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Assessing the Impacts and Spillover Effects of Multifaceted Graduation Programs in a Pastoralist Region

By

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DISSERTATION

Submitted in partial satisfaction of the requirements for the degree of

DOCTOR OF PHILOSOPHY

in

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Approved:

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Abstract

Poverty reduction programs inspired by BRAC's graduation approach aim to develop both tangible productive assets and intangible psychosocial assets such as self-confidence and the aspiration for upward mobility. In the broader global context, recent estimates underscore the urgency of poverty alleviation efforts, especially in a post-COVID-19 world. The surge in extreme poverty, coupled with disproportionate setbacks faced by the vulnerable, emphasizes the necessity of targeted interventions like the graduation approach, including fiscal support.

The first chapter of this thesis seeks to enhance our understanding of how psychosocial factors operate and shape the impact of graduation programs. After formulating a set of hypotheses about the effects of psychosocial constraints based on a dynamic optimization model that considers the choice between a low-income casual wage-labor occupation and a higher-earning entrepreneurial activity, this paper utilizes a randomized controlled trial of a graduation program implemented in the pastoralist regions of Northern Kenya. Key empirical findings reveal that the estimated highly favorable average treatment effects conceal significant heterogeneity, as beneficiaries who initially experienced severe depressive symptoms gained little from the program. The saturation design of the RCT also enables the identification of substantial spillover effects on the asset accumulation of women who were not enrolled in the graduation program. Spillovers are also shown to positively impact non-beneficiary women's preference for upward economic mobility, providing a plausible explanation for their capital accumulation despite not receiving direct support from the graduation program. The paper extracts implications from these findings for the cost-effective design and implementation of graduation programs.

The second chapter delves into the spillover effects of the REAP program through the female kinship network within local communities. In contrast to the previous chapter, which employed a saturation measure on community-level interaction with treated women to assess the impact of treatment intensity on REAP-eligible women, this study investigates spillover effects on control group women whose relatives benefited from the program. The findings reveal increased business assets, savings, and living standards among non-treated women whose relatives were recipients of the REAP program. Importantly, spillover effects are more pronounced for non-head control female respondents compared to female household heads. The study also draws a comparison between spillover effects at the kinship network and community levels. Similar effects are observed in business assets, with unique spillover effects on savings through the kinship network. Overall, this chapter underscores the significance of network structures in poverty alleviation program spillover effects

and calls for inclusive strategies tailored to vulnerable groups and their specific needs. The study enhances our comprehension of local network dynamics and provides insights into the success of interventions within developing economies and the intricacies of spillover mechanisms.

The third chapter delves into the long-term impacts of the REAP program in pastoralist regions of Northern Kenya, taking into account spillover effects. The research discovers substantial positive effects of the program on women's productive assets and financial savings even four years into the program. However, severe depressive symptoms among beneficiaries lead to varying treatment effect trajectories. With the inclusion of endline data, the study demonstrates continuous treatment effects, revealing an asset accumulation and savings trajectory that peaked at around 30 months into the treatment and then declined by about 20% by 48 months. A positive trajectory for income over 48 months of the program is also observed. Importantly, the spillover impact found at the midline is not sustained at the endline. The distinctive characteristics of the program, including direct initial cash transfers and collaborative business management, introduce complexities in impact estimation, exacerbated by external shocks such as floods, locusts, and COVID-19. Challenges in measuring jointly owned business assets arise from group-based enterprises. These findings provide insights into the intricate dynamics of poverty alleviation interventions in challenging contexts.

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I am deeply indebted to many people, especially my main advisor, Michael Carter, who has exhibited a tremendous amount of patience with me throughout the years. I want to thank him for his thought-provoking ideas and insightful discussions; he acted as a safety net for my assertions and sometimes wild ideas. A single discussion with him could leave me pondering for months. Thank you, Michael. This journey has felt more like an apprenticeship to me. I have learned not only how to conduct research, but also attitudes toward career, life, and loved ones from you. I hope I have shown that I have, at the very least, become a bit more like an economist through my dissertation, and if that's the case, it's largely due to your influence.

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I am truly grateful to my parents, who have provided me with endless support throughout my life. They've given me the best the world has to offer – their love, kindness, trust, and unwavering support. Rather than simply showing me the world, they've guided me to approach it with courage, excitement, and curiosity, rather than fear. Without a doubt, they are the best parents I could have ever asked for. My aspiration is to emulate even a fraction of their exceptional parenting.

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Lastly, I want to thank myself for being brave, maintaining a positive outlook, and always embracing challenges with a warm heart. I am proud of the person I've become, and it's amazing to see how far I've come!

Contents

[1]The chapter is co-authored with Michael R. Carter, Nathaniel D. Jensen & Laurel Krovetz. This study was made possible through the generous support of the American people through the United States Agency for International Development Cooperative Agreement No. AID-OAA-L-12-00001 with the BASIS Markets, Risk and Resilience Feed the Future Innovation Lab. The contents are the responsibility of the authors and do not reflect the views of the US Government. We thank seminar participants at the BRAC, the UN FAO, the University of California, Davis, the BOMA Project and the 2022 Evidence to Action Conference. We also thank our respondents and our implementing partners. The projects activities were ruled exempt under Category 2 by the IRB at the University of California, Davis, project number 1627031. This RCT was registered in the American Economic Association Registry for randomized controlled trials under trial number AEARCTR-0005822.

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CHAPTER 1

Introduction

1.1. Extreme Poverty and Graduation Programs

Recent estimates (World Bank, 2022) show a surge in extreme poverty—living on less than $2.15 per person daily at 2017 purchasing power parity—by 70 million in 2020, surpassing 700 million post-COVID-19. This is the largest annual increase since the 1990 inception of global poverty monitoring. Before COVID-19 and Ukraine's conflict, extreme poverty consistently decreased for three decades. It's clear the 2030 goal to eradicate this poverty will not be met; projections indicate around 574 million people, almost 7% of the global population, will remain below $2.15 a day by 2030, mainly in Africa.

The Poverty and Shared Prosperity 2022 Report underscores another harsh reality: the poorest 40% experienced double the income loss compared to the wealthiest 20%. Unequal setbacks in education and health disproportionately affected the most vulnerable, leading to significant learning deficits and shortened lifespans. Without policy intervention, these setbacks will have lasting impacts on income and development. The report advocates decisive actions, including creating fiscal space for targeted transfers to the impoverished — a crucial strategy to aid those in need.

1.2. The Graduation Approach and its Impacts

As one of the targeted transfer approaches that have been applied in various contexts, the "graduation" program is a multi-faceted intervention that combines entrepreneurship training, cash transfers, and savings promotion. It begins by targeting the poorest households, providing them with initial productive assets, consumption support, and training in profitable asset management. The program encourages saving, offering ongoing coaching and mentoring throughout the treatment period.

Existing evidence suggests positive average treatment effects of the "graduation" program. In six evaluated sites (Banerjee et al., 2015), the program generates cost-effective and sustained positive impacts on earnings, consumption, and welfare measures over three or more years. Trajectories continue to diverge from the control group in locations with long-term follow-ups (Banerjee et al., 2021; Bandiera et al., 2017). In fact, according to the conditional quantile treatment effects presented in the literature, the average treatment effects mask the reality that some of the treated individuals did not benefit from the programs.

1.3. Cost-Effectiveness and Heterogeneous Impacts

The success of the graduation approach has prompted governments and organizations to consider replication in various contexts, with the goal of lifting millions out of extreme poverty, as advocated in the Poverty and Shared Prosperity 2022 Report. However, life skills coaching and business management training often come at a significant cost, accounting for 60%-80% of supervision costs (Banerjee et al., 2015). To assess cost-effectiveness prior to scaling up, it is essential to comprehend potential spillover effects and the diverse impacts among different segments of the population. Approximately 25%-30% of beneficiaries did not experience the expected benefits, underscoring the necessity to explore variations in program outcomes.

1.4. Research Questions

The thesis endeavors to address the following research questions:

- Does the program enhance the psychological and material well-being of beneficiaries in both the short and long term, particularly in a pastoralist region prone to environmental shocks like droughts?
- Do the enhancements observed among treated women extend to non-treated individuals through chance community interactions or local kinship ties? If so, do differences exist in these spillover effects?
- Do psychological factors account for the variations observed in treatment effects? Do psychological pathways serve as mediators in conveying the program's impact on both women who received the treatment and those who did not?

1.5. Samburu County and REAP program

This thesis investigates using a randomized control trial the woman-focused "graduation" program by NGO BOMA Project in Northern Kenya. The study area includes seven "mentor areas" designated by BOMA in Samburu County, one of Kenya's poorest with a 75.8% poverty rate and high child food poverty (63.5% in 2015-16). Many households (60%-70%) engage in livestock production, allocating 60% and more income to food.

Extreme weather events due to pastoralism's vulnerability result in low population densities, limited market access, and income loss. An ongoing impact evaluation project initiated in 2018 assesses the effects of the Rural Entrepreneur Access Project (REAP), a variant of BOMA's graduation program. REAP, a two-year program, assists women in establishing 3-person businesses. It follows a sequence:

(1) Targeting participants for three-woman business groups
(2) Providing a $US200 seed capital jump-start grant with initial training
(3) Offering two years of mentoring by BOMA mentors, including weekly visits and monthly evaluations
(4) Forming savings groups among business groups, with a $US100 grant if the business survives after 6 months
(5) Granting credit access through government-registered formal savings groups

REAP's objective is addressing ASAL's low income, irregular cash flows, and limited financial access to lift households from extreme poverty, particularly empowering ultra-poor women. It's distinct from other programs, offering an initial cash transfer, combining transfer and cash support over two years, and focusing on collaborative enterprise management.

1.6. Outline of the Thesis

Data was collected in three rounds: baseline in 2018, midline in 2020 (pre-COVID), and endline in 2022 for evaluating the REAP program's impact. Chapter 1 employs a rollout and saturation RCT design. It introduces a dynamic stochastic optimization model illustrating heterogeneous treatment impacts of the graduation approach, showing how it may alleviate constraints through endogenous preferences and create social spillovers. The chapter assesses average treatment effects on household earnings, women's business assets, and savings using midline data, focusing on first-wave enrollees two years into the program. Heterogeneous effects based on baseline depression status and spillovers to non-treated women are explored, linked to changes in preferences via psychological channels.

In Chapter 2, endline data on the local kinship network is used to examine potential spillover effects through this network, contrasting with random interactions in Chapter 1. Between midline and endline, Kenya faced floods, locusts, and COVID-19, closing markets and affecting business. While we analyze treatment effects compared to controls, exogenous shocks can undermine outcomes. The program aims for cash income from market activities, challenged by market closures causing beneficiaries to close businesses. Chapter 3 examines program effects and spillovers using endline data during this critical period, discussing longer-term impacts in a shock environment.

CHAPTER 2

Psychosocial Constraints, Impact Heterogeneity and Spillovers in a Multifaceted Graduation Program in Kenya[1]

[1]The chapter is co-authored with Michael R. Carter, Nathaniel D. Jensen & Laurel Krovetz. This study was made possible through the generous support of the American people through the United States Agency for International Development Cooperative Agreement No. AID-OAA-L-12-00001 with the BASIS Markets, Risk and Resilience Feed the Future Innovation Lab. The contents are the responsibility of the authors and do not reflect the views of the US Government. We thank seminar participants at the BRAC, the UN FAO, the University of California, Davis, the BOMA Project and the 2022 Evidence to Action Conference. We also thank our respondents and our implementing partners. The projects activities were ruled exempt under Category 2 by the IRB at the University of California, Davis, project number 1627031. This RCT was registered in the American Economic Association Registry for randomized controlled trials under trial number AEARCTR-0005822.

2.1. Introduction

Multifaceted "graduation" programs transfer tangible productive assets and also intensively mentor transfer recipients to build up their skills, self-confidence and aspirations.[2] Asset transfers are meant to relax capital constraints, whereas mentoring is meant to relax what Bossuroy et al. (2022) call psychosocial constraints to asset accumulation and the shift to more productive livelihoods and occupations. While capital constraints are relatively well-understood, our goal in this paper is to better understand psychosocial constraints, how they operate and how they shape the direct and spillover impacts of graduation programs.

We begin with a dynamic stochastic optimization model of the choice between a low income, casual wage-labor occupation and a higher earning but risky business activity that relies on capital and entrepreneurial acumen. This model elucidates the potential sources of the pronounced heterogeneity in impacts that has been empirically observed in evaluations of graduation programs (e.g., Bandiera et al., 2017; Gobin, Santos, and Toth, 2017). When combined with insights from the economics of depression (de Quidt and Haushofer, 2016), the theory suggests an empirical strategy for identifying one important source of that heterogeneity, namely the severity of initial depressive symptoms in the beneficiary population. In addition, we also use the theoretical occupational choice model to illustrate how a graduation program might be expected to relax psychosocial constraints through an endogenous preference mechanism that operates through the "sour grapes" or adaptive preferences described by Elster (1983) in which preferences adapt to constraints. Importantly, the theory illustrates how this endogenous preference mechanism would be expected to generate social spillovers.

To examine the empirical veracity of these ideas, we implemented a randomized controlled trial of a woman-targeted graduation program implemented by the BOMA Project NGO in the pastoral regions of Northern Kenya. We first show that conventionally measured average treatment effects (ignoring spillovers) are sizable, as treated women's holdings of productive assets, family cash income and financial savings all significantly increase by 414%, 12% and 500% 24 months after the program began. However, hiding beneath these average treatment effects is substantial impact heterogeneity. First using an a-theoretic conditional quantile analysis, we show that approximately 25% of the beneficiary population experienced no benefits form the program, a finding similar to those in the Bandiera et al. (2017) and Gobin, Santos, and Toth (2017) studies. Digging deeper, and following the lead of our theoretical modeling, we show that the almost

[2]This approach was pioneered by the NGO BRAC which had noticed that their signature micro-credit programs were not appropriate for the poorest. BRAC thus developed their "Targeting the Ultra-Poor," or TUP program that intended to build the tangible and intangible assets of the poorest such that they were ready to graduate to micro-credit (see the discussion in Hulme and Moore (2008) and Hashemi and Montesquiou (2011)).

20% of the beneficiary population that exhibited severe depressive symptoms at baseline had drawn down about half the assets transferred to them, had experienced no income gain, but had built up savings stocks at a rate similar to that of non-depressed women.

To explore endogenous preference effects and social spillovers, we leverage the rollout and saturation design of the RCT which allows us to define an exposure measure to beneficiaries of the BOMA graduation program. Defined as the probability that a random social interaction would be with a BOMA project beneficiary during the 24-month period between the baseline and follow-up, this measure varies between 0 and 60%. Using this measure reveals statistically significant spillovers on both beneficiary and non-beneficiary women.[3] For non-beneficiary women, the impacts suggest an asset accumulation rate at 25% of that for beneficiary women, even though the former received no direct asset grant or other support from the BOMA program. Employing a ladder-of-life-based measure of the desire for upward economic mobility, we show that a plausible explanation for these real spillovers is a statistically significant impact of exposure to BOMA beneficiaries on the non-beneficiaries' preference for upward mobility.

The remainder of the paper is organized as follows: Section 2 presents the dynamic optimization model of occupational choice and highlights its key implications regarding psychosocial constraints. Section 3 describes the BOMA Project graduation program and the research design employed to assess its direct and spillover impacts. Section 4 estimates average treatment effects and impact heterogeneity related to baseline mental health indicators. Section 5 introduces the spillover exposure measure and estimates its effects on economic variables and preferences. Finally, Section 6 concludes by discussing the implications of these findings for designing more cost-effective graduation programs.

2.2. Theoretical Perspectives on Psychological Assets and Graduation Programs

While the overall empirical validity of poverty trap models is still subject to debate, they offer a compelling framework to understand the situations of individuals targeted by graduation programs who seem trapped in chronic poverty. This section begins by presenting a dynamic stochastic model of occupational choice. It is demonstrated that this model predicts systematic heterogeneity in the outcomes of graduation programs, which can potentially be identified. Furthermore, the latter part of this section illustrates how endogenous adaptive preferences would be expected to operate and generate spillover effects from treated to non-treated populations.

[3]Taking these spillovers into consideration and calculating the total causal effect of the BOMA program raises the benefit-cost ratio by 29%.

2.2.1. A Dynamic Theory of Occupational Choice and the Heterogeneous Impact of Graduation Programs. Consider an economy comprised of individuals each endowed with an initial level of wealth (k_{j0}) and a level of entrepreneurial skill (α_j), as suggested by Buera, Kaboski, and Shin (2014). Note that for individuals who have never been entrepreneurs, α_j is a latent variable. In this model, individuals can devote their resources to one of two different occupations:

- *Casual Wage Labor* which generates income $F^w_{jt} = w_0 + f^w(k_{jt})$; or,
- *Entrepreneurial Occupation* which generates income $F^e_{jt} = (w_0 - A) + \alpha_j f^e(k_{jt})$.

Note that w_0 is the returns from full-time work in the causal labor market. Income can also be earned from accumulated capital wealth, through either a low-return investment associated with the wage labor occupation (F^w), or a higher returning entrepreneurial investment (F^e) that requires withdrawal of a discrete amount of time from the labor market (to run the business), $\frac{A}{w_0}$, and is sensitive to the agent's level of entrepreneurial skill. We assume that both investment functions are increasing and concave in k,[4] that $f_e(k) > f_w(k) \forall k$ and that $0 \leq A \leq w_0$. Combining these two income generation processes yields a a non-concave income possibility set with locally increasing returns to scale: $F(\alpha, k) = max\,[F^w, F^e]$.[5]

Following Ikegami et al. (2019), we assume that capital wealth is subject to shocks and evolved according to:

$$k_{jt+1} = (k_{jt} + f(k_{jt}) - c_{jt})\,(\theta_{jt+1} - \delta)$$

where c_{jt} is consumption, $0 \leq \theta_t \leq 1$ is a random capital depreciation shock with known probability distribution function and δ is a standard, fixed rate of capital depreciation.[6]

[4] This assumption of course allows f^w to be linear in k, as it would be if capital under the casual wage labor were put into a simple savings, or buried in the backyard, earning no interest.
[5] Note that even an individual trying to join the entrepreneurial class will operate the wage labor process until she has sufficient capital to make it worth while to begin to run the entrepreneurial technology.
[6] Under this specification, both terms of capital are subject to depreciation through theft or other mechanism of loss.

To study the dynamics of occupational choice and consumption dynamics, we assume that individuals solve the following inter-temporal maximization problem:

$$\max_{c_{jt}} \qquad E_\theta \sum_{t=0}^{\infty} \beta^t u(c_{jt})$$

subject to:

$$\begin{aligned} & c_{jt} \leq k_{jt} + F(\alpha_j, k_{jt}) \\ & F\left(\alpha_j, k_{jt}\right) = max\left[F^w, F^e\right] \\ & k_{jt+1} = \left(k_{jt} + f(k_{jt}) - c_{jt}\right)\left(\theta_{jt+1} - \delta\right) \\ & k_{jt} \geq 0 \end{aligned} \tag{2.1}$$

where E_θ is the expectation taken over the distribution of the negative shocks and β is the time discount factor. $u(c_{jt})$ is the utility function defined over consumption.

The key question that this model allows us to address is from which positions in the α, k space is it optimal to accumulate capital and adopt the higher income, entrepreneurial occupation. Figure 2.1, taken from Ikegami et al. (2019), provides the answer to that question based on numerical analysis of dynamic optimization problem 2.1. The horizontal axis measures the entrepreneurial skill parameter, α, while the vertical axis measures initial tangible productive capital, k. For purposes of this graph, we assume that decision makers know their true skill parameter. The probabilities associated with the heat map (and captured in the side bar in the figure) measure the chances that the individual will end up in the long-run end up in the high earning, entrepreneurial equilibrium.[7]

As can be seen, the probability of escaping the low, casual labor occupation (and chronic poverty) is 0 for initial positions on the west side, or yellow-colored portion of the diagram. The boundary between this portion of the diagram and the portion where that probability falls becomes greater than 0, demarcates what Ikegami et al. (2019) call the Micawber Frontier. From asset positions west/south-west of that boundary, it is not dynamically optimal in the sense of optimization problem 2.1 to even attempt to become an entrepreneur.

[7]To derive these probabilities, we numerically solve the model for a wide array of initial asset positions over a number of randomly drawn shock sequences. Specifically, for each of 1500 initial positions evenly distributed across the initial endowment space shown in Figure 2.1. The infinite horizon model was solved for each asset position, generating an optimal consumption value as well an optimal asset holding. A random shock was then generated, assets were updated and infinite horizon model was again solved for each updated asset position. This procedure was repeated 60 times, yielding a single history of consumption, income and assets for each initial asset position. At the end of each 60-year, an indicator variable was formed indicating whether or not the individual was pursuing the wage labor or the entrepreneurial livelihood in period 60.
This entire process was then repeated 1000 times, generating 1000 histories for each of the 1500 initial endowment positions. The heat map in Figure 2.1 displays the probability that an individual at the indicated initial asset position will end up at the higher income entrepreneurial occupation across the 1000 histories.

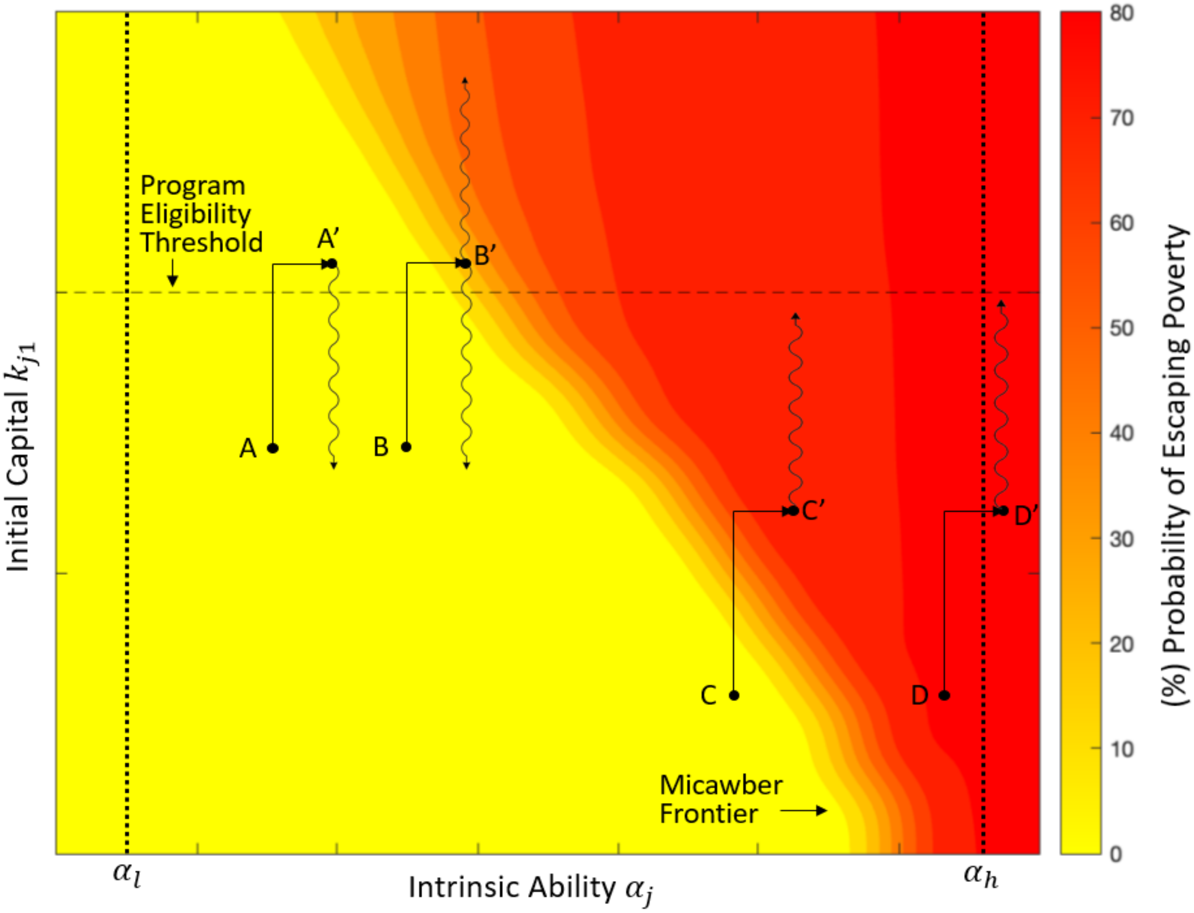


FIGURE 2.1. Optimal Occupational Choice & The Heterogeneous Impact of Asset Building

To the north/northeast of that boundary, it is optimal to try to accumulate and advance to the higher income entrepreneurial equilibrium. However, as can be seen, not everyone who attempts to advance will succeed in the stochastic environment captured in the optimization problem as the probabilities of escaping poverty just to the right of the frontier between 1 and 0.

As can also be gleaned from the figure, there is a critical minimum value of α_ℓ below which it is not optimal to stay at the high equilibrium even if the individual were gifted a large capital stock. Foreshadowing later discussion, this would be true even for an individual who incorrectly perceives her entrepreneurial capabilities to be below α_ℓ. Note also, that individuals with values of $\alpha > \alpha_h$ will (almost) always escape chronic poverty and move to the high income entrepreneurial equilibrium.

2.2.2. Implications of Dynamic Model for Graduation Program Impact Heterogeneity. We can use Figure 2.1 to derive predictions about the impact of a graduation program like that implemented by the BOMA Project in Northern Kenya. By design, graduation programs are means-tested based on observable characteristics. For illustrative purposes, we assume that any individual observed with capital below the dashed horizontal line is eligible for the graduation program, whereas those above it are not. Under the numerical parameterization used to derive Figure 2.1, this horizontal line is approximately mid-way between the low and high steady state capital stocks. As discussed earlier, multi-faceted graduation programs transfer tangible assets, moving beneficiaries to the north in the figure. They also transfer intangible assets including hard entrepreneurial skills, as well as soft skills intended to boost beneficiaries confidence in their own capabilities. This second transfer is portrayed as a rightward shift in Figure 2.1.

We have projected onto the figure four types of asset positions, labelled A to D. We illustrate the first order impact of the graduation program as a shift to the northeast, moving a beneficiary at A to A', B to B', *etc.* We can use the probabilities shown on the graph to predict the expected outcome of the graduation program on each of these stylized positions. If we assume that individuals are distributed across the entire domain of α values shown in the Figure, then we can immediately appreciate the heterogeneous impacts that this stylized, multi-faceted graduation program will have. Four kinds of impacts are possible depending on the individual position in the tangible-intangible asset space (impacts means impact relative to an otherwise identical individual who did not receive and was uninfluenced by the program):

(1) *Position A:* The asset building transfers move the individual to point A', but are inadequate to move the individual above the Micawber frontier. The optimization problem above implies that the optimal policy for the individual is to draw down (consume) their tangible assets such that in the long-run the individual ends up at the low level equilibrium. Were we to compare that individual to an otherwise identical person who did not receive the transfers, we might observe short-term effects on consumption and income, but in the long run the impact would be zero.

(2) *Position B:* The Asset transfers move the individual into the multi-colored band, where she may or may not succeed in reaching the high equilibrium (note that B' is in a position where the chances of escaping the low equilibrium is only 25%). Average causal impacts of a graduation program for individuals in this position would be a mix of those who escaped the low level equilibrium and those who did not.

(3) *Position C:* Graduation program asset transfers would move this person to position C', which is firmly in the zone where the probability of reaching the entrepreneurial equilibrium in the long term poverty is quite high. Impacts for individuals in this position (compared to someone at C who did not receive the program) would be expected to be substantial over the long-term as beneficiaries would likely move from the low to the high equilibrium.

(4) *Position D:* Individuals beginning at point D would also be predicted to move the high equilibrium in the long run. While graduation program transfers would help them approach the high equilibrium more quickly than a control person at position D, the longer term impact would be predicted to be zero as the control person is also predicted to escape poverty and also end up at a similar long-run position

As this overview of the possible impacts of a graduation program makes clear, any average treatment effect estimated from a well-balanced sample would be expected to be a data-weighted average of these different, and highly heterogeneous, effects.

2.2.3. Economics of Depression and the Heterogeneous Impact of Graduation Programs. As is apparent from Figure 2.1, the expected impact of a graduation program will depend fundamentally on an individual's initial wealth and their entrepreneurial acumen. While conditional quantile analysis can reveal some information about impact heterogeneity (see Section 2.4.2), one barrier to a more structural analysis of heterogeneity stems from the fact that entrepreneurial skill (α) is not directly observable. Importantly, entrepreneurial skill is not only unknown to the econometrician, but it is also unknown to most chronically poor women who do not have lived experience as entrepreneurs. While we might imagine that in the absence of empirical evidence, most women would hypothesize themselves to have an average level of α, there is one, observationally distinguishable group that we would expect to systematically understate their true entrepreneurial skill.

Citing psychiatrist Aaron Beck's exposition on depression (Beck, 1967), De Quidt (2019) notes that depression leads individuals to underestimate their intrinsic abilities. Depression, considered one of the most common mental illnesses, specifically major depressive disorder, encompasses a constellation of disruptive symptoms affecting mental well-being. This well-being is defined as 'a state in which the individual realizes their abilities, can cope with life's normal stresses, remains productive, contributes to their community' (World Heath Organization, 2017). Formally modeled by these authors, depression prompts individuals to behave as if their entrepreneurial efficacy or skill (analogous to α in our model) is lower than its actual level.

Given that impoverished women trapped in the low equilibrium lack personal experiences to counterbalance this underestimation, we can hypothesize that depression influences decisions as if individuals are situated on the left side of Figure 2.1. Even assuming that there is no intrinsic correlation between entrepreneurial skill and depression, this perspective suggests that women with depressive symptoms will disproportionately act as if they are low skill types.[8] Section 2.5.3 will return to use this insight to see if lower graduation program impacts are indeed found amongst those with high baseline levels of depressive symptoms.

2.2.4. Adaptive Preferences and the Psychosocial Spillovers of Graduation Programs. While graduation programs could generate an array of spillover benefits (see Section 2.5 below), this section lays the conceptual foundation for a specific type of spillover that could be particularly relevant in our study area, namely one that operates through an endogenous preference mechanism. Specifically, we build on the notion of adaptive preferences put forward by Elster (1983). Elster argues that although conventional economic analysis views the preferences that guide utility maximization as independent from the constraints that limit the maximum achievable utility, the latter in fact can reshape the former. To illustrate the notion that preferences adapt to constraints, Elster draws on the Aesop's fable, "Sour Grapes." In the fable, a fox wants to enjoy a bunch of grapes hanging from a vine. Despite multiple attempts to reach the desired grapes, the fox can never reach the not quite low-hanging fruit and ultimately walks away saying he did not really want the grapes after all because they were sour. In the fable, constraints cause preferences to change.

One approach to encapsulate Elster's concepts is by proposing that we deduce our potential highest achievable living standard by observing individuals whom we perceive as similar to ourselves. This aligns with Appadurai's notion of an 'aspiration window,' which individuals use to assess whether others are sufficiently akin to them to serve as credible models for adopting reasonable levels of living standards (Appadurai, 2004). For a person i we write this perceived maximum attainable living standard as $\tilde{c}(c_{g(i)})$, where $c_{g(i)}$ is the vector of observed living standards of those in i's social reference group $g(i)$.[9] The notion of adaptive preferences is that we sour on living standards that we see as unattainable, undervaluing living standard advances beyond $\tilde{c}$ relative to what we value them if we thought they were attainable. In other words, adaptive preferences suggest an endogenous consumption threshold beyond which the utility function flattens out, indicating that

[8]de Quidt and Haushofer (2016) also note that depression can lower effective labor supply. Incorporating this insight into the model above as an decrease in w_0, will result in a further shift right in the Micawber threshold, again making it more likely that depressed people will find themselves in a position like A in Figure 2.1.

