# Community Matters: Psychological Well-Being and Spillovers in an Asset-Building Anti-Poverty Program in Paraguay

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**Abstract**

Asset-building anti-poverty programs that follow BRAC's graduation approach generally yield positive average treatment effects on economic outcomes. However, these averages often mask substantial heterogeneity, and the psychological effects of such programs remain understudied. Leveraging a randomized controlled trial with a staggered rollout and saturation design, I examine how and for whom the Paraguayan government's graduation program works. Midline findings indicate that while the program improves key economic outcomes for most treated households, impacts vary widely across participants. I also find that the program worsens the psychological states of beneficiaries at midline, with measures of depression, locus of control, aspirations, and self-efficacy suggesting that the expectation for program participants to transform their livelihoods may induce stress. A saturation analysis reveals that this psychological decline seems to be attenuated in communities with a higher share of beneficiary households, highlighting the importance of community dynamics in supporting participants. Psychological factors may act as an important source of spillover effects, as beneficiaries in higher-saturation communities experience better economic outcomes than those in communities where fewer neighbors receive the program. The paper discusses what these findings imply for the cost-effective design and implementation of graduation programs.

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# 1 Introduction

Graduation programs are multifaceted interventions, initially developed by the NGO BRAC two decades ago, designed to help households escape poverty by building physical productive assets alongside intangible business and life skills. These programs provide low-income households with an asset transfer and intensive coaching to support a transition from casual wage labor to higher-earning entrepreneurial activities that offer more sustainable livelihoods. The design of such interventions aligns with the theory of multiple-equilibria poverty traps, which holds that poverty becomes self-reinforcing when the low-equilibrium behaviors of the poor perpetuate low living standards (Duflo, 2012; Balboni et al., 2022). That is, by providing assets and coaching, the approach seeks to shift households away from low-equilibrium behaviors that can perpetuate poverty—such as reliance on low-productivity work, underinvestment in assets and technology, limited financial planning, and a lack of clear goals—and toward behaviors more consistent with sustained economic progress. Theoretical support exists for strong complementarities between tangible and intangible assets that underpin these poverty traps (Barrett et al., 2019). The multifaceted nature of graduation programs seeks to build both types of assets to facilitate a livelihood transition that leads participants to a higher equilibrium and improved well-being.

In practice, this means helping participants shift behaviors away from low-return activities—such as reliance on casual wage labor or low-productivity self-employment—toward more profitable and sustainable enterprises, including raising animals, running small shops, or offering services such as laundry or food preparation. Coaching further aims to strengthen forward-looking behaviors, such as financial planning, saving, and investing in business growth, while also addressing constraints linked to aspirations, resilience, and confidence. The program thus aligns with the theory of multiple-equilibria poverty traps, which holds that poverty becomes self-reinforcing when the low-equilibrium behaviors of the poor perpetuate low living standards (Duflo, 2012; Balboni et al., 2022).

Impact evaluations from several countries show that graduation programs effectively help families begin a path out of poverty. Across studies, most have found positive effects on income, with estimated gains ranging from approximately 5% to over 30%, depending on the context and intensity of the program. (Banerjee et al., 2015a; Bandiera et al., 2017). Research also shows that these gains can persist in the long term (Banerjee et al., 2021). The strength of this evidence has led governments worldwide to adopt the graduation approach. Despite these successes, researchers have limited understanding of the mechanisms through which these interventions achieve their outcomes and which participants benefit most (Karlan, 2020).

While graduation programs can directly increase physical assets through a transfer, raising psychological assets is a more complex goal. These assets, such as self-confidence and conflict-resolution abilities, are not directly transferable, and interventions aimed at improving them can fall short or even backfire. This is particularly concerning in the context of anti-poverty programs, as psychological well-being is not only intrinsically valuable but may also be critical for sustaining economic progress. A growing literature shows how low stocks of psychological assets can lead individuals to make decisions that perpetuate poverty (De Quidt and Haushofer, 2016; Wuepper and Lybbert, 2017; Lybbert and Wydick, 2018; Moya and Carter, 2019). Such constraints can manifest through depression that reduces willingness to plan ahead, low aspirations that dampen effort, and weak self-efficacy or a low sense of agency that undermines entrepreneurial initiative and persistence.

The nature of psychological assets underscores a tension at the core of the graduation approach: while the program seeks to strengthen these assets, it can simultaneously place psychological strain on participants by setting expectations for them to rapidly transform their livelihoods. Several factors contribute to why these pressures can be burdensome. For many households, the business venture they pursue as part of the graduation program represents uncharted territory, requiring them to navigate steep learning curves. Moreover, these programs often target women, who may encounter resistance when assuming new or unconventional

roles within their communities (Fletschner and Carter, 2008). As participants attempt to apply new technical and soft skills to succeed in their business ventures, they may find that increased effort does not immediately translate into better economic prospects or improved well-being, leading to frustration.

Despite the centrality of psychological assets in the graduation approach, the ways in which the program interacts with the psychological state of participants remain understudied. In most impact evaluations, mental health is treated as a secondary outcome and reported using a single index, which can obscure variation across underlying indicators such as depression and self-efficacy.[1] Prior research has emphasized the mediating role of baseline psychological state in shaping program impacts (Correa, 2021; Zheng et al., 2023). Other work highlights the importance of the coaching component designed to transfer business and life skills, with evidence suggesting that versions of the program lacking this element yield muted outcomes (Sedlmayr et al., 2020; Banerjee et al., 2022).

Another potential pathway for building psychological assets, however, lies in social interactions and spillovers within communities. Program participants might benefit from a range of social mechanisms that build psychological assets and create an environment more conducive to success, such as exposure to positive role models, peer effects, social learning, networks of cooperation and support, and improvements in collective identity and empowerment (Feigenberg et al., 2013; Macours and Vakis, 2014; BenYishay and Mobarak, 2019; Bandiera et al., 2020; Carter et al., 2021). Beyond differences driven by baseline levels of social cohesion or networks, having more neighbors who participate in the program may itself generate spillovers by increasing opportunities for interaction, learning, and mutual support that shape psychological well-being. Yet, the literature has rarely examined how these externalities contribute to mental health outcomes.]

While this paper documents the impacts of the Paraguayan graduation program on key eco-

[1] Effect sizes on mental health range from improvements of 0.1 to 0.24 standard deviations (Ridley et al., 2020).

nomic outcomes, its primary focus is on the effects of the intervention on psychological well-being and on how community-level program coverage rates shape these outcomes through potential spillover effects. Using measures of depression, locus of control, aspirations, and self-efficacy, I examine how this asset-building anti-poverty program affects the psychological well-being of participants and whether these psychological factors contribute to spillover effects within communities.

To address these questions, I implemented a randomized controlled trial (RCT) of the graduation program led by the Paraguayan government, *Tenondera*.[2] The RCT design features a staggered rollout and a saturation component—whereby we exogenously vary the share of households participating in *Tenondera* within a community—to rigorously evaluate program impacts. This design allows me to identify two types of spillovers: those among treated households, where program impacts may vary with the number of treated neighbors but without biasing conventional estimates; and those onto non-treated households, where exposure to treated neighbors can bias those estimates. Both types of spillovers matter for optimal program design, since they affect how benefits accumulate among participants and whether non-participants are influenced in unintended ways. Spillovers may arise for both economic and psychological outcomes, though the mechanisms are likely to differ substantially between domains.

Midline findings reveal promising average treatment effects in conventional economic outcomes. Ten months into the program, treated households saw their assets rise by 60%, monthly per capita income by 7%, and savings by 32% as a result of program participation. These averages, however, mask substantial impact heterogeneity. A conditional quantile treatment analysis shows that about 25% of beneficiaries experienced no significant income gains, and 10% saw no significant asset gains (even though all participants received direct asset transfers). Additionally, the top quartile of participants recorded asset increases 6.7 times larger than those

[2]The program name means "onward" in Guarani, an indigenous language widely spoken in Paraguay and one of the country's two official languages.

in the bottom quartile. These disparities underscore how differently households are affected by the program and are consistent with findings from the broader literature (Bandiera et al., 2017). Unlike previous studies, however, I find no compelling evidence that baseline psychological characteristics explain this variation.

In contrast to the improvements in economic outcomes, when examining these psychological variables as program outcomes, I find a decline in psychological well-being by midline. The share of beneficiaries with symptoms of depression rose by 6%, those with low internality (a locus of control subscale) by 7%, with low aspirations by 5%, and with low self-efficacy by 11%. I further explore these patterns in relation to social dynamics, leveraging variation in community saturation rates to capture exposure to neighbors who also receive the program. Because psychological assets are nonrival, improvements in, for instance, confidence among some participants can spill over as others observe and learn from them, allowing neighbors to build up their own psychological assets without diminishing those of the first. Consistent with this intuition, the analysis reveals evidence of spillovers in high-saturation communities, where a larger share of neighbors participate in *Tenondera*.

Higher saturation rates positively impact the economic outcomes of treated households, as beneficiaries in these communities perform better than those in communities where fewer neighbors receive the program. For psychological outcomes, I find suggestive evidence that the deterioration in well-being among treated households is less severe in higher-saturation communities, indicating that psychological factors may be a significant source of spillovers. These findings suggest that mechanisms such as role-model effects, peer learning, reciprocity, and collective identity may play an important role in shaping program impacts. Such community-level externalities, currently underappreciated within the graduation approach, have important implications for its design and implementation.

The rest of the paper is organized as follows. Section 2 introduces the program intervention, describes the research design, and provides details on the integrity of the RCT implementation.

Section 3 presents the empirical strategy and main findings, including both standard impact estimates and the saturation analysis. Section 4 concludes.

# 2 Study Framework

This paper uses data from an RCT of Paraguay's government-led graduation program, *Tenondera*, designed and implemented in collaboration with the Ministry of Social Development (MDS) and local partners Fundación Capital and Instituto Desarrollo. The RCT includes three planned waves of data collection: a 2021 baseline, a 2022 midline, and a 2024 endline, with each household intended to be surveyed in all three rounds. The analysis in this paper relies on the baseline and midline data, as the endline data were not yet available at the time of writing.

## 2.1 Program Intervention

*Tenondera* was first implemented as a pilot program in 2014 and is currently undergoing a major scale-up in previously unreached areas of Paraguay. To be eligible, households must be participants in the government's conditional cash transfer (CCT) program, which delivers 72 monthly payments over a fixed six-year cycle, after which benefits end.[3] Starting in year four—the second half of the CCT program schedule—households are invited to join *Tenondera*, based on the government's geographic prioritization strategies.[4] The program aims to support households in strengthening their livelihoods so that they can sustain themselves economically after they stop receiving CCT payments.[5] Following the model of other comprehensive graduation programs, *Tenondera* adopts a dual strategy of providing tangible business assets while

[3]Eligibility for the CCT program is based on geographic targeting and a household-level proxy means test. To be invited to participate, households must include children, youth, pregnant women, persons with disabilities, or be of indigenous status.

