Project Description and Research Design

Can Asset Transfer & Asset Protection Policies Alter Poverty Dynamics in Northern Kenya? Research Design Summary

1. Hypotheses to be Tested

The pastoralist regions of Northern Kenya have seen a number of interventions intended to address the deep poverty and the extreme vulnerability with which it is intertwined. This research examines two of those interventions that have recently received a great deal of public support and recognition.[1]

- *IBLI Asset Protection Program*, which developed a satellite-based index that triggers payouts to households that experience climatic shocks, allowing them to protect their assets.

- *REAP Asset Transfer (or Graduation) Program*, which transfers and helps poor women build a suite of tangible and intangible (or psychological) assets; and,

While there is evidence that both programs are individually effective in the short to medium term, the programs have the potential to complement each other and in combination offer a package that fundamentally alters poverty in the region. Having secured the cooperation of the program implementers, the research proposed here will test the efficacy of this package and how best and most cost-effectively to assemble it.

The primary objectives of the proposed research are to (1) measure the impact of each intervention on the extent and depth of poverty at the community level, and (2) compare that to the impacts of a package that combines both interventions. Explicitly, we will test the following hypothesis:

> *Hypothesis 1*: The REAP intervention improves the welfare of its participants.
> *Hypothesis 2*: IBLI insurance coverage improves household welfare.
> *Hypothesis 3*: The REAP intervention and IBLI coverage are complimentary, meaning that together they have positive welfare impacts that are larger than the sum of their impacts individually.

There are also three secondary research objectives:

- Given the costliness of graduation programs like REAP, and given that many of the benefits of graduation programs can be hypothesized to spillover and assist others who are not in the program, is there an optimal program enrolment (or program saturation) rate beyond which additional benefits do not justify the additional costs? That is, can most of the benefits of a graduation program be achieved by directly treating only 30% or 40% or 50% of eligible women?

- While our focus is on household welfare and poverty more generally, the research will seek to uncover any gender-differentiated impacts. This agenda will include impact analysis of the three treatments (REAP, IBLI, REAP+IBLI) on the welfare and empowerment of women in treatment households.

[1] Citations available in full proposal.

- Neither program has undergone an analysis of long-term impacts. The end-line survey will be collected nearly two years after the first cohort of REAP participants have graduated from the program while the final and fifth cohort will be just be graduating from the program. At the same time, those households treated with insurance subsidies will have been in the program for four years. This will allow an analysis of the persistence and trajectory—growth or decline—of impacts over time.

2. Interventions to Be Evaluated

The NGO BOMA has implemented the **REAP (Rural Entrepreneur Access Project)** graduation intervention in the Kenyan ASALs following a model relatively similar to the BRAC ultra-poor programs. REAP provides a support package consisting of cash grants for micro-enterprise start-up run by small women groups, as well as intense one-on-one training and mentoring. Concretely, the graduation program targets the most vulnerable women and helps them to establish a sustainable income and savings stream through a two-year program of sequenced interventions:

1. Targeting of participants for the development of three-woman business groups;
2. Two years of mentoring;
3. Business skills training;
4. A seed capital jump grant of $US 200 to start the business;
5. Savings group training and a $US 100 progress grant (if the business remains in operation); and,
6. Access to credit through membership in savings groups

The objective of the REAP program is to address three constraints faced by the poorest households in the ASALs: low income, irregular cash flows, and scarce access to financial services. The goal is to lift households out of extreme poverty, i.e. for graduating women to be able to meet basic needs such as accessing food security, paying education and medical expenditures, and building a small saving base. While the price of this intervention is somewhat modest compared to continuing cash transfers, the cost and intensity of the REAP and other graduation program raises the question as about how to lower its cost and more rapidly spread its benefits.[2]

Index Based Livestock Insurance (IBLI) has been piloted in Marsabit since 2010 as a contingent safety net intervention, with the objective of compensating pastoralists for lack of forage related to droughts.[3] The insurance relies on an NDVI satellite index to measure lack of pasture and make insurance payments. Since its inception, IBLI has expanded to include five additional counties in Kenya and the Borena Zone of Ethiopia. The government of Kenya has also initiated its Kenya Livestock Insurance program (KLIP) modeled on the IBLI program. Findings from various studies suggest that IBLI has the potential to alter poverty dynamics by preventing descent and promoting ascent of poor households. [4] These important impacts coupled with the low demand for the commercial product suggest an opportunity for public support in the form of premium subsidies as well as other institutional and policy support.