[9]For example, $\tilde{c}$ could be the mean, median or max of the vector $c_{g(i)}$.

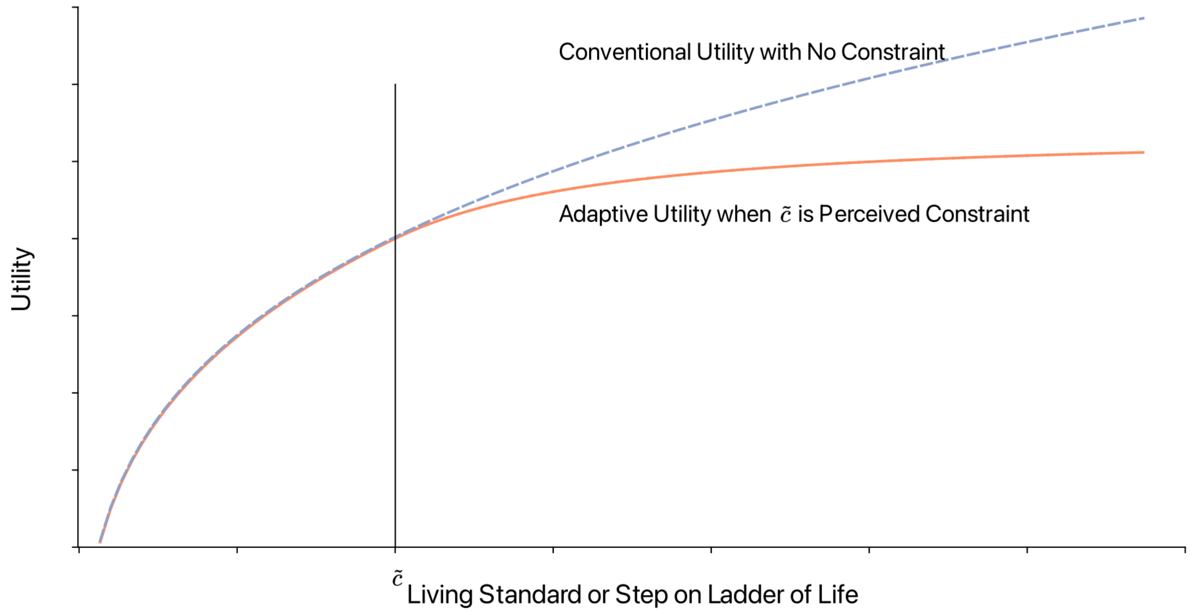


FIGURE 2.2. Adaptive, Sour Grapes Preferences

we expect few gains in our subjective well-being from material advance beyond the threshold level:

$$u(c_{it}) = \begin{cases} u^{\ell}(c_{it})\; if\; c_{it} < \tilde{c}(c_{g(i)}) \\ u^{h}(c_{it})\; otherwise \end{cases} .$$

Figure 2.2 illustrates this preference structure. The upper, dashed-blue curve is a standard constant relative risk aversion utility function. The lower solid, orange curve represents an adaptive preference function for an individual who sees her maximum attainable living standard as $\tilde{c}$. Adapting to that constraint, the individual ascribes little incremental value to advance beyond $\tilde{c}$ as the utility function flattens out beyond that level.

The notion of a threshold consumption level where preferences discontinuously change has featured in a number of models of endogenous preferences (*e.g.*, Genicot and Ray, 2017; Lybbert and Wydick, 2018). Adaptive preferences as presented here is built on the notion that we are happier (less frustrated) when we do not highly value unattainable living standards and instead are happy enough with the attainable. Elster suggests that this tendency to devalorize what we see as unobtainable is consistent with cognitive dissonance theory.[10]

[10]Economic analyses that draw on cognitive dissonance theory include Montgomery (1994) and Laajaj (2017).

Section 2.5.3 below presents a strategy for empirically measuring endogenous adaptive preferences. We close this section by using the dynamic occupational choice model (2.1) to illustrate the behavioral implications of a shift in endogenous adaptive preferences. The probabilities for upward mobility illustrated in Figure 2.1 were derived assuming adaptive preferences with a low critical value, set in the vicinity of the steady state income for someone pursuing the poor, informal wage-labor occupation. We then ask what happens if the threshold level shifts up to the steady state level of the entrepreneurial occupation, perhaps because members of the community begin to move forward economically. Figure 2.3 shows the results of resolving the dynamic model using these new preferences. The color scale shows the change in the probability of reaching the entrepreneurial equilibrium after the preference shift. The black solid curve projected across the figure is the Micwaber Frontier from Figure 2.1. As can be seen, the largest change in the probability of economic advance are for those endowment positions that are just to the west of the original Micawber Frontier. A shift in preferences fundamentally alters behavior for individuals at these endowment positions, indicating that the preference change would induce them to try to accumulate productive assets and reach the higher income entrepreneurial equilibrium Note also that the preference change has no effect on the behavior of those far away from the original Micawber Frontier.

2.3. Program Intervention and Research Design

This study examines the impacts of the REAP program on its participants and those in their community. It is part of a larger agenda that is assessing not only the impacts of REAP, but also the impacts of the Index Based Livestock Insurance (IBLI)[11] product, and will test for synergies between the two. The larger agenda included randomized treatments of both REAP and IBLI and has three waves of data collection: 2018, 2020, and 2022. This manuscript focuses on the heterogeneity of REAP's impacts and its spillovers on others. As will be discussed in the following section, the research design was developed specifically to examine REAP's spillovers, but does so using only the 2018 and 2020 data. At the same time, the assessment of IBLI requires data from the 2022 collection, which is after several droughts had moved through the region. For this reason, we leave the discussion of IBLI and related treatments out of this manuscript, except for those cases in which it is relevant for this study.

[11]IBLI is a commercial insurance product that households can purchase from Takaful Insurance of Africa. IBLI policies last 12 months and make payouts if a proxy for forage conditions in the insured area fall below a threshold. The proxy is based on the Normalized Differenced Vegetation Index (NDVI) collected by NASA through sensors located on satellites and processed by the USGS. More on IBLI can be found at https://ibli.ilri.org/. For more on NDVI, visit https://www.usgs.gov/landsat-missions/landsat-normalized-difference-vegetation-index.

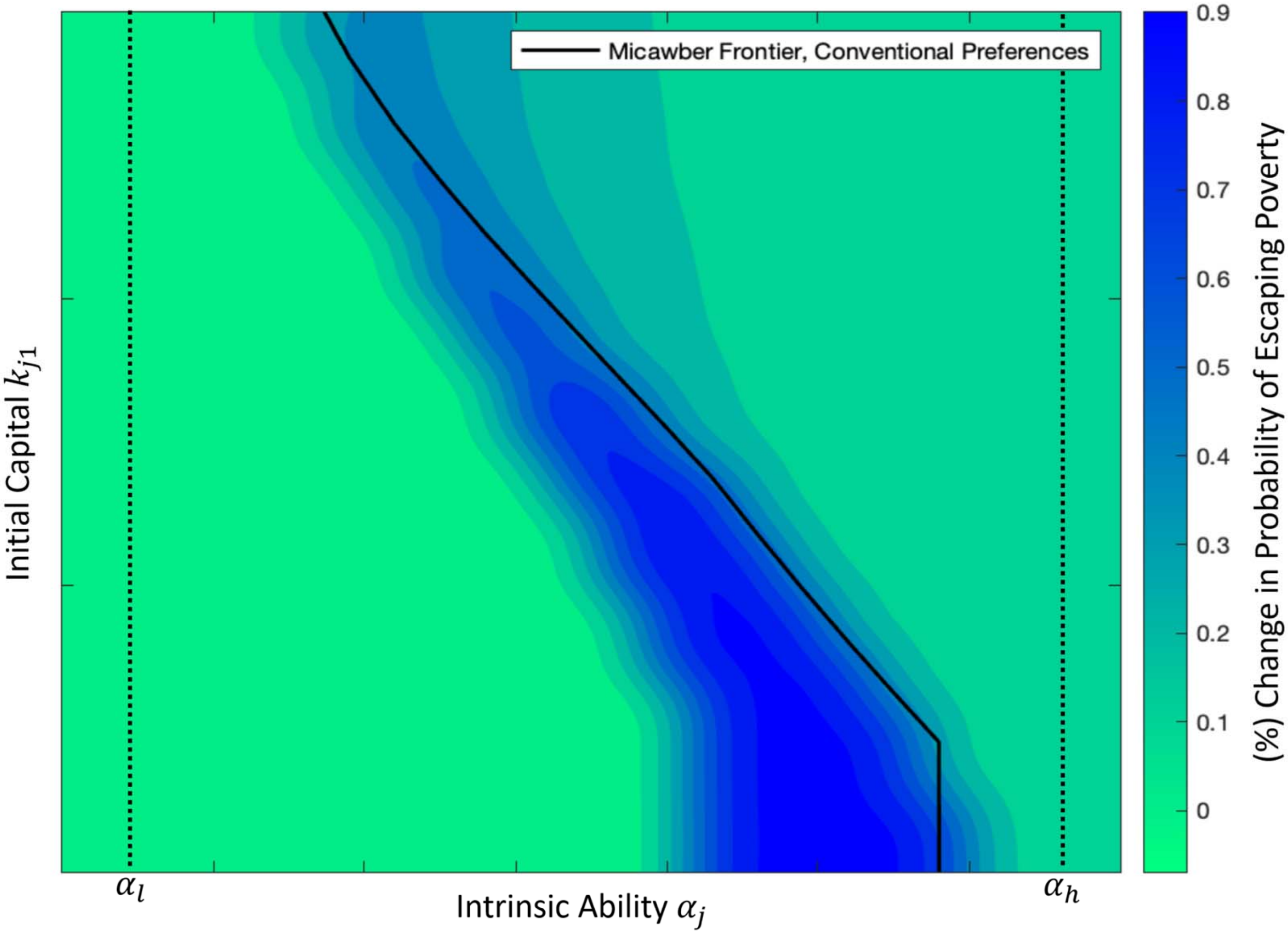


FIGURE 2.3. Endogenous Preferences & Change in the Prospects of Upward Mobility

2.3.1. The REAP Graduation Program. The REAP program aims to help poor women move out of, and stay out of, poverty by providing financial and business training, and mentoring participants as they develop and launch a local business. The REAP intervention targets the poorest women using a community-based assessment procedure known as the Participatory Rural Appraisal (PRA). [12] Once identified through the PRA, BOMA staff then visit each of the PRA-qualified households to verify that they are consistent with their PRA classification and to determine if they have a member that is eligible for the REAP program.

[12]The PRAs involve working with community members to identify locally defined wealth groups and then allocating community members into them. It is common for communities to agree on four to five wealth categories and allocate about half of the households into the lowest two categories. No matter the number of wealth categories that the communities uses, BOMA targets women from the lowest two.

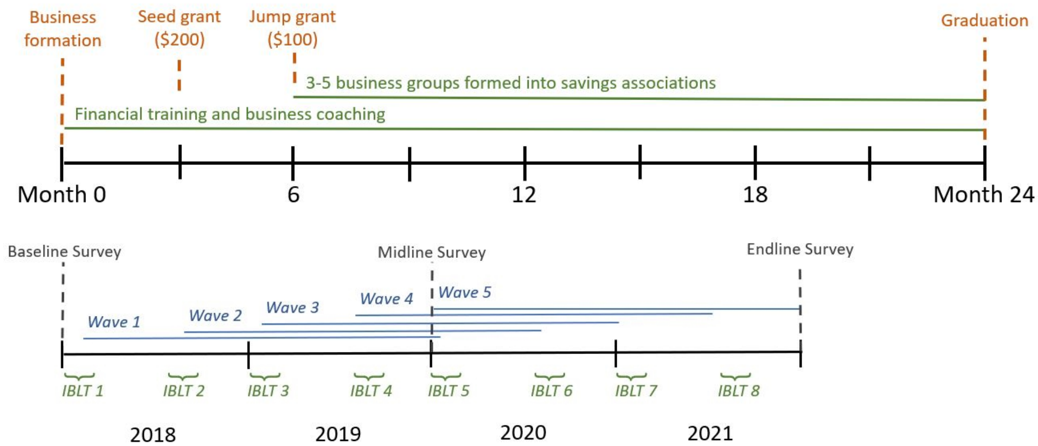


FIGURE 2.4. Intervention and Study Design

Eligibility requires that the participant is female, is of productive age, is of sound mind, is a permanent resident of the community, and does not suffer from drug addiction. [13]

Once enrolled, participants of the REAP program receive mentoring and skills training from a BOMA staff member throughout the duration of the program. BOMA staff also help participants form into 3-person business groups and to develop a business plan. Upon approval by the mentor, the 3-person business receives a seed grant worth approximately USD200 with which to purchase any needed assets and the starting inventory for the business. A "jump grant" worth approximately USD100 is provided to the business after three months of operation as long as they have been adhering to the principles of the program. The left panel of Figure 2.4 coarsely illustrates the timelines of the REAP intervention.

BOMA relies on full-time staff, called mentors, to implement the REAP program. Each mentor is assigned a catchment area and is responsible for the training and monitoring of participants in their area. For practical reasons, the mentors enroll and implement the REAP intervention in waves, so that at any point a mentor might be working with women that are at several different stages of the REAP program in the same community. These waves are commonly rolled out every six months.

2.3.2. Research Design. This research was implemented in northern Samburu County, Kenya. This study region was selected because the BOMA Project had plans to launch REAP there (relevance) but had not yet done so (uncontaminated). The pastoral and agro-pastoral communities of northern Samburu County are similar to the communities that BOMA works with across northern Kenya; they are remote,

[13] Reportedly, alcoholism is common disease in some regions that BOMA operates in and can cause considerable dysfunction among the business members.

suffer from high levels of poverty, and they are often unable to take advantage of and, or are overlooked by government and development interventions. Most households live in relatively small clustered settlements called manayattas. In the remainder of this paper, we will simply refer to manyattas as communities.

The study's research design and sample selection was performed to ensure comparability between the treatment and control groups. To begin with, BOMA completed the PRA and verification process, and then provided the research team with a roster of REAP-eligible women. The research team then stratified the roster by community, and used a randomization process to distribute participants into those that would participate in the study and those that would enter the pool of REAP-eligible, but non-study, individuals. Within each community, those that were selected to participate in the study were provided with a random rank. When it was time to enroll new participants in REAP, those with the lowest rank were offered the opportunity to enroll in REAP. If they excepted, they were then designated "anchor women". Each anchor woman would then select two women from the pool of REAP-eligible non-study individuals to participate in their 3-person REAP business. This approach allowed us to maintain an uncontaminated and relevant control group, while also maximizing the power of the sample size by splitting all the REAP-treated study participants into different REAP businesses. Further, it provided a clear and exogenous process for identifying study participants that would be recruited for the next wave of REAP in a context in which we expected reasonably high attrition.[14]

In total, 88 communities participated in the project at baseline and the number of control and anchor women selected in each community was set in proportion to the number of REAP-eligible women in each community. A sample of 1,502 REAP-eligible women enrolled in the study, and the study had a target of 700 anchor women that would participate in the REAP program. The study also included a sample of 373 "vulnerable" women from the same 88 communities that were drawn from the population identified in the PRAs as being from the wealth category just above the REAP-eligible threshold. We call these women "vulnerable" because they are close, but not, in the REAP-eligible (poorest) categories. These vulnerable women were not eligible to receive the REAP treatment but could be impacted by spillovers from it and were eligible to receive the IBLI treatment. As Section 2.5.1 discusses in detail, the RCT was designed to detect spillovers on to all sub-populations.

[14]During the study design phase of the project, we assumed that attrition rates between the 2018 and 2020 study would be a relatively high rate (10%) in part because the population is migratory by nature and we planned to drop households that moved out of the study region and becuase there would be a large number of study participants–both those that would be control and those that would not enroll in REAP unitl later waves–that were not recieving any engagements from the project at all.

2.3.3. Integrity of the experimental design. Table A2 in A.2 present the baseline test for the outcome variables used as the primary outcome measures in the study. Panel A presents the mean comparisons and t-tests for equality of means between the treatment and control groups of poor households in the sample. The treatment status is defined based on assignment to treatment by the endline survey. We observe no significant difference in either the primary outcome variables or the household characteristics at the baseline. While the difference of means of treatment and control poor households on household earnings is significant at the 10% level, the aggregate test, reported in Panel B of Table A2, finds that we are not able to reject the equality of means across all the measures (p-value = 0.11). Overall, the sample balance was good between the treatment and control poor groups.

Table A3 in A.2 presents an analysis of survey attrition for both midline and endline data collections. The follow-up rate was excellent. We managed to survey 92% of baseline respondents in the midline, and 86% in the endline (Panel A). We do not observe any significant differences in attrition rates across treatment and control groups. Panel B of Table A3 presents an analysis of the characteristics of respondents who were more likely to be resurveyed during the midline and endline. Panel C presents a test of whether being assigned to receive treatment affected the type of person who completed both midline and endline surveys. We did not find evidence that the treatment caused a sample composition bias by affecting attrition rates. We fail to reject that the treatment status indicator and all the outcome variables interacted with treatment status were zero. The p-values for the test are 0.66 (midline) and 0.17 (endline), thus supporting the contention that survey attrition did not lead to a different sample frame across treatment and control groups.

2.4. Average Treatment Effects and Impact Heterogeneity by Baseline Depressive Symptoms

This section presents our empirical analysis using the standard approach commonly found in the literature, which does not account for spillover effects. However, Section 2.5 will delve into the consideration of spillovers. In this section, our focus is solely on significant material outcomes, namely women's business assets, family cash income, and women's savings. Regarding women's business assets, we acknowledge that some participants only reported their private business assets instead of collective assets. To address this, we employed a conservative measurement for imputing the missing data on business assets. For detailed information on the imputation procedures, please refer to A.3. Results for several key psychological outcomes are presented in Appendix Table A6. Despite the program's intensive mentoring, these results indicate no impact on depressive symptoms, as measured by the Center for Epidemiological scale. However, there was an observed increase in individuals' perceived control over events that influence their lives, indicating an internal locus of control, as defined by Rotter (1966).

2.4.1. Conventional Average Treatment Effects. We start our discussion first by presenting standard intent-to-treat (ITT) treatment results, estimating the following ANCOVA model:

$$y_{hm} = \alpha_0 + \alpha_1 y^0_{hm} + \beta^a W^a_{hm} + \beta^b W^b_{hm} + \varepsilon_{hm} \tag{2.2}$$

where y_{hm} is the 2020 outcome variable of interest for individual h in community m, y^0_{hm} is the 2018 baseline value of that same variable, W^a_{hm} and W^b_{hm} are binary indicator variables for assignment to waves 1 or 2 and 3 or 4, respectively of the BOMA program.[15] The error term ε_{hm} is clustered by community. Under this specification, the control is comprised of women selected for eligibility for REAP, but not assigned to any of the first four treatment waves. A fraction of these women were later assigned to later treatment waves, but they did not their status at the time of the 2020 follow-up survey.

The β^w $(w = a, b)$ parameters identify the intent-to-treat impact of the program under two assumptions: random assignment to treatment and no spillovers between treatment and control households. Later, we will relax the latter assumption by exploiting our saturation design. We can compare the treatment effects obtained from this initial estimation with the treatment effects that take into consideration the spillover effects on the control women within the same cluster.

The first and last columns of Table 2.1 report the OLS estimates of the β^w parameters in equation 2.2. As can be seen, the REAP program on the key economic outcomes. For waves 1 and 2 women, who on average have been enrolled in the program for 20 months by the time of 2020 data collection, women's total business assets increased by $PPP 190, household cash income by $PPP 98 and amount deposited to savings increased by $PPP 56. All these estimated impacts are significant at the 1% level and represent increases of 414%, 12% and 500% over the 2018 baseline levels.[16]

We find no significant impact on household earnings for women who enrolled in waves 3 and 4 (who on average had been in the REAP program for 9 months) but their reported total business assets increased by $PPP 125 and the amount deposited to savings also increased by $PPP 24.8. For women who have enrolled in the program for 18-24 months, we document some increase in their business assets. Moreover, we find

[15]After estimating the model with separate parameters for each of the four waves, we combined them into two groups as the coefficients on waves 1 and 2 were similar, as were those for waves 3 and 4.
[16]Hinting at some spillover effects, these percent increases are only 251%, 18% and 325% over control group 2020 levels.

Table 2.1. Average and Conditional Quantile Treatment Effects

	Treatment Waves 1-2					Treatment Waves 3-4
	Average	Conditional Quantile Estimates				*Average*
	Impact (OLS)	*Q25*	*Q50*	*Q75*	*q90*	*Impact (OLS)*
Women's Business Assets ($PPP)	190***	NE	161***	346***	534.9***	125***
	(22.3)	NE	(13.76)	(34.57)	(54.33)	(18.6)
Household Income ($PPP)	98***	60.06*	74*	124.7	203.4	4.4
	(34.2)	(20.75)	(37.24)	(106.5)	(129.8)	(36.3)
Women's Savings ($PPP)	56***	NE	NE	87***	189.9***	25***
	(8.27)	NE	NE	(12.69)	(24.54)	(6.2)
Observations				1385		

Notes: Regressions include baseline levels of the dependent variable. Standard errors for the average treatment effects are clustered at the community level. For the quantile regressions, standard errors were calculated using the bootstrap method with 20 replications. For some lower quantiles, impacts were not estimable (NE) because there was no variation in the dependent variable. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

evidence on transfer of business assets of the women group businesses into their household earnings and savings.

The REAP program, similar to other graduation programs, involves providing cash grants to women. By the time of the midline assessment, women enrolled in waves 1 and 2 would have received both a jump grant (amounting to $PPP 163 as an individual share from the three-woman business group) and a progress grant (amounting to $PPP 82 as an individual share from the three-woman business group if the business survives after 6 months). When comparing the treatment effects on women from waves 1 and 2 with the amount transferred through the program, we observe a decrease in the business assets held by the women's business groups. However, there is an increase in the reported cash income of these women's households and the amount deposited into savings.

For women who enrolled in the program during waves 3 and 4, we do not observe a significant increase in their household cash income compared to the cohorts who started the program earlier. Additionally, the amount deposited into savings by these women is only 44% of what women from waves 1 and 2 have. These findings suggest that the accumulation of business assets among women in waves 3 and 4 may not have fully translated into higher household earnings for them.

2.4.2. Conditional Quantile Analysis. We now present the Conditional Quantile Treatment Effects (QTEs) to emphasize the substantial heterogeneity in treatment effects that will be further explored in subsequent sections. As elucidated in the theoretical framework section, individuals display considerable variation in their initial ability levels. In the absence of engagement in entrepreneurial activities, these individuals frequently lack opportunities to realize their full potential. This diversity can result in highly heterogeneous treatment effects of the REAP program, which may not be fully captured by the overall Intent-to-Treat (ITT) effects.

By examining the conditional quantile function of economic outcomes in relation to the treatment indicators, we can evaluate whether the dispersion in earnings, total business assets, and savings increases or decreases with the REAP program. It's important to note that while the treatment effects on the percentiles we estimate here represent the residual distribution conditional on baseline levels and treatment wave indicators, our aim is to illustrate the diverse impacts that can occur within the targeted population. As demonstrated in the remaining section of Table 2.1, the treatment effect on the 90th percentile of the conditional distribution of total business assets is approximately three times that of the 50th percentile. Similarly, the disparities in household income between the 90th percentile and the 25th percentile of the conditional distribution are roughly 3.5 times the level observed at the 25th percentile.

For instance, among women falling within the 75th percentile and above in the total business assets distribution, provided their assignment to waves 1 and 2 and conditioned on the baseline business assets level, they begin accumulating greater business assets (1.4 - 2 times the initially transferred grants). This insight illuminates the diverse impacts of the program within the ultra-poor population, which is crucial for comprehending how to enhance the program's cost-effectiveness. In the subsequent sections, we will delve into exploring the observed heterogeneity in treatment impacts by examining the psychological pathways available for testing.

2.4.3. Unpacking Impact Heterogeneity by Baseline Depression. Using data from both the baseline and midline assessments, our intention is to observe how the treatment effects of REAP vary based on the respondents' baseline psychological assets, specifically their level of depression. Depression is measured in terms of the 10-point Center for Epidemiological Studies Depression (CES-D) score, which quantifies depressive symptoms. The CES-D (Radloff, 1977) score is a widely used measure in studies focusing on the impact of economic interventions or shocks on mental health. For instance, Christian, Hensel, and Roth (2019) employs CES-D scores from waves 4 and 5 of the Indonesian Family Life Survey (IFLS) conducted in 2007 and 2014 as the depression measure.

In our study, CES-D scores range from 0 to 30, with higher scores indicating more severe depressive symptoms. A CES-D score of 12 is employed as a threshold to indicate clinically diagnostic depression. Notably, Baron, Davies, and Lund (2017) discuss studies conducted in the US and China that identify depression cut-off scores ranging from 8 to 16. In their own study in South Africa, the authors establish

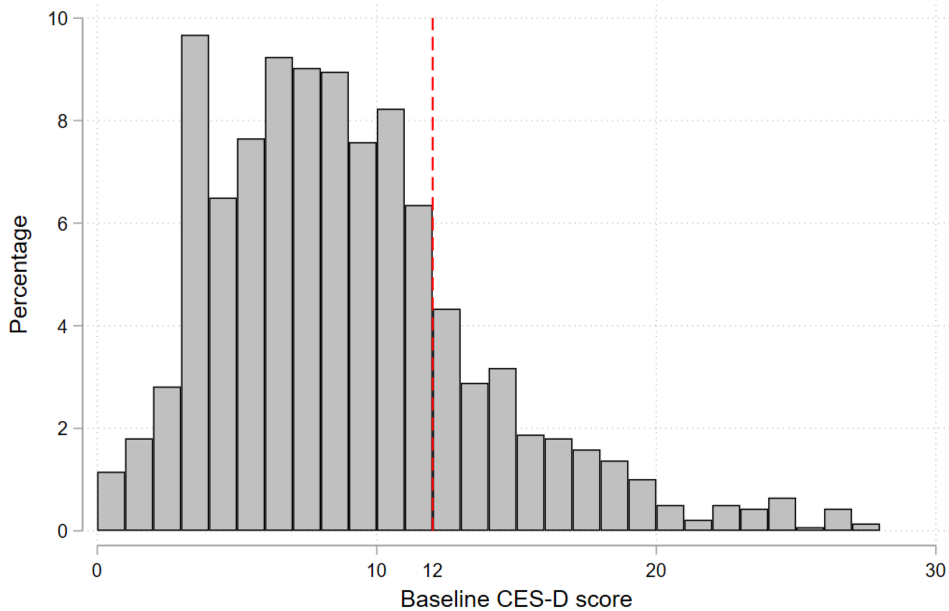


FIGURE 2.5. Histogram of baseline CES-D scores

cut-off scores of 11, 12, and 13 based on the specific population. Hence, the depression cut-off score likely varies due to geographic, cultural, and population differences.

In Kilburn et al. (2018), it is revealed that 37% of the youth (aged between 15 and 25) in their Kenyan sample had a CES-D score of 10 or higher, with a mean CES-D score of 8.61. In our sample of the poor group, 35.6% of women had a CES-D score of 10 or higher, and the mean score is 8.41. We adopt a threshold of 12 to indicate severe depression. Figure (2.5) displays the histogram of the baseline CES-D scores for the respondents, demonstrating that just under 20% of the poor group had a CES-D score exceeding 12.

As discussed earlier, depression might create an added vulnerability for women in poverty that will limit their ability to benefit from REAP. The model presented by Ikegami et al. (2019) suggests that those not suffering from depression prior to treatment should be more likely to follow an upward trajectory towards the higher equilibrium outcome in the long run following treatment. On the other hand, the group considered depressed at baseline should be less likely to be placed on an upward trajectory, despite the program.

To study this, we modify regression equation 2.2 by adding an interaction between treatment and baseline depression dummy variable (D_{hm}) to identify the impacts on two sub-populations, baseline depressed and non-depressed groups:

Table 2.2. Heterogeneous Impacts by Baseline Depression, ITT Estimates

	Treatment Waves 1-2		**Treatment Waves 3-4**	
	Not Depressed	*Depressed*	*Not Depressed*	*Depressed*
Women's Business Assets ($PPP)	209***	93	137***	55
	(23.1)	(45)	(26)	(54)
Household Income ($PPP)	121	-21	2.9	4.1
	(39)	(76)	(43)	(91.2)
Women's Savings ($PPP)	56***	51	25***	17
	(7.6)	(15)	(8.5)	(17.8)
Observations		1385		

Notes: Regressions include baseline levels of the dependent variable. Standard errors for the average treatment effects are clustered at the community level. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

$$y_{hm} = \alpha_0 + \alpha_1 y^0_{hm} + \beta^a W^a_{hm} + \beta^b W^b_{hm} + D_{hm} \times \left[\delta^c + \delta^a W^a_{hm} + \delta^b W^b_{hm}\right] + \varepsilon_{hm} \tag{2.3}$$

The new binary depression indicator variable takes on the value of one for baseline CES-D scores greater than 12. When an individual who is baseline depressed receives treatment in wave w, their treatment effect from the program will be $\beta^w + \delta^w + \delta^c$ $(w = a, b)$.

Table 2.2 summarizes the results from estimating equation 2.3, displaying the expected treatment effects for both the depressed and non-depressed subpopulations. The complete regression results can be found in Appendix Table A4. Treated women with a baseline CES-D score greater than 12 experienced smaller impacts from the program on their business assets and earnings compared to their non-depressed counterparts at baseline, resulting in a substantial divergence in outcomes between these two groups. Table A4 demonstrates that, consistent with the direct and spillover effects discussed in the previous section, participants in the REAP program experienced positive and statistically significant treatment effects. The interaction term is negative and statistically significant for Waves 1 and 2 concerning total business assets. The negative coefficient of the interaction term indicates that the group with baseline depression experienced a smaller impact on business assets compared to the group without baseline depression. This divergence suggests that the non-depressed group is accumulating more assets than the depressed group. The estimated impact for the baseline non-depressed group is $PPP 209 (se = 23.1), while for the baseline depressed group, it is $PPP 93. To facilitate a direct comparison between the impacts on the baseline depressed and non-depressed treated women, we normalized the treatment impacts on the two groups as a percentage of the intent-to-treat (ITT) impacts estimated in column 1 of Table 2.1. Figure 2.6 illustrates the comparison between the baseline depressed and baseline non-depressed groups for the three economic outcomes.