[4]Because *Tenondera* is offered during the latter half of the CCT cycle, many participating households still receive the CCT when they enroll. However, the timing of entry implies that some households may continue receiving the CCT alongside *Tenondera*, while others may complete the CCT cycle before their *Tenondera* participation ends.

[5]To the best of my knowledge, layering a graduation program on top of a CCT is a relatively uncommon practice, with experiences concentrated in Latin America—most notably *Haku Wiñay* in Peru and *Emprendiendo una Vida Mejor* in Honduras.

simultaneously developing intangible skills through intensive mentoring.

The total program duration is 24 months. *Tenondera* begins with a 4- to 6-week induction stage, during which MDS social workers, known as socioeconomic promoters, introduce households to the program. During this period, beneficiaries and promoters collaborate in group workshops to create a business profile and a life plan for each household. These documents outline the goals households aim to achieve through the program. The seed capital transfer occurs shortly afterward, around the third month. The one-time transfer amounts to 3 million Paraguayan guaranis (about USD 390) per household.[6] Participants then have two weeks to use the funds to purchase assets that will support an income-generating activity of their choice.[7] The remainder of the program consists of periodic coaching sessions that cover technical skills for livelihood management, financial topics such as budgeting, bookkeeping, and saving, as well as life skills aimed at building confidence and strengthening decision-making.

During the coaching stage, promoters are responsible for identifying the needs, aspirations, and constraints of each household and providing tailored support based on their observations. The program intends to help vulnerable households overcome barriers to success across multiple dimensions, including low productivity, limited access to technology, credit constraints, poor market access, and psychological barriers. Coaching takes place through both group workshops and individual home visits. The group workshops cover eight topics: (i) life plan, (ii) business idea, (iii) business costing, (iv) financial planning and cash flow, (v) savings, debt, and payment methods, (vi) sales and marketing, (vii) assertive communication, and (viii) resilience. The individual home visits are intended to reinforce specific topics where households may require additional support.[8]

[6]The transfer amount is identical across households, does not vary by activity type or investment needs, and does not require participant co-investment.

[7]Common purchases include animals, products to stock general small-scale stores or specialized businesses such as bakeries, and materials for providing services such as laundry or manicures.

[8]Group workshops are organized at the community level. Promoter workload–determined by the number of communities and households each promoter covers—varies widely, and promoters exercise substantial discretion in how workshops are delivered. While the program design calls for eight separate sessions, in practice promoters often adapt the format: in some communities all eight are held, in others topics are combined into fewer meetings, and group size is adjusted depending on how many households the promoter decides to bring together. Workshops

Promoters are assigned geographically, with each promoter responsible for enrolling and coaching all households within their designated area. They implement the program in quarterly waves, meaning that at any given time, a promoter might be working with households at different stages of *Tenondera* within the same area.

According to MDS administrative data, in nearly 80% of households, the main *Tenondera* enrollee is a woman. Although the program is not explicitly targeted toward women, the CCT program from which *Tenondera* beneficiaries are drawn prioritizes registering a woman as the household recipient. While women are commonly the main beneficiaries on paper, business ventures are often run jointly with their partners in practice.

Unlike other graduation programs, *Tenondera* includes both rural and urban households. This broader coverage results in a diverse range of investment choices for the seed capital transfer. In cohorts prior to this study, around 75% of beneficiary households used their transfer for raising livestock, primarily dairy cattle, chicken, and pigs. Most of the remaining households invested in small-scale retail stores, while a small minority pursued other ventures such as agricultural production or artisanal handcrafts.

## 2.2 Research Design

This study was implemented in three departments of Paraguay—Caaguazu, Caazapa, and San Pedro—which were designated as priority zones by MDS, as shown in Figure 1. The staggered design leverages a five-year national scale-up plan for *Tenondera*, under which cohorts are enrolled in quarterly waves. All households in the study sample were, or will be, offered the opportunity to join the program, with randomization determining the timing of each invitation.

To construct the study sample, MDS initially provided a shortlist of potential participating households. Eligibility for inclusion on the shortlist required that the household: (i) was lo-

are concentrated in the first few months of the program and are expected to be completed by month 10. After the workshops, coaching continues through periodic home visits, whose frequency and timing are also determined largely by promoter discretion, with workload and scheduling being the main constraints. Promoters formally stop working with households once the two years of the program are completed.

Figure 1: Departments of Paraguay Included in the Sample




cated in a district within one of the three priority departments; (ii) had a *Tenondera* promoter available in the district to fully implement the program; (iii) was a recipient of the CCT program and in the second half of its transfer cycle at the time; (iv) was not receiving other government transfers; and (v) had not previously participated in *Tenondera*. During the baseline survey, enumerators attempted to visit every household on the shortlist. The final study sample consists of the 2,864 households that were successfully located.[9]

Through a multi-stage randomization procedure, I assigned each household in the sample to one of three groups: Early Treatment, Middle Treatment, and Late Treatment. Figure 2 summarizes the study timeline. Early Treatment households were offered the program in the first quarter of 2022, immediately following the baseline survey. Middle Treatment households were offered the program one year later, in the first quarter of 2023, following the midline

[9]Several communities within the three departments had already received the program under the government's earlier prioritization strategies. Our RCT focuses on communities where *Tenondera* had not yet been implemented. Within these communities, all eligible households were included on the shortlist and targeted for the baseline survey.

Figure 2: Study Timeline




survey. Late Treatment households will be offered the program only after the study concludes, in the first quarter of 2025.[10] At midline, Early Treatment households had been enrolled in the program for 10 months. Since *Tenondera* is front-loaded in its delivery of core components, this stage represents an appropriate point at which to measure early program impacts.

In the first stage of the randomization procedure, I assigned each of the 246 neighborhoods or localities in the sample (which I will refer to as communities) to a specific saturation scheme, as summarized in Tables 1 and 2. That is, within each community, I randomly determined the proportion of households to be assigned to each of the three treatment groups. In the second stage, I assigned individual households to their treatment group according to the saturation scheme designated for their community.

Specifically, I imposed variation in the shares of households assigned to each treatment group across communities, creating variable "saturation rates". Here, the saturation rate is a community-level measure, defined as the share of study households within a community that have been offered the program at a given point in time. In some communities, all sampled households were treated early into the study, resulting in a 100% saturation rate at midline. At the other extreme, in communities where no households received treatment before the end of the study, the midline saturation rate is 0%. This setup generates random variation in the extent to which study households are exposed to neighbors participating in *Tenondera*, allowing

[10]Middle Treatment and Late Treatment households were not officially informed at baseline that they would join *Tenondera* at a given date. Some may nonetheless have anticipated eventual participation, given that other CCT households in their communities were already starting to receive the program. To assess this possibility, the endline survey includes new questions on anticipation effects.

Table 1: Randomization Schemes in the Saturation Design Group

| Scheme | Share of households in Early Treatment | Share of households in Middle Treatment | Share of households in Late Treatment | Number of communities |
|---|---|---|---|---|
| A | 100% | 0% | 0% | 22 |
| B | 80% | 20% | 0% | 22 |
| C | 20% | 80% | 0% | 21 |
| D | 0% | 80% | 20% | 20 |
| E | 0% | 20% | 80% | 22 |
| F | 0% | 0% | 100% | 22 |

Table 2: Randomization Schemes in the Non-Saturation Design Group

| Scheme | Share of households in Early Treatment | Share of households in Middle Treatment | Share of households in Late Treatment | Number of communities |
|---|---|---|---|---|
| G | 67% | 33% | 0% | 59 |
| H | 0% | 33% | 67% | 60 |

for the estimation of spillover effects.

Tables 1 and 2 present the first-stage randomization allocations, including the number of communities assigned to each scheme. I conducted this step separately for two subsamples, based on community population size. In both high- and low-population communities, a preliminary analysis showed that saturation assignment would not translate monotonically into spillover exposure, meaning that applying the same scheme to two communities of markedly different sizes would result in different levels of exposure to neighbors participating in *Tenondera*. To address this issue, I defined a saturation design group consisting of mid-sized communities (between 50 and 200 households), where a given saturation scheme would more consistently yield comparable levels of spillover exposure across communities.[11] This group

[11] Saturation in this RCT is defined relative to *Tenondera*-eligible households, as randomization determines the proportion of eligible households assigned to Early, Middle, and Late Treatment groups. This is the relevant definition for the analysis, since the spillovers of interest arise among program-eligible households, not among all households in the community. Importantly, this RCT includes all eligible households within the sample communities, so concerns that additional program-eligible households outside the sample might alter saturation rates do not apply.

An alternative definition of saturation—based on the fraction of all households in a community that are offered the program—can differ substantially from the measure I use whenever the share of eligible households varies across communities. Indeed, the only case in which the two saturation definitions coincide is if the fraction of eligible households is identical across communities, which is not true in practice. Table A1 in Appendix Adocuments this heterogeneity in eligibility rates across communities by reporting the mean number of eligible households per community, the mean share of all households that are eligible, and the coefficient of variation (CV) of eligibility shares across communities. The results show that eligibility rates vary meaningfully across communities, as expected. Moreover, CVs are systematically higher in the non-saturation design group (Schemes *G* and *H*) com-

includes six schemes, ranging from full Early Treatment assignment (Scheme *A*) to full Late Treatment assignment (Scheme *F*). The non-saturation design group comprises the largest and smallest communities and includes two schemes.[12] The randomization procedure for the non-saturation design group followed the same steps as in the saturation design group.

## 2.3 Randomization Checks

In Table 3, I present baseline balance tests for the outcome variables used in the study, along with key household demographic characteristics. The table reports mean comparisons and t-tests for equality of means between treatment and control groups, based on household assignment status at midline (i.e., "Treatment" refers to the Early Treatment group, while "Control" refers to the Middle Treatment and Late Treatment groups). Overall, the sample appears appropriately balanced. All outcome variables and most household characteristics show no significant differences at baseline. The only measure with a significant difference at the 1% level is rural location, with a higher share of treatment households residing in rural areas compared to control households. However, a joint test of equality of means across all variables yields a p-value of 0.38, indicating that balance across groups cannot be rejected.