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[2] According to the BOMA management team, the total cost per-beneficiary is approximately 56,000 KSh ($550) for the two-year intervention.

[3] Chantarat, Mude, Barrett, & Carter, 2013.

[4] Jensen, Barrett, & Mude, 2017; Janzen & Carter, 2013.

For this study, a private insurance company will offer IBLI contracts in the study area beginning in January 2018 using a novel subsidy design. At the heart of this alternative design is a smoother, less discriminatory rate of subsidy on individually scalable contingent social protection. This design would provide a level of insurance that households can adjust to their level of TLU holding. The level of subsidies is progressively phased out in order to avoid the discouragement effect that is characteristic of the KLIP (where the price jumps from 0 to 100% of the market price after a household receives an allotment of free insurance. While subject to final negotiation with the insurance company, the proposed IBLI intervention will offer the following sliding subsidy scheme to households randomized into the IBLI insurance treatment:

- 100% subsidy on the first TLU[5] unit of insurance;
- 80% subsidy on purchased insurance for 2-5 additional TLUs;
- 40% subsidy on insurance for TLUs 6-9;
- 20% subsidy on TLUs 10-15;
- 0 subsidy for units of insurance beyond 15.

3. Basic Experimental Design

As described above, the study will focus on two sub-populations of interest: (i) Poor households who are eligible for the REAP program; and, (ii) Vulnerable, non-poor households who are not REAP-eligible but whose assets and living standards place them just above the eligibility threshold. To study these two sub-populations, a research design has been constructed to be consistent with BOMA's REAP implementation protocol. Table A3.1 in Annex 3 lists the basic timeline, which includes a baseline in January, 2018, followed by implementation of the insurance and REAP interventions, followed by a midline in January 2020 and an endline in January 2022.

The salient features of the implementation protocol as they shape or constrain the research design are the following:

Constraint 1. The study area will be comprised of 7 "mentor areas" in Samburu North Sub-county;

Constraint 2. Each mentor area is comprised of (approximately) 10 villages or communities that are serviced by a single REAP program mentor;

Constraint 3. Each mentor will organize 20 new businesses in their area in each of 5 rollout waves, for a total of 100 new businesses in each mentor area and 10 businesses (on average) in each community;

Constraint 4. No more than 100 [10] new businesses will be established in any mentor area [community] even though there will be enough poor, eligible women to populate up to 2.5 times that number of new businesses;

Constraint 5. The first rollout wave will begin in March, 2018, and a new wave will begin every 6 months until a total of 5 waves is reached, with the last wave being initiated in March, 2020; and,

[5] A TLU or tropical livestock unit (one cattle or its equivalent in terms of sheeps, goats or camels) is the unit in which IBLI contracts are sold.

Constraint 6. Women's eligibility for the REAP program will be based on the standard BOMA 2-step process.[6]

The basic goal of the experimental design is to randomly select and allocate poor, REAP-eligible women to the 4 primary treatment arms shown in Table A2.1. Similarly, the design will randomly select and allocate vulnerable, REAP-ineligible women to the 2 treatment arms shown in Table A2.3. Vulnerable, REAP-ineligible households are defined as those in the third wealth ranking group (see footnote 6).

Total sample sizes that will be available for midline data collection in January 2020 are shown by the **bold** numerals in the two tables. These numbers are used in the basic power calculations developed and discussed in detail below.

