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 variables, though these figures obscure sizable heterogeneity, and the psychological effects of the 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 the distribution of participants. I also find that the program worsens the psychological state of beneficiaries mid-program, 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 shows that this psychological decline seems to be attenuated in communities with a greater share of beneficiary households, highlighting the role of community dynamics in supporting participants. In fact, psychological factors may act as an important source of spillover effects, as beneficiaries in these 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

The graduation program is a multifaceted intervention, 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. It provides low-income households with an asset transfer and intensive coaching to facilitate a transition from casual wage labor to higher-earning entrepreneurial activities, promoting a more sustainable livelihood. The program aligns with the multiple-equilibria poverty trap theory, 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)). There is theoretical support for strong complementarities between tangible and intangible assets that underpin these poverty traps (Barrett et al., 2019). The multifaceted nature of the program addresses both capital and psychosocial constraints that households may face, aiming to move participants toward a higher equilibrium that improves their well-being. However, this process is not always smooth; the expectation of the program for participants to transform their livelihoods may induce stress, particularly as households attempt to apply new technical and soft skills to succeed in their ventures. Despite the prominence of the intangible component within the graduation approach, the ways in which the program interacts with the psychological state of households remain relatively understudied.

Impact evaluations from several countries show that graduation programs effectively help families begin a path out of poverty (Banerjee et al. (2015), Bandiera et al. (2017), Banerjee et al. (2021)). The evidence on these programs has led governments worldwide to adopt them. Despite these successes, there is limited understanding of the mechanisms through which these interventions achieve their outcomes and which participants benefit most. Some studies have shown that baseline psychological conditions can mediate the economic outcomes of the program (Correa (2021), Zheng et al. (2023)). Indeed, researchers have proposed that low stocks of psychological assets may 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)). This paper instead focuses on the direct effects of the graduation program on psychological outcomes and how these interact with community dynamics—a dimension that is currently underexplored.

Specifically, using measures of depression, locus of control, aspirations, and self-efficacy, I examine how this asset-building anti-poverty program affects psychological well-being and whether these psychological factors contribute to spillover effects within communities. To address these questions, I implemented a randomized controlled trial (RCT) of the Paraguayan government's graduation program, *Tenondera* (Guarani for "onward"). The RCT design includes a staggered rollout and a saturation component to rigorously evaluate program impacts.

Midline findings reveal promising average treatment effects in economic terms: 10 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. Additionally, the top quartile experienced asset increases 6.7 times larger than those of the bottom quartile. These results highlight the disparities in how the program affects different subgroups and are consistent with the broader literature (Karlan (2020)). Unlike previous studies, however, I find no strong evidence that baseline psychological characteristics drive this variation.

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 4%, and with low self-efficacy by 9%. I further consider these characteristics in relation to social dynamics, leveraging varying levels of community saturation rates—a measure of the share of households participating in *Tenondera* within a given community—to capture exposure to neighbors who receive the program. This analysis reveals significant spillover effects 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 psychological well-being among treated households is less severe in higher-saturation communities, indicating that psychological factors may be an important source of spillover effects. Beneficiary households in communities with a large share of neighbors participating in the program might engage in networks of learning and support, which help mitigate psychological stress and foster economic success. This dynamic, currently underappreciated in such programs, has important implications for their design and implementation.

The rest of the paper is organized as follows: in Section 2, I present the program intervention, research design, and details on the integrity of the experimental design of the RCT. Next, I present the empirical strategy and results of the study, including the standard impact and saturation analyses, in Section 3. Finally, Section 4 concludes.

2 Study Framework

This study examines the impacts of Paraguay's government-led graduation program, *Tenondera*, on its participants and their communities. It includes three waves of data collection—a 2021 baseline, a 2022 midline, and a 2024 endline. Each household is intended to be surveyed thrice. The results in this manuscript use only data from the baseline and midline waves, as data collection for the endline is still ongoing. The study is being conducted in collaboration with the Ministry of Social Development (MDS), which is the implementing agency of the program, and local partners Fundación Capital and Instituto Desarrollo.