Table A2 in Appendix B provides a survey attrition analysis for the midline round. Follow-up was largely successful, with 90% of baseline households resurveyed at midline. I find no significant differences in attrition rates between the treatment and control groups. In the most saturated specification used to test for attrition-related imbalances, the joint test fails to reject the null hypothesis that the treatment indicator and all interactions between baseline outcomes and treatment status are equal to zero (p-value $= 0.30$). This result supports the conclusion

pared to the saturation design group (Schemes *A*–*F*), reflecting greater heterogeneity in eligibility rates among the largest and smallest communities. This variation underscores why defining saturation relative to eligible households is preferred, so that the measure captures exposure to program-eligible neighbors who are treated and is not confounded by the variation in how many households in the community are program-eligible.

[12] Schemes were constructed as shown to fulfill multiple objectives: to generate variation in saturation rates at both midline and endline; to ensure, per MDS request, that no community had households receiving the program at widely different times; and to maintain a roughly even distribution of households across the three treatment groups.

Table 3: Baseline Balance

| Variable | (1) Control Mean/(SE) | (2) Treatment Mean/(SE) | T-test P-value (1)−(2) |
|---|---|---|---|
| Monthly per capita income | 59.50 | 57.24 | 0.19 |
| | (1.01) | (1.37) | |
| Household business assets | 415.41 | 452.32 | 0.22 |
| | (18.01) | (22.95) | |
| Household savings | 9.18 | 10.10 | 0.60 |
| | (0.98) | (1.49) | |
| CES-D 10 score | 6.33 | 6.24 | 0.63 |
| | (0.11) | (0.15) | |
| Depressed (%) | 0.21 | 0.19 | 0.13 |
| | (0.01) | (0.01) | |
| Aspirations index | 0.38 | 0.36 | 0.26 |
| | (0.01) | (0.02) | |
| Low aspirations (%) | 0.38 | 0.36 | 0.26 |
| | (0.01) | (0.02) | |
| Self-efficacy index | 0.29 | 0.29 | 0.97 |
| | (0.04) | (0.05) | |
| Low self-efficacy (%) | 0.29 | 0.29 | 0.99 |
| | (0.01) | (0.01) | |
| Internality score | 17.81 | 17.95 | 0.39 |
| | (0.09) | (0.13) | |
| Low internality (%) | 0.19 | 0.18 | 0.32 |
| | (0.01) | (0.01) | |
| Mental health index | 0.40 | 0.45 | 0.53 |
| | (0.04) | (0.06) | |
| Poor mental health (%) | 0.32 | 0.31 | 0.56 |
| | (0.01) | (0.02) | |
| HH in rural area (%) | 0.60 | 0.66 | 0.01*** |
| | (0.01) | (0.02) | |
| Number of HH members | 4.25 | 4.35 | 0.13 |
| | (0.04) | (0.05) | |
| Homeownership (%) | 0.76 | 0.77 | 0.46 |
| | (0.01) | (0.01) | |
| Respondent is married (%) | 0.60 | 0.61 | 0.75 |
| | (0.01) | (0.02) | |
| Observations | 1,918 | 946 | |

*Note*: Monetary variables are in USD (exchange rate: 1 USD = 7,692 Paraguayan guaranis). Monthly income per capita, household business assets, and savings are winsorized at the 95th percentile. The F-statistic for the joint test of baseline balance is 1.07 (p-value = 0.38). Cutoffs for “depressed" and “low internality" status are 10 and 15 points, respectively. P-values are from two-sided t-tests. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

that attrition did not differentially affect the composition of the treatment and control groups.

MDS demonstrated a high level of compliance with treatment assignment during program implementation. By midline, treatment status was upheld for 94% of households. Among those assigned to Early Treatment and scheduled to receive *Tenondera* in the first quarter of 2022, 10% had not yet received the program. Additionally, 4% of households in the Middle Treatment and Late Treatment groups received the program in early 2022, despite not being scheduled to do so.

# 3 Results

First, I present the empirical results of the RCT using a standard approach that does not account for spillover effects. In Section 3.1, I estimate average treatment effects for economic outcomes, comparing households in the Early Treatment group to households in the Middle and Late Treatment groups, which together serve as the control group, under the assumption of no spillovers. Section 3.2 then examines psychological outcomes of *Tenondera*, again presenting average treatment effects using this same comparison. Next, in Section 3.3, I show results that do account for spillovers, based on the variation in saturation intensity across communities, for both economic and psychological variables. In this approach, I distinguish between control households in communities with non-zero saturation at midline and those in communities with zero saturation at midline. The latter group, which I refer to as the "super-control," corresponds to households in Schemes *D*, *E*, *F*, and *H* in Tables 1 and 2. Finally, in Section 3.4, I document the existing heterogeneity in economic impacts and discuss psychological factors as a potential source.

Table 4: Average Treatment Effects for Economic Outcomes (USD)

| Variable | ITT Estimate |
|---|---|
| Monthly per capita income | 4.00* |
| | (2.39) |
| Household business assets | 255.71*** |
| | (33.93) |
| Household savings | 3.06* |
| | (1.72) |
| Observations | 2,584 |
| District FEs | ✓ |

*Note:* Baseline mean values of monthly per capita income, household business assets, and household savings are 58.75, 427.60, and 9.49 USD, respectively. For each regression, only the ITT impact parameter is reported. All regressions control for baseline levels of the dependent variable. Standard errors in parentheses are clustered at the community level. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

## 3.1 Average Treatment Effects for Economic Outcomes

For this initial set of treatment effect results, I estimate a classic intent-to-treat (ITT) ANCOVA model:

$$y_{hd} = \alpha_0 + \alpha_1 y_{hd}^0 + \beta Treat_{hd} + \gamma_d + \varepsilon_{hd}, \tag{1}$$

where $y_{hd}$ denotes the outcome of interest at midline for household $h$ in district $d$, $y_{hd}^0$ is the baseline value of the same variable, and $Treat_{hd}$ is an indicator for assignment to *Tenondera* as part of the Early Treatment group. The error term $\varepsilon_{hd}$ is clustered at the community level, and $\gamma_d$ represents district fixed effects in my preferred specification.[13] Under this specification, the control group comprises households eligible for *Tenondera* but assigned to either Middle Treatment or Late Treatment. The parameter $\beta$ identifies the ITT effect of the program, under the assumption of no spillovers between treatment and control households. I relax this assumption in Section 3.3.

For key economic outcomes, Table 4 presents OLS estimates of the ITT impact parameter

[13]District fixed effects capture, among other factors, variation in state capacity across districts. For example, some districts are covered by more *Tenondera* promoters than others, implying variation in promoter workload and thus their availability to support beneficiaries.

from Equation 1. At midline—10 months into *Tenondera*'s 24-month duration—offering the program raised monthly per capita income by USD 4, business assets by USD 255.71, and savings by USD 3.06, relative to control households at midline. These effects are statistically significant at the 10%, 1%, and 10% levels, respectively, and represent gains of 6.8%, 59.8%, and 32.2% when expressed relative to the corresponding baseline means.[14]

The seed capital transfer provided by *Tenondera* amounts to USD 390 per household. Comparing this figure to the estimated treatment effects suggests that, on average, some depletion of purchased assets occurred. Households retained about two-thirds of the value of the assets they initially received. At the same time, the observed increases in income and savings point to a partial transformation of the initial asset transfer into these other economic gains.

## 3.2 Average Treatment Effects for Psychological Outcomes

*Tenondera* seeks to build intangible psychological assets by incorporating life skills training into its workshop curriculum, with the goal of strengthening the confidence and decision-making abilities of participants. For instance, the training material for the group workshops that kick-start the program emphasizes improving goal-setting, resilience to setbacks, and communication skills. Accordingly, beyond analyzing economic outcomes, this study also evaluates the impact of the program on psychological dimensions, including depression, locus of control, aspirations, and self-efficacy. While some of these outcomes can be viewed as directly related to the program's objectives, others serve more broadly as important measures of individual well-being, even if they are not explicit program goals. Overall, if the life skills training and coaching components are effective, one would expect to observe improvements in measures of psychological well-being. Appendix C presents histograms of baseline psychological measures, revealing substantial variation across households prior to program implementation. The appendix also includes supporting evidence on the validity of these measures, including cross-

[14] In standard deviation units relative to the control group at baseline, these impacts amount to 0.09 SD for income, 0.32 SD for business assets, and 0.07 SD for savings.

sectional correlations across constructs and test–retest correlations between survey rounds.

Depression is measured using the Center for Epidemiological Studies Depression (CES-D) scale (Radloff, 1977), widely used in studies assessing the impact of economic interventions on mental health. The scale consists of 10 questions about emotions and general well-being experienced by respondents during the week preceding the survey, capturing symptoms consistent with depression. In the baseline sample, CES-D scores range from 0 to 28, with higher scores indicating more severe depressive symptoms. I use a commonly applied cutoff of 10, above which respondents are classified as exhibiting symptoms consistent with clinical depression (Andresen et al., 1994). Based on this threshold, 20.67% of respondents in the baseline sample are categorized as depressed.

The locus of control construct comprises three subscales: *internality*, which measures the confidence of an individual in their abilities and belief in personal control over life outcomes; *powerful others*, which captures the extent to which one perceives their life as being controlled by people with greater power or advantage; and *chance*, which assesses the degree to which one attributes life events to luck or randomness (Levenson, 1981). In the empirical analysis that follows, I focus on the internality subscale, which aligns most directly to the objectives of *Tenondera*, as the program seeks to strengthen personal agency and entrepreneurial initiative. In the baseline sample, internality scores range from 0 to 24, with higher scores reflecting a stronger sense of personal control over one's life. I use a cutoff of 15, below which respondents are classified as having low internality. Based on this threshold, 18.58% of respondents fall into the low internality category at baseline.

Aspirations are measured using an index constructed from a series of statements regarding respondents' satisfaction with their current situation and their intentions for business growth or improvement. The survey items are adapted from Lybbert and Wydick (2019). Lastly, self-efficacy is quantified using an index based on respondents' perceived capabilities and confidence in achieving their goals, drawing from the New General Self-efficacy Scale developed

by Chen et al. (2001). Based on the distribution of index values, I define natural cutoffs and find that, at baseline, 37.50% of respondents exhibit low aspirations, while 29.16% report low self-efficacy.

In addition to analyzing each psychological variable separately, I construct a composite mental health index that aggregates standardized values of depression, internality, aspirations, and self-efficacy. This index provides a summary measure of overall psychological well-being and allows for a more general assessment of the impact of the program on mental health.[15] The share of respondents with poor mental health at baseline is 31.81%.[16]

Table 5 presents average treatment effects on depression, aspirations, self-efficacy, internality, and an overall mental health index, estimated using the specification in Equation 1. Panel A reports effects on continuous scores, while Panel B categorizes participants based on the thresholds for low psychological states defined earlier. Contrary to expectations derived from the design of the program, the results reveal a consistent pattern: on average, participants exhibit a deterioration in psychological well-being at this stage of the program. Specifically, offering the program increased the probability of being classified as depressed, and as having low aspirations, low self-efficacy, and low internality, by 6, 5, 11, and 7 percentage points, respectively. It also increased the probability of being classified as having low overall mental health, based on the index, by 7 percentage points.[17] These findings suggest that, while the program's economic benefits are evident, its psychological effects are more complex and merit

[15] I constructed indices for aspirations, self-efficacy, and mental health using the first principal component from each corresponding set of survey items. I recoded any necessary variables so that higher values reflected more positive psychological states. I used unrotated principal component analysis on standardized inputs, retaining the first component as a summary measure of each construct, which explained 46.2% of the total variance for aspirations, 52.7% for self-efficacy, and 23.0% for mental health.