TABLE A2.1: Sample Structure: Treatment Status & Duration of Poor Households at Midline

		IBLI	
		NO	*YES*
REAP	*NO*	**330**	**330**
	YES	**330**	**330**
	24 months*	*83*	*83*
	18 months*	*83*	*83*
	12 months*	*82*	*82*
	6 months*	*82*	*82*

* Duration of treatment in months since REAP program enrollment

Because the REAP program is rolled out in waves, women recruited into the REAP program in different waves will have differing durations under in the REAP program by the time of the midline survey. The lower half of Table A2.1 subdivides treated women by the treatment duration they will have received by midline. Women with a 24-month treatment duration are those enrolled in wave 1 initiated in March, 2018; those with an 18-month treatment duration are those from wave 2 initiated in September 2018; *etc*. Procedurally, prior to initiation of the REAP treatment, all women in each study community will be divided into three groups:

Group 1. Poor, REAP-eligible;
Group 2. Vulnerable non-poor, REAP-ineligible; and,
Group 3. Better off households of no interest to the study.

Following the saturation protocol detailed in the next section, women in the first group will be allocated randomly to the following 5 groups prior to initiation of the first REAP wave:

1. Wave 1 households to be offered REAP enrollment in March, 2018;
2. Wave 2 households to be offered REAP enrollment in September 2018;

[6] First, BOMA implements a Participatory Rural Appraisal (PRA) for classifying *all* households in each village into 4-6 wealth categories using village-defined wealth categories. Regardless of the criteria used by the village for the wealth categories, only women in categories 1 and 2 (the poorest two categories) can be eligible for the REAP. Based on BOMA's previous experience in Marsabit County and Samburu East and West Subcounties, we expect 50% of the women will fall into these lowest two wealth categories. Those women are then further screened for their interest and capability to potentially succeed in the REAP program using the Participant Targeting Tool (PTT) and BOMA Locational Committees (BLC). If a potential candidate is deemed not appropriate, the BLC will request that BOMA replace them with new a candidate.

3. Wave 3 households to be offered REAP enrollment in March 2019;
4. Wave 4 households to be offered REAP enrollment in September 2019;
5. Wave 5 households to be offered REAP enrollment in March 2020; and
6. Permanent Control Households

To achieve these sample sizes, we will require a total of 66 communities selected from the 7 mentor areas in Samburu where BOMA is rolling out the REAP program in early 2018. In each community, the sample will be comprised of the following:

- One woman selected at random from each of the 10, 3-women businesses that REAP will initiate in rollout waves 1-4. Half of these women—and their households—will be allocated the insurance treatment. That is, the survey sample will consist of 660 women who across the first four waves will receive the BOMA treatment, of which 330 will also receive the insurance treatment available from the outset.
- Ten women selected at random from the women randomized into the "control households" group. Half of these women—and their households—will be offered the insurance treatment. That is, the survey sample will consist of 660 BOMA eligible women who will not receive treatment during the duration of the study. Of these, 330 will receive the insurance treatment available from the outset.
- In wave 5, 100 of the 660 women who had previously been classified as sample "control households" will be selected to join BOMA groups and thus will switch to the BOMA treated category in the sample. Half of these women (50) will have been treated with insurance and 50 will not. This transition, happening in March 2020, will occur just after the midline. As indicated in Table A2.2 below, this will mean 760 of the sample households will be BOMA-treated at endline, and 560 will remain untreated.
- Five women from Group 2 (least well-off fraction of the REAP-ineligible group). Half of these women (2 or 3)—and their households—will be offered the insurance treatment. Specifically, as shown in Table A2.3, the survey will consist of a sample of 330 group 2 women, half of which (165) will receive the insurance treatment.

The italicized numbers in the lower half of Table A2.1 show the distribution of treatment durations that the above procedures will obtain at midline. We discuss in the power calculation section below how we will analyze these different treatment durations. Table A2.2 shows these modified numbers as well as the implied distribution of treatment durations at endline.