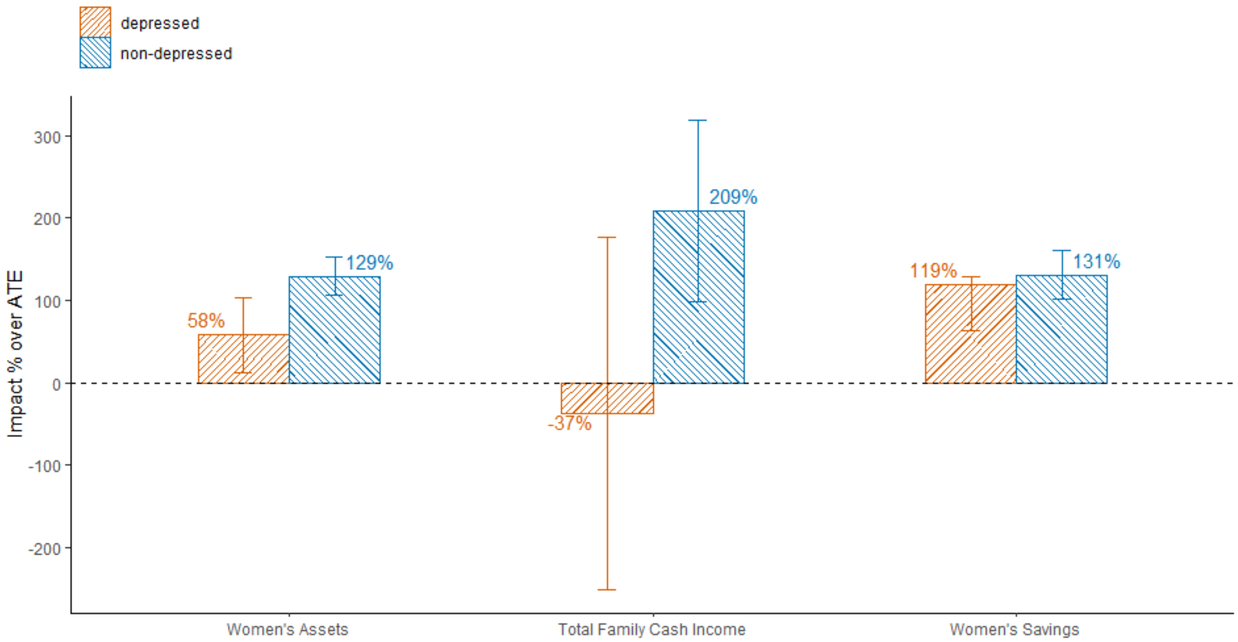
For the baseline non-depressed group, the average impact on women's assets is 129% of the average treatment effect estimated. However, for the baseline depressed women, the increase in women's assets is less than half (45%) of that. Furthermore, while the treatment impacts on family cash income for the baseline depressed women are not statistically significant, the increase in income for the baseline non-depressed women is 209% of the average treatment effects. As observed, except for savings, the treatment impacts on the non-depressed group are substantially larger. The p-values from the Wald tests assessing the equivalence of treatment effects on the depressed and non-depressed groups are 0.075 for total family cash income and 0.015 for women's assets.

It is possible that this divergence could be a result of the second grant that successful businesses received, and whether a business was successful was driven by the level of depression of the women running it. Thus, it could be that these asset accumulation patterns are reflecting the second grant. If it is true that the non-depressed were those who had successful businesses and thus received the second grant, this suggests that the depressed had a harder time benefiting from the first grant and the other components of the program, which supports the theory that the depressed are in a position that makes it harder for them to benefit from a program such as REAP.

The results also show that, though smaller compared to the non-depressed-at-baseline group, the impact of the program on business assets is nonetheless positive for the depressed-at-baseline group. The next point of interest is how an increase in assets will impact the earnings of the depressed-at-baseline group compared to the non-depressed-at-baseline group. More specifically, in relation to the theory discussed in 2.2.1, the depressed-at-baseline group should be less likely to translate an increase in their assets into an increase in earnings due to the added vulnerability to poverty depression creates. In agreement with the direct and spillover impact on earnings, only Waves 1 and 2 women have experienced a statistically significant treatment effect on earnings. The coefficients on the interaction terms are not statistically significant, however they are negative and either fully or nearly cancel out the positive wave specific impact of the program on earnings for Waves 1 and 2 women. This again suggests that the depressed-at-baseline group is falling behind the non-depressed-at-baseline group in terms of earnings.

The absence of divergence in savings between these two groups may be attributed to several factors. One possibility is that non-depressed women were able to reinvest their savings into their businesses, whereas depressed women faced challenges in accumulating substantial savings. Another consideration is that savings

FIGURE 2.6. Treatment effect between baseline depressed and baseline non-depressed groups



accumulated by treated women within the savings groups underwent rigorous monitoring. Each individual group member was required to adhere to the established rules of the savings groups to access their savings. This mechanism could have functioned as a protective measure for women experiencing depression, particularly when uncertainty surrounded the specific utilization of the funds. However, it could also introduce additional inflexibility in situations requiring prompt access to funds for emergent needs. Section 3.6 will delve further into the implications of these findings for the design, targeting, and implementation of graduation programs.

2.5. Spillovers

There are a number of mechanisms by which an asset building graduation program could generate spillovers and influence others. In the first instance, there could be relatively straightforward pecuniary spillovers in which increases in the number of program beneficiaries influence the returns other individuals receive from their own economic activities. These pecuniary spillovers would be negative if more beneficiaries congests the market and lowers the prices that any individual can get for producing a good service. In our study area, many graduation program beneficiaries establish local shops or kiosks that service their local community with limited demand. Pecuniary spillovers could also be positive if they create an agglomeration

Table 2.3. Distribution of communities across saturation groups

Scheme	% of Communities	% of businesses started in each wave				
		Wave 1	Wave 2	Wave 3	Wave 4	Wave 5
A	20%	60%	10%	10%	10%	10%
B	20%	10%	10%	10%	10%	60%
C	50%	10%	30%	20%	30%	10%
D	10%	10%	10%	60%	10%	10%

economies. For example, other beneficiaries establish livestock fattening and trading businesses that service a larger market. In these cases, a growth in local businesses may make it easier to bulk purchase inputs or transport services at favorable prices.

In addition, spillovers could take place through psychosocial channels. Like other graduation programs, the BOMA REAP program intends to build both tangible physical assets as well as intangible psychosocial assets. A key difference between these two types of assets is that the former are rival goods whereas intangible assets are not, meaning that beneficiaries' psychological assets, such as self-confidence and aspirational preferences can spillover and be shared without reducing beneficiaries' stock of self-confidence or aspirations. While our reduced form identification strategy does not allow us to pin down the precise source of spillovers, we will provide evidence that at least some of the estimated reduced from spillover effects take place through psychosocial channels.

2.5.1. Saturation Design & Measurement. To study the impacts of spillovers from REAP participants to other REAP participants or to non-participants, the distribution of businesses started across the four treatment waves was randomly varied between communities. This was done by randomly allocating the communities across four different implementation schemes. Table 2.3 provides the intended distribution of communities across the four REAP distribution schemes. For example, in Scheme A communities, 60% of the REAP participants were enrolled in the first wave (March 2018), which led to relatively high saturation rates of REAP treated women (i.e., receiving the REAP training and mentoring) and REAP businesses by the 2020 data collection period. For communities in scheme B, most REAP treatments would not start until well after the 2020 survey so that there was a relatively low saturation of REAP participants and businesses in those communities during the 2020 survey. By the end of the fifth wave, all communities were to have similar saturation levels, but there was considerable variation at the time of the survey in 2020.[17]

[17]The 5th wave of REAP groups was launched just after the 2020 followup survey. By the end of wave 5, approximately 20% of all REAP-eligible women in each community had been offered the chance to participate in REAP.

By the time of midline data collection, BOMA women started the programs in different waves had been enrolled for different durations. (*i.e.* wave 1 women would have been in the program for 24 months, wave 2 for 18 months, wave 3 for 12 months and wave 4 for 6 months). To capture the treatment duration in our saturation measures, we use a treatment-duration-weighted saturation measure (S), which we define as the probability that a random social interaction with a REAP-eligible women in the community over the 24 months between baseline and midline would have been with a woman in the REAP Program:

$$S_m = \frac{\sum_{w=1}^{4} n_m^w \times 3 \times d^w}{24 \times P_m},$$

where n_m^w is the number of businesses assigned to community m in wave w, so the number of treated women is three times that number as each business is comprised of 3 women, as described above. The d^w terms are the duration weights, which equals 24, 18, 12 and 6 for businesses established in waves 1-4, respectively. The numerator is the number of REAP-eligible women (P_m) with whom interactions are possible, weighted by the full 24-month period. Note that the weighted saturation measure S_m would be 100% if all eligible women in the community were treated in wave 1, and 0 if no women had been treated through wave 4. The mean value of this weighted saturation measure is 0.19, with a standard deviation of 0.08 and minimum and maximum values of 0 and 60%.

2.5.2. Econometric Analysis of Spillovers. To measure the intent-to-treat (ITT) impact of assignment to the REAP program and the spillover effects on both the treated and non-treated women, we estimate the following modified version of equation 2.2:

$$\begin{aligned} y_{hm} =& \alpha_0 + \alpha_1 y_{hm}^0 + \beta^a W_{hm}^a + \beta^b W_{hm}^b + \\ & S_m \times \left[\delta^c I_{hm}^c + \delta^a W_{hm}^a + \delta^b W_{hm}^b\right] + +\varepsilon_{hm} \end{aligned} \tag{2.4}$$

where S_m is the community level saturation measure. I_{hm}^c is an indicator variable for an eligible woman in the control group at midline. The estimated direct impacts for a woman who enrolled in waves 1 and 2 in community m thus can be represented as: $\beta^a + \delta^a \times S_m$. The estimated spillover effects for a within-cluster non-treated woman in community m are $\delta^c \times S_m$. Following Baird et al. (2018), we define the total causal effect of being assigned to treatment wave w in community m as $\beta^w + \delta^w \times S_m + \delta^c \times (1 - S_m)$.

Table 2.4 presents the primary treatment and spillover results for the three key economic outcome variables that the program aimed to improve. The full regression results can be found in Appendix Table

Table 2.4. Impacts and Spillovers at Different Saturation Levels

	Control		**Treatment Waves 1-2**		**Treatment Waves 3-4**	
	Zero Saturation	*Mean Saturation*	*Zero Saturation*	*Mean Saturation*	*Zero Saturation*	*Mean Saturation*
Women's Business Assets ($PPP)	28.2	51*	258***	240***	195***	171***
	(19.4)	(26.6)	(57.2)	(28.8)	(58)	(24)
Household Income ($PPP)	493.1***	14.0	293***	137.1**	35	17
	(50.5)	(50.5)	(103.1)	(62.6)	(120)	(59.8)
Women's Savings ($PPP)	7.8	10.8	73**	67.2***	55**	34.5***
	(6.7)	(8.3)	(30)	(11.7)	(27)	(8.5)
Observations			1385			

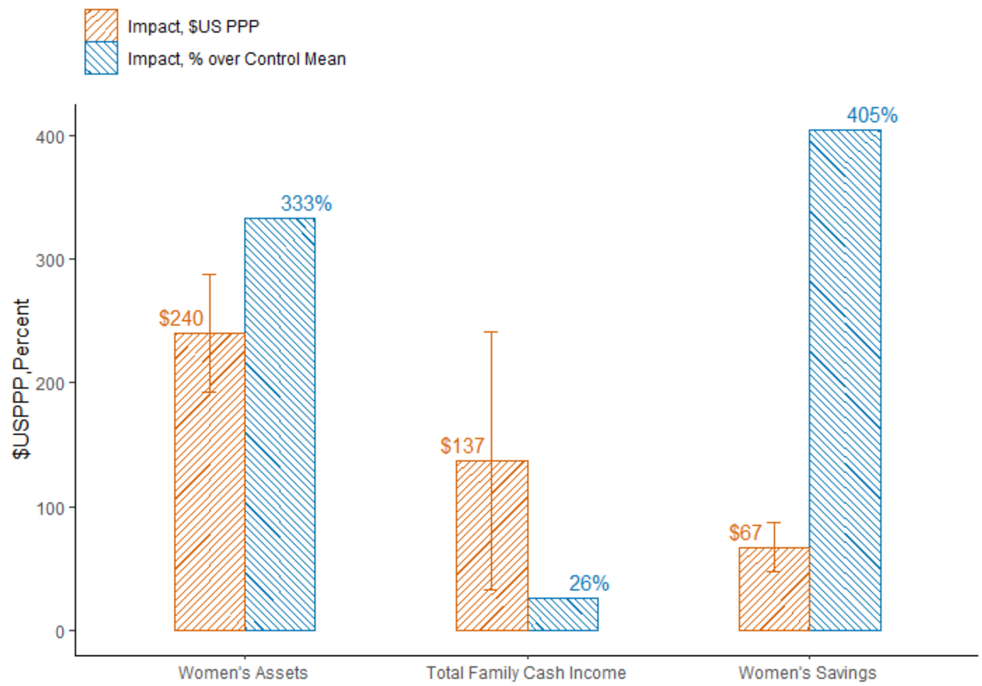
Note: Estimates are in the Appendix Table A5. Control zero saturation results are estimated at the mean level of control variables at midline.

A5. To ease discussion, we focus primarily on estimated impacts for women assigned to receive treatment in waves 1 and 2. Beginning first with business assets, at the median duration-weighted saturation level of 0.19, this implies a large and statistically significant increase for those women treated in waves 1 and 2 ($PPP 240.34, se=$PPP 28.8, with a control group mean of $PPP 72.2). In addition, we see that spillovers are estimated to lead non-treated, within-cluster control households to accumulate business assets. The estimated coefficient ($PPP 270) is statistically significant and at the median duration-weighted saturation level, this implies an impact of $PPP 51.3 on the accumulation of those non-treated households. In sum, spillover impacts on the non-treated control households are about 20% of the impact on the treated.

We also find a statistically significant increase in the reported annual cash earnings ($PPP 137.1, se=$PPP 62.6, with a control group mean of $PPP 535) as well as in the amount deposited into savings ($PPP 67.16, se=$PPP 11.68, with a control group mean of $PPP 16.6). However, we observe a significant negative spillover impacts on treated households. These impacts suggest that increased competition from other REAP businesses reduces earnings. At the median duration-weighted saturation level, these estimates imply a $PPP 155.42 drop in earnings. This drop is of a magnitude that could overturn the entire amount of increase in the earnings. Since we cannot reject the hypothesis that there are no spillover effects onto the treated women, this pattern suggests that the potential impacts of REAP are reduced as more community members are treated. On the other hand, we do not observe statistically significant spillover impacts on non-treated control women for earnings.

It thus seems that while greater exposure to REAP neighbor role models spurred asset accumulation, greater competition from them led to a pattern of declining income benefits from those assets. This pattern generally holds up for both directly treated households and for non-treated households that were subject to spillover effects. The pattern for savings broadly parallels that for earnings (as would be expected if increase

Figure 2.7. Causal impacts on key economic outcomes



earnings are the source of increased savings). Impacts on savings are negative for treated and non-treated households, and in this case are uniformly statistically insignificant.

Figure 2.7 shows the impacts on the women who received treatment in waves 1 and 2 accounting for the spillover effects on them graphically. Note that the average value of the asset transfer of the program was $PPP 245.43 (US$100) per participant. The measured impact on the treated women including the spillover effects roughly equals to the amount that was transferred to them. This reflects no further accumulation of assets yet at the median saturation level villages and suggests that those treated women have managed to preserve their capital stock while generating income and savings.

To summarize, we see that increased saturation spurs asset accumulation, but lowers the income impact of those assets. One way to evaluation this tradeoff is to evaluate to compare the present value of the multi-year total causal effect to the present value of the full program cost. We project out the impacts of the BOMA graduation program out over a ten year horizon.[18] Full details on this approach are given in Appendix A.6. While the analysis rests on a number of assumptions (namely that the benefits estimated after

[18]The ten year horizon follows the analysis in Sedlmayr, Shah, and Sulaiman (2020) of a graduation-like program in Uganda. While that study ignores spillovers and considers only direct impact on the treated, it does suggest a defensible way of projecting the benefits of asset-building forward into the future.

24 months persist for another 6 years before they begin to dissipate), it does allow us to gauge the impact of the program as a function of the saturation rate. We also use the Table 2.1 average treatment effects to calculate the benefit-cost ratio we would have obtained had we ignored spillovers. From the perspective of the implementing program, these benefit-cost numbers represent their return on investment in the poor population in the pastoralist regions.

The results of this analysis are as follows. Ignoring spillovers, we obtain a benefit-cost ratio of 1.7. In words, every dollar invested in the REAP program generated $1.7 in benefits for REAP eligible women. That same measures rises to 2.2 when we evaluate imapcts at the average saturation level of 19%. Finally, if we calculate the total causal effect at a lower saturation ratio of only 15%, then the benefit-cost ratio rises to just over 3. While program design is more complex than maximizing the benefit-cost ratio, these data indicate that a lower saturation level may be called for in the program being evaluated.

2.5.3. Do Adaptive Preferences Explain Spillovers? While the negative impacts of more businesses on cash earnings is not necessarily surprising given that most BOMA program beneficiaries live in small, remote communities where it is easy to imagine saturating the market,[19] the more intriguing result is the finding that women who were not in the REAP program began to changed their behavior and accumulate tangible assets. While this behavioral change could be explained by simple exogenous social effects (*e.g.*, asset accumulation became more profitable when neighbors began to build up their own businesses and income)[20], we here explore Section 2.2.4's suggestion that treated women showed their untreated peers that women could start businesses and obtain higher living standards for themselves and their families. In the notation of Section 2.2.4, this demonstration effect shifted out the perceived income ceiling, $\tilde{c}$, inducing an increase in the perceived marginal utility of economic advancement, as suggested by the theory of adaptive preferences.

While measurement of the subjective marginal utility of income is non-trivial, we built our approach on a five-step ladder of life that portrayed different levels of living standards of local communities.[21] This ladder of life approach is adapted from the Cantril ladder, which asks respondents to evaluate their current life as a whole using the mental image of a ladder. The Gallup World Poll, which remains the principal source of data in the World Happiness Report (Helliwell et al. (2022)), uses the responses to this question as

[19]Indeed, prior to this study, the REAP program purposefully limited the number of women treated in any community to no more than one-third of the eligible population.
[20]See discussion in Manski (1993) on social effects
[21]The REAP program uses this same ladder approach in its participatory poverty assessment that is used to determine graduation program eligibility.

	Ultra - Poor 1: Losipu	Poor 2: Ldoropu	Vulnerable 3: Loikash	Middle Income 4: Loata	Well off 5: Lparakuo
Livestock	No livestock	Few livestock: - 10 shoats - No cattle	Some livestock: - 50 shoats - 15 cattle	Many livestock: - 100 shoats - 30 cows	Many livestock: - 300 shoats - 100 cattle
Business	No business	Petty trading: - Tobacco - Charcoal	Small business: - Miraa	Business: - Retail - Kiosk	Large business: - whole sale - livestock trade with a lorry
Food	1 meal a day	2 meals a day	2 meals a day	3 meals a day	3 meals a day

Figure 2.8. Ladder of Life.

life evaluations of the measurement of subjective well-being. We characterized each step of the ladder over three dimensions: livestock, business and food based on community understandings of different standards of living for who is destitute, poor, vulnerable, middle income and well-off. Respondents were then asked to place themselves on the ladder described using Figure 2.8 (most on steps 1 & 2) and told us how important (on a scale from 1 to 5) it was to work hard to advance to each one of the higher steps. We then standardize the importance index to the next steps to be our measure on respondents' beliefs on the importance of moving up the social ladder. The underlying assumption is that the importance assigned by any respondent to moving up the ladder is an analogue measure of marginal utility.

Table 2.5. Regression results on Ladder of Life

VARIABLES	Importance to get to ladder 3	Importance to get to ladder 4	Importance to get to ladder 5
ITT Wave 1 & 2	0.48**	0.37*	0.30*
	(0.21)	(0.19)	(0.18)
ITT Wave 3 & 4	0.14	-0.13	-0.28
	(0.25)	(0.25)	(0.22)
ITT w1-w2 * Saturation	-1.08	-0.96	-0.68
	(0.7)	(0.75)	(0.67)
ITT w3-w4 * Saturation	1.08	2.12**	2.43**
	(0.99)	(1.07)	(1.08)
Control * Saturation	0.62	0.84*	0.57
	(0.62)	(0.48)	(0.52)
Baseline level of outcome variables	-0.017	0.02	0.012
	(0.029)	(0.034)	(0.024)
Constant	-0.13	-0.15	-0.099
	(0.11)	(0.092)	(0.097)
Observations	830	1,353	1,382
R-squared	0.013	0.009	0.007

Note: Standard errors clustered at the community level. Importance scale (1-5 very important) standardized at control mean.$^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Using the same empirical approach outlined in the spillover equation (2.4) in Section 2.5.2, we examine the direct and spillover effects of the REAP program on individuals' valuations of upward mobility in life. As depicted in Table 2.5, our analysis reveals that, in comparison to a pure control group, the treatment exerts a substantial and statistically significant influence (0.48 standard deviations, se = 0.21) on the aspiration to progress to step 3, accompanied by a slightly smaller impact (0.37 standard deviations, se = 0.19) on advancing to step 4. Furthermore, we note a similar albeit diminished impact on control women (0.84 standard deviation increase, se = 0.48 for step 4). We present the Ordinary Least Squares (OLS) regression results, as the interpretation of the coefficients remains relatively straightforward. Additionally, the results of the ordered probit regression are provided in the appendix, under Table A7, utilizing the importance scale (ranging from 1 to 5, with 5 indicating high importance) as the dependent variable, yielding qualitatively consistent outcomes. The overarching goal of graduation programs like REAP is to enhance the resilience of program participants by alleviating constraints. Furthermore, our findings suggest that the program not only influences participants' desires, but also triggers a social spillover or demonstration effect among their neighbors. While we have yet to ascertain the exact proportion of the estimated impacts attributable to social multipliers, a discernible pattern is emerging between these shifts in preferences and the cascading effects on capital accumulation.

2.6. Conclusion

The arid and semi-arid pastoralist regions of the Horn of Africa constitute one of the most challenging environments for graduation programs attempt to reduce poverty by build productive assets for women. We indeed confirm that the variant of the graduation program model developed by the BOMA Project NGO indeed works and exhibits a benefit-cost ration in excess of 2.

However, the goal of this evaluation steps beyond simply stress-testing the graduation model in difficult environment. In particular, the evaluation was designed to allow exploration of the psychosocial channels through which graduation programs operate. Drawing on equal measures of the economics of poverty traps (Ikegami et al., 2019) and the economics of depression (de Quidt and Haushofer, 2016), we show that consistent with theory the impressive average treatment effects are primarily driven by beneficiaries who exhibited strong baseline mental health. The 20% of the beneficiaries with severe depressive symptoms in fact benefit not at all from the program, drawing down on the assets transferred to them and experiencing no income gain. While we cannot claim that this finding explains the often observed impact heterogeneity in which 25-30% of beneficiaries experience no benefits (e.g., see Bandiera et al., 2017 and Gobin, Santos, and Toth, 2017), the consistency of the theory and empirical evidence is suggestive.

The second psychosocial mechanism we explore is an endogenous preference mechanism (adaptive, or sour grapes preferences modeled on Elster, 1983) that is shown theoretically to generate substantial behavioral change in untreated population. Using the empirical study's randomized saturation design, we are able to document spillovers from the treated to untreated that result in increased business asset accumulation by the latter. We further find evidence that increased exposure to treated women increases the subjective value that women assign to economic advancement. These spillover effects are economically substantial and they increase the estimated benefit-cost ratio of the program from 1.7 to 2.2. There is also evidence that the existing program modestly over-saturates the remote communities in the study area as a lower saturation rate would increase the benefit-cost ratio to just over 3.

A final observation is that these reported benefit-cost numbers include the drag of the women who lacked the psychological assets to benefit from the program. One implication is that the program could generated more benefits per-dollar expended if additional potential beneficiaries were screened for the kind of severe depressive symptoms that seem to limit program impact. Another, and perhaps more palatable implication, would be to design a two-track program in which women without severe depressive symptoms would move forward with the program as currently designed. Recent work on cognitive behavioral therapy reported in Barker et al. (2022) suggests that low cost interventions may be able to resolve the mental health issues that afflict other women. Resolving those health issues first, in a second track graduation program, may lead to better average outcomes, while still making those benefits available to all women who need them based on an economics means test.

In closing, as is clear from the theory of poverty traps analyzed in Section 2.2, another source of impact heterogeneity stems from different exposure to shocks. In forthcoming analysis, we use a second follow-up survey to study the impact of an index insurance contract to ward off the ill effects of shocks on women's ability to preserve asset built by the BOMA program.

APPENDIX A

A.1. Numerical Parameterization of Occupational Choice Model

TABLE A1. Functional Forms and Parameters used in Numerical Simulations

Production Technology and Parameters
$F^w_{jt} = w_0 + k_{jt}^{\gamma_L}$ $F^e_{jt} = (w_0 - A) + \alpha_j k_{jt}^{\gamma_H}$ $\gamma_L = 0$ $\gamma_H = 0.56$ $A = 3.95$ $w_0 = 3.95$
Utility Function and Parameters
Adaptive preferences utility function: $u(c_{it}) = \begin{cases} u^\ell(c_{it})\, if\, c_{it} < \tilde{c}(c_{g(i)}) \\ u^h(c_{it})\, otherwise \end{cases}$ Conventional prefeneces utility function: $u^l(c_t) = \frac{c_t^{1-\rho_l}-1}{1-\rho_l}$ $\beta = 0.95$ $\rho_l = 0.75$ $\rho_h = 2.5$
Distribution of Shocks
The probability of θ_{jt} is assumed to be: density of $\theta_{jt} = \begin{cases} 0.3 & \theta_{jt} = 0.11 \\ 0.18 & \theta_{jt} = 0.021 \\ 0.13 & \theta_{jt} = 0.031 \\ 0.11 & \theta_{jt} = 0.041 \\ 0.10 & \theta_{jt} = 0.051 \\ 0.02 & \theta_{jt} = 0.061 \\ 0.01 & \theta_{jt} = \{0.071, 0.081, ..., 0.191\} \end{cases}$

A.2. Balance and Attrition Tables

TABLE A2. Baseline Balance

Panel A: T-test comparing means of baseline characteristics by endline treatment status				
	Control mean [standard error]	Treatment mean [standard error]	p-value from t-test	Normalized difference
Baseline Reported Annual Cash Earnings (KES)	33313.454	36319.322	0.071*	-0.097
	[1100.568]	[1258.628]		
Baseline Total Business Assets (KES)	1880.193	2242.171	0.5	-0.036
	[290.374]	[472.487]		
Baseline Amount Deposited Into Savings (KES)	466.494	665.586	0.212	-0.067
	[90.057]	[137.083]		
Tropical Livestock Units BL	3.108	3.057	0.796	0.014
	[0.128]	[0.156]		
Baseline Household Dietary Diversity Score	3.1	3.019	0.166	0.075
	[0.040]	[0.042]		
Ladder of Life Step	2.089	2.111	0.446	-0.041
	[0.019]	[0.021]		
Locus of control scores (higher - more external)	33.883	33.432	0.214	0.067
	[0.248]	[0.263]		
Baseline Internal Locus of Control Score	17.278	17.154	0.291	0.057
	[0.080]	[0.085]		
Baseline CES-D	8.397	8.429	0.901	-0.007
	[0.171]	[0.200]		
Baseline importance to move to ladder 3	4.579	4.585	0.841	-0.012
	[0.021]	[0.023]		
Baseline importance to move to ladder 4	4.643	4.65	0.82	-0.012
	[0.019]	[0.019]		
Baseline importance to move to ladder 5	4.731	4.69	0.133	0.081
	[0.018]	[0.021]		
Other people sharing business knowledge	-0.016	-0.353	0.241	0.063
	[0.132]	[0.272]		
Age of HH head	43.821	43.342	0.618	0.028
	[0.643]	[0.716]		
Age of respondent	36.101	35.159	0.286	0.058
	[0.605]	[0.639]		
% respondent with husbands	0.612	0.583	0.273	0.059
	[0.018]	[0.020]		
HH head yrs of schooling	1.195	1.177	0.916	0.006
	[0.112]	[0.125]		
HH size	5.437	5.331	0.345	0.051
	[0.076]	[0.081]		
	# of obs control	# of obs treatment		
Panel B: Regression of Treatment on all outcomes				
F-test from regression of treatment on all outcome variables listed above	1.49			
p-value	0.11			

Table A3. Attrition: Dependent Variable: Completed Survey, OLS

Panel A		
	Midline	Endline
Treatment Status	-0.0071	-0.00013
	(0.013)	(0.017)
Observations	1,874	1,874
R-squared	0.052	0.063
Outcome mean	0.92	0.86
Panel B		
Treatment Status	-0.0059	0.0012
	(0.013)	(0.017)
Household Reported Cash Earnings(KES)	8.3e-08	2.3e-07
	(1.1e-07)	(1.5e-07)
Total Business Assets(KES)	7.1e-08	-2.1e-07
	(3.1e-07)	(4.1e-07)
Savings(KES)	4.6e-07	-1.6e-06
	(1.4e-06)	(1.8e-06)
Household Dietary Diversity Score	-0.00021	0.0026
	(0.0063)	(0.0083)
Ladder of Life Step	-0.0035	-0.010
	(0.025)	(0.033)
Locus of Control	-0.00018	-0.00022
	(0.0011)	(0.0014)
Internal Locus of Control	-0.00073	-0.00046
	(0.0033)	(0.0043)
Shared with Business Knowledge	-0.050**	-0.033
	(0.0014)	(0.0018)
CES-D Score	-0.0042***	-0.0039**
	(0.0014)	(0.0018)
TLU (Tropical Livestock Units)	-0.00072	-0.0013
	(0.0018)	(0.0024)
Importance to move to ladder 3	0.0089	0.0062
	(0.017)	(0.023)
Importance to move to ladder 4	-0.0036	-0.00042
	(0.019)	(0.024)
Importance to move to ladder 5	-0.0049	0.011
	(0.018)	(0.023)
Observations	1,874	1,874
R-squared	0.062	0.070
Outcome mean	0.92	0.86
Panel C		
Treatment Status	0.36	0.50
	(0.25)	(0.32)
Baseline characteristics	YES	YES
Baseline characteristics interacted with Treatment	YES	YES
Observations	1,874	1,874
R-squared	0.069	0.080
Outcome mean	0.92	0.86
P-value from test that Treatment and all other variables above interacted with Treatment are jointly 0	0.66	0.17

A.3. Imputing the missing data on BOMA total business assets

During the midline data analysis, we encountered a discrepancy between the reported total business assets from the survey responses of women assigned to the treatment group and the BOMA administrative data from February 2020. To investigate this further, in March 2021, we conducted a follow-up round of discussions with BOMA mentors to understand the reasons behind these discrepancies.

In summary, out of the total sample of 241 women, 8 (approximately 3%) were confirmed as attrition cases, either due to non-participation at the time of the survey or never having participated in the program. Among the remaining 233 women, around 14% (33 women) experienced various disruptions during the midline data collection period, such as forest evictions, security concerns, or temporary travel/migration.

According to the mentors' feedback, there were several reasons for the reported zero business assets. First, some participants did not realize that the mentors were referring specifically to their BOMA businesses, leading to confusion in their responses. Second, a significant proportion of women (approximately 66% based on the provided Excel sheet) either did not fully understand the question or had difficulty comprehending it. Additionally, some women admitted to not being fully focused during the interview or feeling shy, which could have affected their responses. Among the women, 10% (23 women) were mentioned by mentors as being less active or not directly involved in the day-to-day operations of the business during the midline survey period. Finally, mentors suggested that a subset of women (approximately 12%, or 27 women) intentionally withheld information from the enumerators, even if they understood the questions.

Imputation of missing values. For the cases where participants reported zero business assets but had business value recorded in the BOMA administrative data, we made a change in our analysis. Specifically, we marked the zero total business assets as missing for those participants who were identified by the BOMA mentors as Did not understand the question only. Among the total sample of 241 cases, 135 (approximately 56%) were identified by the BOMA mentors as having non-zero business assets.

However, we did not make any changes to the reported zeros for the other categories of participants. This decision was made because we observed instances of zero business assets reported by the control group, even when they self-identified as owning businesses. Additionally, we lacked information regarding the reasons behind the control group's reported zero business assets. Therefore, we chose not to alter the reported zeros for these categories during the analysis.

