032)
Observations	1095	1095	1095
R-squared	0.074	0.022	0.199
Control group mean	0.168	0.581	0.526

Note: Estimates from a linear probability model. In Panel A, the treatment group is group A and control group is groups B and C combined. In Panel B, the treatment group is group B and control group is group C. In Panel C, the treatment group is group A and control group is group C. Groups A, B, and C refer to REAP participants who received funding in March/April 2013, Sept./Oct. 2013, and March/April 2014, respectively. Regressions include controls for the number of REAP businesses in a manyatta at baseline, location fixed effects and baseline levels of the outcome variable (with the exception of *probability that consumption per adult equivalent is above the poverty line* for which baseline levels are not available). Robust standard errors, clustered at the sub-location level, are shown in parentheses. The Kenyan rural poverty line used is as defined by the Kenya National Bureau of Statistics (2007) which, after conversion, is estimated to be \$77.069 per month and per adult equivalent in PPP 2014 terms. Asterisks *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

the probability that beneficiaries are above the poverty line by 13.8% (6.5%), an effect that represents a 81.7% (38.7%) increase over the control group probability of being above the poverty line. The effects are similar at one year, with beneficiaries being 13.2% more likely to have incomes above the poverty line (a 78.6% increase over the control group). Hence, the results are consistent with the idea that REAP can serve as a “cargo net” (Barrett, Carter, and Ikegami 2008) for those who have fallen out of the direct livestock economy by allowing beneficiaries to establish a different livelihood, through microenterprises outside pastoralism.

Examining the impact that REAP has on the probability that a beneficiary has expenditure or consumption above the poverty line (columns [2] and [3], respectively) reveals a small increase in this probability in the treated group at midline and negligible changes at endline. 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 8 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).³⁷ In figure J.1 in appendix J of the supplementary material online, we

³⁷ 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). These regressions also include the same control variables as those in equation (1). 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* for some quantiles at six months due to a large proportion of individuals with zero savings.

Table 8. The Quantile Treatment Effects of REAP

Outcome	(1) OLS Estimates	(2) 10th percentile	(3) 25th percentile	(4) 50th percentile	(5) 75th percentile	(6) 90th percentile
Panel A: Treatment effects at six months (at midline)						
<i>Monthly income per capita</i>	11.084*** (2.639)	3.101*** (0.762)	4.443*** (1.354)	7.306*** (1.776)	9.830*** (2.960)	13.482*** (5.333)
<i>Monthly expenditure per capita</i>	5.668 (3.933)	1.918* (1.051)	1.618 (1.273)	0.584 (1.955)	-0.018 (3.486)	7.056 (9.400)
<i>Monthly consumption per capita</i>	2.773 (2.335)	0.380 (0.916)	0.529 (1.060)	0.908 (1.099)	3.622* (2.135)	6.403* (3.840)
<i>Savings per capita</i>	1.077 (0.714)	- -	- -	- -	0.662 (0.637)	1.842 (1.254)
<i>TLU per capita</i>	-0.011 (0.044)	0.002 (0.025)	0.016 (0.036)	0.013 (0.041)	-0.002 (0.063)	0.009 (0.068)
<i>Durable asset index</i>	0.526 (0.392)	-0.033 (0.432)	0.564* (0.293)	0.655* (0.379)	1.074* (0.596)	0.555 (0.681)
Panel B: Treatment effects at six months (at endline)						
<i>Monthly income per capita</i>	8.744*** (3.209)	1.852** (0.768)	2.372** (1.086)	4.439*** (1.575)	5.877* (3.465)	13.145* (7.360)
<i>Monthly expenditure per capita</i>	2.446 (3.963)	1.595 (1.477)	-0.619 (2.098)	-1.536 (3.130)	-5.098 (3.989)	4.003 (10.086)
<i>Monthly consumption per capita</i>	0.038 (2.134)	1.110 (0.975)	0.275 (0.987)	0.991 (1.348)	0.708 (2.088)	-3.589 (5.384)
<i>Savings per capita</i>	1.536** (0.607)	- -	- -	0.319 (0.414)	1.244** (0.572)	4.324*** (1.293)
<i>TLU per capita</i>	0.180* (0.099)	0.055 (0.035)	0.112*** (0.042)	0.059 (0.064)	0.219* (0.117)	0.505*** (0.155)
<i>Durable asset index</i>	0.751* (0.401)	0.203 (0.337)	0.326 (0.299)	0.487 (0.413)	1.463** (0.676)	1.264* (0.703)
Panel C: Treatment effects at one year (at endline)						
<i>Monthly income per capita</i>	7.769*** (2.067)	2.785*** (0.751)	3.824*** (0.991)	6.481*** (1.367)	10.478*** (2.621)	12.361*** (5.859)
<i>Monthly expenditure per capita</i>	-1.565 (3.444)	0.873 (1.397)	-0.423 (1.913)	-1.327 (2.779)	-3.002 (3.645)	0.881 (11.022)
<i>Monthly consumption per capita</i>	-2.195 (2.202)	-0.328 (1.063)	-1.566 (0.977)	-2.246* (1.284)	-1.641 (3.195)	2.094 (4.547)
<i>Savings per capita</i>	5.836*** (0.765)	2.478*** (0.336)	3.408*** (0.435)	4.928*** (0.595)	6.762*** (1.117)	8.025*** (1.174)
<i>TLU per capita</i>	0.158* (0.086)	0.094*** (0.034)	0.134*** (0.040)	0.104* (0.056)	0.129* (0.078)	0.142 (0.279)
<i>Durable asset index</i>	0.994*** (0.372)	0.418 (0.306)	0.516* (0.299)	0.881* (0.459)	1.255* (0.643)	1.995*** (0.787)