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 join the program, participants must first be recipients of the government's conditional cash transfer (CCT) program, which follows a six-year schedule.[1] Starting in year three—the second half of the CCT program schedule—households are invited to join *Tenondera* according to the government's geographic prioritization strategies. *Tenondera* aims to help households improve their livelihoods so that their economic activities can sustain them after the CCT program ends. Following the model of other comprehensive graduation programs, *Tenondera* incorporates a dual strategy of providing tangible business assets while 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 (called socioeconomic promoters) introduce households to the program. Over these weeks, beneficiaries and promoters collaborate in group workshops to create a business profile and a life plan for each household. These documents outline what households expect to achieve through the program. The seed capital transfer takes place shortly after, at approximately the third month. The one-time transfer amount is 3 million Paraguayan guaranis (about USD 390) per household. Household members then have two weeks to spend the money on assets that will support an income-generating activity of their choice.[2] The rest of the program consists of periodic coaching sessions, covering technical skills to help with livelihood management, financial topics including budgeting, bookkeeping, and saving, as well as life skills to build confidence and enhance decision-making.

During this coaching stage, promoters are tasked with identifying the needs, aspirations, and limitations of households, and providing customized support based on their observations. The program intends to help vulnerable households overcome barriers to success in multiple

[1] CCT program beneficiaries are primarily households with school-age children or members with disabilities.

[2] Common purchases include animals, products to stock general small-scale stores or specialized ones such as bakeries, and materials to support the offering of other services such as laundry and manicure.

dimensions, such as low productivity, insufficient technology, credit constraints, low 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 any particular topics where the households need support.

Promoters are distributed on a geographic basis, with each promoter responsible for the enrollment and coaching of all households in their assigned area. They implement the program in quarterly waves, meaning that at any point a promoter might be working with households at different stages of *Tenondera* in the same area.

According to MDS administrative data, in close to 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 join 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 setup results in households choosing to invest their seed capital transfer in a diverse range of ventures. In cohorts prior to this study, around 75% of beneficiary households spent their transfer on beef, poultry, or pork production. Most of the remaining households invested in small-scale stores, while only a few invested in other ventures such as agricultural production and artisanal handcrafting.

2.2 Research Design

This study was implemented in three departments of Paraguay, deemed priority zones by MDS—Caaguazu, Caazapa, and San Pedro, as shown in Figure 1. The staggered design of

Figure 1: Map of Paraguay that highlights the departments included in the sample

the study leverages a five-year *Tenondera* scale-up plan across the national territory, which involves cohorts joining the program in quarterly waves. Each household in the study sample was or will be offered to join the program at some point, with the randomization procedure determining when each household is invited.

To construct the sample for the study, MDS initially provided a shortlist of potential participating households. Eligibility for the shortlist required that the household was located in a district within one of the three priority departments, that a *Tenondera* promoter was available in the district to fully implement the program, that the household was a recipient of the CCT program and in the second half of its transfer schedule at the time, that it was not receiving other government transfers, and that it had not previously received *Tenondera*. During the baseline survey, enumerators attempted to visit every household in the shortlist. The study sample consists of the 2,864 households that were successfully located.

Through a multi-stage randomization procedure, I assigned each household in the sample to

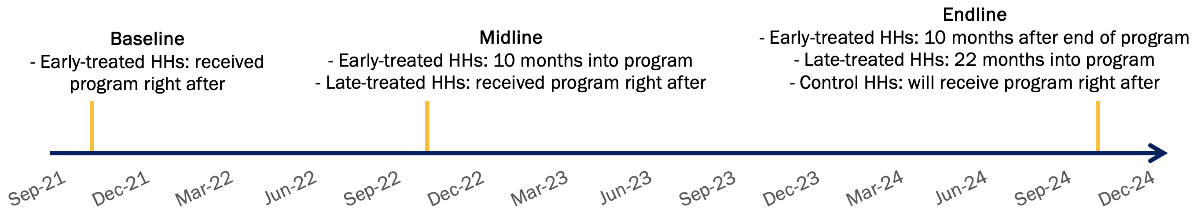

Figure 2: Study timeline

one of three groups—Early Treatment, Late Treatment, and Control. The program was offered to Early Treatment households in the first quarter of 2022, right after the baseline. The program was offered to Late Treatment households in a subsequent wave, during the first quarter of 2023, right after the midline. Finally, the program will be offered to Control households only after the end of the study, in the first quarter of 2025. Figure 2 summarizes the study timeline. At midline, Early Treatment households were 10 months into the program. Since *Tenondera* is front-loaded in the delivery of its components, this is a reasonable time at which early program impacts can be found.