Predictive mean matching. We used predictive mean matching to impute those missing values. In STATA, it is under multiple imputation method. Predictive mean matching (PMM) is a partially parametric method that matches the missing value to the observed value with the closest predicted mean (or linear prediction). It was introduced by Little (1988) based on Rubin's (1986) ideas applied to statistical file matching. PMM combines the standard linear regression and the nearest-neighbor imputation approaches. It uses the normal linear regression to obtain linear predictions. It then uses the linear prediction as a distance measure to form the set of nearest neighbors (possible donors) consisting of the complete values. Finally, it randomly draws an imputed value from this set. By drawing from the observed data, PMM preserves the distribution of the observed values in the missing part of the data, which makes it more robust than the fully parametric linear regression approach.[1]

[1]For detailed information on this step: STATA manual mi impute pmm Methods and formulas.

A.4. Impact Regression Tables

Table A4. Full Regression Results

	Standard ITT			Baseline Depression Heterogeneity		
	Business Assets	*Family Income*	*Savings*	*Business Assets*	*Family Income*	*Savings*
Waves 1 & 2	190***	98.2***	56.3***	209***	121***	56.0***
	(22.3)	(34.2)	(8.27)	(23.1)	(39.2)	(7.64)
Waves 3 & 4	125***	4.43	25.0***	137***	2.89	25.1***
	(18.6)	(36.3)	(6.19)	(25.6)	(43.4)	(8.45)
Waves 1 & 2 × Depression				-111**	-133	1.77
				(55.9)	(94.6)	(18.4)
Waves 3 & 4 × Depression				-76.9	10.6	-1.19
				(64.4)	(109)	(21.2)
Depression				-4.74	-9.37	-6.66
				(29.7)	(50.3)	(9.79)
Baseline Level Dep. Variables	0.13*	0.11***	0.20**	0.13***	0.10***	0.20***
	(0.064)	(0.029)	(0.092)	(0.036)	(0.020)	(0.040)
Constant	66.2***	449***	14.5***	66.9***	452***	15.7***
	(10.7)	(28.0)	(2.94)	(12.3)	(26.2)	(4.03)
Observations	1,385	1,385	1,385	1,385	1,385	1,385
R^2	0.071	0.027	0.066	0.076	0.030	0.066

Notes: Baseline levels of the outcome of interest and an indicator for vulnerable group are included in both specifications for ITT and quantile regressions. Total income was measured by adding reported cash income from sales of livestock, livestock products, crops, casual labor, salaried employment, and business, duka, and petty trading for each household. Assets were calculated by summing up the cash, stock, assets, savings, and credits related to each individual business that the respondents own. Standard errors clustered at the community level. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

Table A5. Impact Estimates Accounting for Spillovers

VARIABLES	Business Assets ($PPP)	Family Income ($PPP)	Savings ($PPP)
ITT Wave 1 & 2	258***	293***	72.6**
	(57.2)	(103)	(30.4)
ITT Wave 3 & 4	195***	35.4	54.9**
	(58.0)	(120)	(27.5)
ITT w1-w2 * Saturation	-92.1	-818**	-28.7
	(220)	(409)	(118)
ITT w3-w4 * Saturation	-123	-96.7	-107
	(283)	(523)	(122)
Non-treated Control * Saturation	270*	73.7	57.0
	(140)	(266)	(43.6)
	[0.207]	[0.414]	[0.243]
Baseline Level of Dependent Variables ($PPP)	0.12*	0.11***	0.20**
	(0.064)	(0.029)	(0.092)
Constant	19.2	436***	4.59
	(19.3)	(54.2)	(6.57)
Observations	1,385	1,385	1,385
R-squared	0.073	0.030	0.067
Control Mean of Dependent Variable	72.2	535	16.6

Note: Standard errors clustered at the community level. Sharpened q-values for estimated coefficients on within-cluster control reported in square brackets. $^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$.

A.5. ATE on Psychological Variables

Table A6 presents results on psychological outcomes. While accounting for spillovers does not change the result that the REAP program had no impact on women's CES-D score. We do see a consistent pattern of positive program impacts on mental health (lower numbers mean less likely to be depressed, and lower numbers means more internal locus of control) and improvement in the life evaluation on the Waves 1 and 2 women (measured as the current step on the ladder of life). Nor did we find any statistically significant improvement on the within-cluster control women on the CES-D score. On locus of control, which measures the degree to which people believe that they have control over the events that affect their lives — internal locus of control —, as opposed to external forces — external locus of control — (Rotter (1966)). We find that the REAP program reduces the locus of control measure for the participants (which moves them towards more internal locus of control). However, accounting for the spillover effects undercut the direct effects and we find no sign of spillovers to control on locus of control, either.

Table A6. Treatment effects on secondary outcomes

VARIABLES	CES-D	Locus of Control	Life Evaluation
ITT Wave 1 & 2	-0.018	-0.34*	0.35**
	(0.18)	(0.18)	(0.15)
ITT Wave 3 & 4	-0.31	-0.42	-0.13
	(0.19)	(0.27)	(0.24)
ITT w1-w2 * Saturation	-0.32	1.32*	-0.69
	(0.73)	(0.79)	(0.57)
ITT w3-w4 * Saturation	1.27	2.59*	1.31
	(0.82)	(1.38)	(1.35)
Non-treated Control * Saturation	-0.073	0.14	0.27
	(0.47)	(0.49)	(0.48)
Baseline level of outcome variables	0.11***	-0.019	0.23***
	(0.028)	(0.027)	(0.034)
Constant	0.013	-0.025	-0.048
	(0.093)	(0.095)	(0.088)
Observations	1,384	1,378	1,385
R-squared	0.015	0.008	0.061

Note: Standard errors clustered at the community level. Outcome variables standardized at control mean. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

Table A7. Ordered Probit results on Ladder of Life

VARIABLES	Importance to get to ladder 3	Importance to get to ladder 4	Importance to get to ladder 5
ITT Wave 1 & 2	0.654**	0.491**	0.401**
	(2.35)	(2.18)	(1.96)
ITT Wave 3 & 4	0.178	-0.087	-0.309
	-0.52	-0.31	-1.11
ITT w1-w2 * Saturation	-1.5	-1.375	-0.874
	(1.56)	(1.55)	(1.15)
ITT w3-w4 * Saturation	1.598	2.38*	3.088**
	(1.12)	(1.90)	(2.05)
Control * Saturation	0.991	1.015*	0.875
	(1.26)	(1.71)	(1.38)
Baseline level of outcome variables	-0.051	0.051	0.035
	(0.75)	(0.63)	(0.51)
Observations	830	1,353	1,382

Note: Standard errors clustered at the community level. Importance scale (1-5 very important) estimated using ordered probit.$^{***}p < 0.01$,$^{**}p < 0.05$,$^{*}p < 0.1$.

A.6. Benefit-Cost Methodology for Total Causal Effects

One way to gauge the effectiveness of the REAP program and these individual impacts is to calculate the estimated discounted present value of all benefits (direct & indirect) and compare those benefits to the present value of the program cost. We evaluate the return on investment ratio (ROI) taking into account this admittedly complex pattern of spillovers and the fact that non-treated households receive spillover benefits without any additional expenditure. We calculate the ROI using the following formula:

$$ROI(S) = \sum_{t=1}^{10} \left[\frac{TCE_t(S)}{n \times \widetilde{S} \times c_t} \right] = \sum_{t=1}^{10} \left[\frac{S(\beta^w + S\delta^w) + (1-S)(S \times \delta^c)}{\tilde{S} \times c_t} \right] \tag{A.1}$$

where c is the cost per-treated person. Note that the TCE(S) in the numerator is, as before, the total benefits to a community of n eligible people where a fraction of S are treated. The denominator gives the investment needed to receive that impact (number of people treated, $n \times \widetilde{S}$ times the cost per-fully treated person. The new term $\tilde{S}$ is a slightly modified version of the duration weighted saturation measure that better captures the relative expense of treating individuals in waves 1, 2 and 3 compared to wave 4.[2] The expanded expression of the ROI expression shows that the ROI is just the per-capita benefit (weighted average of direct and indirect benefits) divided by the average cost of all people treated ($s \times c + (1-s) \times 0$).

[2]Costs of treating a REAP beneficiary is not quite proportional to time in the program. For example, a wave 4 woman was in REAP for only 25% of the time as a wave 1 women, but the expenditures on a wave 1 woman was about 37% of the cost of a wave 1 woman. The new term \tilde{S} replaces the time weights used to define S with these relative expenditure weights.

Note further that if there are no spillover impacts and $\delta^{w,c} = 0$, then this expression reduces down to the more standard looking $\frac{\beta^w}{c}$.

Using the cost per household converted to \$PPP 713.21, the benefit-cost ratio is 2.2:1, estimated from BOMA's point of view. In words, every dollar invested in the REAP program generated \$2.2 in benefits for REAP eligible women. This increase compared to the simple measure had we estimated a naive ITT results from valuing the spillovers to non-treated eligible women, which in turn causes some undercounting of the benefits to treated women. We have not considered impacts on women in BOMA communities who are not eligible for the program.

We take total causal effect estimated for reported annual cash earnings for wave 1 and wave 2 participants and assume that those treatment effects persist constant until the end of seven years after the intervention begins, then gradually decline over the next three years. While we do not model any changes to business assets or savings through years 2 through 7, starting in year 8 we assume that the participants liquidate their savings and business assets over a period of three years as their businesses wind down. We also assume that any other differences between participants and nonparticipants because of the REAP program disappears by the 10-year mark. We assume that one third of the savings and asset stocks generated (and presumably held) enter as cash flow in year 8, one third in year 9 and the final third in year 10. We assume a discount rate of 5% per year. At the median duration-weighted saturation level of 0.19, the total discounted value of the intervention is estimated to be \$PPP 350.99 by this approach. According to BOMA's estimation on cost to deliver the program, the cost per participant for Wave 1 participants (who had graduated from the program by midline) is \$322.46 per household (after discounting to the present value). This number includes all program delivery, managerial and administrative costs associated with the REAP program, as reported by BOMA administrative data.

CHAPTER 3

Spillover Effects of Graduation Program through Local Kinship Network

3.1. Introduction

In this chapter, my objective is to examine whether the REAP program generates any spillover effects through the female kinship network in local communities to women who were not assigned to receive treatment. Specifically, this paper empirically investigates whether there exists any spillover effects of multi-facted "graduation" programs through the extended family for the eligible women in local communities.

In the previous chapter, we quantified the spillover effects using common study designs that aim to identify such effects by clustering treatment at the village level and assuming no spillovers across villages. To capture the treatment duration and overall intensity of treatment within local communities, we utilize a "duration of treatment-weighted" measure. This measure intends to intuitively capture the spillover effects on any REAP-eligible woman through random social interactions with REAP-treated women in the local community between the baseline and midline surveys. The community-level saturation measure seeks to account for these spillover effects.

Meanwhile, it is reasonable to hypothesize that spillover effects could also occur through interactions with REAP-treated women with whom the REAP-eligible woman has deep and prolonged connections, such as those within her kinship network. In his seminal paper Granovetter (1973), Granovetter suggests that the "strength" of an interpersonal tie could be assessed through the following dimensions: 1) the amount of time spent together, 2) the emotional intensity between those two ; 3) the intimacy (mutual confiding) and 4) the reciprocal services of those ties. The spillover effects through the local kinship network structure of those women is elicited and used as the interpersonal links through which spillover effects could happen is because: 1) they are predominantly exogenous to the treatment as the friendship through other dimensions could be altered as women form business groups and 2) female kin ties stand for a higher proportion of women's personal networks compared to men in local context Moore (1990).

Different dynamics may exist regarding spillover effects on random associations within a community compared to those with relatives, especially for women in developing contexts, such as those within local communities. Their social roles, in general, tend to restrict the composition of their networks, which can influence the nature of spillover effects.

I utilize the random variation generated by the RCT design of the REAP program evaluation to examine the intensity of treatment on the kinship network of control group women. This allows me to estimate the spillover effects on key material well-being outcomes, including earnings, total business assets, and savings. By comparing the outcomes of women who had no BOMA relatives with those who did, I find that randomly assigning non-treated BOMA-eligible women's relatives to receive the REAP program leads to an increase in the total business assets and savings of their cohort relatives. On average, having two or more relatives treated by BOMA results in an over 60% increase in the total business assets and savings of control group women compared to those who did not have any relatives treated by BOMA by the endline. Additionally, I also observe a significant increase in these women's self-perceived living standards and household dietary diversity compared to their cohorts who had no BOMA-treated relatives by the endline.

While I have identified significant spillover effects through women's kinship network structure, these impacts on material and subjective well-being were primarily found among female respondents in the control group who were not the household heads of their families. Interestingly, I did not find any significant spillover effects through the kinship network for households with female head. Additionally, they were no more likely to receive shared knowledge on running a business, even if they had relatives treated by BOMA, compared to control women who had no relatives treated by BOMA.

In comparing the spillover effects quantified in the last chapter, which utilized the random variation in community-level saturation, with the spillover effects investigated in this chapter through the local kinship network, several interesting findings emerge. On the material outcomes, when examining the spillover effects on total business assets, the results suggest that the effects observed through the local kinship network are comparable in magnitude to the average spillover effects observed in a community with a median saturation level. Moreover, I find positive spillover effects on savings through the kinship network, which were absent when estimating the spillover effects at the community level.

Interestingly, the study also uncovered that treated women were sharing business management knowledge with their untreated cohorts at the median saturation community by the midline survey. However, these spillover effects only became evident at the endline through the local kinship network. This finding highlights the efficiency of information diffusion among random interactions with any REAP treated woman and suggests that spillover effects on knowledge diffusion may occur through local female kinship network at a slower pace. While random interactions within the community facilitate the initial spread of information, local kinship network plays a crucial role in the idea elaboration and decision-making stages, where discussions and feedback from trusted individuals become more relevant.

I conclude with a discussion of the broader implications of the findings for understanding economic vulnerability and inequality within poverty alleviation interventions, such as "graduation programs." These findings provide valuable insights into the dynamics of spillover effects and the role of different network structures in facilitating information diffusion and resource sharing. Considering the spillover effects on the non-treated group would further enhance the cost-effectiveness of these expensive programs.

Furthermore, it is essential to recognize that economically vulnerable groups of women, especially those who are single-handedly supporting their households, may require additional support or special consideration during the targeting process. It is crucial to understand the underlying mechanisms that hinder these women from benefiting from the spillover effects of the program and the greater challenges they may face in accessing resources and opportunities.

By comparing community-level spillover effects with kinship network spillover effects, we can highlight the unique contributions and mechanisms of each network structure. This emphasizes the importance of considering both aspects in order to fully understand the overall impact of interventions on non-treated individuals.

The findings presented in this paper contribute to a broader body of research examining the impacts of the multi-faceted graduation program[1]. Previous studies have documented positive effects on both the material well-being (such as increased consumption levels driven mainly by income growth) and psychological well-being of targeted households across diverse geographic and institutional contexts (Banerjee et al., 2015; Blattman et al., 2016; Bandiera et al., 2017; Ridley et al., 2020; Bossuroy et al., 2022). Most of these studies, using a village-level clustering approach and assuming no spillovers across villages, found no significant spillover effects on non-treated households. However, the results presented here underscore the significance of considering the unique dynamics of spillover effects that can be mediated through the local structure of social networks. This local structure can give rise to distinct patterns of economic interaction that may have an impact on the program's outcomes.

This study also contributes to the expanding body of research on the roles of network-based processes in facilitating economic activities in developing economies, where formal institutions and markets may be

[1]initially designed and implemented by BRAC, a large Bangladeshi NGO

absent or inadequate to meet people's needs (see Breza et al., 2019, for a review). Previous studies that focused on information provision, including learning and diffusion (Banerjee et al., 2013; Cai, Janvry, and Sadoulet, 2015; Banerjee et al., 2021; Beaman et al., 2021) have generally assumed that the diffusion of knowledge occurs through an exogenous network within the local social network structure, with connections between households pooled through certain dimensions hypothesized to facilitate diffusion.

Compared to men, women may be more confined to their neighborhood or their kinship network structure. Women tend to experience fewer network benefits due to their limited social roles, which can restrict their network positions and impact their connections and relationships. As noted by the findings in Beaman and Dillon (2018), women were left out of the knowledge diffusion process in local villages where the program intervention targeted individuals with higher centrality in the network. The findings in this paper suggest that in scenarios where women are intended beneficiaries, some women were still excluded from the spillover channels of information and resources, even though kin ties formed a significant part of their personal networks. Therefore, when evaluating policies related to the diffusion and learning of women in local communities within developing economies, additional hurdles or obstacles may arise from a policy evaluation perspective.

The remainder of this paper is organized as follows. Section 2 presents a conceptual approach for spillover effects and potential mechanisms behind spillover through local kinship network. Section 3 presents summary statistics on the data used for the analysis. Section 4 presents the empirical strategy used for the analysis. Section 5 then presents the reduced form results on the spillover effects. Finally, Section 6 concludes by reflecting on the implications of these findings for the design of more cost-effective graduation programs.

3.2. Conceptual Approach

In this section, I will describe two key dimensions of the analytical approach in this paper. Firstly, I will provide a more detailed explanation of how the impacts of the REAP program can potentially spill over through the local kinship network, affecting the non-treated women's material outcomes and other well-being indicators. Secondly, I will compare and contrast the different spillover dynamics that occur through random interactions within local communities with any woman treated by BOMA, and those that occur specifically through the kinship network.

3.2.1. Female Kinship Network. Given that the REAP program exclusively treated women within local communities, and the sample consists solely of female respondents, this paper's focus is to investigate whether the treatment of female relatives in the REAP program generates spillover effects on their non-treated female relatives. The study explores various kinship relationships, including first-order connections characterized by genetic proximity, such as mother-daughter and sister-sister relationships, as well as second-order connections like mother-in-law, sister-in-law, daughter-in-law, and co-wives. Through the examination of these connections, the paper aims to illuminate the potential spillover effects within the kinship network.

Moore (1990) found that women's personal networks usually is focused on family compared to men's and that originates partly from social structural locations of men and women, which lead to distinct differences in network composition. Kin ties stand for a higher proportion of women's personal networks compared to men.

In an ethnographic study conducted by Spencer (2003) in the Samburu community, an extremely patriarchal society with a polygamous culture, close relationships among married women and their kin were observed. Despite the prevailing patriarchal norms, co-wives were found to develop friendships instead of rivalries, often based on their shared opposition to the elders. Furthermore, female relatives from the same clan were expected to provide mutual support when they married into different clans, and it was not uncommon for women from the same clan to live together in the same homestead.

In this pastoralist society, where gender inequalities exist, women play a significant role as caregivers, particularly during challenging circumstances such as drought. When drought strikes and livestock herds perish, leaving families without food, cash, and means of income, men often embark on long journeys in search of grazing areas, while women and children remain in the villages for extended periods, sometimes up to six months. In these circumstances, women are compelled to come together and find ways to secure livelihoods for themselves and their children, fostering a sense of unity and collective action. This highlights the resilience and resourcefulness of women in the face of challenging conditions, as they work collectively to support each other and navigate the difficulties posed by their pastoralist lifestyle.

3.2.2. Spillover effects of REAP through Local Kinship Network. Compared to evidence on spillover impacts of direct cash transfer programs (Egger et al., 2022; de Milliano et al., 2021; Carraro and Ferrone, 2023), the "graduation" program has a distinct approach. While direct cash transfers focus on injecting income into communities, the "graduation" program teaches beneficiaries about business fluctuations and challenges through hands-on experience. It recognizes that business success depends on multiple factors, including market conditions, customer demand, skills, and resources. By imparting this knowledge, the program aims to build beneficiaries' capacity to overcome challenges and promote sustainable economic growth at the individual and community level.

Interviews with women in the field suggest that they noticed the changes that happened to their closely-related relatives after the REAP program started. After enrolling in the REAP program, their friends started to pay more attention to personal hygiene of each household member. Their treated friends inspire them to maintain the tidiness and sanitation of the dwelling and changing clothes for their children from time to time.

If the treated beneficiaries of the REAP program experience successful businesses and generate positive income, it is possible that they may "crowd out" the informal safety nets provided by the local kinship structures. As a result, they may require less mutual support from their non-treated kinship ties. However, at the same time, they may increase the amount of direct transfers from the REAP beneficiaries to their non-beneficiary relatives who are connected through kinship ties. In this way, they could act as a safety net for their non-beneficiary relatives within their kinship network.

Additionally, the increased economic participation of women in local communities through the REAP program may generate positive general equilibrium effects on local markets. For instance, by creating higher demand for labors and reflected by increased wage. This could potentially increase the household earnings of some of the non-treated relatives. This could also lead to increased interactions and transfers among treated women and their non-treated relatives.

Furthermore, the business groups and savings groups established by REAP incentivize women to collaborate with their non-treated relatives for productive investments. For instance, they may collectively accumulate livestock units from their allotted herd and aspire to start another livestock sales business together. Even if their treated relatives do not initiate cooperation to pool resources, the success of their BOMA relative entrepreneurs may still serve as inspiration for the non-treated relatives. Their relatives' income levels may enter the non-treated woman's "aspirations window" as in Ray (2006), which could be formed from the woman's zone of similar and attainable individuals. If the women have reference-dependent preferences ("keeping up with the Joneses") (see Bursztyn et al., 2014), this encouragement could motivate them to strive and plan for a better future. The non-treated relatives may aspire to achieve the consumption level that the treated relatives can afford, such as clothing, educational and medical expenses for their children, and other durable goods for the household, following in the footsteps of their successful role models.

Not only do the treated BOMA relatives serve as role models for their non-treated relatives, but they also have the potential to share valuable insights, practical business management tips, and relevant market information. This knowledge exchange between treated and non-treated relatives can benefit the non-treated individuals in their journey towards becoming female entrepreneurs and earning a livelihood. The diffusion of knowledge, a non-rival good, is well-documented in cases where there are no supply constraints on the good (Banerjee et al., 2013; Miller and Mobarak, 2015; Beaman and Dillon, 2018).The shared experiences and guidance from the treated relatives can contribute to the growth and development of their non-treated relatives' entrepreneurial endeavors.

Once non-treated relatives experience positive spillover effects from their treated BOMA relatives, such as improvements in their material well-being, it is reasonable to expect second-order improvements in other well-being measures. These may include increased food diversity and nutrition, as the improved economic situation may enable access to a wider variety of nutritious food. Additionally, enhanced self-perceived living standards may arise as non-treated relatives witness the positive changes in their treated relatives' lives.

These dynamics highlight the potential for the REAP program to have a significant impact not only on the treated beneficiaries but also on their non-treated relatives within the kinship network, improving their material well-being and fostering positive changes in various dimensions of their overall well-being.

3.2.3. Female headship households among the kinship network. However, it is important to recognize that female-headed households often face additional challenges and have fewer pathways to resilience compared to women living in male-headed households. Buvinić and Gupta (1997) found that female-headed households are disproportionately represented among the poor. This disparity can be attributed to various factors, including higher dependency ratios and lower average earnings of the main breadwinner, typically the female household head, or a combination of both. Female-headed households may encounter discrimination in accessing employment opportunities and resources, exacerbating the gender-based discrimination they already face. These households also contend with social and economic pressures that further limit their opportunities.

The anecdotal evidence collected from the field suggests that female-headed households can primarily be attributed to three factors: 1) women who are living with children outside of marriage. 2) women who have returned to their family clan after the death of their husbands and 3) widows who continue to reside with their husbands' clans.

In Samburu County, evidence from household economic analysis conducted by USAID[2] indicated that poor female-headed households own fewer livestock and derive less income from livestock and related products. Low-income women, in particular, often occupy weaker market positions and can be vulnerable to gender-based exploitation due to their limited economic and social power. Factors such as the inability to trade in major livestock assets, limited savings, restricted mobility due to household responsibilities, and the need to prioritize childcare and household duties further hinder their ability to engage in economic activities and access resources.

For female-headed households, especially those facing economic constraints, pooling resources with their treated relatives for productive investments can be challenging. Despite their aspirations to start businesses like their treated relatives, social and economic pressures may hinder their efforts. These households often lack the necessary resources and have limited access to information, even if their relatives have benefited from the REAP program. The combination of these factors perpetuates their marginalization and restricts their opportunities for economic advancement.

It is crucial to acknowledge the unique circumstances and barriers faced by female-headed households within the kinship network. Efforts to support and empower these households should consider their specific needs, such as targeted interventions to enhance their economic opportunities, provide access to resources, and address the systemic factors contributing to their marginalization.

3.2.4. Different Spillover Dynamics through Community Interaction and Kinship Network. In a developing context like Samburu, strong ties, such as kinship networks, inevitably play an important role in structuring economic activities, especially in women's daily social lives. Spencer (2003, p.160) observed that women often gather together under a tree, engaging in conversations, gossiping, and engaging in handiwork. However, weak ties also hold significance in information diffusion.

In "The Strength of Weak Ties" Granovetter (1973), social connections' importance in information diffusion, especially in labor markets, is emphasized. Interpersonal relationships with lower intensity, confiding, and reciprocal support (weak ties), act as bridges connecting various parts of a social network. Recent research (Rajkumar et al., 2022) supports the importance of those ties in job switching, with their impact varying by industry, while those interpersonal relationships with higher intensity, more frequency and high reciprocal support remain influential in less digital industries.

[2] https://www.advancingnutrition.org/sites/default/files/2023-03/Household-Economic-Analysis.pdf

High treatment intensity of the REAP program increases non-treated women's chances of encountering BOMA women with whom they maintain weak ties in the community, serving as bridges for information diffusion and potential spillover effects. Strong ties, like kinship and close friendships, also contribute significantly to social networks, fostering local cohesion and mutual support (Gee et al., 2017). While weak ties efficiently disseminate business management information within a village, reaching a broader audience than strong ties, they expose individuals to diverse perspectives, fostering creativity. However, strong ties, such as kinship networks, play a crucial role during the idea elaboration phase, offering sustained support and engagement (Mannucci and Perry-Smith, 2022). Understanding the dynamics of information diffusion within both weak and strong ties provides insights into how social connections influence economic choices and outcomes for local women.

3.3. Data

3.3.1. Spillover effects through kinship network. In Chapter I, we explored the spillover effects within local communities by assuming random interactions among people and assumed no spillover effects across different clusters. In this chapter, the focus shifts to analyzing the spillover effects through the local kinship network onto non-treated women. This approach is motivated by the assumption that the network structure itself is exogenous to the treatment intervention. In empirical projects that utilize an existing network structure and consider pre-existing links as influential for economic outcomes (see Banerjee et al., 2013; Cai, Janvry, and Sadoulet, 2015; Breza and Chandrasekhar, 2019), it is typically assumed that the network structure is not influenced by the treatment intervention. This assumption allows us to study the impact of the treatment within the given network context. Therefore, this study concentrates on the kinship network, as it is less likely to be altered by a specific intervention.[3]

3.3.2. Number of relatives treated by BOMA data. During the endline data collection, I included additional questions asking respondents whether they knew any BOMA women who were their immediate or non-immediate relatives. Immediate relatives were defined as individuals' first-order relatives such as grandmothers, granddaughters, mothers, daughters, and sisters. Non-immediate relatives were defined as her second-order relatives including individuals such as mothers-in-law, co-wives, and sisters-in-law on the husband's side, and daughters-in-law and wives of brothers on the respondent's side.

[3]In the appendix to this chapter, I provide documentation for an alternative network structure that I initially intended to investigate when designing the survey. However, the collected data did not exhibit sufficient variation for me to test the hypothesis.

Table 3.1. Descriptive Statistics: number of BOMA relatives

Frequencies of count variables	Mean	0	1	2	3 or more
Number of BOMA relatives	0.86	50%	28%	13%	9%
Male head	0.82	52%	27%	13%	8%
Female head	0.80	53%	27%	13%	7%

Note: The data is from the endline 2022 survey round, where respondents reported the number of relatives who they knew were BOMA women. The frequencies of the number of BOMA relatives are shown for two subgroups: respondents who reported the household head as female or male. The p-value for a two-sample Kolmogorov-Smirnov test for equality of distribution between those two groups is 0.96.

Table 3.2. Descriptive Statistics: Male head vs. Female head households

	Male head		Female head		
	Mean	Std. dev.	Mean	Std. dev.	p-values from ttest
Head years of education	1.61	3.56	0.71	2.27	0.00
Respondent's age	31.31	12.52	43.78	18.19	0.00
Dummy: lives with husband	0.97	0.17	0.02	0.14	0.00
Head age	43.81	15.82	44.34	17.97	0.56
Household size	5.93	2.18	4.88	1.97	0.00
% of hh members under 12	0.44	0.19	0.42	0.26	0.05

Table 3.1 presents the distribution of connectivity between respondents' kinship networks and BOMA beneficiaries, demonstrating significant variation in the level of connectivity. This variation is crucial for identifying the coefficients of interest described in the empirical strategy outlined below.

Among the respondents, approximately 50% reported having no relatives treated by BOMA. In contrast, 28% reported having one relative treated by BOMA, and an additional 22% reported having two or more relatives treated by BOMA. This variation in the number of relatives treated provides an opportunity to examine the spillover effects on non-treated women within different levels of connectivity to the BOMA program. Moreover, the variation in the number of relatives treated between those women who reported household heads being male were not statistically different from those who reported themselves as female heads of the households.

Table 3.2 lists the household characteristics that are included in the regression analysis later to account for the potential omitted variables that could potentially affect both the outcome of interest and the number of treated relatives. It is shown that on average, respondents who identify themselves as the head of the household are older, less likely to live with their husbands and have smaller household sizes.

3.4. Empirical Strategy

To examine the potential spillover effects to the relatives of treated BOMA women, I estimate the following regression equation for outcome variable y_{hmt} of respondent h in village m in time period t for the control group women:

$$
(3.1) \quad \begin{aligned} y_{hmt} = {} & \delta_1 \left(OneRelativeTreat_{hm,t=2} \right) + \delta_2 \left(Two^{+} RelativeTreat_{hm,t=2} \right) \\ & + \delta^c Vul_{hm} + \mathbf{X}_{hmt} \gamma + {}_{s=1}^{2} \sum \omega_s 1(s = t) + \theta_m + \varepsilon_{hmt} \end{aligned}
$$

where respondents with $OneRelativeTreat_{hm,t=2} = 1$ indicate that one of their relatives was treated by REAP, while respondents with $Two^{+} RelativeTreat_{hm,t=2} = 1$ indicate that at least two of their relatives were assigned to receive treatment by REAP by the endline. Since treated women have the option to choose other eligible women from a pool (a randomly generated list containing the names of other eligible women) as their BOMA business partners, the number of relatives treated by BOMA among the treatment group could be influenced by self-selection. Therefore, I limit the regression analysis to the control group only.

Vul_{hm} is an indicator variable for an ineligible woman h who is in the control group in manyatta m. The vector of controls $\mathbf{X}_{hmt}$ includes baseline characteristics of the respondents' household sizes, which are used to control for the effects of kinship network size.[4] This is because households with larger kinship networks are likely to have more kins in the treatment group. Additionally, the vector of controls includes a measure of the community-level saturation rates throughout the treatment period. As women in different communities started to enroll at different times, the community-level saturation could be correlated with both the number of treated women in local kinship networks and the outcome variables. ω_s represents survey round dummies for the midline ($s = 1$) and endline ($s = 2$). Finally, the θ_m terms represent the community/village-level fixed effects to account for village-level time-invariant factors that could affect both the number of BOMA relatives and the outcome variables. ε_{hmt} is the error term. Standard errors are clustered at the manyatta level.

I will assume that: 1) the treatment assignment would not change the local kinship network structure, and 2) there are no other omitted variables that affect both the number of relatives treated in one's local kinship network and the outcome variables throughout the rounds t. The regression coefficients on the terms involving the number of relatives treated by BOMA can be interpreted as the causal effect of having one and two or more BOMA relatives.