TABLE A2.2: Sample Structure: Treatment Status & Duration of Poor Households at Endline

			IBLI	
			NO	*YES*
	NO		280	**280**
REAP	*YES*		380	**380**
		48 months*	*83*	*83*
		42 months*	*83*	*83*
		36 months*	*82*	*82*
		30 months*	*82*	*82*
		24 months*	*50*	*50*

* Duration of treatment in months since REAP program enrollment

Finally, Table A2.3 shows the sample structure for vulnerable, non-poor households. Prior to initiation of the treatments, 330 Group 2 households will be randomly selected for inclusion in the study and randomly and equally allocated between the control and treatment groups.

TABLE A2.3: Sample Structure: Treatment Status of Vulnerable Households at Midline & Endline

IBLI	
NO	*YES*
165	**165**

4. Experimental Design: Differential Saturation at the Community Level

As discussed above there are a number of important and highly policy-relevant questions about the spillovers of both the business creation and also the intensive personal mentoring that are hallmarks of BOMA's REAP program. To study the impact of spillovers (be they positive, negative or non-monotonic), our research design will use the wave rollout structure of the BOMA implementation model to create differential program saturation in communities over the baseline to midline period. The saturation design is subject to the following constraint imposed by BOMA's program implementation logic:

Constraint 7. Each community must get at least one new business every wave.[7]

Table A2.4 lays out the proposed saturation scheme. *Constraint 3* (above) and *Constraint 7* impose limits on the saturation design. Saturation Scheme A in Table A2.4 is the largest amount of saturation that can be realized given these constraints, whereas Saturation Scheme B is the minimum amount of saturation that can be realized. Saturations Schemes C and D are less extreme than A and B. The far-right column of the table shows the number of communities in each mentor area that would be allocated to each saturation scheme.

The figures in the square brackets in the first column of Table A2.4 give two alternative measures of the total treatment intensity or saturation under each scheme. The first figure in each row (which ranges from 180 to 540) is the total number of months that women in the community would have been

[7] This constraint insures that the mentor remains active, present and connected in each community at all times.

enrolled in the REAP program by the time of the midline survey.[8] The second indicator variable (ranging from 3 to 8) is the total number of new REAP businesses that would have been started by the time of the midline. Here we assume that a new business does not become operational until 6 months of program enrollment. Hence only the businesses from waves 1-3 would be operational by the time of the midline survey.

While there is no single best measure of saturation level within a community, the first measure (total treatment months) may be more appropriate for gauging aspirational and other psychological spillovers to non-treated women, while the second measure may be more relevant for gauging pecuniary externalities that economically affect treated and non-treated households. Note that an equal rollout strategy across all communities would imply 360 treatment women months in all villages and 6 new businesses at midline. In contrast, the variable saturation rate strategy implies variation in both saturation measures. Specifically, under the variable rollout scheme, 60% of treated communities will have 360 women months of treatment, while 20% will have 50% more treatment (540 months) and 20% will have 50% less (180 months). In terms of new businesses formed by REAP, 50% of communities will have 6 new businesses, 30% will have 8, and 20% will have only 3.

TABLE A2.4: Community-level Saturation Levels within a Single Mentor Area

Saturation Scheme [Saturation Rates]	**New Businesses Per-community** *Wave 1*	*Wave 2*	*Wave 3*	*Wave 4*	*Wave 5*	*Number of Communities*
A *[540, 8]*	6	1	1	1	1	2
B *[180, 3]*	1	1	1	1	6	2
C *[360, 6]*	1	3	2	3	1	5
D *[360, 8]*	1	1	6	1	1	1
TOTAL IN MENTOR AREA	**20**	**20**	**20**	**20**	**20**	**10**

5. Statistical Analysis and Power of the Binary Treatment Model

Consider the following ANCOVA regression equation for analyzing the impacts of the proposed interventions in Samburu County, on the livings standards of poor, REAP-eligible households (y^p_{hvt}):

$$y^p_{hct} = \alpha^p_1 y^p_{hc0} + \beta^p_I I_{hct} + \beta^p_G G_{hct} + \beta^p_{IG} I_{hct} G_{hct} + \delta^p_1 S_{ct} + \delta^p_2 S_{ct} G_{hct} + \varepsilon_{hct},$$

where the subscript *h* designates household, *c* community, and *t* time period. The superscript *p* denotes measures and parameters for poor, REAP-eligible households. The binary treatment variable I_{hct} takes on the value of 1 for household *h* receiving the insurance premium discount coupon treatment and, G_{hct} does the same for households offered the REAP program. The saturation variable, S_{ct}, is the saturation