[16] For depression, I rely on the standard CES-D cutoff of 10 established in the literature. For the other psychological variables, I set thresholds so that the share of respondents classified as having low scores is comparable to the share classified as depressed, thereby providing a consistent benchmark across indicators. Results presented in this subsection are robust to the use of alternative but similar thresholds. Moreover, beyond the choice of cutoffs, the findings based on raw scores are consistent with those from the classification variables, with statistically significant impacts across every indicator.

[17] When expressed in standard deviation units relative to the control group at baseline, treatment effects on raw scores correspond to a 0.10 SD increase in depressive symptoms, a 0.17 SD decrease in aspirations, a 0.30 SD decrease in self-efficacy, a 0.27 SD decrease in internality, and a 0.34 SD decrease in overall mental health.

Table 5: Average Treatment Effects for Psychological Outcomes

| Variable | ITT Estimate |
|---|---|
| **Panel A** | |
| CES-D 10 Score | 0.48** |
| | (0.23) |
| Aspirations | -0.22** |
| | (0.10) |
| Self-Efficacy | -0.49*** |
| | (0.13) |
| Internality | -1.07*** |
| | (0.32) |
| Mental Health Index | -0.64*** |
| | (0.18) |
| **Panel B** | |
| Depressed (percentage points) | 5.71** |
| | (2.20) |
| Low Aspirations (percentage points) | 5.25* |
| | (2.73) |
| Low Self-Efficacy (percentage points) | 10.41*** |
| | (2.80) |
| Low Internality (percentage points) | 7.17** |
| | (2.89) |
| Low Mental Health (percentage points) | 7.27*** |
| | (2.71) |
| Observations | 2,046 |
| District FEs | ✓ |

*Note:* Regressions are run on the subsample in which the same respondent was surveyed in both rounds. For each regression, only the ITT impact parameter is reported. All regressions control for baseline levels of the dependent variable. Standard errors in parentheses are clustered at the community level. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

closer examination.[18]

Several potential explanations may account for this decline. One possibility is that the program imposes burdensome expectations on participants to rapidly transform their livelihoods through business success. This pressure may generate distress, particularly when early efforts do not lead to meaningful returns. At midline, many participants are still acquiring and apply-

[18]This decline, though it may appear surprising, is in line with results from some other development interventions, indicating that programs requiring new responsibilities or investments can temporarily worsen psychological well-being even as they improve economic circumstances. For example, an RCT of expanded microcredit access in South Africa found increases in perceived stress despite income gains (Karlan and Zinman, 2010).

ing new technical and life skills, and progress may feel slow or uncertain. The resulting gap between effort and reward could lead beneficiaries to internalize their struggles as personal failures rather than structural or programmatic challenges, possibly explaining the decline in well-being observed at this stage. Another explanation involves implementation shortcomings, particularly in the delivery of life skills training content intended to support psychological resilience. Finally, anticipation effects among non-beneficiaries could also be at play. Individuals not yet enrolled in the program may have experienced temporary psychological gains driven by optimism about their future inclusion in the program, making beneficiaries appear worse off by comparison. Questions newly added to the endline survey will support a more detailed investigation of these potential mechanisms.[19]

It is important to acknowledge that program impacts on these psychological dimensions may be complex. Taking on new responsibilities and risks could initially heighten stress or anxiety, and effects may follow non-monotonic trajectories as households adjust over time. Moreover, psychological and economic outcomes are likely to interact—for instance, declines in well-being could worsen short-run economic gains, while improvements in confidence or aspirations could reinforce them. In this paper, I analyze economic and psychological outcomes separately, but understanding these feedback loops and their longer-term evolution remains an important direction for future research.

## 3.3 Spillover Analysis

Having examined standard ITT estimates that do not account for spillovers, I now leverage the saturation design of the study to investigate the presence and magnitude of such effects. Spillovers can bias program effect estimates when using standard impact evaluation techniques. The saturation design enables the identification of spillovers by comparing outcomes

[19] To better distinguish between the mechanisms behind the midline decline in psychological well-being, the endline survey includes new questions on program exposure and anticipation. These questions record the frequency and timing of training and home visits, as well as respondents' expectations and behavioral changes prior to receiving *Tenondera*, which will allow us to assess whether implementation gaps or anticipation effects contributed to the midline results.

for both treated and indirectly treated households—those that did not receive treatment but have treated neighbors within their communities—to a "super-control" group consisting of households in communities with no program exposure at midline (Schemes *D*, *E*, *F*, and *H* in Tables 1 and 2). For each community, I calculate the midline saturation rate, defined as the number of households assigned to treatment by midline divided by the total number of households in the sample within the community. The super-control group corresponds to communities with a midline saturation rate of zero.

The imposed saturation intensities (outlined in Tables 1 and 2) were designed to yield a limited set of midline saturation rates. In practice, however, these values served as targets, and implementation processes introduced greater variability in the actual rates observed across communities.[20] Consequently, the saturation rate is treated as a continuous variable in the analysis, ranging from 0 to 1, with a mean of 0.32 and a standard deviation of 0.37.

Based on their assigned saturation rate at midline, I categorize communities into four groups: zero, low, medium, and high saturation. This categorical variable allows for the examination of spillover effects across different levels of program exposure.[21] While a similar analysis could be conducted using saturation rate as a continuous variable instead, the categorical approach offers several advantages. The categorical variable helps uncover potential nonlinearities in spillover effects and facilitates a more straightforward interpretation of the estimated impacts from a policy perspective.

To assess the ITT impact of *Tenondera* assignment and the spillover effects on both treated and non-treated households, I estimate the following modified version of Equation 1, adapted

[20] For example, in Scheme *B*, while the target midline saturation rate was 0.8, the average among study households in these communities is 0.81, with a standard deviation of 0.09. Other schemes exhibit similar patterns.

[21] Using the distribution of midline saturation rates (see Appendix D), I set cutoff points at 0.6 to distinguish low from medium saturation and 0.75 to separate medium from high saturation, resulting in 51% of sampled households falling into the zero group, 9% into the low group, 22% into the medium group, and 18% into the high group. The results presented in this section are robust to the use of alternative, similar cutoff values. A discussion of baseline balance across saturation groups is provided in Appendix E.

from Baird et al. (2018):

$$y_{hd} = \alpha_0 + \alpha_1 y_{hd}^0 + \Sigma_{s\in(Low,Medium,High)} S_s \times \left[\theta_{0,s} + \theta_{1,s} Treat_{hd}\right] + \gamma_d + \varepsilon_{hd}, \quad (2)$$

where $S_s$ denotes a set of indicator variables that turn on when household $h$ is in a community assigned to saturation group $s$. The omitted category is the zero-saturation group, which serves as the reference group for estimation. In this setup, $\theta_{0,s}$ captures spillover effects for a control household in a community assigned to saturation group $s$, while $\theta_{1,s}$ represents the impact on a treated household in the same group.

Table 6 displays the estimated direct treatment and spillover effects for key economic outcomes using the full study sample.[22] The top panel of impacts on control households reports spillover effects on non-treated households in treated communities, corresponding to $\theta_{0,Low}$, $\theta_{0,Medium}$, and $\theta_{0,High}$, respectively, each measured relative to non-treated households in zero-saturation communities. The bottom panel of impacts on treatment households displays the treatment effects on households in each saturation group, corresponding to $\theta_{1,Low}$, $\theta_{1,Medium}$, and $\theta_{1,High}$, respectively, each measured relative to non-treated households in the same group. Differences across these treatment effects capture whether program impacts vary with saturation intensity—for instance, for income, $\theta_{1,High} - \theta_{1,Low} = 13.91$ reflects the additional gain for treated households in high-saturation versus low-saturation communities, while $\theta_{1,Medium} - \theta_{1,Low} = 5.12$ captures the same comparison for medium-saturation versus low-saturation communities. The constant term captures the expected value of the outcome for a household in the super-control group (households in zero-saturation communities) with a baseline outcome of zero and located in the omitted district, given the inclusion of baseline controls and district fixed effects.

Among the monthly per capita income estimates, two coefficients are statistically signifi-

[22] Results using only the saturation design subsample are provided in Appendix F, though these are less precise due to the smaller sample size.

Table 6: Impacts and Spillovers at Different Saturation Categories for Economic Outcomes (USD), Full Sample

| | Monthly per capita income | | Household business assets | |
|---|---|---|---|---|
| *Impacts on Control Households* | | | | |
| Low Saturation | 2.97 | (4.92) | 98.57* | (52.76) |
| Medium Saturation | 4.53 | (4.05) | -67.40* | (39.63) |
| High Saturation | -9.89* | (5.85) | -34.01 | (54.60) |
| *Impacts on Treatment Households* | | | | |
| Low Saturation | -1.61 | (4.63) | 190.50** | (92.43) |
| Medium Saturation | 3.50 | (4.61) | 315.79*** | (48.97) |
| High Saturation | 12.30* | (5.46) | 297.79*** | (68.77) |
| *Other Parameters* | | | | |
| Baseline Level of Outcome | 0.28*** | (0.02) | 0.38*** | (0.06) |
| Constant | 40.79*** | (2.00) | 205.09*** | (33.00) |
| Observations | 2,573 | | 2,573 | |
| District FEs | ✓ | | ✓ | |

*Note:* The rows for impacts on control households correspond to estimates of $\theta_{0,Low}$, $\theta_{0,Medium}$, and $\theta_{0,High}$, respectively, which measure spillover effects on non-treated households in each saturation group relative to non-treated households in zero-saturation communities. The rows for impacts on treatment households correspond to estimates of $\theta_{1,Low}$, $\theta_{1,Medium}$, and $\theta_{1,High}$, respectively, which measure the effect of treatment relative to non-treated households in the same saturation group. Share of households in each saturation group are 51% for zero, 9% for low, 22% for medium, and 18% for high. Standard errors in parentheses are clustered at the neighborhood/locality level. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

cant at the 10% level, both corresponding to high-saturation communities. Households in these communities—where a large share of neighbors participate in *Tenondera*—experience contrasting outcomes depending on their own treatment status. Non-treated households in high-saturation communities show significantly lower income relative to super-control households and also fare worse than non-treated households in communities with lower saturation levels. In contrast, treated households in high-saturation communities earn significantly more than treated households in low-saturation communities. Notably, income for treated households in low-saturation and medium-saturation communities is statistically indistinguishable from that of their non-treated counterparts. Results for household business assets follow a similar pattern: high and medium saturation levels lead to stronger economic gains among treated households, but negative spillover effects for non-treated households.