The regression coefficients of interest are δ_1 and δ_2, which represent the spillover effects of the REAP program on control women who have one and two or more BOMA relatives, compared to those who do not have any BOMA relatives. Positive values of δ_1 and δ_2 suggest positive spillovers on control women who

[4]These baseline characteristics consist of an indicator variable for a female household head, an indicator variable for whether the respondent lives with her husband, the household head's years of schooling, the age of the respondent, and household size.

have BOMA-treated relatives compared to those who have zero BOMA relatives. A test on $\delta_1 = \delta_2$ would tell us whether having more relatives would have different effects on the control women.

It would be ideal if I could match the treatment waves of those control women's assigned-to-treat relatives, as not all women were treated at once in the experiment. Unfortunately, the data was not collected at the time of the survey. Therefore, one can only interpret the spillover effects on the control group as a simple average of the spillover effects over their treated relatives in all different waves.

3.5. Results

This section presents results from estimating equation 3.1 above. Additionally, alternative specifications are examined, along with tests for alternative mechanisms that might underlie these findings. One caveat to keep in mind is that by the midline, only waves 1-4 women were treated, and those women assigned to receive treatment at wave 5 were not treated until after midline data collection. However, the data on the waves of the treated relatives are not available for analysis. Therefore, the spillover effects from having BOMA relatives could be understood as an average spillover effect across relatives who were treated for different periods. As discussed in the last chapter and in the next chapter, the treatment duration plays an important role in the total business assets of treated women and should be considered in later works if possible.

3.5.1. Material Wellbeing Outcome Variables. In Table 3.3, columns under "All" show the results from the entire control group. I show the regression results on three main outcomes for respondents' material wellbeing. I find no statistically significant spillover effects on household reported cash earnings.

For respondents who have at least two relatives treated by BOMA, compared to their counterparts who had no BOMA relative, on average, there is an increase in their total business assets (PPP$ 49.4, s.d. = PPP$ 23)[5]. This represents approximately a 56.9% increase compared to the control group who have no BOMA relatives at the endline. Furthermore, it accounts for 45% of a simple intent-to-treat (ITT) effects of REAP estimated averaging across waves 1-5 women by endline (PPP$ 109.2, s.d. = PPP$ 16)[6]. In other words, having two or more relatives treated by BOMA for a control woman would result in an increase in her total business assets by the endline, comparable to a 45% increase if the women were enrolled in BOMA.

[5]PPP$ (Purchasing Power Parity) values were derived using the PPP conversion factor, private consumption (LCU per international $) from World Development Indicators, specifically 42.54 for 2018 and 45.79 for 2020 and 46.41 for 2022. We convert all monetary figures from local currency to USD PPP, at the year of the program's inception for cost data, and the year of the relevant survey for our results data. We then convert from USD PPP for that year to 2018 USD by multiplying by the ratio of the 2018 U.S. Consumer Price Index (CPI) to the U.S. CPI for the year in question. The U.S. CPIs used for 2018, 2020 and 2022 are, respectively, 115.157, 118.69 and 124.3.

[6]Estimated using a simple regression with a dummy treatment indicator and an indicator for the vulnerable group, with manyatta and survey rounds fixed effects at the endline.

The spillover impacts on amount deposited to savings are large and positive on women who have one relative treated by BOMA. The increase in savings is 62.3% higher compared to women who were not connected with any BOMA treated relative. In comparison to the average intent-to-treat (ITT) effects on waves 1-5 women (PPP$ 41.2, s.d = PPP$ 5.4), the spillover effects on non-treated women account for 37.5% of those women who were assigned to receive the treatment by the endline. However, there is no statistically significant difference between the coefficients for one relative and two relatives, as indicated by the p-value of the difference between δ_1 and δ_2.

Overall, for women who have one relative treated by BOMA but were not assigned to treatment themselves, the increase in savings could be interpreted as a temporary decrease in consumption since there is no statistically significant increase in earnings. However, for control women who have two or more relatives treated by BOMA, there is an increase in total business assets, indicating that they are starting to accumulate business assets.

One caveat to consider is that the reported cash earnings are collected for the entire household rather than specifically for the woman respondent. To address this, I further divide the control group into two subgroups: those respondents who identified themselves as the female household head and those who reported the household head as male. Table 3.3 provides the regression results for both of these subgroups.

Upon examining the results within the two subgroups, it appears that the spillover effects are primarily observed among respondents who live in a household with male head. For women who identified themselves as the household heads, I did not observe a significant increase in any of the three outcomes.[7] However, I find a moderate increase in earnings for households with a male head, particularly if they had at least two BOMA relatives by endline. Additionally, these households experienced a significant increase in their total business assets and the amount deposited to savings. For households with a male head, the increase in savings with one BOMA relative was also significantly substantial (PPP$ 21.4), with a saving amount nearly 75% higher compared to households with a male head but no BOMA relative. This increase in savings represents approximately 52% of the simple average treatment effects estimated across the five waves for women mentioned earlier.

3.5.2. Spillover effects on savings through membership in savings groups. One possible hypothesis behind the increase in savings is that as their relatives start saving groups with their BOMA partners, those non-treated women could also become members of savings groups or other forms of self-help groups

[7] It is worth noting that in nearly all cases (99%), respondents who reported a female household head also identified themselves as the household head.

Table 3.3. Spillover impacts via kinship network

	Household Reported Earnings (PPP$)			Total Business Assets (PPP$)			Amount Deposited to Savings (PPP$)		
	All	Male Head	Female Head	All	Male Head	Female Head	All	Male Head	Female Head
One relative	54.862	66.647	27.406	11.721	15.814	28.644	15.453**	21.370**	5.465
	(36.975)	(52.412)	(45.190)	(24.062)	(38.306)	(22.984)	(6.830)	(10.178)	(5.367)
Two+ relatives	45.171	93.295*	-16.201	49.401**	84.900**	3.026	7.185	9.361*	12.271
	(35.660)	(52.392)	(46.029)	(23.729)	(38.841)	(17.392)	(4.522)	(5.157)	(9.489)
p-value, $\delta_1 = \delta_2$	0.79	0.63	0.45	0.28	0.23	0.31	0.25	0.25	0.47
Control mean	820.63	832.81	794.40	86.81	88.95	82.20	24.81	28.41	17.06
Survey-round FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Manyatta FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.12	0.11	0.10	0.03	0.04	0.03	0.04	0.04	0.06
Observations	3,579	2,414	1,165	3,579	2,414	1,165	3,579	2,414	1,165

Regressions estimates from equation 3.1. Household reported cash earnings were calculated by summing up the reported cash income from various sources such as sales of livestock, livestock products, crops, casual labor, salaried employment, and income from businesses, dukas, and petty trading for each household. Earnings are winsorized at 5% to avoid extreme impacts from outliers. Additionally, assets were computed by summing up the cash, stocks, assets, savings, and credits associated with each individual business owned by the respondents. Amount of savings include savings with bank, mpesa, savings groups and cash savings, does not include savings with businesses. The covariates included in the analysis were an indicator variable for a female household head, an indicator variable for whether the respondent lives with her husband, the household head's years of schooling, the age of the respondent, and household size. Sharpened q-values for estimated coefficients reported in square brackets. Control mean represents the average levels of the women who had no BOMA relatives by the endline. Standard errors were clustered at the manyatta level. *** p<0.01, ** p<0.05, * p<0.1.

among women. Anecdotal evidence suggests that BOMA saving groups might be limited to BOMA women only. Therefore, it is more likely that treated women initiate other social groups with their non-treated relatives, or the non-treated relatives form their own social groups.

In fact, Table 3.4 provides evidence suggesting that compared to the no BOMA relative group, control women who had one relative have a higher probability (2% more likely to have savings with a savings group by the endline, compared to 3% among the no BOMA relative groups). Moreover, they are more likely (7%, s.d. = 0.02) to become members of a social group. Social groups include savings groups, cooperatives, self-help groups, or groups related to water or pasture. Women reported participating in collective activities besides regular meetings with those social groups.[8]

3.5.3. Additional Outcome Variables. In Table 3.5, I discover evidence of spillover effects on the control group in terms of their household dietary diversity and self-reported living standard. Across the entire sample of the control group, I observe a statistically significant increase in household dietary diversity when the respondent has two or more relatives treated by BOMA. Specifically, compared to control group

[8]These collective activities include hiring labor, purchasing agricultural or livestock inputs, business inputs or stock, purchasing or negotiating transportation for goods or animals, raising funds for the group through business or social activities, and providing assistance to those in need collectively.

Table 3.4. Spillover effects on saving groups membership

	Has any forms of savings			Has savings with savings group			Membership with social group		
	All	Male Head	Female Head	All	Male Head	Female Head	All	Male Head	Female Head
One relative	0.054***	0.079***	0.008	0.020**	0.027**	-0.004	0.070***	0.088***	0.007
	(0.019)	(0.025)	(0.038)	(0.010)	(0.012)	(0.020)	(0.018)	(0.021)	(0.036)
Two+ relatives	0.034	0.055**	0.020	-0.005	0.010	-0.032	0.047***	0.089***	-0.042
	(0.023)	(0.028)	(0.040)	(0.012)	(0.015)	(0.021)	(0.016)	(0.021)	(0.032)
p-value, $\delta_1 = \delta_2$	0.40	0.47	0.76	0.04	0.29	0.13	0.28	0.96	0.15
Control mean	0.21	0.20	0.23	0.03	0.04	0.02	0.10	0.11	0.08
Survey-Round FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Manyatta FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.02	0.02	0.04	0.03	0.02	0.03	0.04	0.05	0.03
Observations	3,579	2,414	1,165	3,578	2,414	1,164	3,578	2,413	1,165

Regressions estimates from equation 3.1. Any forms of savings is an indicator suggests that the woman has savings with bank, businesses, cash, savings group or mpesa. Has savings with savings group is 1 if the respondent suggested that she had any savings with savings group. Membership with social group is the answer to the question "Are you a member of a group, whether it be a savings group, a cooperative, self-help group or group related to water or pasture?" All the outcome variables are dummy variables range from 0 to 1. Control mean represents the no BOMA relative group mean at the endline. Standard errors are clustered at the manyatta level.

respondents without any BOMA relatives, women with two or more BOMA relatives experience a 0.11 standard deviation increase in household dietary diversity. These spillover effects are even larger than the simple average treatment effect of BOMA estimated by averaging across the five waves (0.08 SDs with s.e. = 0.04). The same trend applies to the self-reported living standard, where the simple ITT averaged across five waves estimated by endline is 0.13 SDs with s.e. = 0.04.

It is important to note that there is a statistically significant difference between the coefficients for having one relative and having two or more relatives, as indicated by the p-value of the difference between δ_1 and δ_2. This suggests the possibility of a threshold model in the spillover effects, where it may be necessary to have at least two relatives to confirm the information and facilitate aggregation (Beaman and Dillon, 2018).

My findings on these non-economic wellbeing outcomes indicate that the spillover effects primarily arise from the subgroup of households where the respondents did not identify themselves as the household head at the baseline. This observation aligns with the results obtained for the material outcomes.

3.5.4. Non-immediate relatives versus immediate relatives. I examine whether having immediate and non-immediate relatives (referred to as first-order and second-order relatives above) would have different spillover effects on the material and other indicators discussed above. In Appendix C, Table C.1 - Table C.8, I present results estimated using the same specification as in 3.1, but utilizing the count of treated immediate relatives and non-immediate relatives separately for non-treated women. I observe a significant

Table 3.5. Spillover impacts via kinship network: other indicators

	Household Dietary Diversity Scores			Self-reported current living standard			Any other woman shared knowledge with me		
	All	Male Head	Female Head	All	Male Head	Female Head	All	Male Head	Female Head
One relative	-0.006	0.044	-0.098	0.039	0.027	0.042	0.129**	0.152**	0.025
	(0.045)	(0.057)	(0.092)	(0.053)	(0.067)	(0.097)	(0.054)	(0.070)	(0.087)
Two+ relatives	0.106**	0.161***	0.022	0.162***	0.169**	0.093	0.098*	0.097	0.144
	(0.041)	(0.049)	(0.062)	(0.052)	(0.068)	(0.095)	(0.059)	(0.070)	(0.098)
p-value, $\delta_1 = \delta_2$	0.02	0.03	0.28	0.05	0.10	0.59	0.64	0.51	0.27
Control mean	0.05	0.05	0.05	0.15	0.26	-0.10	0.02	0.04	-0.04
Survey-round FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Manyatta FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.05	0.04	0.06	0.07	0.05	0.05	0.02	0.02	0.01
Observations	3,579	2,414	1,165	3,579	2,414	1,165	3,570	2,407	1,163

Regressions estimates from equation 3.1. Any other women share business knowledge represent the answer to the question: Did any other woman share knowledge with you about how to run a business? All the outcome variables are standardized to a z-score by subtracting the control group with zero BOMA relatives at the corresponding survey round and dividing by the control group with zero BOMA relatives' standard deviation at the survey round. Standard errors are clustered at the manyatta level.

increase in the amount deposited into savings, Household Dietary Diversity Scores, self-reported living standards, social group membership, and sharing of business knowledge through connections with immediate BOMA relatives. However, the spillover effects on total business assets are observed through non-immediate relatives. This suggests that the accumulation of total business assets for the non-treated women might occur with the help of males in their kinship, as most of the non-immediate relatives are connected either through their husbands or their brothers.

3.5.5. Learning versus Alternate Mechanisms. The results support the notion that the REAP program has successfully disseminated business management practices among local women. Table 3.5 provides evidence of spillover impacts in the form of increased knowledge sharing on how to run a business. The results indicate that control women with one or more BOMA relatives received more information on business management compared to control women without any BOMA relatives. However, it is worth noting that for female-headed households (as shown in the last two columns of Table 3.5), there are no statistically significant impacts on the knowledge-sharing mechanism, even if they had relatives treated by BOMA. This could suggest that female-headed households are isolated from their kinship network, even if it represents a large proportion of the female head's network structure. On the other hand, it could also suggest that these households lack complementary investment sources, even if they are highly integrated into the kinship network but still cannot take advantage of the spillover effects. Therefore, they receive less information on business management.

Table 3.6. Spillover impacts via relatives of REAP: transfer received from other individuals

	Amount of transfer received (KES)		
	All	Male Head	Female Head
One relative treated by BOMA	694.212	1636.303	-1265.395
	(1041.070)	(1573.194)	(959.112)
Two or more relatives treated by BOMA	-285.354	507.086	-1681.299
	(775.769)	(1190.045)	(1044.959)
Control mean	6198.36	5809.60	7035.68
Survey Round FE	YES	YES	YES
Manyatta FE	YES	YES	YES
Covariates	YES	YES	YES
R-squared	0.01	0.01	0.03
Observations	3579.00	2414.00	1165.00

Regressions estimates from equation 3.1. Household reported the value of any gifts, transfers from other households or individuals (including remittances from non-household family members who are working or living away from home) in the past 12 months. The covariates included in the regression were an indicator variable for a female household head, an indicator variable for whether the respondent lives with her husband, the household head's years of schooling, the age of the respondent, and household size. Control mean represents the average levels of the women who had no BOMA relatives by the endline. Standard errors were clustered at the manyatta level. *** p<0.01, ** p<0.05, * p<0.1.

Another potential channel through which spillovers may occur is resource transfers from treated BOMA relatives to their non-treated relatives. However, as shown in Table 3.6, I find that transfers received from other individuals are not significantly different between control women who had no BOMA relatives and those who did in the 12 months period before the survey.

3.5.6. Comparison with spillover effects measured at the midline at the community saturation levels. The spillover effects measured at the midline using community saturation levels and BOMA relatives appear to be similar in magnitude. At the midline, I find that the spillover effects are more prominent in communities with higher levels of saturation (weighted by duration of enrollment) in terms of total business assets. This suggests that as the number of treated women and their duration of treatment increase within a community, the within-cluster spillover effects on total business assets for the control group become more significant by the midline. In fact, as detailed in chapter two, we observe that the spillover effects on total business assets for untreated women are approximately 20% of the impact seen in the treated women within a community with a median saturation level.

In Table 3.7, I estimate a specification that includes interactions between survey round indicators and two groups of BOMA relatives (indicators for one relative and at least two BOMA relatives). Controlling for manyatta fixed effects and changes in saturation rates over time, these results also capture the within-cluster spillover effects but through connection with BOMA relatives. Compared to control women with no relatives treated by BOMA, the estimated spillover effects on total business assets at the midline amount to PPP$ 54, which is approximately 29% of the simple average ITT estimated by pooling waves 1-4 women together. This finding is comparable, but slightly higher, than the spillover effects observed in chapter two, where we found that the spillover effects on total business assets, using community-level saturation rates, were about 20% of the treatment effects on BOMA women.

Moreover, at the endline, the spillover effects through one BOMA relative increased to PPP$ 73.6, corresponding to approximately 30% of the simple average ITT estimated by pooling women from the five waves. As discussed in the next chapter, we do not observe significant spillover effects on total business assets through community-level saturation rates at the endline, using a similar specification as in chapter two. This is reasonable if, in the long run, non-treated women in the community would adapt to random interactions with BOMA-treated women who were not closely connected to them, and the spillover effects might eventually fade. However, what we observe here is that the effects within the kinship network stayed and increased over time.

Furthermore, I observe positive spillover effects on savings through the kinship network at the midline, which were not evident at the community level. Having one BOMA relative by the midline resulted in a PPP$ 23 increase in savings, which is approximately 49% of the simple ITT by the midline on savings when pooling women from waves 1-4 together. However, these spillover effects on savings were no longer statistically significant by the endline.

By the midline, we also observed spillover effects on within-cluster control women in terms of whether other women shared knowledge on how to run a business with them. The observed spillover impacts amounted to approximately 14.25% of the treatment effects at the midline, as shown in the table in the last chapter. However, as indicated in Table 3.7, no spillover effects were found through the kinship network by the midline.

On the other hand, significant and substantial spillover effects through the kinship network were observed for women with one or two or more relatives treated by BOMA by the endline. In fact, by the endline, compared to non-treated women with zero BOMA relatives, women with one BOMA relative were 20% more likely to receive knowledge on how to run a business. The likelihood increased to 46% for women who had two or more BOMA relatives.

Table 3.7. Spillover impacts with more flexible specifications

	Household Reported Earnings (PPP$)	Total Business Assets (PPP$)	Amount Deposited to Savings (PPP$)	Any other women shared knowledged with me
Midline	-136.846	-81.766	-7.887	-0.042
	(85.536)	(75.582)	(11.101)	(0.111)
Endline	125.187	-96.860	-0.118	-0.047
	(117.029)	(101.535)	(16.098)	(0.152)
One relative	41.076	-23.484	4.906	0.074
	(45.156)	(21.972)	(9.243)	(0.059)
One relative $\times Midline$	-31.398	54.449*	22.842**	0.001
	(67.116)	(28.000)	(9.598)	(0.084)
One relative $\times Endline$	85.703	73.614**	14.962	0.204*
	(87.558)	(32.551)	(17.968)	(0.112)
Two+ relatives	24.880	31.403	2.310	-0.012
	(52.299)	(26.109)	(6.252)	(0.071)
Two+ relatives $\times Midline$	-93.398	-8.639	2.957	-0.033
	(62.665)	(35.350)	(8.434)	(0.097)
Two+ relatives $\times Endline$	180.953	75.975	14.658	0.457***
	(116.410)	(45.796)	(9.619)	(0.114)
Manyatta-level Saturation	-688.425*	371.457	17.725	0.208
	(385.085)	(325.162)	(54.325)	(0.466)
R-squared	0.12	0.04	0.04	0.02
Observations	3,579	3,579	3,579	3,570
Control mean	813.84	95.45	24.08	0.07

Data involve all three rounds from 2018, 2020 and 2022. Dependent variables are the same as in table 3.3. Regressions are based on modified version of equation (3.1) with two survey round indicators interacted with the number of BOMA relative indicator groups. Same set of covariates and manyatta fixed effects are included as in table 3.3. Standard errors are clustered at the manyatta level.

3.6. Conclusion

The graduation approach, which includes the transfer of related productive assets, as well as life skills and training around a selected livelihood, has been found successful in improving the lives of the poorest in both the short term and the long term, across various geographical locations (Bandiera et al., 2017; Banerjee et al., 2015). Positive improvements in material well-being (assets, savings, and consumption levels) and self-reported psychological well-being have been documented in different variations of graduation programs (Banerjee et al., 2015; Blattman et al., 2016; Gobin, Santos, and Toth, 2017).Given the evidence of success, governments and development agencies have launched plans to replicate the graduation approach in different developing contexts, potentially helping millions of people graduate from extreme poverty.[9]

However, the life-skill coaching and business management training components of these graduation programs are, on average, very costly. They require regular and frequent visits from supervising staff who provide quality support to the treated households. The training and staff salaries account for approximately

[9]A new initiative plans to scale up the Graduation Approach from 2020 to 2025. https://alleviate-poverty.org/

60%-80% of the total supervision costs. If positive spillover effects exist on non-treated individuals in local communities, this could further enhance the cost-effectiveness of these programs. Therefore, quantifying the spillover effects before scaling up these programs would be beneficial.

Moreover, spillover effects at the village level through random interactions between acquaintances may have different dynamics compared to spillover effects through those within local women's female kinship network. It would be worth investigating the dynamics of spillover effects and the role of different network structures in facilitating information diffusion and resource sharing within local societies.

To explore these ideas, I collected information on the number of treated relatives through individual-level kinship networks in a "graduation program" in Kenya. By exploiting the random exposure in control households' kinship networks introduced by the randomly assigned treatment status under the REAP program, I first demonstrate strong evidence of positive spillover effects through the kinship network of the program on control women, as they started to accumulate business assets and savings.

Furthermore, these spillover effects are substantial and significant for women living in male-headed households, but no effect is found for female-headed control households. The local female kinship structure leads to strikingly different consequences of the program, suggesting the importance of recognizing that certain economically vulnerable groups of women, particularly those single-handedly maintaining their households, may require additional support or special consideration during the targeting process. These women may not benefit from the spillover effects of the program and could face greater challenges in accessing resources and opportunities.

These findings suggest that spillover effects occurring through random interactions with any person in the community and those conversations within the female kinship network may operate through different channels. The delayed spillover effects observed within the kinship network regarding knowledge sharing are intriguing. They may indicate that community-level random interactions are more efficient in diffusing information. "Weak ties" often serve as 'bridges' between different groups of people, providing a diversity of information. In contrast, female kinship ties may lack such diversity. Nevertheless, these kinship ties may assume a more critical role at a later stage when women begin to digest new information, requiring in-depth and sustained support and engagement in discussions with their trusted contacts.

Lastly, I compared the spillover effects through the kinship network with what was estimated in the chapter two using community-level saturation variation. At the midline, interesting differences emerged in how the diffusion process of business knowledge differed between random interactions at the village level and female kinship ties. As discussed in the next chapter, we did not observe significant spillover effects using community-level saturation by the endline. However, we did observe increasing spillover effects by the endline on total business assets for non-treated women through the kinship network. This highlights the importance of social organization in better understanding and predicting the impacts of poverty-alleviating programs across different contexts and patterns of economic exchange.

APPENDIX B

Marriage Network

B.1. Marriage Network Data

During midline data collection, a network module was added asking each respondent in the study to list her closest psychological friends and to provide some basic information about these friends. Up to six psychologically close[1] friends are elicited together with five additional types of interactions for a given survey respondent: (1) who visit her house and whose house(s) she visits, (2) from whom she borrows or to whom she lends food if in urgent need (e.g. unga, maize or rice) (3) with whom she discusses on financial issues (4) whom she welcomed when the person moved to this village because of marriage and (5) who welcomed her when she moved to this village because of marriage. Answers to these five question provides a broad description of the interactions across households.[2]

When piloting the network survey in the field, I discovered that girls typically get married at a young age and that when they do get married, women from their husband's family would take on the role of their mother or sister when they first move to live with their husband's family. In local culture, an elder woman from the husband's family, such as the husband's mother or the husband's brother's wife, would take the responsibility of taking care of the newlywed girl, looking after her daily routines, giving her directions around the community, and helping her fetch water and food. As a result, the newlywed girl would develop a close relationship with this woman, which I will refer to as the "marriage network" for convenience. Some women learned in great detail about their "marriage network" friends' BOMA businesses, such as their friend's business type and enrollment duration with REAP.[3]

I intended to focus on the local marriage network for two reasons. Firstly, this type of social connection existed long before the introduction of the REAP program in the local communities. As most of the women in our sample have been married for more than two years, this connection is unlikely to be affected or altered by

[1]The question we used to elicit the psychological friendship is "Who would you discuss with about your personal issues (including problems about your private life such as sharing a personal problem, emotional feelings, your plan for the future, your aspirations etc.?"

[2]Those mentioned friends' names then will be matched to the study sample and a link exists between households if any member of a household is linked to any other member of another household.

[3]The marriage network would include both the woman's friends who welcomed her when she got married and moved to her husband's manyatta, as well as those whom she welcomed when they joined her family.

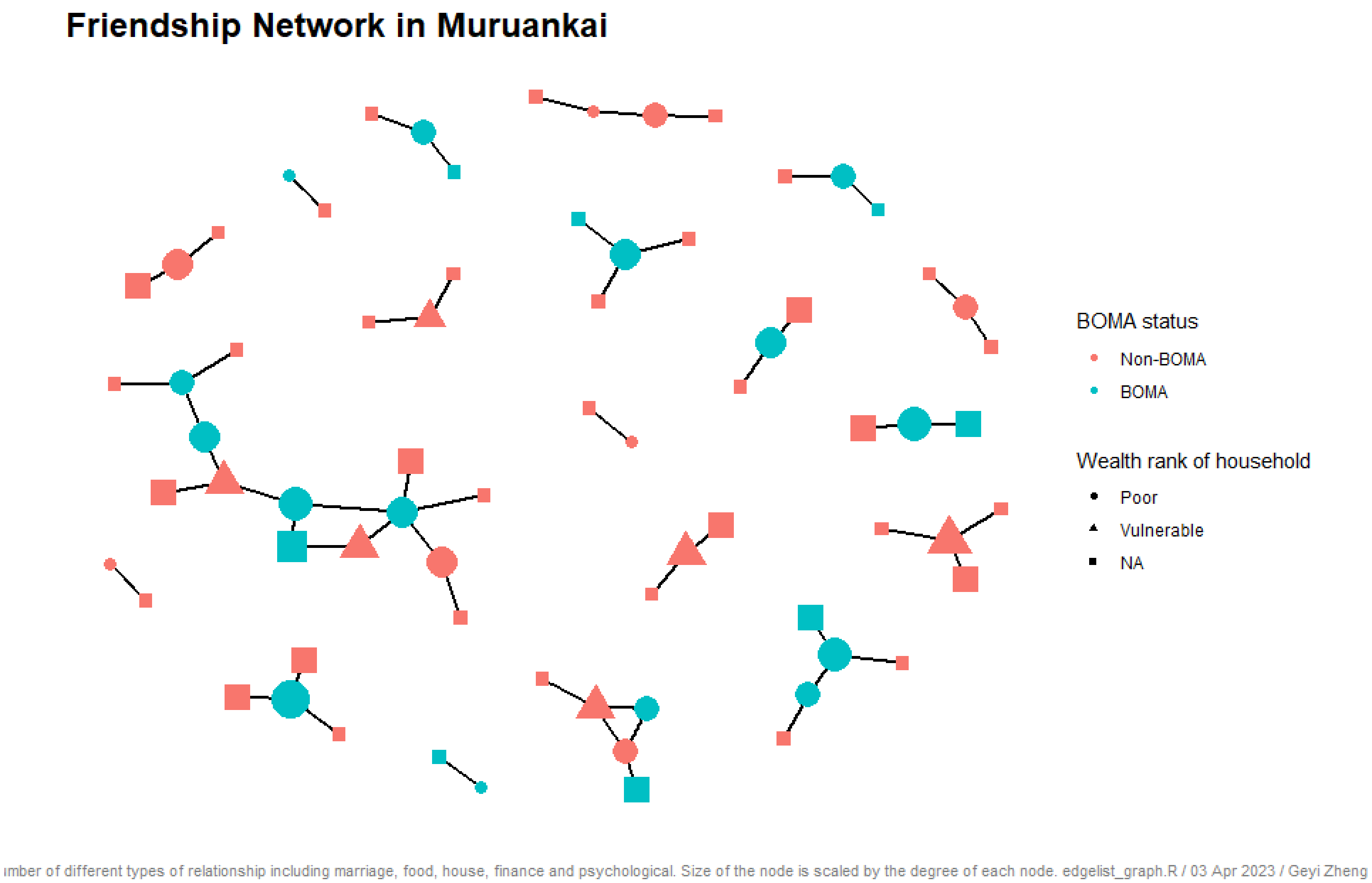


FIGURE B.1. Close Psychological and Marriage Network Friends

the REAP program. Therefore, the network structure is exogenous to the treatment assignment. Secondly, the woman who takes on the role of a mother or sister in the husband's family usually establishes a deep and close psychological relationship with the newlywed girl, similar to that of a biological mother. Hence, I hypothesize that spillover effects (especially those on psychological indicators) could mediate through this relationship.

Figure B.1 shows the graphic representation of the marriage network and psychological friends in one of the local communities. As could be seen here, the marriage network is sparse in the sense that there is not a very large connected component in the graph. This together with other studies in the same literature has the limitation that the network is only partially observed. In our case, we miss all the potential connections from the male side as we only observe network connections from female participants.

B.1.1. Network level saturation measure. The idea was to develop a similar enrollment duration-weighted measure saturation rates on marriage network and compare the regression results with those using the community level saturation rates.

$$S_n^t = \frac{\sum_{w=1}^{t} n_n^w \times d_w^t}{D_t \times N_n} \forall t = mid, end \tag{B.1}$$

where d_w^{mid} is 24 months for $w = 1$, 18 for $w = 2$, 12 for $w = 3$ and 6 for $w = 4$. D_{mid} is 24 to normalize the saturation measure.[4] n_n^w is the number of women who were assigned to enroll in wave w who were reported to be connected with the respondent through the marriage relationship. N_n is the total number of women connected with the respondent through the marriage relationship.

The collected data on marriage and psychological network has very low number of friends treated. In other words, there is not enough variation in the independent variable to identify the spillover impacts. About 84% of the women respondents reported zero friends treated by BOMA by midline and about 81% still had zero marriage friends treated by the endline.

The lessons learned from the collection of the network data is that the matching of the names were not very successful even if we provided the roster of names for the respondents among different communities. Photos would be much more helpful but also more costly. This results in that a lot of women reported marriage network friends who are not within our sample. If I then further restrict the sample to the individuals whose reported marriage friends who were successfully matched to women in our sample. This shrinks the sample down to 329 individuals with three rounds of data. That reduces the proportion of women who reported zero treated friends to 70%, which is close to 30% treatment saturation in average communities.

[4] For endline, d_w^{end} is 48 months for $w = 1$, 42 for $w = 2$, 36 for $w = 3$, 28 for $w = 4$ and 22 for $w = 5$ and D_{end} is 48.