[8] Recall that each business comprises three women. For scheme s, the total treatment months will be $T_s = \sum_{w=1}^{4} 3 n_{sw} m_w$, where n_{sw} is … and m_w is the number of months enrolled for each wave up until the time of the baseline survey.

measure (defined in the prior section) for community *c*. This simple model allows us to measure the basic impacts of the two programs (β_I^p, β_G^p,) and any household level complementarity (β_{IG}^p,). The term δ_1^p allows us to measure whether graduation training spills over to non-treated households, while δ_2^p allows us to see if spillovers occur between households selected for graduation.

Note that this regression equation treats the REAP program as a single, binary treatment, whereas the rollout pattern (detailed in Table A2.1 above) means that different households will have been enrolled in the program for different periods of time. While we might imagine that the impacts are different depending on period of time in the program (a fact that is exploited in the continuous treatment model discussed in Section 6 below), we here consider the statistical power for detecting an average ITT effect, understood as an average across households and enrollment durations. Based on the prior study by Gobin *et al.* (2017) on the income impacts of the REAP program, we can anticipate that ITT treatment impacts might be as large as large as 20-30% only 12 months after REAP enrollment (6 months after initiating a new business). As we shall see, our study is adequately powered to pick up economically meaningful impacts even if average effects are diluted by households with low exposure to the program. A similar but simpler expression can be written for vulnerable, non-poor households, which are not eligible for the REAP program:

$$y_{hct}^v = \alpha_1^v y_{hl0}^v + \beta_I^v I_{ht} + \delta_1^v S_{ct} + \varepsilon_{hct}.$$

The coefficient β_I^v is the primary coefficient of interest for this group of households, indicating the impacts of the insurance on y_{hct}^v.

We obtain minimum detectable effects (MDEs) for our two binary treatment terms using standard procedures. For this exercise, we focus on household consumption, as this measure is especially noisy and thus makes our power analysis conservative. Key parameters are derived from the IBLI impact evaluation panel survey of pastoralist households in nearby Marsabit County. As already mentioned, we have individual randomization for both REAP[9] and insurance treatments and will have 330 observations per-treatment arm for poor, REAP-eligible households and 165 households per-arm for vulnerable REAP-ineligible households. Our specific assumptions are as follows:

- For REAP eligible households (which we define as households in the two lowest expenditure quartiles in the IBLI panel data), we find that the average level of real consumption is 10,571 KSH with a coefficient of variation of 28%. Note that these figures are averages across 5 survey rounds which included a mix of good and bad years.
- For Vulnerable, REAP-ineligible households (which we define as households in the middle two expenditure quartiles), we have average consumption of 16,023 with a coefficient of variation of 21.7%.

Figure A2.1 shows the MDEs for our study design. MDE's (for 80% power using hypothesis tests with a 5% Type I error probability) for the poor, REAP-eligible sample are shown by the red, solid curve as a function of net compliance. The MDE's for the vulnerable, REAP-ineligible households are shown by the blue, dashed curve. Despite the smaller sample size, the less variable consumption pattern of this better-off group means that the MDE's are quite similar to those to the poorer group.

Figure A2.1 displays the MDE's as a function of net compliance of the treatments in order to fully

[9] Strictly speaking, our saturation design makes adds an element of quasi clustering to the REAP program as some community clusters will have more early-enrolled households than others. With a total of 66 community clusters, penalizing the power analysis for this partial clustering makes very little difference using IBLI panel estimates that show a modest intra-cluster correlation.