An asset-building anti-poverty program can generate spillover effects through multiple channels. Some spillovers may be pecuniary, occurring when increased program coverage affects the returns others receive from their own economic activities. These effects can be negative—arising from congestion or competition—or positive, through agglomeration benefits when economic activities cluster and complement one another. Spillovers may also have a psychosocial dimension: psychological assets such as confidence and aspirations are non-rival and can permeate across individuals within a community, potentially amplifying the impact of a program beyond its direct beneficiaries.

While market forces may contribute to the spillovers observed in this section, they are unlikely to fully explain the patterns. The program does not seem to generate a broad-based agglomeration effect that lifts all households equally within the community. Instead, a substantial share of spillovers may be psychosocial in nature.

The differential effects by saturation level suggest that psychosocial spillovers could play a central role in shaping both positive and negative community dynamics. In high-saturation communities, several social mechanisms may be at work. These include the presence of role models, peer effects, social learning, and shared norms—all of which can reinforce program messages and behaviors among participants. When many neighbors engage in the same program, they may form a network that helps participants internalize aspirations, build confidence, and sustain motivation through companionship and mutual encouragement. These community dynamics can amplify program impacts by fostering conditions conducive to livelihood transformation.

However, high levels of program coverage may also generate adverse spillovers for non-beneficiaries. When many neighbors appear to be receiving support or making visible progress, those left out may experience frustration, envy, or feelings of exclusion. These feelings can stem from a relatively disadvantaged position within a changing social landscape, shaped by social comparison, altered community hierarchies, or reduced access to shared informal resources. As

a result, some households may even disengage from common spaces or networks, potentially weakening the broader social fabric.

The presence of both positive and negative spillovers within the same community underscores the importance of saturation not only as a design parameter, but also as a potential source of impact heterogeneity, as I further discuss in Section 3.4. A deeper understanding of these mechanisms is essential for interpreting program effects and for designing interventions that are both inclusive and responsive to community dynamics.

The hypothesis that psychosocial factors are an important source of spillovers finds support when examining psychological outcomes. Table 7 reports results from regressions of the same structure as Equation 2 for different psychological well-being indicators. I find that treated households fare better on psychological measures in high-saturation communities relative to those in low-saturation communities, while the opposite pattern appears for non-treated households. Specifically, the prevalence of depression, low self-efficacy, and overall poor mental health is lower among treated households in high-saturation communities compared to their counterparts in low-saturation communities. This pattern does not hold for non-treated households, suggesting that positive psychosocial spillovers are concentrated among program participants. These findings should be interpreted as suggestive, however, as the effects are not statistically strong.

At this stage, I cannot definitively identify the sources of the observed spillover effects. However, using endline data, I will conduct further analysis to assess whether the introduction of the program led to shifts in local market composition. For example, the program may have encouraged diversification, with households investing in ventures that were previously uncommon in their communities, or alternatively, increased concentration around familiar enterprise types. Isolating these patterns will help clarify the relative contributions of congestion, competition, and agglomeration effects, as compared to psychosocial spillovers. This investigation will provide a clearer understanding of how different spillover channels shape the overall impact of

Table 7: Impacts and Spillovers at Different Saturation Categories for Psychological Outcomes (%), Full Sample

| | Depressed | | Low aspirations | | Low self-efficacy | | Low internality . | | Poor mental health | |
|---|---|---|---|---|---|---|---|---|---|---|
| *Impacts on Control Households* | | | | | | | | | | |
| Low Saturation | -5.86 | (4.20) | 1.07 | (5.11) | 2.86 | (4.61) | 1.68 | (3.97) | -0.47 | (4.65) |
| Medium Saturation | 6.06 | (3.80) | -5.30 | (4.44) | 5.61 | (4.37) | 0.73 | (4.37) | 7.26* | (3.87) |
| High Saturation | 4.56 | (6.86) | 0.69 | (9.02) | 7.17 | (7.29) | 4.70 | (7.25) | 11.73 | (7.42) |
| *Impacts on Treatment Households* | | | | | | | | | | |
| Low Saturation | 8.66 | (8.33) | 5.08 | (10.21) | 6.55 | (8.47) | 0.65 | (6.69) | 6.27 | (9.38) |
| Medium Saturation | -0.76 | (4.59) | 7.35 | (4.49) | 5.50 | (4.81) | 8.40* | (4.66) | 0.87 | (4.18) |
| High Saturation | 2.76 | (7.17) | 5.67 | (8.57) | 4.96 | (6.65) | 2.56 | (8.14) | -2.28 | (6.28) |
| *Other Parameters* | | | | | | | | | | |
| Baseline Level of Outcome | 16.59*** | (2.91) | 2.68 | (2.11) | 5.20** | (2.20) | 6.99** | (2.77) | 8.15*** | (2.23) |
| Constant | 21.16*** | (1.33) | 42.57*** | (2.03) | 33.77*** | (1.98) | 23.32*** | (1.85) | 33.96*** | (1.76) |
| Observations | 2,036 | | | | | | | | | |
| District FEs | ✓ | | | | | | | | | |

*Note:* The rows for impacts on control households correspond to estimates of $\theta_{0,Low}$, $\theta_{0,Medium}$, and $\theta_{0,High}$, respectively, which measure spillover effects on non-treated households in each saturation group relative to non-treated households in zero-saturation communities. The rows for impacts on treatment households correspond to estimates of $\theta_{1,Low}$, $\theta_{1,Medium}$, and $\theta_{1,High}$, respectively, which measure the effect of treatment relative to non-treated households in the same saturation group. Share of households in each saturation group are 51% for zero, 9% for low, 22% for medium, and 18% for high. Standard errors in parentheses are clustered at the neighborhood/locality level. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

*Tenondera*.

## 3.4 Impact Heterogeneity in Economic Outcomes

Above, I presented average treatment effects of *Tenondera*. Yet evidence from evaluations of development interventions shows that such averages can mask substantial heterogeneity in impacts across participants. For example, studies of both microfinance and graduation programs find that while some households achieve substantial gains, others benefit little or not at all (Banerjee et al., 2015a,b; Bandiera et al., 2017; Meager, 2019). To examine whether similar patterns emerge in this setting, I estimate conditional quantile treatment effects, which allow program impacts to vary across different points of the outcome distribution.

Table 8 presents conditional quantile treatment effect estimates for income and assets, documenting substantial heterogeneity in the impacts of *Tenondera* across the distribution of participants. For income, estimated effects range from essentially zero at the 10th percentile to 0.24 standard deviations relative to the control group at baseline at the 90th percentile. For assets, the range is even wider, from 0.06 to 0.59 standard deviations across the same percentiles. Corresponding plots are provided in Appendix G. Estimates for savings are not reported due to limited variation in the dependent variable.

These treatment effects have two key implications for household inequality and program cost-effectiveness. First, the program does not transform the economic prospects of all households. For roughly one-tenth of beneficiaries, there are no statistically significant gains in assets, despite the large transfer component at the core of the program. Moreover, for roughly half of beneficiaries, the program does not lead to a statistically significant increase in income. While these results may reflect the fact that households are only midway through the program, clear disparities in outcomes are already evident at this stage.

Second, even among households that experience improvements in their material conditions, the dispersion in outcomes is substantial. For income, the effect at the 90th percentile of the

Table 8: Conditional Quantile Treatment Effects for Economic Outcomes (USD)

| Variable | Q10 | Q25 | Q50 | Q75 | Q90 |
|---|---|---|---|---|---|
| Monthly per capita income | -1.11 | -0.32 | 4.41* | 8.21** | 10.79** |
| | (1.02) | (1.76) | (2.64) | (4.14) | (4.97) |
| Household business assets | 4.52 | 48.38*** | 173.30*** | 324.18*** | 463.63*** |
| | (6.49) | (14.13) | (26.25) | (48.91) | (77.94) |
| Observations | | | 2,584 | | |
| District FEs | | | ✓ | | |

*Note:* Baseline mean values of monthly per capita income and household business assets are 58.75 and 427.60, respectively. For each regression, only the ITT impact parameter is reported. All regressions control for baseline levels of the dependent variable. Standard errors in parentheses are clustered at the community level. Estimates for savings are not reported due to limited variation in the dependent variable. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

conditional distribution is 2.4 times larger than the effect at the median. Similarly, for assets, the effect at the 75th percentile is 6.7 times greater than at the 25th percentile. Notably, conditional on baseline asset levels and treatment assignment, households in the top decile of the asset distribution accumulate business assets exceeding the value of the seed capital transfer.

This variation raises the question of what factors drive differential impacts across households. Theory points to at least three likely sources of impact heterogeneity in graduation program effects: initial endowments in human and physical capital, baseline psychological capabilities, and exposure to climate shocks and risk (Barrett et al., 2019; Ikegami et al., 2019). Prior literature has identified baseline psychological state as a meaningful contributor to heterogeneity in program outcomes. In Appendix H, I show that, by midline, there is no evidence that baseline psychological state drives variation in economic outcomes in this setting.

While baseline psychological state does not seem to explain heterogeneity in economic outcomes by midline, the community dynamics discussed above may offer an alternative explanation. In addition to formal program components designed to strengthen psychological assets—such as life skills training and tailored mentoring—social networks may play a critical role in influencing both psychological well-being and economic outcomes. These informal dynamics

may not only strengthen the motivation and sense of agency of participants, but also interact with the broader pathways through which psychological and economic outcomes influence one another. Improved mental health can enable better economic decisions, while economic progress may sustain psychological gains when effort is perceived to yield a commensurate reward. Although further research is needed to unpack these interactions, understanding how community dynamics shape the experiences of program participants may be key to explaining some of the observed heterogeneity in program impacts.

# 4 Conclusion

This study provides insights into the economic and psychosocial impacts of the Paraguayan graduation program, *Tenondera*. Through an RCT, I find that the program generates significant economic benefits for participating households, including increases in income, assets, and savings. However, these average effects mask considerable heterogeneity, as some program participants see limited or no economic benefit while others experience more pronounced gains. Prior research has highlighted baseline psychological state as a key factor that leads to heterogeneous impacts in graduation programs. In this setting, however, I find no evidence that baseline psychological state explains variation in economic outcomes by midline. This null result—though potentially due to estimation noise—suggests that other mechanisms, particularly community dynamics, may play a more central role.