APPENDIX C

Immediate vs. Non-immediate relatives

TABLE C.1. Material outcomes: spillover impacts via kinship network - had some immediate relatives

	Household Reported Earnings (PPP)			Total Business Assets (PPP)			Amount Deposited to Savings (PPP)		
	All	Male Head	Female Head	All	Male Head	Female Head	All	Male Head	Female Head
Have immediate BOMA relatives	53.762	89.667*	-1.184	36.844	65.519	-25.041	13.112**	16.402**	8.886
	(38.788)	(46.069)	(48.180)	(30.688)	(47.265)	(20.271)	(5.757)	(7.567)	(8.075)
Manyatta-level saturation	-637.085	-558.126	-924.537*	406.037	74.078	1116.541	25.400	32.830	26.930
	(384.701)	(439.513)	(523.542)	(328.967)	(182.402)	(898.380)	(55.016)	(64.758)	(96.688)
Control mean	820.63	832.81	794.40	86.81	88.95	82.20	24.81	28.41	17.06
Survey Round FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Manyatta FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.12	0.11	0.10	0.03	0.04	0.03	0.04	0.04	0.05
Observations	3579.00	2414.00	1165.00	3579.00	2414.00	1165.00	3579.00	2414.00	1165.00

Regressions estimates from equation 3.1. Household reported cash earnings were calculated by summing up the reported cash income from various sources such as sales of livestock, livestock products, crops, casual labor, salaried employment, and income from businesses, dukas, and petty trading for each household. Earnings are winsorized at 5% to avoid extreme impacts from outliers. Additionally, assets were computed by summing up the cash, stocks, assets, savings, and credits associated with each individual business owned by the respondents. Amount of savings include savings with bank, mpesa, savings groups and cash savings. The covariates included in the analysis were an indicator variable for a female household head, an indicator variable for whether the respondent lives with her husband, the household head's years of schooling, the age of the respondent, and household size. Sharpened q-values for estimated coefficients reported in square brackets. Control mean represents the average levels of the women who had no BOMA relatives by the endline. Standard errors were clustered at the manyatta level. *** p<0.01, ** p<0.05, * p<0.1.

Table C.2. Spillover impacts via kinship network Material - had some non-immediate relatives

	Household Reported Earnings (PPP)			Total Business Assets (PPP)			Amount Deposited to Savings (PPP)		
	All	Male Head	Female Head	All	Male Head	Female Head	All	Male Head	Female Head
Have nonimmediate BOMA relatives	31.363	51.572	2.699	25.458*	43.604*	24.459	3.776	4.937	9.478
	(30.655)	(38.653)	(52.516)	(15.060)	(22.508)	(18.635)	(3.933)	(5.255)	(7.161)
Manyatta-level saturation	-645.711*	-581.477	-924.128*	399.863	56.023	1120.812	23.553	29.312	27.844
	(384.955)	(441.803)	(524.461)	(330.836)	(187.054)	(901.166)	(54.990)	(64.164)	(97.028)
Control mean	820.63	832.81	794.40	86.81	88.95	82.20	24.81	28.41	17.06
Survey Round FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Manyatta FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.12	0.11	0.10	0.03	0.04	0.03	0.04	0.04	0.06
Observations	3579.00	2414.00	1165.00	3579.00	2414.00	1165.00	3579.00	2414.00	1165.00

Regressions estimates from equation 3.1. Household reported cash earnings were calculated by summing up the reported cash income from various sources such as sales of livestock, livestock products, crops, casual labor, salaried employment, and income from businesses, dukas, and petty trading for each household. Earnings are winsorized at 5% to avoid extreme impacts from outliers. Additionally, assets were computed by summing up the cash, stocks, assets, savings, and credits associated with each individual business owned by the respondents. Amount of savings include savings with bank, mpesa, savings groups and cash savings. The covariates included in the analysis were an indicator variable for a female household head, an indicator variable for whether the respondent lives with her husband, the household head's years of schooling, the age of the respondent, and household size. Sharpened q-values for estimated coefficients reported in square brackets. Control mean represents the average levels of the women who had no BOMA relatives by the endline. Standard errors were clustered at the manyatta level. *** p<0.01, ** p<0.05, * p<0.1.

Table C.3. Spillover impacts via kinship network: other indicators - immediate relatives

	Household Dietary Diversity Scores			Self-reported current living standard			Any other woman shared knowledge with me		
	All	Male Head	Female Head	All	Male Head	Female Head	All	Male Head	Female Head
Have immediate BOMA relatives	0.073*	0.126**	-0.018	0.107**	0.121**	0.049	0.164***	0.155**	0.158*
	(0.044)	(0.054)	(0.062)	(0.045)	(0.052)	(0.095)	(0.052)	(0.061)	(0.095)
Manyatta-level saturation	3.035***	2.899***	3.458***	0.788	1.087*	0.358	0.345	0.635	-0.314
	(0.735)	(0.794)	(0.963)	(0.536)	(0.567)	(0.848)	(0.468)	(0.662)	(0.712)
Control mean	0.05	0.05	0.05	0.15	0.26	-0.10	0.02	0.04	-0.04
Survey Round FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Manyatta FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.05	0.04	0.06	0.07	0.05	0.05	0.02	0.02	0.01
Observations	3579.00	2414.00	1165.00	3579.00	2414.00	1165.00	3570.00	2407.00	1163.00

Regressions estimates from equation 3.1. Any other women share business knowledge represent the answer to the question: Did any other woman share knowledge with you about how to run a business? All the outcome variables are standardized to a z-score by subtracting the control group with zero BOMA relatives at the corresponding survey round and dividing by the control group with zero BOMA relatives' standard deviation at the survey round. Standard errors are clustered at the manyatta level.

Table C.4. Spillover impacts via kinship network: other indicators non-immediate relatives

	Household Dietary Diversity Scores			Self-reported current living standard			Any other woman shared knowledge with me		
	All	Male Head	Female Head	All	Male Head	Female Head	All	Male Head	Female Head
Have non-immediate BOMA treated relatives	0.034	0.062	-0.022	0.095**	0.078	0.096	0.032	0.034	0.052
	(0.034)	(0.040)	(0.073)	(0.042)	(0.056)	(0.083)	(0.045)	(0.057)	(0.072)
Manyatta-level Saturation	9.386***	8.567***	10.648***	0.935	1.934	-0.439	0.808	2.579	-2.489
	(2.079)	(2.190)	(2.470)	(1.571)	(1.768)	(2.194)	(1.176)	(1.747)	(1.841)
Control mean	0.05	0.05	0.05	0.15	0.26	-0.10	0.02	0.04	-0.04
Survey Round FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Manyatta FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.05	0.04	0.07	0.07	0.05	0.05	0.02	0.02	0.01
Observations	3579.00	2414.00	1165.00	3579.00	2414.00	1165.00	3570.00	2407.00	1163.00

Regressions estimates from equation 3.1. Any other women share business knowledge represent the answer to the question: Did any other woman share knowledge with you about how to run a business? All the outcome variables are standardized to a z-score by subtracting the control group with zero BOMA relatives at the corresponding survey round and dividing by the control group with zero BOMA relatives' standard deviation at the survey round. Standard errors are clustered at the manyatta level.

Table C.5. Spillover impacts via kinship network: saving group indicators number of immediate relatives

	Has any forms of savings			Has savings with savings group			Membership with social group		
	All	Male Head	Female Head	All	Male Head	Female Head	All	Male Head	Female Head
Have immediate BOMA relatives	0.019	0.041*	-0.025	0.005	0.018	-0.022	0.044**	0.073***	-0.024
	(0.021)	(0.024)	(0.037)	(0.011)	(0.015)	(0.016)	(0.018)	(0.023)	(0.029)
Manyatta-level Saturation	-0.085	0.120	-0.413	0.063	0.113	-0.021	-0.012	-0.129	0.177
	(0.213)	(0.261)	(0.365)	(0.089)	(0.117)	(0.172)	(0.201)	(0.209)	(0.294)
Control mean	0.21	0.20	0.23	0.03	0.04	0.02	0.10	0.11	0.08
Survey Round FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Manyatta FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.02	0.01	0.04	0.02	0.02	0.03	0.03	0.04	0.03
Observations	3579.00	2414.00	1165.00	3578.00	2414.00	1164.00	3578.00	2413.00	1165.00

Regressions estimates from equation 3.1. Any other women share business knowledge represent the answer to the question: Did any other woman share knowledge with you about how to run a business? All the outcome variables are standardized to a z-score by subtracting the control group with zero BOMA relatives at the corresponding survey round and dividing by the control group with zero BOMA relatives' standard deviation at the survey round. Standard errors are clustered at the manyatta level.

Table C.6. Spillover impacts via kinship network: saving group indicators number of non-immediate relatives

	Has any forms of savings			Has savings with savings group			Membership with social group		
	All	Male Head	Female Head	All	Male Head	Female Head	All	Male Head	Female Head
Have nonimmediate BOMA relatives	0.036*	0.044**	0.059	-0.005	0.002	-0.014	0.038***	0.068***	-0.025
	(0.020)	(0.021)	(0.038)	(0.010)	(0.010)	(0.018)	(0.014)	(0.015)	(0.028)
Manyatta-level Saturation	-0.089	0.106	-0.404	0.063	0.110	-0.022	-0.020	-0.153	0.174
	(0.214)	(0.260)	(0.369)	(0.089)	(0.116)	(0.172)	(0.201)	(0.209)	(0.294)
Control mean	0.21	0.20	0.23	0.03	0.04	0.02	0.10	0.11	0.08
Survey Round FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Manyatta FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Covariates	YES	YES	YES	YES	YES	YES	YES	YES	YES
R-squared	0.02	0.01	0.05	0.02	0.02	0.02	0.03	0.04	0.03
Observations	3579.00	2414.00	1165.00	3578.00	2414.00	1164.00	3578.00	2413.00	1165.00

Regressions estimates from equation 3.1. Any other women share business knowledge represent the answer to the question: Did any other woman share knowledge with you about how to run a business? All the outcome variables are standardized to a z-score by subtracting the control group with zero BOMA relatives at the corresponding survey round and dividing by the control group with zero BOMA relatives' standard deviation at the survey round. Standard errors are clustered at the manyatta level.

Table C.7. Non-immediate relatives on Business Knowledge Sharing

	Business Training		
	All	Male Head	Female Head
One non-immediate BOMA relative	0.040	0.028	0.069
	(0.049)	(0.062)	(0.080)
Two+ non-immediate BOMA relatives	0.015	0.046	0.012
	(0.066)	(0.078)	(0.114)
p-value, $\delta_1 = \delta_2$	0.71	0.82	0.65
No BOMA relatives endline mean	0.02	0.04	-0.04
Wave FE	YES	YES	YES
Manyatta FE	YES	YES	YES
Covariates	YES	YES	YES
R-squared	0.02	0.02	0.01
Observations	3,570	2,407	1,163

Regressions estimates from equation 3.1. Household reported cash earnings were calculated by summing up the reported cash income from various sources such as sales of livestock, livestock products, crops, casual labor, salaried employment, and income from businesses, dukas, and petty trading for each household. Earnings are winsorized at 5% to avoid extreme impacts from outliers. Additionally, assets were computed by summing up the cash, stocks, assets, savings, and credits associated with each individual business owned by the respondents. Amount of savings include savings with bank, mpesa, savings groups and cash savings. The covariates included in the analysis were an indicator variable for a female household head, an indicator variable for whether the respondent lives with her husband, the household head's years of schooling, the age of the respondent, and household size. Sharpened q-values for estimated coefficients reported in square brackets. Standard errors were clustered at the manyatta level.

Table C.8. Immediate relatives on Business Knowledge Sharing

	Business Training		
	All	Male Head	Female Head
One immediate BOMA relative	0.185***	0.201***	0.097
	(0.052)	(0.067)	(0.100)
Two+ immediate BOMA relatives	0.087	-0.029	0.337*
	(0.092)	(0.091)	(0.176)
p-value, $\delta_1 = \delta_2$	0.26	0.02	0.20
No BOMA relatives endline mean	0.02	0.04	-0.04
Wave FE	YES	YES	YES
Manyatta FE	YES	YES	YES
Covariates	YES	YES	YES
R-squared	0.02	0.03	0.01
Observations	3,570	2,407	1,163

Regressions estimates from equation 3.1. Household reported cash earnings were calculated by summing up the reported cash income from various sources such as sales of livestock, livestock products, crops, casual labor, salaried employment, and income from businesses, dukas, and petty trading for each household. Earnings are winsorized at 5% to avoid extreme impacts from outliers. Additionally, assets were computed by summing up the cash, stocks, assets, savings, and credits associated with each individual business owned by the respondents. Amount of savings include savings with bank, mpesa, savings groups and cash savings. The covariates included in the analysis were an indicator variable for a female household head, an indicator variable for whether the respondent lives with her husband, the household head's years of schooling, the age of the respondent, and household size. Sharpened q-values for estimated coefficients reported in square brackets. Standard errors were clustered at the manyatta level.

CHAPTER 4

The Longer Term Impacts of Graduation Program in Pastrolists Regions

4.1. Introduction

In Chapter 2, we demonstrate that, once the spillover effects onto the non-treated women are taken into account, the REAP program has significant positive impacts on women's productive assets, family cash income, and financial savings. Two years after the program commenced, the program resulted in a remarkable increase of 333% in women's productive assets, a 26% increase in family cash income, and a substantial 405% increase in financial savings. Notably, among the beneficiaries, about 16.7% who suffered from severe depressive symptoms experienced a distinct outcome. These individuals not only lost around half of the transferred assets but also did not witness any income gain

With the endline data collected at the beginning of 2022, we aim to examine the medium to long-term effects of the graduation program in the pastoralist regions. At the time of the endline data collection, four years had passed since the first wave of women enrolled in the REAP program. There is a growing interest in understanding the medium to long-term impacts of this multi-faceted graduation program. The emerging literature has documented that substantial, time-limited transfers of assets, coupled with intensive training components, can have persistent effects in alleviating poverty. However, as discussed in Bouguen et al. (2019), there is limited evidence on the long-term impacts (at least four years after the program's commencement) of multifaceted graduation programs compared to other poverty alleviation programs involving cash transfers, such as unconditional cash transfer programs and entrepreneurial grant programs. Bandiera et al. (2017) demonstrated a similar pattern of positive impacts on per capita consumption, asset accumulation, savings, and loan activities together with income and revenues at the end of four years in Bangladesh compared with Banerjee et al. (2015), after three years from six countries. Similarly, Banerjee, Duflo, and Sharma (2021) presented evidence from a TUP program in West Bengal, India, showing that the treated group experienced higher per capita consumption and income compared to the control group, both seven and ten years after the initial asset transfer.

We gathered three rounds of data from household surveys conducted during a randomized control trial in the pastoralist regions of Northern Kenya. The trial was implemented by the BOMA Project NGO from the year 2018 to the year 2022. In our analysis, we initially demonstrate that conventionally measured average treatment effects (without taking into consideration the spillover effects) remain substantial four years into the program. Specifically, compared to the control group, women's holdings of productive assets and savings increased by 340% and 102%, respectively. Additionally, we observed a 9% increase in household income for the treated women, although this finding is not statistically significant. Unfortunately, household consumption was not measured, so we are unable to comment on that aspect.

In addition, the REAP program was rolled out in waves for practical reasons. The random allocation of women into different waves of receiving treatment provided us with an opportunity to adopt a continuous treatment analysis approach. This approach allowed us to trace out the trajectory of program impacts for those women through various stages of treatment duration. We find an inverse U-shape of the asset accumulation process, peaking somewhere between 2.5 and three years into the program, with the impact starting to decrease after that, although still remaining above the initial amount of asset transfer at four years into the program. Moreover, we documented a smoothly increasing trajectory for the treatment impacts on household income, reaching about 10% of the baseline income level for treated households. As for the treatment impacts on savings, they also peaked at around two to 2.5 years before starting to decrease but remained significant, amounting to about 215% of the baseline control level.

More interestingly, as we trace out the treatment impacts trajectory for the baseline depressed women and those with no baseline depressed symptoms, we find that the trajectory of those who experienced severe depression never really managed to accumulate productive assets that exceeded the amount initially transferred to them throughout the treatment duration. Nevertheless, we fail to reject the hypothesis that the trajectory of the treatment impacts is statistically significantly different for the baseline depressed and baseline non-depressed groups, primarily due to the wide confidence intervals of the estimates for the baseline depression group.

Lastly, we leverage the saturation design of the RCT, which allows us to define an exposure measure for beneficiaries of the BOMA graduation program. This exposure measure is defined as the probability that a random social interaction would be with a BOMA project beneficiary during the 48-month period between the baseline and endline, and it varies between 10% and 60%. Using this measure reveals statistically significant spillovers on both beneficiary and non-beneficiary women. For non-beneficiary women, the impacts imply asset accumulation at 22% of the rate observed for beneficiary women who had been in the program for four years, despite the fact that the former received no direct asset grant or other support from the BOMA program. Moreover, for the treated women, the direct impacts estimated considering spillover effects are statistically different from those estimated at the mean level saturation rates for both midline and endline on asset accumulation.

Described in Gobin, Santos, and Toth (2017), this program distinguishes itself from other ultra-poor programs in two significant ways: Firstly, REAP offers the initial transfer in cash rather than a physical asset. In contrast, other programs typically provide this initial transfer in the form of a physical asset, often a productive resource like livestock. Moreover, to discourage beneficiaries from quickly consuming the transferred asset, these other programs often supply consumption support in the form of cash for a duration of two years. REAP combines both aspects into a single initial cash transfer. Secondly, the program seeks to advance women's business groups and underscores their collaborative management of enterprises.

These aspects present potential challenges when estimating program treatment impacts in our case. Firstly, we examine the impact of this multi-faceted grant-based program over a critical period of time between the midline and endline, during which Kenya has faced a triple threat of floods, crop-eating locusts, and COVID-19. Especially with COVID-19, the complete closure of local markets limited opportunities for doing business and increased the cost of transporting goods. While we are discussing treatment effects compared to control groups throughout the paper, such exogenous shocks could easily undermine any treatment effects. The program is not designed to teach households how to flourish as subsistence households but rather promotes market-level activities to generate cash income. When faced with the complete closure of markets, such engagement may not be viable, leading beneficiaries to close down their businesses. Although cash transfers provide flexibility to the beneficiaries in the form that they want to invest, they might also be easy to split and consumed during difficult situations.

Secondly, the group enterprises could place additional social accountability around running the businesses, which may also introduce additional challenges in the analyses. We document some data issues in measuring the business assets for the jointly owned businesses. On one hand, we find that a significant part of the treated women at midline reported zero business assets as they do not consider the jointly owned businesses as their own. On the other hand, some women reported their own share of the business, while others reported the total business assets for the entire enterprises. We discuss these issues in more detail in Section 4.5 of this chapter and reflect on the possible impacts on the analyses.

The remainder of the chapter is organized as follows: Section 2 presents a literature review on the short, medium to long term treatment impacts of the graduation programs. Section 3 describes summary statistics on the main economic outcomes for the three rounds data. Section 4 estimates simple average treatment effects. Section 5 describes the continuous treatment analysis and the results. Section 6 estimates spillover effects on economic variables. Finally, Section 7 concludes by discussing the implications of these findings for designing more cost-effective graduation programs.

4.2. Literature Review

The multi-faceted "graduation" program, instrumentally developed by BRAC, a leading international nonprofit, starts by identifying the poorest households within a community. The selected households then receive an initial transfer of productive assets, along with consumption support and regular training on how to manage the assets profitably. Participants are encouraged to save money, and these households continue to receive ongoing coaching and mentoring throughout the treatment period.

Positive impacts on beneficiaries' economic and psychological well-being have been well-documented in the literature across various countries. Banerjee et al. (2015) find that treated households successfully increased assets, income, and consumption three years after receiving productive asset transfers in six countries. Similarly, Bandiera et al. (2017) observe a similar pattern of effects after four years in Bangladesh, using similar outcome indices. They also document a significant increase in the beneficiaries' mental health. In fact, Ridley et al. (2020) compare the impacts of multi-faceted programs studied in 10 articles with that of cash transfers studied in 18 articles and find that, on average across different studies, the impacts of multi-faceted programs on psychological well-being are higher than the average effects found in cash transfer studies. Specific to REAP program, Gobin, Santos, and Toth (2017) document a 30.8% increase to the control group in income one year after the program started, no significant effect on consumption and a 131.4% increase in savings.

Two recent studies reveal contrasting outcomes when examining the longer-term effects of multi-faceted programs. Barker et al. (2023) find that in Ethiopia after seven years, the treatment effects on wealth and consumption declined from the two- and three-year results. Their argument is that the dissipation of treatment effects stems primarily from the catch-up progress of control households, rather than losses experienced by the treated households. In contrast, Banerjee, Duflo, and Sharma (2021) showcase substantial improvements in average household per capita consumption and income for treated households compared to the control group, both at years seven and ten in India. This is despite the control households gradually moving out of poverty over time.

Regarding the distribution of outcomes, both Bandiera et al. (2017) and Banerjee et al. (2015) demonstrate that the treatment effects on consumption are non-negative at each centile during the four-year follow-up. These effects are significantly larger at higher centiles compared to lower centiles. A similar pattern of effects is observed for assets and savings. Similarly, Banerjee et al. (2015) also show that the effects on consumption per capita, income, and revenue index all increase with the quantiles.

Although it has been argued that the complementarities between the components of the multi-faceted program (the transfer of the initial productive asset, consumption support, the training and mentoring component, and the savings accounts) are crucial to the observed large and significant treatment impacts, most studies show the impacts of the entire bundle of the treatment. Some recent studies try to examine the treatment effects of each individual component of the multi-faceted programs in order to better understand the underlying mechanisms through which the program works. In Ghana, Banerjee et al. (2022) find that the savings component alone led to significant treatment effects on financial inclusion and consumption at the two-year mark, but these effects diminished after three years. On the other hand, the productive asset transfer component alone did not show any positive impacts on treated households at either the two-year or three-year follow-up.

In Uganda, Sedlmayr, Shah, and Sulaiman (2020) found that simplified versions of the multi-faceted program tended to erode its impacts. The positive effects disappeared when the program was reduced to mere transfers. Combining cash transfers with a light-touch psychological intervention (rather than regular mentoring and training visits), however, seemed to have a positive impact on psychological well-being and productive assets, yet no improvements were observed in consumption.

In Niger, Bossuroy et al. (2022) find that combining cash transfers with life-skills training and community sensitization on aspirations and social norms was the most cost-effective approach compared to combining cash transfers with a lump-sum cash grant or a combination of both the cash grant and psychosocial interventions with the monthly cash transfers. This highlights the importance of psychosocial interventions in the multi-faceted interventions for the ultra-poor.

Finally, the impacts of graduation programs are primarily examined under stable settings, with limited evidence on how these programs assist women during challenging shocks, including those arising from conflict or adverse weather events. The core focus of the multi-faceted program is to educate households on the effective management of productive assets while receiving consumption assistance. This approach holds the potential to alleviate negative consequences of conflicts or shocks by diversifying income sources and establishing supplementary assets that can act as buffers during labor and credit market demands.

This approach could also render beneficiaries more vulnerable, especially if their business investments are adversely affected by such shocks. Additionally, the effectiveness of the multi-faceted program for the ultra-poor might be constrained if its execution is significantly disrupted during difficult circumstances. For instance, the program could face pauses or execution challenges if markets are forced to shut down or if mentors are unable to regularly visit households.

A couple of recent studies focus on the impacts of variants of graduation programs within civil-conflict settings. In the context of the civil war in Yemen, Brune et al. (2022) discover modest positive outcomes four years into the program. Using one round of follow-up data after the baseline, they find improvements in assets and savings for the treated women. In South Sudan, Morel and Chowdhury (2015) examine program impacts where conflict emerged during the program. They note a 16% higher consumption among treated women six months after the program's start. Nevertheless, this impact on consumption did not persist beyond two years. Notably, participants did experience significantly elevated levels of livestock assets after two years. In a study within Afghanistan's fragile and conflict-ridden setting, Bedoya et al. (2019) reveal significant and substantial impacts on consumption. Two years into the program, a noteworthy 30% increase in consumption is observed compared to the control group.

In summary, the literature provides substantial evidence concerning the effects on economic and psychological well-being in the short to medium term due to the multi-faceted program. Nevertheless, the overall treatment effects might obscure variations among beneficiaries. Information on the program's extended impact and its functioning during periods of shocks remains relatively restricted. Moreover, though there is ample evidence highlighting the synergies between diverse program components, it becomes evident that the psychosocial intervention significantly influences the outcomes.

4.3. Data

In this section, we address two major issues regarding the business assets data we collected and present summary statistics on the main economic outcomes from the three rounds of data.

4.3.1. Premature termination of the program and business failures within BOMA. We found a significant portion of women undergoing treatment reporting zero business assets during the midline data collection. In March 2021, a follow-up investigation with the BOMA mentors revealed that about 3% (8/241) of women enrolled in the program did not actively participate and eventually dropped out without graduating from BOMA.

During the endline data collection, we conducted a series of follow-up questions to address this issue. Specifically, we asked additional questions to respondents who were identified as recipients of the REAP but reported zero total business assets for any co-owned businesses. We find that based on the status of BOMA women, approximately 4% (25 out of 584 women) left the program prematurely due to business failures. These women reported themselves as participants of the BOMA program at some point in time but discontinued their involvement before completing the program due to the failure of their businesses. Based on the evidence provided regarding the total business assets reported on co-owned businesses, it was found that approximately 6% of women reported premature termination of the program. This was due to either their disengagement from the co-owned businesses or the businesses themselves ceasing operations.

On average, it can be inferred that there is a certain likelihood, ranging between 4% and 6%, that business failure would lead to premature termination among women participating in the BOMA treatment. This estimation aligns closely with the information gathered from the follow-up conducted by BOMA mentors after the midline assessment, although slightly higher.

In terms of the impacts on the variable measures used in this study, during the midline analysis, we observed that out of the 520 women assigned to the treatment group, 241 reported zero business assets. This number is significantly larger than what we would expect from premature termination. After consulting with BOMA mentors, we discovered that some of the reported zero total business assets were inaccurate, primarily due to misunderstandings by the BOMA women when answering the questions.

To address this, we performed imputation for a portion of the zero total business assets reported at midline. These imputations were based on the BOMA administrative data, which indicated non-zero values for the respondents' share of total business assets. Approximately 26% of the treated women at midline (around 135 cases) had their total business assets imputed using this method. Detailed information regarding the imputation process can be found in section A.3 from chapter one.

It is important to highlight the conservatism of our imputation process. We specifically imputed values for around 80% of the treated women who reported zero total business assets, identified through follow-up with their BOMA mentors due to their lack of clarity on the questions. The remaining 20% of treated women (106 out of 520) who reported zero total business assets were left unaltered.

Regarding the treated women who cited reasons other than a misunderstanding for their zero total business assets, we did not include them in the imputation process. This approach is aimed at providing a conservative estimation of the treatment effects at midline, considering the unaddressed cases.

4.3.2. Reporting total business assets from co-owned businesses. During endline data collection, we also investigated how women reported their total business assets. We wanted to determine whether they reported only their share in the businesses or if they reported the assets for the entire businesses.

Of those 456 respondents, who reported total business assets co-owned with other women, 66% reported the total business assets for the entire group, while 34% reported the portion they owned from the business. Among the co-owned businesses, 86% involved a total of three individuals, and an additional 6.8% had two individuals. The number of individuals ranged from 2 to 32.

When calculating the total business assets for the endline, we followed this specific approach. If the respondents indicated that they were answering for the entire business, we calculated the total business assets by dividing it by the number of individuals involved. On the other hand, if the respondents reported only their share of the business, we considered that portion as separate. It is noteworthy that around one-third (34%) of the women reported only their share of the co-owned business, while two-thirds (66%) reported for the entire business.

Regarding the total business assets in the midline, if we assume that the women reported following the same proportion as in the endline (with 1/3 reporting for their personal share and 2/3 reporting for the entire business), it's important to note that we might overestimate the proportion of women's total business assets for those who reported for the entire business. This overestimation occurs because we counted the total business assets reported by the respondents as their personal share of the business assets.

Since we had 279 women who reported positive total business assets at the midline, (as discussed in the last section, we had to impute for some of the rest women who were assigned to treatment but did not report any total business assets according to the discrepancy between the BOMA administrative data and the self-reported total business assets data), we might overestimate 186 respondents' total business assets, assuming that 2/3 of the women reported for the entire businesses.

In relation to the underestimation of total business assets by treated respondents who reported zero total business assets at midline, the overestimation caused by women reporting for the entire business resulted in, on average, similar levels of total business assets compared to what we obtained from BOMA administrative data.

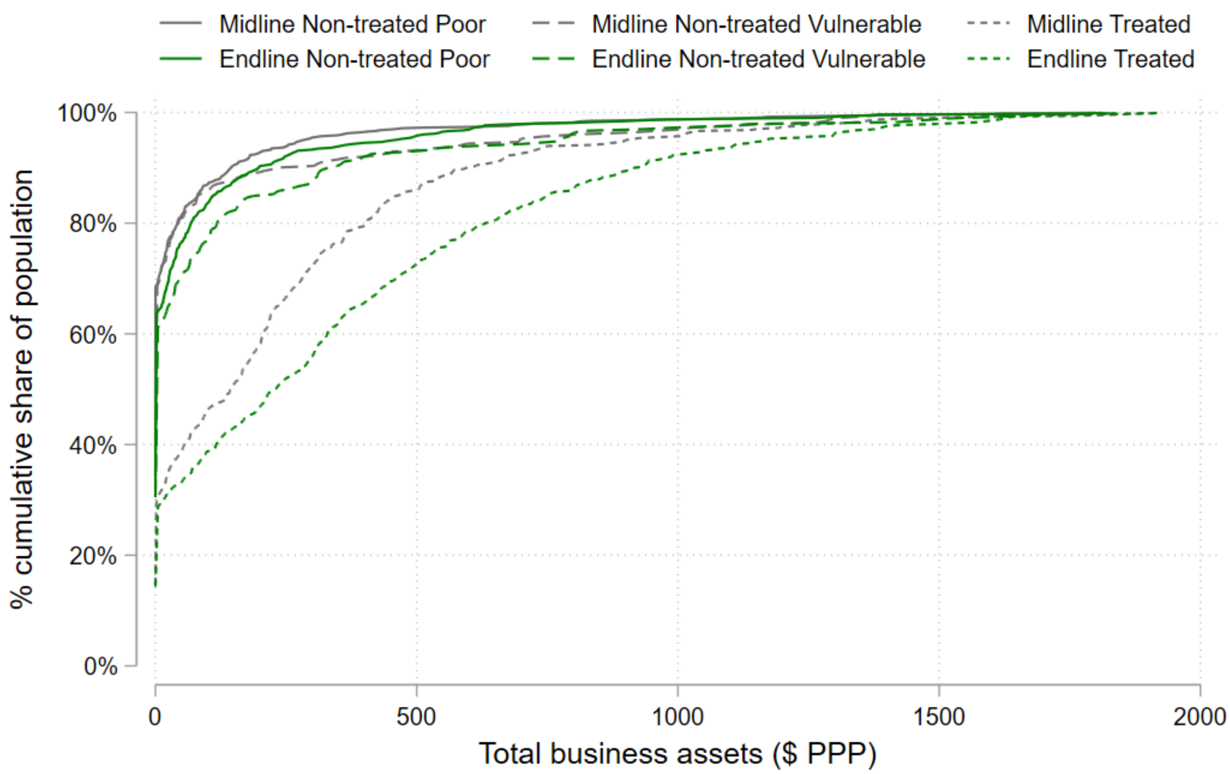
4.3.3. Summary Statistics on main economic outcomes. Figure 4.1 displays the cumulative distribution functions (CDFs) for the total business asset levels of both the treated and control groups at midline and endline. Upon examining the graph, it is evident that the non-treated groups exhibit nearly identical total business asset levels between midline and endline. However, both the midline and endline treated groups demonstrate significant positive shifts across the entire distribution compared to the non-treated groups. Moreover, the distribution of total business assets for the endline treated group is entirely shifted to the right of the midline treated group's distribution. For example, at midline, 80% of the treated groups have total business assets exceeding $PPP 480, whereas by endline, 80% of the treated women possess total business assets surpassing $PPP 520. More interestingly, as indicated by the long-dashed lines for the vulnerable group, who had a relatively higher wealth rank at baseline compared to the poor group eligible for the REAP program, the CDF of the total business assets shows that by endline, the distribution of the treated women's total business assets completely shifts to the right of the endline vulnerable group's total business assets. This hints at a catch-up from the poor group to the vulnerable group after enrolling in the REAP program on asset accumulation.