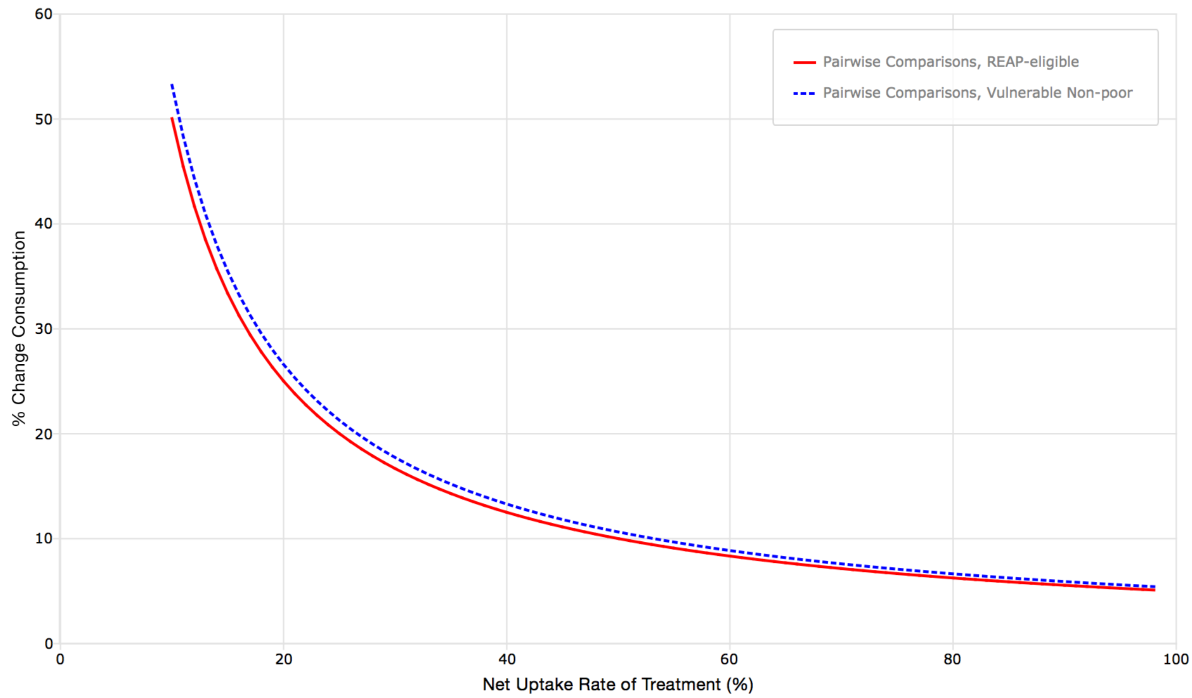
illustrate the sensitivity of our design to our assumptions about uptake. Based on historical reports from BOMA, we can anticipate a 95% participation rate. At this level of participation, the design is quite well-powered to detect even modest impacts of 6% changes in household living standards.

While uptake of insurance contracts is notoriously problematic (although IBLI in Marsabit has done much better than most programs), the insurance uptake encouragement design explained in Section 2 (which includes an element of free insurance) above will induce very high levels of participation.[10] We assume that 80% of households will accept the highly subsidized insurance units (the one free unit plus the two units offered at an 80% subsidy). Given average livestock of poor households (5 TLU) and vulnerable households (10-15 TLU), the implied insurance coverage levels are 60% of livestock wealth for poor households and 20-30% for vulnerable households. As can be seen in Figure A2.1, the implied MDEs are around 8%, which seems quite reasonable given these coverage levels and the risk exposure of Samburu households.

The other key parameter of interest is the slopes of the saturation rate terms (δ_1 and δ_2). An economically meaningful slope coefficient would be one that implies that full saturation would deliver at least half the benefits of full treatment. Given that the Gobin *et al*. study identified an impact of REAP as 30% increase in living standards (raising incomes from roughly 10,000 KSH to 13,000 KSH), we would find a full saturation spillover effect that raised the incomes of non-treated households to 11,500 KSH to be economically meaningful. A spillover impact of this magnitude would imply a slope $\delta_1 = 1{,}500$. Following Dupont and Plummer (1998), we calculate the minimum detectable slope (MDS) under our proposed study design. For the critical δ_1^p parameter that measures spillovers from treated to non-treated households, the MDS is 1,192, while it is 1,062 for δ_2^p. The proposed study is thus adequately powered to detect economically significant spillover benefits should they occur.