While the program demonstrates positive economic impacts on average, the findings reveal a complex relationship between *Tenondera* and the psychological well-being of participants. Despite the goal of the program of building intangible psychological assets through life skills training, midline results show declines in several psychological measures, including higher rates of depression, low aspirations, low self-efficacy, and low internality. Possible explanations include challenges in delivering the life skills component or burdensome expectations around business success, which may cause distress if economic progress falls short of household goals.

Additionally, implementation hurdles or anticipation effects, such as optimism among non-participants about eventual program inclusion, may also play a role. Newly added survey modules at endline will allow for a deeper analysis of these psychological outcomes and inform potential program adjustments.

The analysis also underscores the importance of community-level saturation in shaping outcomes for both treated and non-treated households. Treated households in high-saturation communities achieve the most substantial economic gains, suggesting positive spillovers likely mediated by psychosocial channels such as shared learning, motivation, mutual encouragement, and informal support from peers. In contrast, non-treated households in those same communities experience declines in income, potentially due to exclusion from these psychosocial benefits or exposure to competition or congestion effects. While baseline psychological state does not explain heterogeneity in economic outcomes, the surrounding community environment may alter program impacts. Community dynamics, including social comparison and shifting local norms, may influence both economic and psychological outcomes. These feedback loops, where improved mental health enables better decisions and economic progress reinforces psychological gains, could help explain why saturation effects differ by treatment status.

These findings raise important considerations for program design, especially regarding how support is delivered to participants with varying psychological and social resources. While midline results do not show that baseline psychological state drives heterogeneity in economic outcomes, future endline data may offer more precise insights. In the meantime, the evidence on saturation effects suggests that strengthening support structures, such as coaching, peer learning, and community-based activities, could improve both economic and psychological outcomes by fostering an environment more conducive to success. Policymakers should also consider adjusting program delivery models to account for the risks of negative spillovers. High saturation appears to benefit participants but may lead to unintended consequences for non-

beneficiaries, including worsened economic or psychological outcomes. Targeting entire communities or ensuring that excluded households receive alternative support could help mitigate these risks. Such strategies may improve the overall cost-effectiveness of *Tenondera* by better aligning resources with household needs and minimizing the likelihood of adverse spillovers.

In conclusion, *Tenondera* demonstrates the potential of asset-building anti-poverty programs to improve economic prospects, though its impacts are more complex when factoring in psychological well-being and community dynamics. Understanding the interplay between economic assistance, psychosocial factors, and local community context is essential for improving the effectiveness of such programs in lifting individuals out of poverty and building resilience to shocks. The upcoming endline analysis will illuminate the persistence of these patterns and help identify the channels through which program impacts are amplified or diminished.

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# A *Tenondera* Eligibility Across Communities

Table A1: Variation in *Tenondera*-Eligible Households Across Saturation Schemes

| Scheme | Mean number of *Tenondera*-eligible households per community | Mean share of households that are *Tenondera*-eligible per community | CV of the share of households that are *Tenondera*-eligible per community |
|---|---|---|---|
| A | 8.27 | 0.0805 | 0.8132 |
| B | 9.64 | 0.0885 | 0.5872 |
| C | 7.71 | 0.0852 | 0.7940 |
| D | 10.90 | 0.1154 | 0.6538 |
| E | 11.00 | 0.1113 | 0.8582 |
| F | 8.24 | 0.0820 | 0.5290 |
| G | 12.17 | 0.1361 | 1.7942 |
| H | 12.03 | 0.0939 | 1.7921 |

*Note:* Saturation in the main analysis is defined relative to *Tenondera*-eligible households. This table reports the mean number and share of eligible households per community, and the coefficient of variation (CV = SD/mean) of the eligibility share, highlighting variation across saturation schemes. Schemes *A*-*F* are part of the saturation design group, while Schemes *G* and *H* belong to the non-saturation design group.

# B Attrition Checks

Table A2: Attrition: Dependent Variable: Completed Survey, OLS

| | |
|---|---|
| **Panel A** | |
| Treatment status | −0.0145 |
| | (0.0118) |
| Observations | 2,864 |
| R-squared | 0.0005 |
| Outcome mean | 0.9029 |
| **Panel B** | |
| Treatment status | −0.0146 |
| | (0.0118) |
| Monthly per capita income (USD) | $2.8e-05$ |
| | (0.0002) |
| Household business assets (USD) | $1.3e-05^{*}$ |
| | $(6.8e-06)$ |
| Household savings (USD) | −0.0001 |
| | (0.0002) |
| CES-D 10 score | −0.0016 |
| | (0.0012) |
| Aspirations index | −0.0013 |
| | (0.0065) |
| Self-efficacy index | $-0.0118^{*}$ |
| | (0.0069) |
| Internality score | 0.0003 |
| | (0.0015) |
| Observations | 2,864 |
| R-squared | 0.0038 |
| Outcome mean | 0.9029 |
| **Panel C** | |
| Treatment status | 0.0640 |
| | (0.0617) |
| Baseline characteristics | ✓ |
| Baseline characteristics interacted with Treatment status | ✓ |
| Observations | 2,864 |
| R-squared | 0.0060 |
| Outcome mean | 0.9029 |
| P-value from joint test that Treatment status and all other variables above interacted with Treatment status are jointly 0 | 0.30 |

*Note:* ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

# C Measurement of Psychological Well-Being

To assess the psychological effects of the program, I administered a battery of questions designed to capture multiple dimensions of psychological well-being, including depression, self-efficacy, aspirations, and internal locus of control. The depression scale is adapted from a standard instrument (the CES-D) and includes 10 items reflecting different emotional states. Aspirations are captured with five items focused on future-oriented goals and expectations. Self-efficacy is measured using seven items adapted from the New General Self-Efficacy Scale. Internality is measured using four items reflecting beliefs about control over one's own life. These items were combined into a standardized index of psychological well-being, with higher scores indicating better outcomes.

The histograms of baseline psychological measures displayed in Figure 3 show substantial variation prior to program implementation. The distributions indicate that the scales capture

Figure 3: Histograms of Baseline Values for Key Psychological Variables

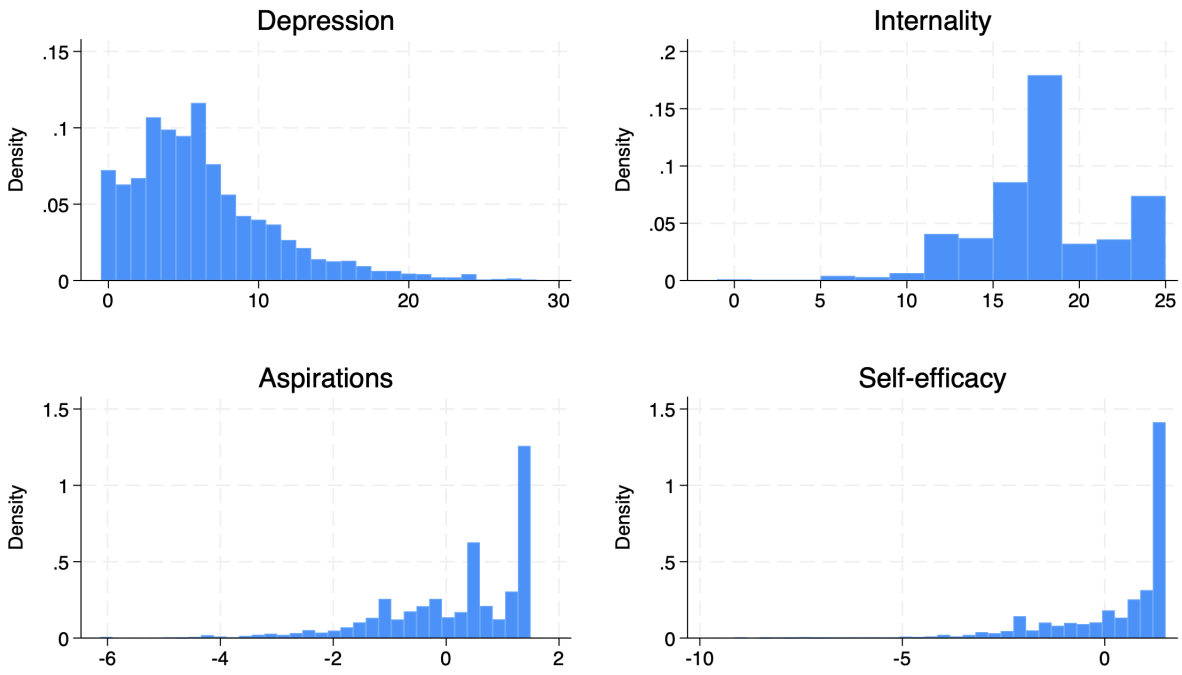


Table A3: Internal Consistency of Psychological Measures

| Measure | Cronbach's Alpha |
|---|---|
| Depression | 0.725 |
| Aspirations | 0.680 |
| Self-Efficacy | 0.839 |
| Internality | 0.357 |
| Mental Health Index | 0.828 |

Table A4: Test–Retest Correlations (Baseline to Midline)

| Measure | Test–Retest Correlation |
|---|---|
| Depression | 0.192 |
| Aspirations | 0.006 |
| Self-Efficacy | 0.027 |
| Internality | 0.016 |
| Mental Health Index | 0.042 |

meaningful heterogeneity across individuals. These figures provide visual support for the use of continuous psychological indices as outcome variables as part of the empirical strategy.

The internal consistency of the scales is generally satisfactory (Table A3). Cronbach's alpha is 0.84 for self-efficacy, 0.73 for depression, 0.68 for aspirations, and 0.83 for the overall mental health index. The internality scale, however, shows weaker internal consistency ($\alpha = 0.36$), warranting caution when interpreting results based solely on this construct. Test–retest correlations between the baseline and midline rounds are low, ranging from 0.006 for aspirations to 0.192 for depression (Table A4). These low correlations likely reflect both the volatility of psychological states amid a process of livelihood transformation and classical measurement error. Nonetheless, the measures exhibit meaningful cross-sectional correlations (Table A5). For example, aspirations, self-efficacy, and internality are positively correlated with each other and negatively correlated with depression, in line with psychological theory.