As shown in Table 4.1, we present summary statistics of the three key economic outcomes for the three rounds of data. Indicating the impact of severe shocks, we observed a decrease of 30% in average household income from baseline to midline for both the poor and vulnerable groups. By the endline, household income levels had recovered to baseline levels for both groups. Household income here might be useful because it allows us to see net effects of REAP on household income while accounting for the fact that women may have earned less money doing other things even while building up their businesses.

Despite a decrease in household income, an upward trend in business assets was observed among women in the poor group, suggesting potential spillover effects discussed in the first chapter. On the other hand, business assets for the vulnerable group decreased by approximately 41% at the midline but subsequently recovered to the baseline level by the endline.

Regarding savings, no discernible upward trends were observed between baseline and midline for the control group. However, the savings level more than doubled between the endline and baseline for the control group. In contrast, the savings level for the vulnerable group decreased by 15% from baseline to midline and remained at that level by the endline.

In summary, we observe a catching-up trend among the control group women from baseline to endline across all three key economic outcomes. For the treated group, apart from their business assets, there was also a significant increase in their savings levels, surpassing their vulnerable counterparts. The average savings level for the treated group was about 40% higher than that of the vulnerable group, and there was no change in the average levels between midline and endline.



Note: Midline non-treated poor include women who were assigned to receive treatment in wave 5. Midline treated women include women who were assigned to receive treatment from waves 1 to 4 and Endline treated women include women who were assigned to receive treatment from waves 1 to 5.

Figure 4.1. Endline vs. Midline Total Business Assets CDF

Table 4.1. Summary statistics on key economic outcomes

	Baseline		Midline			Endline		
	Poor	Vulnerable	Control	Waves 1-4	Vulnerable	Control	Waves 1-5	Vulnerable
	mean	mean	mean	mean	mean	mean	mean	mean
	(s.d.)	(s.d.)	(s.d.)	(s.d.)	(s.d.)	(s.d.)	(s.d.)	(s.d.)
Women's Business Assets ($ PPP)	48.2	226.1	74.4	242.5	133.8	86.5	406.2	220.6
	(233.5)	(1040.40)	(321.59)	(345.82)	(557.02)	(303.76)	(593.55)	(757.17)
Household Income ($ PPP)	815.1	1120.9	551.4	620.1	779.5	840.5	913.7	1057.6
	(725.6)	(937.84)	(540.35)	(595.70)	(736.35)	(803.49)	(832.87)	(1029.85)
Women's Savings ($ PPP)	13.1	52.5	17.1	62.8	44.4	27.9	63.3	44.3
	(69.4)	(271.48)	(89.58)	(134.45)	(169.96)	(184.31)	(169.54)	(167.28)
Observations	1385	700	830	555	350	669	617	324

4.4. Simple Average Treatment Effects

In this section, we present the intent-to-treat (ITT) impacts for waves 1-4 for the midline impacts and waves 1-5 for the endline impacts. It's important to note the timing and progression of the women's participation in the REAP program. At midline, wave 1 women had just graduated from the program, wave 2 women were 18 months into the program, wave 3 women were 12 months in, and wave 4 women had been in the program for 6 months. Wave 5 women, on the other hand, entered the program after the midline and had been in the program for 18-24 months by the time of the endline data collection. We combined waves 1 and 2 and 3 and 4 in this manner due to similar impacts observed between these waves.

We use the following regression specification to estimate causal average treatment effects at midline and endline:

$$y^t_{hm} = \alpha_{0t} + \alpha_{1t} y^0_{hm} + \beta^{at} W^a_{hm} + \beta^{bt} W^b_{hm} + \beta^{ct} W^c_{hm} + \alpha_{2t} Vul_{hm} + \varepsilon_{hm} \tag{4.1}$$

where y^t_{hm} represents the outcome variable of interest for individual h in community m during survey round t, y^0_{hm} is the baseline value of the same variable in 2018, W^a_{hm}, W^b_{hm}, and W^c_{hm} are binary indicator variables for assignment to waves 1 or 2, 3 or 4, and wave 5, respectively, of the BOMA program.[1] Vul_{hm} is an indicator for the vulnerable group. The error term ε_{hm} is clustered by community. When evaluating midline results, the control comprises women eligible for REAP but not assigned to any of the first four treatment waves. When evaluating endline results, the control group includes women eligible for REAP but not assigned to any of the 5 treatment waves.

The β^w $(w = a, b, c)$ parameters identify the intent-to-treat impact of the program under two assumptions: random assignment to treatment and no spillovers between treatment and control households. Later, we will relax the latter assumption by exploiting our saturation design with the addition of the endline data. It is worth pointing out that by the midline data collection, women from waves 1 and 2 had been enrolled in the program for approximately 18 to 24 months, which was about the same treatment length as wave 5 women by the time of the endline data collection. Comparing these two estimates would help us evaluate whether the program has relatively stable average treatment effects under different time and environmental conditions. Additionally, it would be interesting to compare the endline treated women with the vulnerable group of women to determine if the program has improved the well-being of ultra-poor women, potentially bringing them closer to or even surpassing their vulnerable counterparts.

[1]Following the model estimation with separate parameters for the first four waves, we grouped them into two categories since coefficients for waves 1 and 2 were similar, as were those for waves 3 and 4. Wave 5 includes women assigned to treatment after midline data collection. Due to the COVID quarantine, the rollout of the last wave initiated in March 2020 was paused after a group of women had received the grants. The remaining women constituted Wave 6, receiving grants in October 2020. We combined waves 5 and 6 into a single group due to the small number of women receiving grants before COVID.

Table 4.2. Average Treatment Effects Midline and Endline

	Treatment Waves 1-2		**Treatment Waves 3-4**		**Treatment Wave 5**	**Vulnerable**	
	Midline	*Endline*	*Midline*	*Endline*	*Endline*	*Midline*	*Endline*
Women's Business Assets ($PPP)	193.8***	294.5***	129.3***	349.4***	292.6***	9.403	61.88**
	(23.01)	(32.00)	(19.47)	(52.37)	(62.98)	(17.27)	(30.56)
Household Income ($PPP)	98.68***	72.67	0.936	73.91	-27.51	177.0***	149.0**
	(35.25)	(70.11)	(37.25)	(53.63)	(90.61)	(43.56)	(59.01)
Women's Savings ($PPP)	57.49***	28.65***	25.59***	43.93***	30.66*	16.32**	11.52
	(8.524)	(9.160)	(6.427)	(15.75)	(16.91)	(8.049)	(9.784)
Observations	1,735	1,610					

Table 4.2 presents the results obtained from estimating equation 4.1 using both midline and endline data. The complete regression results can be found in the appendix. Firstly, we observe an increase in the average treatment effects on Women's Business Assets. For women in Waves 1 and 2, they accumulate, on average, $PPP 193.8 more compared to the control group by year two into the program, and $PPP 294.5 more by year four into the program. As for women in waves 3 and 4, on average, they accumulate $PPP 129.3 more by 18 months and $PPP 349.4 more by 3.5 years. These findings suggest the presence of non-linear treatment effects over the course of the treatment period, which will be further investigated in the next section.

Wave 5 women, who commenced the program after the midline data collection, experienced a significant increase ($PPP 292.6) in their business asset accumulation compared to the control group. This increase occurred while they were approximately 18-24 months into the treatment at the time of the endline survey. Interestingly, their average treatment effects were about 51% higher than those of Waves 1 and 2 women during the midline assessment. Nevertheless, their earnings did not exhibit a significant increase when compared to the control group, suggesting that the accumulated assets may not have translated into income for these women as of yet.

Comparing the eligible group for the REAP with the vulnerable group, the vulnerable group initially held a higher wealth rank. However, by the midline survey, there was no discernible difference in asset accumulation between the vulnerable group and the control group of eligible women. In contrast, by the endline, the vulnerable group showed a significant increase in asset accumulation. Notably, their increase in asset accumulation only amounted to around 20% of the average treatment effects observed in the REAP women by the endline.

Conversely, the vulnerable group demonstrated a significant increase in household income at both the midline and endline. This increase represented about 20% of the average baseline control levels. Nevertheless, no significant average treatment effects were observed for treated women across different waves. For the endline assessment, the average treatment effects on household income for waves 1 and 2, as well as waves 3 and 4 women, were approximately 8% of the endline control levels, but these differences were not statistically significant. Investigating other labor supply indicators or income from wage labor of the women, as in Bandiera et al. (2017) or Banerjee, Duflo, and Sharma (2021), could provide further insights. Furthermore, the summary statistics in Table 4.1 also indicate that the lack of average treatment effects could be attributed to the control group catching up with the treated women after they completed the program.

Regarding savings, we observe a non-linear but increasing trend similar to what we observed for the average treatment effects on Women's Business Assets. For women in Waves 1 and 2, the average savings are approximately $PPP 57 more compared to the control group by year two into the program. By year four, this amount decreases to $PPP 28.7. For women in Waves 3 and 4, the average savings are approximately $PPP 26 more compared to the control group by 18 months, and this increases to $PPP 44 more by 3.5 years. Notably, the average treatment effects on savings for Wave 5 women are only half of what Waves 1 and 2 achieved by the midline.

Furthermore, when comparing to vulnerable women, who experienced a significant increase in savings levels by the midline (reaching 95% of the control level), BOMA women almost doubled savings levels compared to the vulnerable women 18-24 months into the program (Waves 1 and 2 by midline) and they kept more money in savings 3.5 - 4 years into the program ($PPP 29) compared to their vulnerable counterpart as well. In fact, by the endline, on average, all the treated women have significantly higher savings levels compared to the vulnerable group.

4.5. Continuous Treatment Effects

In this section, we present the results obtained using the continuous treatment estimator. As alluded to in the previous section, the conventional average treatment effects for women enrolled in the REAP at different waves displayed non-linear trends. In our analysis, we make the assumption that women enrolled at different waves follow a consistent trajectory as they progress through various stages of the treatment duration. Since rolling out in waves was built into the program design, women were randomly allocated into different waves after stratifying by manyatta. This grants us exogenous variation in the duration of treatment to estimate the impact dynamics and duration response function, which allows us to recover the medium to long-run impacts of the intervention and their time path.

We begin our continuous analysis by pooling the midline and endline data. Folloiwng Carter, Tjernström, and Toledo (2019), we replace the binary response function with a continuous treatment one:

$$E[y_{hmt}|d_{hmt}] = \lambda_0 + \tau_1 y_{hm0} + \tau_2 t_{end} + \tau_3 Vul_{hm} + f(d_{hmt}) \tag{4.2}$$

where d_{hmt} is the number of months since household h in community m actively enrolled in REAP at survey time t. The duration runs from 0 to 48 months. t_{end} is a survey-round dummy representing the survey round in year 2022. y_{hm0} is the baseline level of the outcomes. We choose a quadratic parametric form to represent the duration impact function, $f(d_{hmt})$.

We assume that after randomly assigning the women into different waves, the duration of treatment should be not correlated with any unobserved characteristics. Replacing $f(d_{hmt})$ in equation 4.2 with a quadratic functional form yields:

$$y_{hmt}(d_{hmt}) = \lambda_0 + \tau_1 y_{hm0} + \tau_2 t_{end} + \tau_3 Vul_{hm} + \zeta_1 d_{hmt} + \zeta_2 d_{hmt}^2 + \vartheta_{hmt} \tag{4.3}$$

We should also take into account that the duration impact function remains constant for women enrolled in different waves. Nonetheless, potential violations of this assumption exist, such as significant environmental shocks like COVID closures, which could affect the later months of the early waves. We argue that COVID closures would likely bias our estimates downwards, serving as a lower bound for the impact response function. Conversely, for the late waves, the majority of women did not receive the initial grant until the COVID closures ended, implying that the impact of COVID should be limited for these waves.

The coefficients derived from equation 4.2 are employed for generating the graphs. These specific coefficients can be found in Table D.2 within the appendix. To create the graphs, a quadratic model is utilized, where the independent variable is the months since the program's initiation. The regression takes into account baseline levels of the outcome variables, community-level saturation rates, and a vulnerability indicator.

The resulting graphs are displayed in Figure 4.2, 4.3, and 4.4. The horizontal axis represents the months since the program's inception. For instance, in the graph illustrating women's business assets, Wave 1 women were 24 months into the program at midline and 48 months into the program at endline. The gray areas surrounding the curve denote the 95% confidence interval estimator for average treatment effects, averaged across women with the same number of months in the program. As data from midline and endline are pooled, the lower panel of figures 4.2, 4.3, and 4.4 depict the distribution of the number of months since funding.

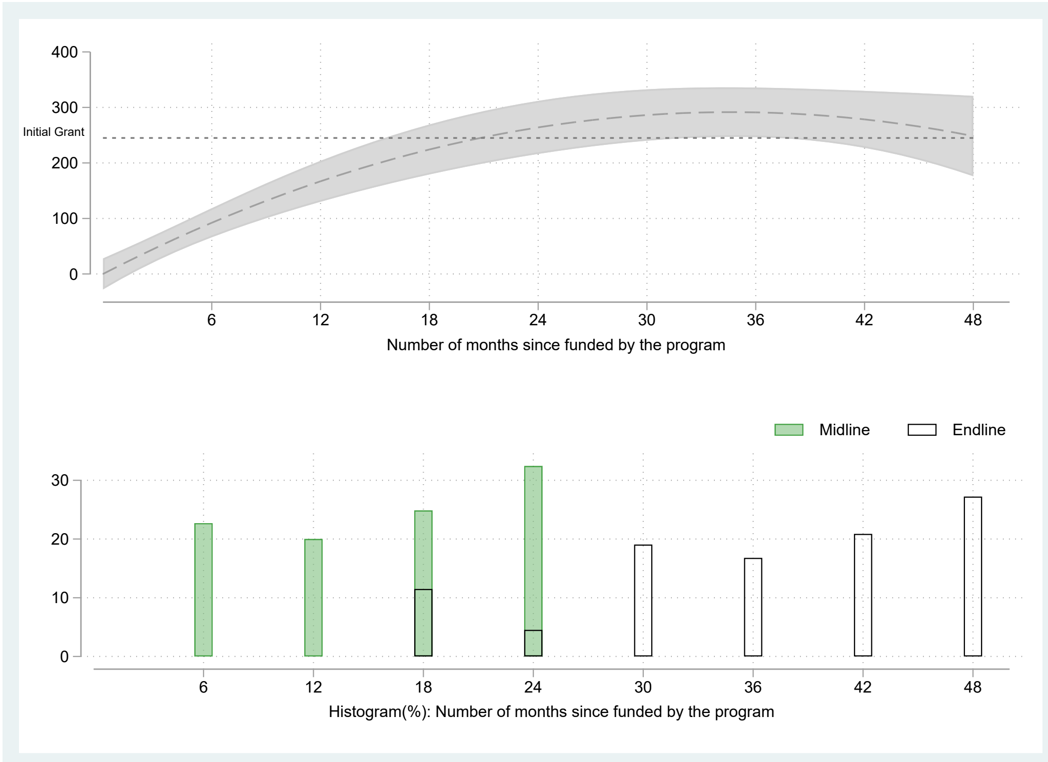
As demonstrated in Figure 4.2, the business assets of women gradually increase as the months since funding progress. They eventually surpass the initial amount transferred through the jump grant and progress grant (totaling PPP$ 245 per woman) between 18 and 24 months into the program. However, the average treatment effects begin to decline around 36 months into the program. To explore whether this reduction in total business assets is attributed to the parametric model, we also estimated a cubic model. The estimates for the cubic term were statistically insignificant and close to zero (as provided in Table D.2).

Treatment effects on household income continue to rise, and the estimated impact at 48 months is near PPP$ 70. This reflects an approximate 9% increase compared to the endline control level. Figure 4.4 displays the continuous treatment estimates on savings, revealing a similar inverse-U shape as the total business assets. It appears that the amount deposited into savings continued to increase up to 24-30 months into the program and then started to decrease. This pattern corresponds to the fact that women are required to save their money with the groups until graduation, which occurs after two years. Subsequently, women begin to draw down their savings and allocate them to other activities.

4.5.1. Heterogeneous Continuous Treatment Estimates by Baseline Depression. To study whether baseline depression might limit eligible women in their ability to benefit from the REAP over a longer time horizon, we introduced two interaction terms between months of treatment and the baseline depression dummy variable when estimating equation 4.2. The baseline depression dummy variable is defined using the baseline Center for Epidemiological Studies Depression (CES-D) score. A CES-D score of 12 is used as a threshold, indicating clinically diagnostic depression. Further discussion on the CES-D scale and the score threshold can be found in section 2.4.3.

Table D.3 presents the results of the heterogeneous continuous treatment estimates on baseline depression. The estimates for women's business assets are depicted in Figure 4.5[2]. As shown in the figure, the average treatment effects of the baseline non-depressed women on the number of months enrolled in the program are similar to those plotted in Figure 4.2, with the estimates above the initial transfer of grants between months 30 and 42. However, what is particularly interesting here is that for the baseline depressed

[2]Figures for earnings and savings are shown in the appendix.



estimated using estimates from table D.2 estimated at mean saturation rate at 0.27.

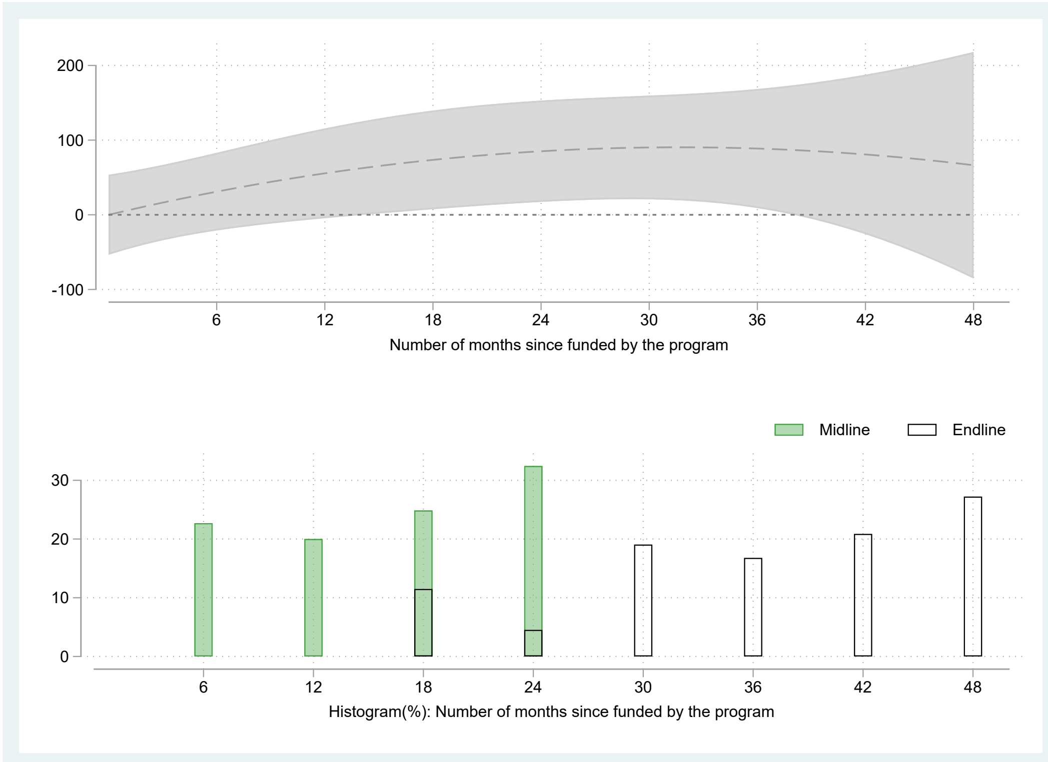
FIGURE 4.2. Women's business assets (PPP$) over months of treatment quadratic

group, the average treatment effects over the 48-month time horizon consistently remain lower than the trajectory for the baseline non-depressed women. Moreover, their accumulation of assets had never increased above the initial level of grant transfer before it started to decline.

The accumulation of assets for the baseline depressed group reached about PPP$ 200 between 30 and 36 months into the program before it started to decrease. By then, the baseline non-depressed group would have accumulated assets above PPP$ 300. Unfortunately, the wide confidence intervals (shown by the vertical bars in the figure) for the baseline depressed group prevent us from suggesting that the estimates are statistically different from each other for those two groups. It is definitely worth investigating the heterogeneity under the baseline depression status for programs similar to this one in different contexts, with this specific design in mind.

4.6. Spillovers

By the endline, as depicted in the right panel of Figure 4.6, the saturation measures we employed ranged from 0.1 to 0.6, as described in section 2.5.1. The mean saturation rate was 0.27, with a standard deviation



estimated using estimates from table D.2 estimated at mean saturation rate at 0.27.

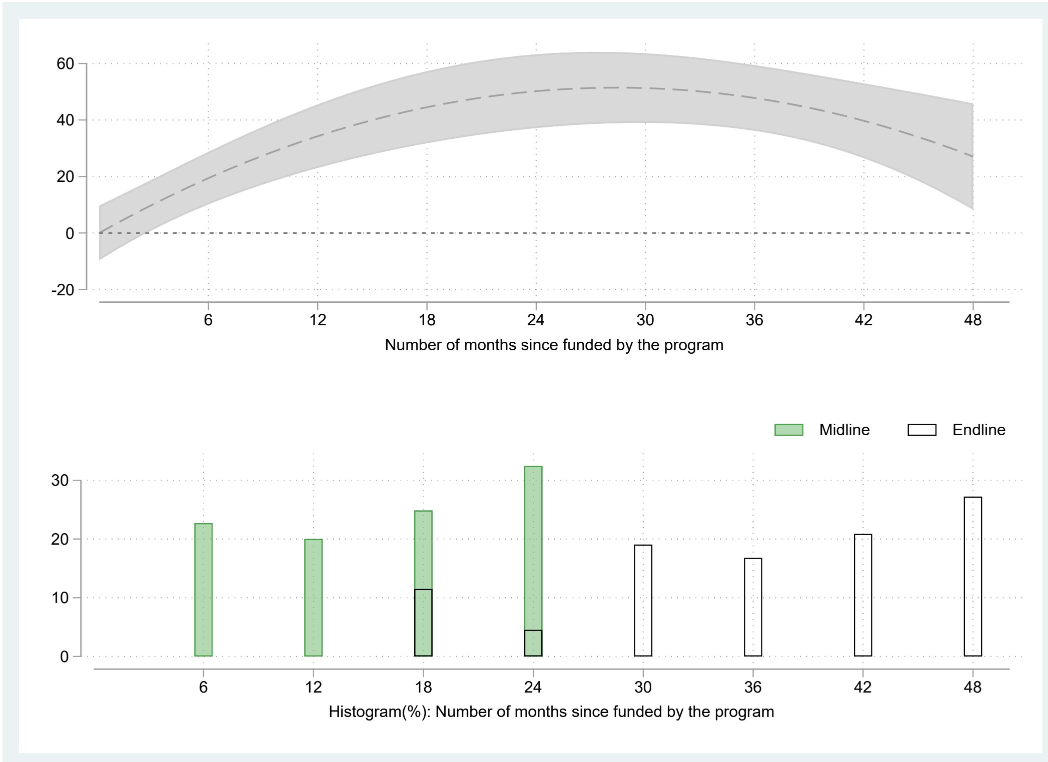
FIGURE 4.3. Household income (PPP$) over months of treatment quadratic

of 0.07. Notably, as shown in the left panel of Figure 4.6 the entire distribution of the saturation rates shifted to the right from midline to endline.

We adopt the following specification, which is a similar econometric approach to the one used in section 2.5.2, to estimate the treatment and spillover effects of being assigned to treatment waves by the endline:

$$
\begin{aligned}
y_{hm} =& \alpha_0 + \alpha_1 y^0_{hm} + \beta^a W^a_{hm} + \beta^b W^b_{hm} + \beta^c W^c_{hm} \\
& S_m \times \left[\delta^d I^d_{hm} + \delta^a W^a_{hm} + \delta^b W^b_{hm} + \delta^c W^c_{hm}\right] + +\varepsilon_{hm}
\end{aligned}
\tag{4.4}
$$

where y_{hm} is the 2022 outcome variable of interest for individual h in community m, y^0_{hm} is the 2018 baseline value of that same variable, W^a_{hm}, W^b_{hm} and W^c_{hm} are binary indicator variables for assignment to waves 1 or 2, 3 or 4 and wave 5, respectively of the BOMA program. S_m is the community level saturation measure at the endline. I^d_{hm} is an indicator variable for an eligible woman in the control group. The estimated direct impacts for a woman who enrolled in waves 1 and 2 in community m thus can be represented as: $\beta^a + \delta^a \times S_m$. The estimated spillover effects for a within-cluster non-treated woman in community m are



estimated using estimates from table D.2 estimated at mean saturation rate at 0.27.

FIGURE 4.4. Women's savings (PPP$) over months of treatment quadratic

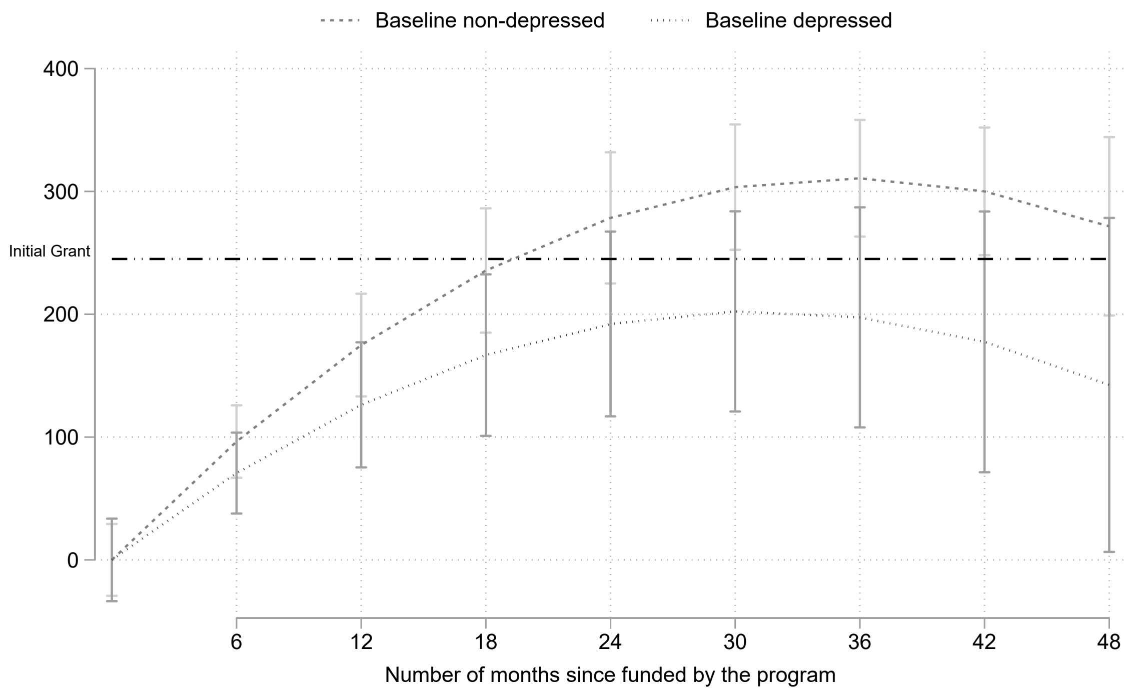
TABLE 4.3. Endline Impacts and Spillovers

	Control		Treatment Waves 1-2		Treatment Waves 3-4		Treatment Wave 5	
	Saturation Low	*Saturation Mean*	*Saturation Low*	*Saturation Mean*	*Saturation Low*	*Saturation Mean*	*Saturation Low*	*Saturation Mean*
Women's business assets ($PPP)	30.1*	81.4*	377.0***	376.8***	420.2**	427.8***	397.8**	369.3***
	(16.9)	(45.6)	(75.5)	(48.0)	(155.3)	(67.2)	(151.1)	(66.5)
Household income ($PPP)	-3.8	-10.3	164.6	80.7	53.7	67.8	132.2	-52.8
	(44.4)	(120.0)	(211.4)	(132.5)	(176.3)	(127.7)	(242.5)	(145.7)
Women's savings ($PPP)	2.8	7.4	45.0	35.4*	39.4	50.0**	16.0	40.7*
	(6.0)	(16.1)	(27.6)	(19.4)	(50.0)	(21.0)	(31.7)	(24.1)
Observations				1,286				

Notes: Direct treatment effects and spillover effects for endline estimated at mean saturation rates 0.27. Low saturation estimated at saturation rates of 0.1. Standard error clustered at the manyatta level. $^{***}p < 0.01,^{**}p < 0.05,^{*}p < 0.1$.

$\delta^d \times S_m$. Following Baird et al. (2018), we define the total causal effect of being assigned to treatment wave $w = a, b, c$ in community m as $\beta^w + \delta^w \times S_m + \delta^d \times (1 - S_m)$.

Table 4.3 displays the estimates obtained from estimating equation 4.4. For the complete regression results, please refer to Table D.4 in the appendix. Given that the minimum level of saturation rates in the



estimated using estimates from table D.3 estimated at mean saturation rate at 0.2.

Figure 4.5. Women's business assets (PPP$) over months of treatment by baseline Depression

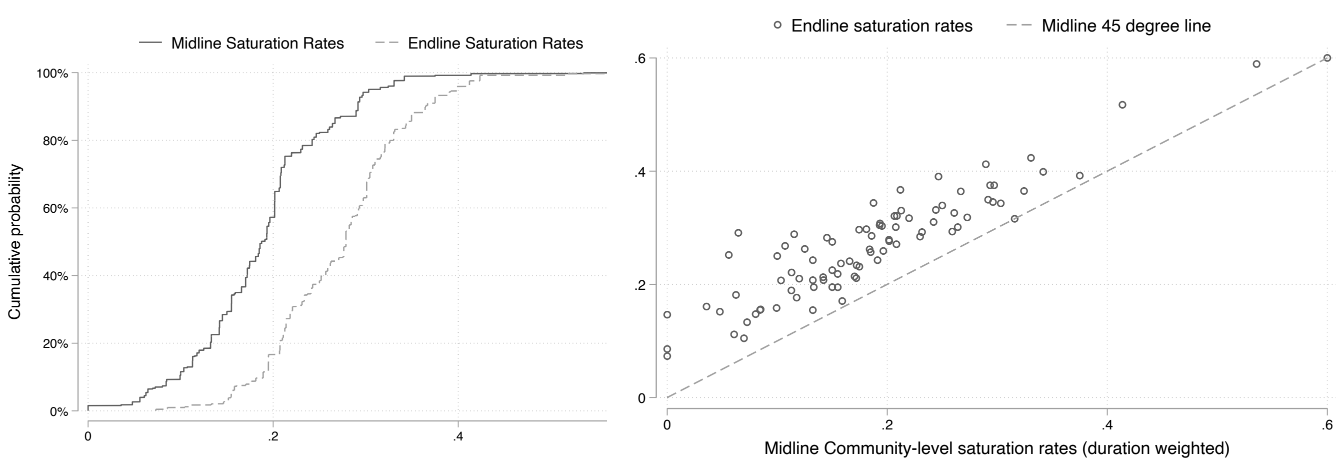


Figure 4.6. Midline vs. Endline Community Saturation Rates

communities by the endline is 0.1, the table also presents the estimated direct impacts and spillover effects for communities with the lowest saturation rates.