[10] In a side analysis, we will be using experimental methods to devise contract designs and marketing especially intended to appeal to women. While not yet proven, we anticipate that this effort will further enhance insurance uptake, especially amongst REAP participants whom we believe will be empowered by that treatment.

FIGURE A2.1. Minimum Detectable Effects for Different Interventions (Household Level)



While these household level impacts are important in and of themselves, we are also interested in the impacts of the social programs on the extent and depth of poverty. We are thus interested in the precision with which we can estimate changes in community level poverty headcounts and gaps.[11] Because we estimate the average community with much greater precision than we have for estimating an individual outcome, we are able to more precisely estimate changes in community level poverty indicators. We should thus be adequately powered to detect changes in the rate and depth of poverty at the community level.

6. Duration or Continuous Treatment Analysis

While binary treatment impacts over a defined time horizon can be informative, programs like REAP and IBLI are interesting precisely because they are hypothesized to induce learning, asset accumulation and behavioral changes that play out over time. Several recent studies have shown the power of continuous treatment or duration analysis to identify the nature of these patterns. To give one example, Carter and Keswell (2014) show that a land redistribution program in South Africa induced a short-term consumption decrease and a long-term or asymptotic consumption increase that was 70% higher than the impacts implied by conventional binary treatment estimates.

The rollout waves employed by BOMA in the REAP program present the perfect opportunity to explore the intertemporal impact patterns which are at the heart of the graduation and insurance program

[11] Consider, for example, the baseline average poverty gap in community c:

$$G_c = \frac{\Sigma_{i=1}^{30}(P - y_i)}{nP},$$

where P is the money metric poverty line. Under our design, we can estimate the average poverty gap with 30 observations of households in each of 66 communities.

logics. Under the randomized allocation of households to rollout waves summarized above, we will enjoy the following distributions of treatment durations and midline and endline for the REAP-eligible households who do NOT receive the insurance treatment:

TABLE A2.5: Treatment Distribution (REAP-eligible households without IBLI treatment)

	TOTAL Sample Size	**Months of Treatment** *(REAP-eligible Households without IBLI Treatment Only)*								
		0	*6*	*12*	*18*	*24*	*30*	*36*	*42*	*48*
Baseline	660	660								
Midline	660	330	82	82	83	83				
Endline	660	280				50	82	82	83	83
TOTAL *n*	**1980**	**1270**	**82**	**82**	**83**	**133**	**82**	**82**	**83**	**83**
[% sample]	**[100%]**	**[64%]**	**[4%]**	**[4%]**	**[4%]**	**[7%]**	**[4%]**	**[4%]**	**[4%]**	**[4%]**

Letting m_{hct} denote the months since enrollment in REAP by household *h*, in community *c* in time period *t*, we can write a simple continuous treatment model (ignoring fixed effects and other potential control variables) as:

$$y_{hct}^{p} = f(m_{hct}) + \lambda_t + \epsilon_{hct},$$

where the function f is a flexible parametric or non-parametric function (see Tjernström *et al*., 2013 and Carter *et al*. 2017 for uses of these methods for the analysis of continuous treatment data in the case of an RCT).

To get an idea of the power of our sample to detect significant impacts of this continuous treatment, we consider he simple case where f is the simple linear function, $\gamma_0 + \gamma_1 m_{hct}$. Following the power calculations procedures summarized in Section 5 above for the spillover/saturation analysis, we find that the minimum detectable slope is conservatively calculated[12] as 36.4. A slope of this magnitude would imply a 12-month impact of 4.1%, and a 24-month impact of 8.2%. As expected, taking advantage of the continuous treatment structure yields a more powerful analysis than the binary estimators considered in Section 5 above.

[12] These calculations utilize the midline treatment distribution shown in Table A2.5 and conservatively assumes that the standard deviation of the regression error is 2959, which is the *un*conditional standard deviation for REAP-eligible households calculated from the IBLI panel data.