Note: Regressions include controls for number of REAP businesses in a manyatta, location fixed effects, and baseline levels of the outcome variable (with the exception of *monthly consumption per capita* for which baseline levels are not available). Robust standard errors, clustered at the sub-location level, are shown in parentheses. All monetary values are reported in 2014 USD, PPP terms. At six months, we are unable to estimate quantile treatment effects of REAP on *savings per capita* for some quantiles due to a large proportion of individuals with zero savings. Asterisks *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

graphically present 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. These results suggest several conclusions.

The first conclusion is that the effects on income are positive and statistically significant at each of the five quantiles reported in [table 8](#), and these effects are increasing with the quantile of the distribution. This is true for both time periods and irrespective of the length of participation in the program. Hence, the evidence strongly suggests that REAP was particularly effective in terms of increases in income and in the short-to-medium-run, for those who were (relatively speaking) better-off, as predicted by our model. The effect of the program estimated at the 90th percentile is more than 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 there are more pronounced effects among individuals in the upper quantiles of the other outcome distributions. These patterns are clearly illustrated in [figure J.1](#) in the [supplementary material online](#). There, 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 endline.

The third conclusion is that the timing of measurement of the program's impact (midline vs. endline) 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), likely reflecting the factors discussed previously. 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. After the promotion of savings groups, we find significant treatment effects across the entire distribution, though the effects are stronger for the upper quantiles.³⁸

³⁸ Note that before the introduction of savings groups, we only observe significant effects on savings in the upper quantiles (75th and 90th at endline) of the savings distribution.

The similarity of the patterns exhibited in [table 8](#) and [figure J.1](#) could suggest 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 outcomes, such as savings or consumption of durable assets. 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 J.1](#) in [appendix J](#) of the [supplementary material online](#), we present the proportion of individuals who are in the 90th percentile of different combinations of outcome variables. The obvious conclusion is 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](#)), though we leave a deeper analysis of these differences for future research. Nevertheless, we note that this conclusion seems to be reinforced by an analysis of the effect of baseline heterogeneity on the effect of the program, where we interact the treatment indicator with baseline indicators of capital (see [tables J.2, J.3 and J.4](#) in [appendix J](#) of the [supplementary material online](#)). At six months (evaluated at endline) households at the higher end of the baseline livestock distribution are saving less compared to those with no livestock at baseline, and at one year, those households who had some savings at baseline own less livestock. Hence, our data suggests that there may be some specialization in terms of future activities, although we would require further follow-up surveys to understand whether such a pattern of behavior persists.