Regarding women's business assets, we observe significant and substantial increases for women assigned to all different waves. For women in waves 1 and 2, the estimated direct impacts were approximately 436% of the endline control mean. Similarly, for women in waves 3 and 4, the estimated direct impacts were around 495% of the endline control mean. For wave 5 women, who joined the program after the midline assessment, the direct impacts were about 427% of the control mean.

Notably, even for the control group women, we estimated a spillover effect of approximately 22% of the direct impacts experienced by wave 5 women. Comparing the impacts at the mean saturation level and low saturation level, it is observed that the saturation rate does not appear to have caused a significant impact on women assigned to waves 1 and 2, as well as waves 3 and 4. Nevertheless, for wave 5 women, the direct impacts decreased as the saturation rates increased from low to mean levels.

By the endline assessment, significant direct impacts and spillover effects on household earnings are not observed. Interestingly, for women enrolled in waves 1 and 2, wave 5, and the within-cluster control women, the direct impacts and spillover effects decreased as the saturation rates increased from low to mean levels in local communities. Conversely, women assigned to waves 3 and 4 experienced an increase in direct impacts when the community-level saturation rates increased from low to mean levels.

Regarding the amount deposited into savings by women, a significant increase is observed at mean saturation levels for women enrolled in all different waves. For women in waves 1 and 2, the estimated direct impacts were approximately 127% of the endline control mean. Similarly, for women in waves 3 and 4, the estimated direct impacts were around 179% of the endline control mean. As for wave 5 women, who joined the program after the midline assessment, the direct impacts were about 146% of the control mean.

Although significant spillover effects on savings at mean saturation levels are not observed, the direct impacts on waves 3 - 5 women appear to increase as the saturation rates increase from low to mean levels. In contrast, the impacts decrease for waves 1 and 2 women as the saturation rates increase.

4.6.1. Treatment and spillover effects between midline and endline. In this section, our aim is to compare the direct impacts and spillover effects between the midline and endline by fixing a midline saturation rate and examining the differences with the endline data. Additionally, we attempted to use incremental saturation rates between the midline and endline, but the results remained largely unchanged.

To compare the direct impacts and spillover effects we obtained for the midline and endline, we use the following econometric specification:

$$
\begin{aligned}
y_{hm,t} &= \alpha_0 + \alpha_1 y_{hm,t=0} + \sum_{w=1}^{2} \beta^w W_{hm}^w + \sum_{w=1}^{2} \delta^w (W_{hm}^w \times S_{mt}) + \delta^c (S_{mt} \times I_{hm,t=1}^c) \\
&\quad + Endline \times (\alpha_2 + \sum_{w=1}^{3} \gamma^w W_{hm}^w + \sum_{w=1}^{3} \omega^w (W_{hm}^w \times S_{mt}) + \eta^c (S_{mt} \times I_{hm,t=2}^c)) + \epsilon_{hm,t}
\end{aligned} \tag{4.5}
$$

where y_{hmt} is the outcome variable of interest for household h in manyatta m at midline ($t = 1$) or endline ($t = 2$). W_{hm}^w is an indicator variable for the assignment of household h in manyatta m to treatment wave w. For instance, $w = 1$ represents women who enrolled in waves 1 and 2, $w = 2$ represents women who enrolled in waves 3 and 4, and $w = 3$ represents women who were assigned to be enrolled in wave 5. I_{hmt}^c is an indicator variable for an eligible woman who is in the control group at midline or endline. Note that households assigned to receive treatment at wave 5 are counted as control at midline ($t = 1$). S_{mt} is the manyatta-level saturation measure at midline or endline. In order to increase the power of the analysis, we include the baseline levels of the outcome variables $y_{hm,t=0}$ (McKenzie, 2012).

In section 2.5.2, we discussed the estimated direct impacts ($\beta^1 + \delta^1 \times S_{mt}$) for a woman who enrolled in waves 1 and 2 in manyatta m, the spillover effects for a within-cluster non-treated woman in manyatta m ($\delta^c \times S_{mt}$), and the total causal effect of being assigned to treatment wave w in manyatta m ($\beta^w + \delta^w \times S_{mt} + \delta^c \times (1 - S_{mt})$) at midline. With the addition of endline data, we can now compare the midline effects with the endline effects.

The estimated difference between the midline and endline direct impacts of REAP for waves 1-2 and 3-4 women can be represented as $\gamma^1 + \omega^1 \times S_{mt}$ and $\gamma^2 + \omega^2 \times S_{mt}$, respectively. The difference between the midline and endline within-cluster spillover effects can be represented as $\eta^c \times S_{mt}$. Furthermore, since the women assigned to receive treatment by wave 5 were in the control group at midline but treated by the endline, the difference between the midline and endline impacts of wave 5 women can be represented by the term $\gamma^3 + (\omega^3 - \delta^c) \times S_{mt}$.

Table D.5 in the appendix presents the results of estimating equation 4.5 for the three key economic outcome variables. First and foremost, we obtained similar coefficients compared to the results in Table A5 of chapter one for the midline impacts of REAP.

Table 4.4 compares the direct impacts and spillover effects between the midline and endline, evaluated at the mean duration-weighted saturation level of 0.19 for the midline and 0.27 for the endline. These coefficients indicate a significant and substantial increase in total business assets between the midline and endline for waves 1 and 2 women (PPP$ 139 with a p-value of 0.01), which is about 3 times the baseline

TABLE 4.4. Impacts and Spillovers Midline vs. Endline

	Control			Treatment Waves 1-2			Treatment Waves 3-4		
	Midline Mean Saturation	*Endline Mean Saturation*	Difference p-value	*Midline Mean Saturation*	*Endline Mean Saturation*	Difference p-value	*Midline Mean Saturation*	*Endline Mean Saturation*	Difference p-value
Women's Business Assets ($PPP)	52.1*	58.6	6.5	245.8***	384.8***	139**	176.2***	436.3***	260.1***
	(27.4)	(41.6)	(0.88)	(29.9)	(54.0)	(0.01)	(24.9)	(74.3)	(0.001)
Household Income ($PPP)	12.6	-12.2	-24.7	138.1**	152.5	14.4	13.7	82.7	69.0
	(52.8)	(117.3)	(0.85)	(65.2)	(135.0)	(0.92)	(63.1)	(135.8)	(0.63)
Women's Savings ($PPP)	11.0	3.0	-8.1	68.4***	38.3*	-30.1	35.3***	59.0**	23.7
	(8.6)	(16.1)	(0.68)	(12.2)	(21.3)	(0.19)	(8.8)	(23.3)	(0.33)
Observations					2,667				

Notes: Direct treatment effects and spillover effects for midline estimated at mean saturation rates 0.19 and endline estimated at mean saturation rates 0.27. Standard error clustered at the manyatta level.
$^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

control level. For waves 3 and 4 women, the increase between the midline and endline in total business assets is twice as large (PPP$ 260.1 with a p-value of 0.001).

Recall that in section 2.5.2, we observe that at midline mean saturation communities, the direct impacts on women's total business assets roughly match the initial amount transferred to them (PPP$ 245.43 or US$ 100 per participant). In contrast, by the endline and at mean saturation rates in villages, the direct impacts on women in waves 1 and 2 surpassed the initial transfer amount, reaching PPP$ 385 (s.e. = PPP$ 54). This occurred four years into the program and two years post-graduation. Similarly, waves 3 and 4 experienced an even greater surge in direct impacts, reaching about 178% of the initial transfer amount and holding statistical significance. Nevertheless, there was no statistically significant difference between the midline and endline within-cluster spillover effects at the mean saturation rates on total business assets. The p-value (0.88) for testing linear combinations of coefficients of $\eta^c \times S_{mt} = 0$ is displayed in the brackets under Table 4.4.

As for reported annual cash earnings, no statistically significant difference between midline and endline was identified at mean saturation rates. For waves 1 and 2 women at mean saturation rates during both midline and endline, the endline direct impact was estimated at PPP$ 152.5 (se = PPP$ 135), indicating a slight increment from midline direct impacts without statistical significance. Waves 3 and 4 women experienced direct impacts approximately half of those for waves 1 and 2 but exhibited a substantial rise from midline, with direct impacts estimated at PPP$ 82.7 (se = PPP$ 135.8).

An interesting observation is a negative spillover effect for within-cluster control women at the endline, particularly for those living in communities at mean saturation rates for both midline and endline. This suggests that the program's impact on reported cash earnings might not significantly extend beyond directly targeted participants and communities.

In terms of midline and endline savings for waves 1 and 2 women at mean saturation rates, no statistically significant difference was found. However, endline direct impacts on savings were approximately half of the midline figure. This suggests that while there was no significant change over time, the program's impact on savings weakened between midline and endline for these women.

Conversely, for waves 3 and 4 women living in communities with mean saturation rates, savings exhibited a substantial increase, reaching 167% of the midline direct impacts level (PPP$ 59, se = PPP$ 23.3). This signifies a noteworthy improvement in savings for these participants compared to earlier program stages.

In summary, women appear to have continued building their business assets between endline and midline evaluations, particularly for waves 3 and 4 in communities with mean saturation rates. They have also augmented their savings; however, this hasn't translated into higher household earnings. As for waves 1 and 2 women, they appear to have drawn down their savings, possibly as a means to balance consumption levels.

4.7. Discussion

We've documented an increase in productive assets and savings four years into a multi-faceted program compared to the control group in a Kenyan pastoralist region. The increase in household income, however, isn't statistically significant. Through continuous treatment analysis, we've uncovered an inverse U-shape in the treatment impacts of assets and savings. This trend began to wane after peaking shortly after two years of the program when most women graduated from REAP, marking the cessation of regular mentoring and training. Additionally, we observed a 22% spillover effect on within-cluster control women at the mean saturation levels at the endline, particularly on women's business assets.

By the endline at mean saturation levels, we no longer observe spillover effects on women's business assets, in contrast to our midline observations. This finding aligns with the hypothesis that as program exposure deepens, individuals adapt their choices and behavior. Over time, they may recalibrate their preferences, especially within the context of the ongoing poverty-alleviating program. Another plausible explanation for the diminishing spillover effects is the dissemination of knowledge and skills among non-participants in the community. As program beneficiaries share their newly acquired insights and techniques, the comparative advantage initially enjoyed by the control group through this knowledge could erode, leading to a convergence of asset levels. This diffusion of skills may contribute to a more equitable resource distribution within the community, thereby reducing observed spillover effects on women's business assets over time.

The sustained and heightened positive effects on business assets at the endline underscore the program's potential for enduring growth and long-term benefits. Significant increases in household earnings aren't observed at the endline, suggesting that program beneficiaries are still in the process of generating profits from their accumulated assets. As mentioned previously, the negative shocks experienced between the midline and endline survey rounds served as a test for the multi-faceted program during challenging periods. Our findings align with similar patterns found in Yemen (Brune et al., 2022), where significant increases in productive assets and savings were also observed after four years of the program in an unstable context. Notably, their program lacks consumption support, which acts as a buffer to prevent premature depletion of productive assets by households.

Further research is warranted to delve into the underlying drivers of these results and to inform strategies for optimizing the program's efficacy and reaching a wider audience. Future investigations should delve into the dynamics of spillover effects under poverty-alleviating programs in systematically challenging macro environments. Our findings, at a minimum, suggest that even in challenging circumstances, the multifaceted program maintains some positive impacts on asset accumulation and savings. Importantly, women have managed to sustain productive asset levels above the initial transfer amount.

Systemic crises or shocks might, to varying degrees, impede households from fully embracing and benefiting from programs like this. To better grasp how multi-faceted programs fare in system-wide crises, future research should explore methods to quantify these shocks. Additionally, studying the synergies between various social protection programs could yield policy tools resilient to challenging macro environments. For instance, combining livestock insurance with this multi-faceted program might offer a potential solution to safeguard the assets that women have begun to accumulate under the present program.

APPENDIX D

D.0.1. Detailed discussions on measurement on total business assets. We discovered that out of the 590 respondents identified as treatment recipients according to BOMA administrative data, (33.2%) 196 of them reported zero business assets with co-owners by the endline.

Among these 196 women, approximately 56.1% (110/196) stated that they were no longer involved in the co-owned businesses, or the businesses themselves had ceased operations by the time of the endline data collection. However, we also observed that around 24% (47/196) of women reported becoming sole owners of the businesses that initially received support from BOMA. Consequently, they were no longer part of any co-owned businesses.

Out of the 110 women who reported no longer co-owning any business despite being identified as REAP recipients according to BOMA's administrative data, 33% (36) of them indicated that they left the program before graduating from BOMA. This suggests that approximately 6% (36/590) of women left the program prematurely. In contrast, none of the women who reported becoming sole owners of co-owned businesses, which were established with BOMA's support, suggested leaving the program before graduating.

On another question that was asked to every respondent regarding their treatment status with BOMA, out of the 1610 participants, 51 (8.7%) women indicated that they had left the program before graduating. The reasons they mentioned for leaving the program varied. Approximately 50% of them stated that their businesses collapsed, attributing the reasons to factors such as the Covid-19 pandemic, drought, theft of funds, or loss of livestock during a raid. Another 15 women (29%) reported leaving the program due to personal-related issues, including old age, sickness, relocation from their original places, using the funds to pay for their children's school fees, disagreements with business partners, or disagreements with their husbands. Some women also mentioned having a wrong perception of BOMA as a reason for their departure.

We also inquired about the reasons for reporting zero business assets among participants who identified themselves as recipients of the BOMA program, whether they had already graduated or were still active participants. It was found that 9% (49 out of 523) of these individuals reported having no businesses. Among these women, approximately 36% (18 out of 49) cited drought as the primary reason for the failure of their

Table D.1. Average Treatment Effects on Economic Outcomes

VARIABLES	Business Assets	Business Assets	Household Income	Household Income	Savings	Savings	CES-D	CES-D
ITT Wave w1-w2	193.8***	294.5***	98.68***	72.67	57.49***	28.65***	-0.370	-0.606
	(23.01)	(32.00)	(35.25)	(70.11)	(8.524)	(9.160)	(0.314)	(0.386)
ITT Wave w3-w4	129.3***	349.4***	0.936	73.91	25.59***	43.93***	-0.280	0.00654
	(19.47)	(52.37)	(37.25)	(53.63)	(6.427)	(15.75)	(0.307)	(0.433)
ITT Wave w5		292.6***		-27.51		30.66*		-0.168
		(62.98)		(90.61)		(16.91)		(0.697)
Baseline level of outcome variables	0.277**	0.398**	0.152***	0.200***	0.259***	0.134	0.110***	0.121***
	(0.119)	(0.171)	(0.0240)	(0.0443)	(0.0504)	(0.0876)	(0.0282)	(0.0275)
Vulnerable	9.403	61.88**	177.0***	149.0**	16.32**	11.52	-0.573*	-0.343
	(17.27)	(30.56)	(43.56)	(59.01)	(8.049)	(9.784)	(0.305)	(0.336)
Constant	61.74***	70.79***	432.5***	682.2***	14.47***	26.52***	8.725***	8.154***
	(12.44)	(13.40)	(27.08)	(36.12)	(2.997)	(8.384)	(0.291)	(0.340)
Observations	1,735	1,610	1,735	1,610	1,735	1,610	1,733	1,607
R-squared	0.167	0.202	0.059	0.041	0.111	0.018	0.015	0.015
Vulnerable	NO	YES	NO	YES	NO	YES	NO	YES

Notes: Standard errors clustered at the manyatta level. $***p < 0.01, **p < 0.05, *p < 0.1$

businesses. Other reasons mentioned by the participants for the lack of business assets included unpaid debts from customers, which may have contributed to financial difficulties and the inability to continue operating their businesses. Additionally, some participants cited personal issues similar to those mentioned earlier, such as using the funds to buy food for their families.

Out of the 456 respondents, who reported total business assets co-owned with other women, 96.7% (441) indicated that the business was started with support from BOMA.

Furthermore, the respondents expressed a high level of confidence in the accuracy of the reported numbers for the total business assets. Fifty percent stated that the number should be correct within 100 KES, while 46% believed it should be correct within 1000 KES. The confidence level appeared similar for both women whose businesses were not started under the support of BOMA and those whose businesses received support from BOMA.

On a side note, regarding respondents who reported total business assets for their personally-owned businesses or those owned within their households, their confidence levels in the accuracy of the reported numbers were also quite high. Sixty-seven percent indicated that the number should be correct within 100 KES, while 31% suggested it should be correct within 1000 KES. It is worth noting that approximately 21% of those personal businesses were initially started with the support of BOMA before becoming personal businesses.

Table D.2. Continuous Treatment Results

	Women's Business Assets ($PPP)	Women's Business Assets ($PPP)	Household Income ($PPP)	Household Income ($PPP)	Women's Savings ($PPP)	Women's Savings ($PPP)
Months of Treatment	15.913***	13.615***	-0.235	-29.047***	3.520***	3.064**
	(2.284)	(4.140)	(3.231)	(7.864)	(0.630)	(1.445)
Months of Treatment^2	-0.213***	-0.064	0.105	1.974***	-0.060***	-0.031
	(0.051)	(0.269)	(0.087)	(0.480)	(0.014)	(0.094)
Months of Treatment^3		-0.002		-0.027***		-0.000
		(0.004)		(0.007)		(0.001)
Baseline levels of outcome variables ($PPP)	0.337**	0.337**	0.174***	0.174***	0.203***	0.203***
	(0.141)	(0.141)	(0.023)	(0.023)	(0.024)	(0.024)
Manyatta level saturation rates (duration-weighted)	237.321**	232.302**	476.812**	413.708*	3.990	2.995
	(96.423)	(97.448)	(238.112)	(236.320)	(36.079)	(35.511)
Indicator: vulnerable or poor	30.845	29.957	177.989***	166.723***	14.554*	14.377*
	(19.688)	(19.487)	(39.282)	(39.164)	(7.464)	(7.454)
Constant	15.417	17.444	431.597***	456.694***	18.790**	19.193**
	(19.579)	(19.863)	(54.147)	(54.593)	(8.381)	(8.248)
R-squared	0.19	0.19	0.06	0.06	0.05	0.05
Observations	3,345	3,345	3,345	3,345	3,345	3,345

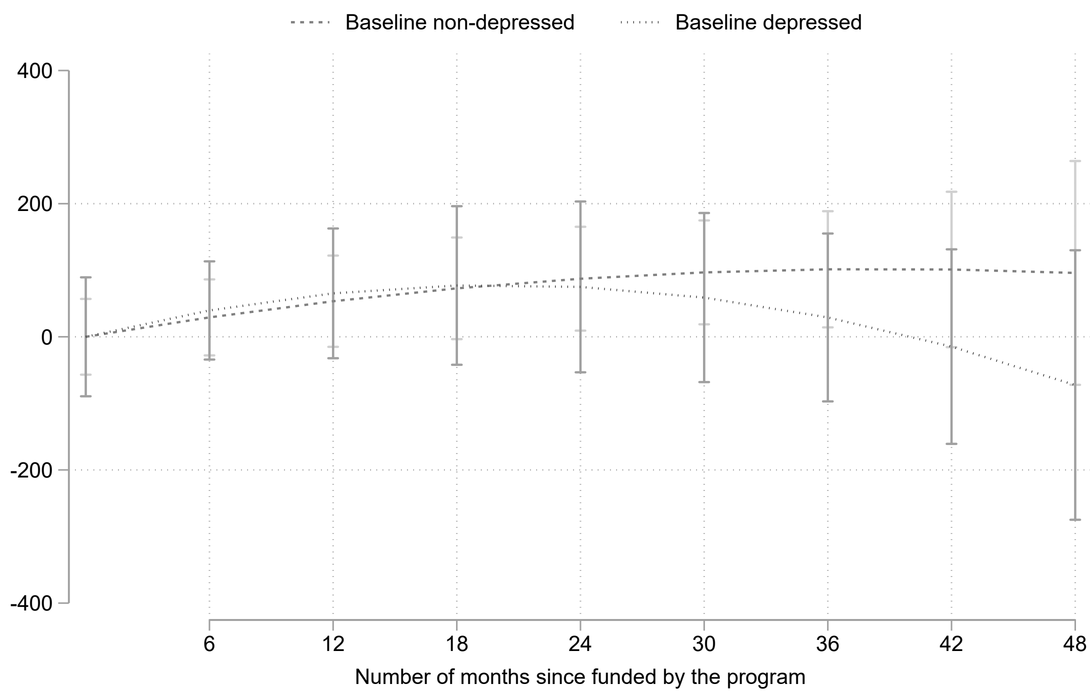


Figure D.1. Family Income (PPP$) over months of treatment by baseline Depression

estimated using estimates from table D.3 estimated at mean saturation rate at 0.2.

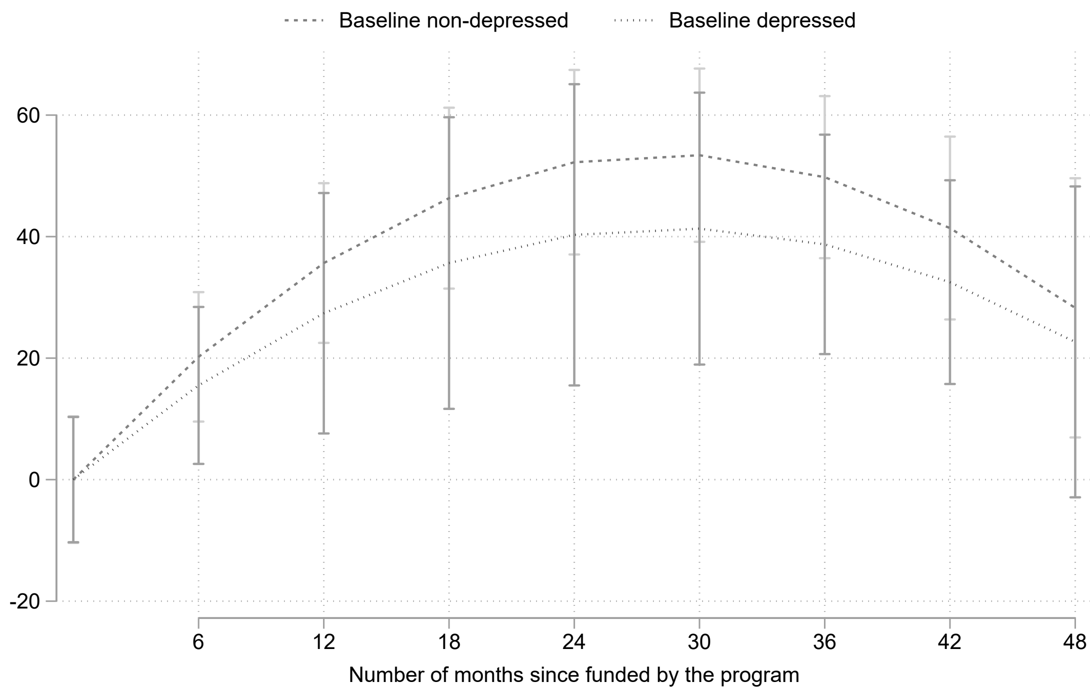


Figure D.2. Women's Savings (PPP$) over months of treatment by baseline Depression

estimated using estimates from table D.3 estimated at mean saturation rate at 0.2.

Table D.3. Continuous Treatment Estimates by Baseline Depression

	Business Assets	Family Income	Savings
Depression	-35.41**	-8.669	-12.09**
	(15.21)	(44.25)	(5.511)
Months of Treatment	16.63***	-0.575	3.661***
	(2.487)	(3.785)	(0.702)
Months of Treatment^2	-0.217***	0.125	-0.0627***
	(0.0560)	(0.102)	(0.0163)
Depression $\times$ Months of Treatment	-4.609	1.845	-0.890
	(3.653)	(8.324)	(1.420)
Depression $\times$ Months of Treatment^2	0.0396	-0.113	0.0161
	(0.0798)	(0.189)	(0.0330)
Baseline level of outcomes (PPP$)	0.337**	0.172***	0.202***
	(0.141)	(0.0235)	(0.0238)
Manyatta level saturation rates (duration-weighted)	223.6**	471.7**	0.667
	(96.43)	(236.6)	(35.62)
Vulnerable	29.66	178.2***	14.16*
	(19.59)	(39.36)	(7.472)
Constant	24.32	435.4***	21.54**
	(20.13)	(54.05)	(8.589)
Observations	3,345	3,345	3,345
R-squared	0.194	0.056	0.049

Notes: Standard errors clustered at the manyatta level.$^{***}p < 0.01,^{**} p < 0.05,^{*} p < 0.1$

Table D.4. Endline Impact Estimates Accouting for Spillovers

	Business Assets ($PPP)	Family Income ($PPP)	Savings ($PPP)
ITT Wave 1&2	377.1***	214.0	50.65
	(105.8)	(296.3)	(35.58)
ITT Wave 3&4	415.7*	45.37	33.17
	(217.8)	(233.0)	(77.98)
ITT Wave 5	414.6*	240.9	1.389
	(213.4)	(340.1)	(44.85)
ITT w1-w2 * Saturation	-1.270	-493.7	-56.40
	(360.0)	(1,003)	(102.0)
ITT w3-w4 * Saturation	44.96	83.12	62.49
	(661.4)	(733.7)	(296.0)
ITT w5 * Saturation	-168.1	-1,088	145.6
	(662.9)	(1,134)	(167.3)
Non-treated Control * Saturation	301.4*	-38.14	27.54
	(168.7)	(444.1)	(59.45)
Baseline Level of Dependent Variables ($PPP)	0.264*	0.156***	0.312**
	(0.154)	(0.0480)	(0.143)
Constant	-2.585	727.0***	17.51
	(38.76)	(118.1)	(18.36)
Observations	1,286	1,286	1,286
R-squared	0.121	0.023	0.027

Notes: Standard errors clustered at the manyatta level in parentheses. $^{***}p < 0.01, ^{**}p < 0.05, ^{*}p < 0.1$.

TABLE D.5. Comparison between Midline and Endline Treatment and spillover effects on key material outcomes

	Total Business Assets (PPP$)	Reported Annual Cash Earnings (PPP$)	Amount Deposited into Savings (PPP$)
ITT Wave w1-w2	262.085***	297.661***	72.852**
	(58.977)	(106.056)	(31.949)
ITT Wave w3-w4	198.096***	36.334	55.817*
	(59.455)	(123.430)	(28.185)
ITT Wave w1-w2 * Saturation	-85.644	-839.667**	-23.539
	(223.098)	(422.423)	(124.065)
ITT Wave w3-w4 * Saturation	-115.132	-118.933	-107.897
	(289.048)	(540.528)	(124.591)
Non-treat Control * Saturation	274.164*	66.249	58.049
	(144.062)	(277.638)	(45.029)
Endline Survey Round (α_2)	-16.633	306.413**	13.553
	(36.510)	(126.010)	(18.591)
ITT Wave w1-w2 * Endline (γ^1)	117.532	-81.698	-19.264
	(130.883)	(304.927)	(51.006)
ITT Wave w3-w4 * Endline (γ^2)	218.345	6.421	-22.059
	(218.930)	(255.762)	(85.800)
ITT Wave w5-w6 * Endline (γ^3)	413.812*	231.837	0.650
	(216.179)	(340.144)	(44.433)
ITT Wave w1-w2 * Endline * Saturation (ω^1)	79.529	355.931	-39.999
	(481.849)	(1112.877)	(166.625)
ITT Wave w3-w4 * Endline * Saturation (ω^2)	154.554	231.683	169.533
	(698.724)	(826.129)	(337.872)
ITT Wave w5-w6 * Endline * Saturation (ω^3)	-166.807	-1035.879	148.528
	(670.220)	(1123.838)	(167.068)
Non-treat Control * Endline * Saturation (η^c)	24.048	-91.660	-29.832
	(163.756)	(473.432)	(71.342)
Baseline Level of Dependent Variables (PPP$)	0.192*	0.134***	0.259***
	(0.101)	(0.023)	(0.083)
Constant	17.712	434.946***	4.322
	(20.121)	(52.193)	(6.779)
R-squared	0.12	0.07	0.04
Observations	2,671	2,671	2,671

Regressions estimates from equation 4.5 for the poor group only. Household reported cash earnings were calculated by summing up the reported cash income from various sources such as sales of livestock, livestock products, crops, casual labor, salaried employment, and income from businesses, dukas, and petty trading for each household. Earnings are winsorized at 5% to avoid extreme impacts from outliers. Additionally, assets were computed by summing up the cash, stocks, assets, savings, and credits associated with each individual business owned by the respondents. Amount of savings include savings with bank, mpesa, savings groups and cash savings, does not include savings with businesses. Standard errors were clustered at the manyatta level. *** p<0.01, ** p<0.05, * p<0.1.

CHAPTER 5

Conclusion

While the "graduation" program has achieved success in other global contexts, it may face significant challenges in its mission to alleviate poverty through women's asset-building in the arduous arid and semi-arid pastoralist regions of the Horn of Africa. The BOMA Project has taken on this challenge by developing a tailored graduation program. Not only does it demonstrate its effectiveness, but it also boasts a remarkable benefit-cost ratio exceeding 2 for women who participated for two years, highlighting its viability even in demanding circumstances. Surprisingly, despite these difficulties, beneficiaries continue to experience noteworthy increases in business assets and savings even four years into the program.

However, the scope of this thesis goes beyond mere program assessment, shedding light on the intricate psychosocial mechanisms underpinning poverty alleviation efforts. Grounded in theories of poverty traps and the economics of depression, a consistent empirical finding emerges: baseline mental health significantly influences program outcomes. Beneficiaries with strong mental well-being achieve remarkable average treatment effects, whereas those grappling with severe depressive symptoms—constituting 20% of beneficiaries—experience no economic gain, depleting assets without corresponding benefits. This revelation underscores the critical interplay between psychological well-being and the success of poverty reduction interventions. These findings might help explain some of the variations in treatment impacts found in program evaluations.

Furthermore, the study unveils the potency of endogenous preferences as a behavioral mechanism. Spillover effects, catalyzed by program exposure, resonate through local kinship networks, amplifying positive impacts on women's business assets and savings. Incorporating these spillover effects significantly elevates the benefit-cost ratio from 1.7 to an impressive 2.2. Adjusting program saturation could potentially enhance this ratio even further, underscoring the nuanced nature of program implementation.

Importantly, the impact assessment encompasses individuals constrained by mental health issues from benefiting fully. To optimize cost-effectiveness, the study suggests screening potential beneficiaries for severe depressive symptoms. Alternatively, a two-track approach is probably preferred, addressing mental health concerns before participants engage in the core program.

In a broader global context, recent estimates (World Bank, 2022) underscore the urgency of poverty alleviation efforts, especially in a post-COVID-19 world. The surge in extreme poverty, coupled with disproportionate setbacks faced by the vulnerable, highlights the necessity of targeted interventions, including fiscal support.

The "graduation" approach, intertwining training, transfers, and savings promotion, consistently yields positive outcomes for the most marginalized. Its replication by governments and development agencies underscores its transformative potential. Yet, the financial challenge posed by life-skill coaching—a significant supervision expense—necessitates careful consideration. Exploring spillover effects at different network levels, village and kinship, reveals nuanced dynamics for tailored interventions.

Systemic crises or shocks might, to varying degrees, impede households from fully embracing and benefiting from programs like this. To better grasp how multi-faceted programs fare in system-wide crises, future research should explore methods to quantify these shocks. Additionally, studying the synergies between various social protection programs could yield policy tools resilient to challenging macro environments. For instance, combining livestock insurance with this multi-faceted program might offer a potential solution to safeguard the assets that women have begun to accumulate under the present program.

In conclusion, this thesis offers theoretical insights and pragmatic guidelines for effective poverty reduction. By addressing psychological well-being, leveraging spillover effects, and refining program design, stakeholders can shape interventions that usher profound change into the lives of the most marginalized. As the global fight against poverty persists, this study illuminates pathways to craft comprehensive, impactful strategies that embrace the intricate tapestry of poverty's challenges.

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