Despite limitations in the stability of these measures over time, I argue that they remain suitable for causal analysis in the context of an RCT. The experimental design ensures that, on average, measurement error is uncorrelated with treatment status, supporting a causal inter-

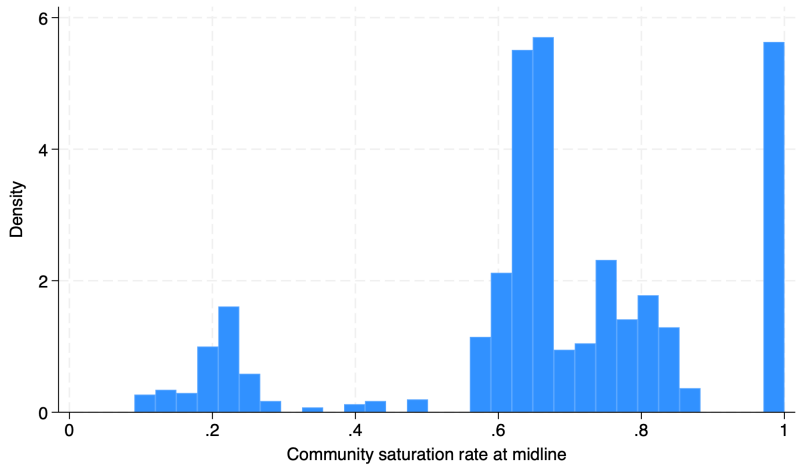
Table A5: Correlations Between Psychological Measures at Baseline

| | Depression | Aspirations | Self-Efficacy | Internality |
|---|---|---|---|---|
| **Depression** | 1.000 | -0.106 | -0.098 | -0.023 |
| **Aspirations** | | 1.000 | 0.391 | 0.239 |
| **Self-Efficacy** | | | 1.000 | 0.368 |
| **Internality** | | | | 1.000 |

pretation of treatment effects. I therefore interpret the observed deterioration in psychological well-being among treated participants as a genuine program effect. Given the complex interplay between economic change and psychological well-being, I view these measures as capturing short- to medium-term shifts in mental states rather than stable psychological traits.

# D Saturation Intensity Histogram

Figure 4: Saturation Intensity at Midline (Excluding Zero)



# E Balance Across Saturation Groups

To assess whether pre-treatment household characteristics differ systematically across the saturation groups I have defined, I examine baseline balance across a set of demographic, economic, and psychological outcomes. I compare households in communities classified as zero, low, medium, and high saturation using pairwise t-tests. Table A6 reports group means and p-values for comparisons among these groups.

Overall, the results indicate that outcome variables are relatively well balanced across saturation groups, particularly psychological measures. Differences in psychological variables are small in magnitude and rarely statistically significant, with a few exceptions in depression and internality between some groups. These findings suggest that the psychological baseline profiles of respondents were broadly comparable across groups.

In contrast, demographic characteristics and economic outcomes exhibit more noticeable variation across saturation groups. These imbalances, while expected given the granularity of the comparisons and sample size, highlight the importance of including baseline covariates in the regression specifications to improve precision and address potential residual confounding. The presence of some statistically significant differences reinforces the need to interpret saturation-level spillover effects with appropriate statistical controls and robustness checks.

Taken together, the evidence suggests that although saturation groups differ on certain demographic characteristics, baseline values for the outcome variables of interest—particularly psychological measures—are largely comparable. This conclusion supports the credibility of the causal estimates presented in the manuscript.

Table A6: Baseline Balance Across Saturation Groups

| Variable | (1) Zero Mean/(SE) | (2) Low Mean/(SE) | (3) Medium Mean/(SE) | (4) High Mean/(SE) | T-test P-value (1)−(2) | T-test P-value (1)−(3) | T-test P-value (1)−(4) | T-test P-value (2)−(3) | T-test P-value (2)−(4) | T-test P-value (3)−(4) |
|---|---|---|---|---|---|---|---|---|---|---|
| Monthly per capita income | 59.79 | 54.70 | 56.74 | 60.30 | 0.10* | 0.14 | 0.82 | 0.52 | 0.10* | 0.16 |
| | (1.16) | (2.70) | (1.67) | (1.94) | | | | | | |
| Household business assets | 416.05 | 522.89 | 377.49 | 479.07 | 0.06* | 0.29 | 0.13 | 0.00*** | 0.44 | 0.01** |
| | (21.56) | (45.15) | (24.90) | (33.10) | | | | | | |
| Household savings | 8.39 | 8.61 | 10.33 | 12.00 | 0.94 | 0.33 | 0.10* | 0.63 | 0.37 | 0.56 |
| | (1.02) | (2.86) | (1.90) | (2.23) | | | | | | |
| CES-D 10 score | 6.33 | 5.75 | 6.72 | 5.96 | 0.08* | 0.09* | 0.13 | 0.01*** | 0.57 | 0.01*** |
| | (0.13) | (0.30) | (0.19) | (0.20) | | | | | | |
| Depressed (%) | 0.21 | 0.19 | 0.23 | 0.17 | 0.36 | 0.45 | 0.03** | 0.19 | 0.50 | 0.01** |
| | (0.01) | (0.03) | (0.02) | (0.02) | | | | | | |
| Aspirations index | 0.18 | 0.15 | 0.22 | 0.26 | 0.75 | 0.51 | 0.22 | 0.46 | 0.26 | 0.58 |
| | (0.03) | (0.08) | (0.05) | (0.06) | | | | | | |
| Low aspirations (%) | 0.38 | 0.35 | 0.39 | 0.35 | 0.28 | 0.80 | 0.16 | 0.25 | 0.98 | 0.15 |
| | (0.01) | (0.03) | (0.02) | (0.02) | | | | | | |
| Self-efficacy index | 0.27 | 0.34 | 0.29 | 0.31 | 0.55 | 0.81 | 0.61 | 0.69 | 0.84 | 0.80 |
| | (0.04) | (0.11) | (0.06) | (0.07) | | | | | | |
| Low self-efficacy (%) | 0.30 | 0.27 | 0.28 | 0.30 | 0.32 | 0.57 | 0.86 | 0.58 | 0.32 | 0.54 |
| | (0.01) | (0.03) | (0.02) | (0.02) | | | | | | |
| Internality score | 17.85 | 17.93 | 17.81 | 17.90 | 0.75 | 0.86 | 0.81 | 0.69 | 0.90 | 0.72 |
| | (0.10) | (0.24) | (0.16) | (0.16) | | | | | | |
| Low internality (%) | 0.19 | 0.14 | 0.20 | 0.17 | 0.08* | 0.59 | 0.41 | 0.05** | 0.28 | 0.25 |
| | (0.01) | (0.02) | (0.02) | (0.02) | | | | | | |
| Mental health index | 0.39 | 0.49 | 0.38 | 0.51 | 0.44 | 0.85 | 0.21 | 0.41 | 0.88 | 0.21 |
| | (0.05) | (0.12) | (0.07) | (0.08) | | | | | | |
| Poor mental health (%) | 0.32 | 0.29 | 0.33 | 0.31 | 0.39 | 0.80 | 0.58 | 0.34 | 0.69 | 0.50 |
| | (0.01) | (0.03) | (0.02) | (0.02) | | | | | | |
| HH in rural area (%) | 0.60 | 0.66 | 0.55 | 0.77 | 0.06* | 0.03** | 0.00*** | 0.00*** | 0.00*** | 0.00*** |
| | (0.01) | (0.03) | (0.02) | (0.02) | | | | | | |
| Number of HH members | 4.24 | 4.16 | 4.45 | 4.25 | 0.40 | 0.01*** | 0.94 | 0.02** | 0.41 | 0.03** |
| | (0.04) | (0.10) | (0.07) | (0.06) | | | | | | |
| Homeownership (%) | 0.75 | 0.82 | 0.74 | 0.79 | 0.02** | 0.40 | 0.10 | 0.01*** | 0.27 | 0.04** |
| | (0.01) | (0.02) | (0.02) | (0.02) | | | | | | |
| Respondent is married (%) | 0.61 | 0.64 | 0.53 | 0.67 | 0.49 | 0.00*** | 0.04** | 0.01*** | 0.43 | 0.00*** |
| | (0.01) | (0.03) | (0.02) | (0.02) | | | | | | |
| Observations | 1,465 | 245 | 648 | 506 | | | | | | |

*Note*: Monetary variables are in USD (exchange rate: 1 USD = 7,692 Paraguayan guaranis). Monthly income per capita, household business assets, and savings are winsorized at the 95th percentile. Cutoffs for “depressed" and “low internality" status are 10 and 15 points, respectively. P-values are from two-sided t-tests. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

# F Spillover Analysis for the Saturation Design Subsample

Table A7: Impacts and Spillovers at Different Saturation Categories for Economic Outcomes (USD), Saturation Design Subsample

| | Monthly per capita income | | Household business assets | |
|---|---|---|---|---|
| *Impacts on Control Households* | | | | |
| Low Saturation | -2.03 | (4.07) | 52.65 | (53.55) |
| Medium Saturation | 13.56 | (31.94) | -228.69*** | (84.96) |
| High Saturation | -7.85 | (6.90) | -129.63* | (65.42) |
| *Impacts on Treatment Households* | | | | |
| Low Saturation | 5.10 | (5.65) | 235.55** | (111.48) |
| Medium Saturation | 34.54 | (52.49) | -119.11 | (123.50) |
| High Saturation | 13.59** | (6.43) | 359.39*** | (67.99) |
| *Other Parameters* | | | | |
| Baseline Level of Outcome | 0.33*** | (0.03) | 0.47*** | (0.04) |
| Constant | 33.96*** | (2.42) | 231.53*** | (32.98) |
| Observations | 1,187 | | 1,187 | |
| District FEs | ✓ | | ✓ | |

*Note:* The rows for impacts on control households correspond to estimates of $\theta_{0,Low}$, $\theta_{0,Medium}$, and $\theta_{0,High}$, respectively, which measure spillover effects on non-treated households in each saturation group relative to non-treated households in zero-saturation communities. The rows for impacts on treatment households correspond to estimates of $\theta_{1,Low}$, $\theta_{1,Medium}$, and $\theta_{1,High}$, respectively, which measure the effect of treatment relative to non-treated households in the same saturation group. In the saturation design subsample, the share of households in each saturation group are 52% for zero, 14% for low, 1% for medium, and 33% for high. Standard errors in parentheses are clustered at the community level. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Table A8: Impacts and Spillovers at Different Saturation Categories for Psychological Outcomes (%), Saturation Design Subsample

| | Depressed | | Low aspirations | | Low self-efficacy | | Low internality . | | Poor mental health | |
|---|---|---|---|---|---|---|---|---|---|---|
| *Impacts on Control Households* | | | | | | | | | | |
| Low Saturation | -10.46** | (4.43) | 1.80 | (5.47) | 2.40 | (4.97) | -0.68 | (4.84) | 1.05 | (5.24) |
| Medium Saturation | 43.55* | (25.30) | 18.81 | (27.39) | -3.63 | (20.49) | 5.61 | (30.01) | -3.22 | (23.20) |
| High Saturation | 4.05 | (8.71) | 11.53 | (9.37) | 9.23 | (8.33) | 10.14 | (9.05) | 17.22** | (7.23) |
| *Impacts on Treatment Households* | | | | | | | | | | |
| Low Saturation | 17.73* | (10.32) | 19.77** | (8.84) | 9.76 | (8.26) | 2.64 | (7.19) | 12.02 | (9.85) |
| Medium Saturation | -35.98** | (17.06) | -30.30 | (46.57) | 35.87 | (27.05) | 27.06 | (45.27) | 42.37 | (28.50) |
| High Saturation | 1.61 | (8.73) | -2.90 | (9.02) | 5.29 | (8.26) | -1.69 | (10.15) | -5.66 | (6.71) |
| *Other Parameters* | | | | | | | | | | |
| Baseline Level of Outcome | 10.70** | (4.11) | -0.97 | (3.14) | 6.30* | (3.37) | 7.17* | (3.63) | 9.30*** | (2.96) |
| Constant | 23.52*** | (2.13) | 41.23*** | (2.73) | 33.53*** | (2.53) | 23.13*** | (2.44) | 32.38*** | (2.26) |
| Observations | 931 | | | | | | | | | |
| District FEs | ✓ | | | | | | | | | |