Comparison of our findings to other studies. Finally, it is important to note that our estimates of the program's impact are of a similar order of magnitude to previous studies, namely [Banerjee et al. \(2015\)](#) and [Bandiera et al. \(2016\)](#). After one year, there is a 30.8% increase in income compared to the control group, which is similar to the increases in income that can be estimated from the results

presented in Banerjee et al. (2015), who find an average increase of 25.7% (22.8%) after two years (three years), and Bandiera et al. (2016), who find a 38% increase in income after four years. Like our work, these studies do not find an important effect on consumption: Bandiera et al. (2016) do not find statistically significant impacts on consumption after two years, while Banerjee et al. (2015) find a relatively small impact (around 5%).

The estimate of the program's impact on savings (131.4% increase) is also similar to those Banerjee et al. (2015) estimated (155.5% increase after two years, and 95.7% increase after three years). Finally, we find that REAP increases the probability of having an income above the poverty line by 13.2%, a result similar to the 11% shift in women out of extreme poverty that Bandiera et al. (2016) estimated.

As with the other ultra-poor poverty graduation programs, our findings are more conservative than those of Blattman et al. (2016), which is the most similar to REAP. These differences may be attributable to both the post-war setting that they study (notably, the low levels of initial business activity and the much lower baseline income), and differences in the set-up of the program (namely, a significantly larger initial grant than in the case of REAP).

Turning to the cost-benefit analysis of this program, we estimate that the cost for one additional woman to be enrolled in REAP in 2015 for two years was approximately \$300 (or PPP \$713.91 at 2014 prices). This figure is well below the direct costs of the six programs that Banerjee et al. (2015) evaluated, as well as the program that Blattman et al. (2016) evaluated. Assuming that the program implementation cost in 2015 was the same as 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 to cover the cost of the program.

Conclusion

In this paper we study a multifaceted approach to poverty alleviation that is gaining recognition for its ability to set ultra-poor households on a sustainable pathway

out of extreme poverty (Banerjee et al. 2015; Bandiera et al. 2016). We show that a variation of the poverty graduation approach, the Rural Entrepreneur Access Project (REAP), which provides disadvantaged women with capital, skills, and ongoing mentorship in enterprise and savings but that excludes consumption support, replaces asset transfers with cash transfers, and targets individuals who are required to form groups of three, enables beneficiaries to run microenterprises that lead to improved household incomes. The short-to-medium-run impacts are economically significant and allow women to meet current household needs (through increased investment in durable assets) and plan for future shocks (through the accumulation of liquid savings). 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. Hence, REAP provides women with greater agency over wealth transfers through cash, and the exact type of business to set up. Simultaneously, these factors do not appear to lead to misuse of funds or suboptimal choices about the type of earning activity to pursue, as designers of other graduation programs have feared.

The estimates of REAP's impact are largely in line with other evaluations of similar programs (Banerjee et al. 2015; Bandiera et al. 2016; Blattman et al. 2016). 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 trials raises the plausible prospect that these impacts should be stable over time.³⁹ A simple cost-benefit analysis shows that, in this likely eventuality, the program would cover costs within a reasonable time horizon (thirteen months).

We are also able to demonstrate the potential applicability of this approach in a different, arguably more extreme context to those already studied. The REAP implementation occurred in locations with very low average population densities, highly variable weather

³⁹ Banerjee et al. (2015) examine two-year and three-year impacts and find no evidence of mean reversion of the impacts. Bandiera et al. (2016) look at two-year and four-year impacts and find more pronounced effects across many outcomes after four years compared to after two years.

conditions, low infrastructure, and limited access to markets, settings which have been robustly shown to be prone to asset-based poverty traps. Yet, women were able to make use of the capital and skills that REAP delivered to establish and run successful enterprises, and obtain a sustainable livelihood outside of the livestock herding economy. This consistency of results provides important support for the robustness of the poverty graduation approach, and further corroborates the external validity of other studies, while suggesting further opportunities for experimentation in the design and implementation of such programs. For example, graduation program designers could consider experimenting with group-based approaches, transferring cash rather than an asset (which can greatly reduce implementation costs), or reducing costs by minimizing initial consumption support.

Supplementary Material

Supplementary material is available at *American Journal of Agricultural Economics* online.

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