*Note:* The rows for impacts on control households correspond to estimates of $\theta_{0,Low}$, $\theta_{0,Medium}$, and $\theta_{0,High}$, respectively, which measure spillover effects on non-treated households in each saturation group relative to non-treated households in zero-saturation communities. The rows for impacts on treatment households correspond to estimates of $\theta_{1,Low}$, $\theta_{1,Medium}$, and $\theta_{1,High}$, respectively, which measure the effect of treatment relative to non-treated households in the same saturation group. In the saturation design subsample, the share of households in each saturation group are 52% for zero, 14% for low, 1% for medium, and 33% for high. Standard errors in parentheses are clustered at the neighborhood/locality level. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

# G Quantile Treatment Effect Plots

Figure 5: Conditional Quantile Treatment Effects of Treatment Assignment for Monthly Per Capita Income

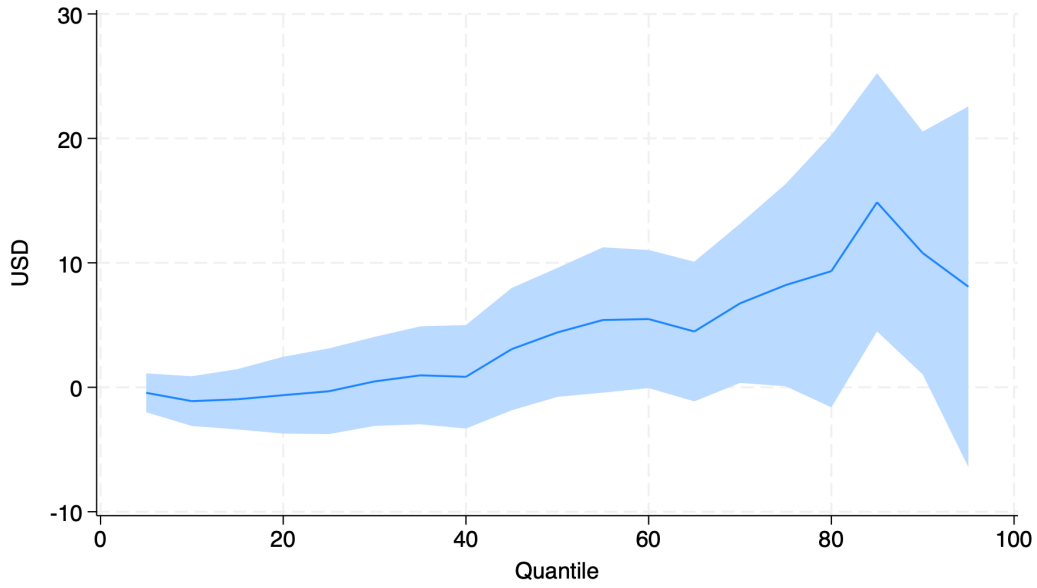
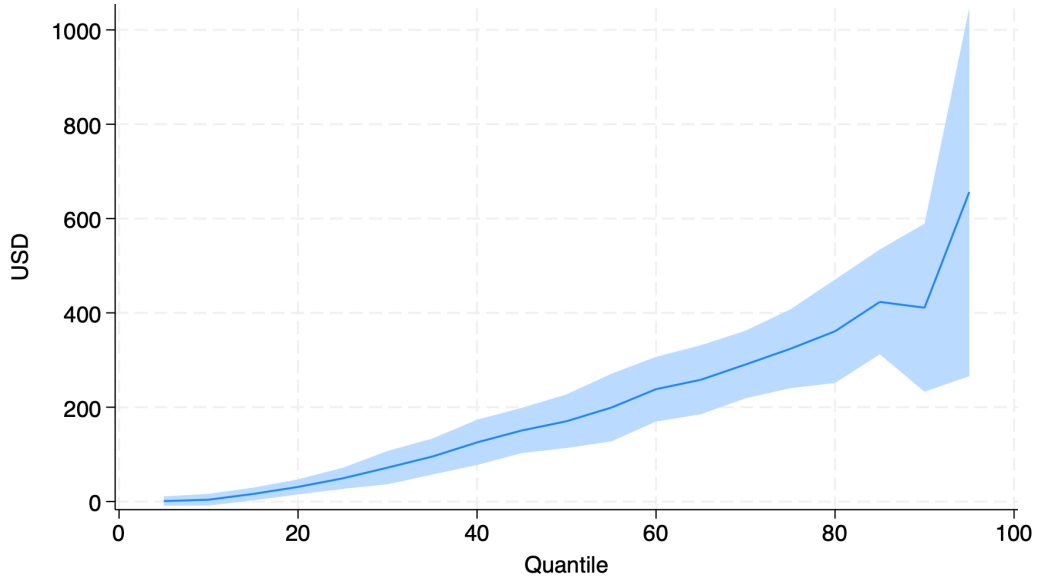



Figure 6: Conditional Quantile Treatment Effects of Treatment Assignment for Household Business Assets



# H Impact Heterogeneity by Baseline Psychological State

I examine whether and how baseline differences in depression, aspirations, self-efficacy, internality, and a composite mental health index lead to varying *Tenondera* treatment effects on key economic outcomes by midline. Given that a core component of the program is the transfer of intangible psychological assets, individuals with different initial psychological states may experience differential impacts. For example, participants with a poor psychological state at baseline may achieve only limited gains from the program.

To investigate whether psychological state at baseline moderates program impacts, I estimate a modified version of Equation 1 that includes an interaction between treatment assignment and an indicator for low baseline psychological state:

$$y_{hd} = \alpha_0 + \alpha_1 y_{hd}^0 + \beta Treat_{hd} + P_{hd}^0 \times [\delta_0 + \delta_1 Treat_{hd}] + \gamma_d + \varepsilon_{hd}, \tag{3}$$

where $P_{hd}^0$ is a time-invariant binary indicator that switches on for respondents with a low level of a given psychological measure at baseline. This specification allows for the identification of differential treatment effects across two subpopulations defined by their baseline psychological state. For instance, among beneficiaries who were depressed versus not depressed at baseline, the treatment effect for the former group is $\beta + \delta_1$, while for the latter it is $\beta$. The differential impact associated with poor psychological state is therefore captured by $\delta_1$.

I estimate Equation 3 separately for each psychological factor, as well as for a composite index constructed from all four measures. For each specification, Table A9 reports the ITT estimate $\beta$ for beneficiaries with a regular psychological state at baseline and the differential impact $\delta_1$ for those with a low psychological state. Overall, the coefficients are imprecisely estimated, providing no conclusive evidence that baseline psychological state is a source of impact heterogeneity in this study. I will revisit this analysis at endline to assess whether improved estimation precision reveals clearer patterns.

Table A9: Heterogeneous Impacts for Economic Outcomes by Baseline Psychological State (USD), ITT Estimates

| Baseline variable | Coefficient | Monthly per capita income | | Household business assets | |
|---|---|---|---|---|---|
| Depression | $\hat{\beta}$ | 4.31 | (2.78) | 251.10*** | (39.29) |
| | $\hat{\delta_1}$ | -2.21 | (5.01) | 14.93 | (63.04) |
| Aspirations | $\hat{\beta}$ | 3.57* | (3.08) | 233.45*** | (39.57) |
| | $\hat{\delta_1}$ | 1.05 | (3.91) | 61.39 | (56.31) |
| Self-Efficacy | $\hat{\beta}$ | 4.13 | (2.69) | 233.42*** | (35.39) |
| | $\hat{\delta_1}$ | -0.43 | (4.76) | 75.35 | (55.00) |
| Internality | $\hat{\beta}$ | 4.04* | (2.43) | 256.72*** | (35.10) |
| | $\hat{\delta_1}$ | 0.08 | (5.61) | -6.23 | (66.42) |
| Mental Health Index | $\hat{\beta}$ | 4.19 | (2.88) | 244.95*** | (38.26) |
| | $\hat{\delta_1}$ | -0.51 | (4.55) | 33.68 | (58.27) |
| Observations | | 2,584 | | 2,584 | |
| District FEs | | ✓ | | ✓ | |

*Note:* Each pair of rows displays estimates from a separate regression. All regressions control for baseline levels of the dependent variable. Standard errors in parentheses are clustered at the community level. ***, **, and * indicate significance at the 1, 5, and 10 percent levels, respectively.

Some point estimates for the differential impact parameter are large relative to the main ITT effects, with many suggesting that participants in poorer psychological states face greater challenges in benefiting from *Tenondera*. For example, beneficiaries with depression or low self-efficacy at baseline experience lower income gains—51% and 10% less, respectively—than those with more favorable baseline psychological profiles. Interestingly, other point estimates present the opposite sign. For instance, those with low aspirations and low self-efficacy at baseline experience a 26% and 32% greater asset gain than their counterparts, respectively, suggesting that these variables may play a more complex role in shaping economic responses to the program.

Despite these findings, the overall effect of the program on individuals with low psychological states remains positive, suggesting that they still benefit from participation—albeit to a lesser extent than their counterparts. If these trends are confirmed at endline with more precise estimates, they may carry important implications for inclusive and cost-effective program design. One approach could be to intensify the coaching component specifically for participants

with lower initial psychological assets, with the goal of supporting more robust economic gains within this subgroup. Alternatively, targeting procedures could be refined to ensure that beneficiaries with poorer psychological states receive additional support prior to program entry, enabling them to fully benefit from participation. Such adjustments could reduce inefficiencies in program delivery and help ensure that resources are allocated to maximize outcomes across diverse participant profiles.