In the first stage of the randomization procedure, I assigned each of the 246 neighborhoods/localities (which I will refer to as communities) that comprise the sample to a different saturation scheme, as summarized in Tables 1 and 2. In other words, within each community, I randomly determined the density with which households would be assigned to one of the three treatment groups. The second stage of the randomization procedure assigned each household to its treatment group, according to the previously determined community-level schemes.

Specifically, I imposed variation in the shares of households belonging 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 the community who have been offered the program at a given point in the study. In some communities, nearly all sampled households will be treated early into the study, resulting in a 100% saturation rate at midline. At the other extreme, in other neighborhoods, no one will be treated until after the end of the study, resulting in a 0% saturation rate at midline. This setup generates

Scheme	Share of households in Early Treatment	Share of households in Late Treatment	Share of households in Control	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 1: Randomization schemes in the saturation design group

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

Table 2: Randomization schemes in the non-saturation design group

random variation in the degree to which study households have been exposed to neighbors participating in *Tenondera* and allows for detecting spillover effects of the program.

Tables 1 and 2 show the first-stage randomization allocation, including the number of communities assigned to each scheme. I performed this randomization step separately for two subsamples based on community population size. In high- and low-population communities, a preliminary analysis revealed that saturation assignment would not monotonically map to spillover exposure, meaning that imposing the same saturation scheme onto two communities with notably different population sizes would translate into different levels of exposure to neighbors participating in *Tenondera*. Therefore, I established a saturation design group, consisting of mid-sized communities (populated by between 50 and 200 households) where the same saturation scheme would arguably capture a similar extent of spillover exposure. This group includes six different schemes, with one extreme where all households are assigned to Early Treatment, and the other where all households are assigned to Control. The non-saturation design group is made up of large and small communities and includes two different schemes.[3]

[3]Schemes were set up this way to fulfill multiple objectives: generating variation in saturation rates at both midline and endline, ensuring that no community had households receiving the program at widely different times as requested by MDS for political reasons, and guaranteeing a roughly even split of the sample across the three treatment groups at the household level.

2.3 Randomization Checks

In Table 3, I present the baseline balance test for the outcome variables used in the study and other household demographic characteristics, by displaying the mean comparison and t-tests for equality of means between the treatment and control groups, where the status of a household was determined by its assignment to treatment at midline. Overall, the sample balance between households in the treatment and control groups was appropriate. All outcome variables and most household characteristics show no significant differences at baseline. The only measure that indicates a difference that is significant at the 1% level is that a larger share of treatment households is located in a rural area compared to control households. However, the equality of means across all variables cannot be rejected, with a p-value of 0.11.

Table A1 in Appendix A provides a survey attrition analysis for the midline round. Follow-up was largely successful—Panel A indicates that 90% of baseline households were resurveyed at midline. I observe no significant differences in attrition rates between the treatment and control groups. Panel B examines the characteristics of households more likely to be resurveyed during the midline. Additionally, there is no evidence that treatment influenced attrition rates, thereby avoiding sample composition bias. Panel C tests whether assignment to treatment affected the type of households who completed the midline survey. The test fails to reject the hypothesis that the treatment status indicator and all outcome variables interacted with treatment status were zero, with a p-value of 0.30. This result supports the conclusion that survey attrition did not lead to a different sample composition between 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 households assigned to early treatment and scheduled to receive *Tenondera* in the first quarter of 2022, 10% had not received it. Additionally, 4% of households in the late treatment and control groups received the program in early 2022, despite not being scheduled to do so.

Table 3: Baseline Balance

Variable	N	(1) Control Mean/SE	N	(2) Treatment Mean/SE	T-test P-value (1)-(2)
Monthly per capita income	1918	59.50	946	57.24	0.19
		(1.01)		(1.37)	
Household business assets	1918	415.41	946	452.32	0.22
		(18.01)		(22.95)	
Household savings	1918	9.18	946	10.10	0.60
		(0.98)		(1.49)	
CES-D 10 score	1918	6.33	946	6.24	0.63
		(0.11)		(0.15)	
Depressed (%)	1918	0.21	946	0.19	0.13
		(0.01)		(0.01)	
Aspirations index	1918	0.10	946	0.15	0.18
		(0.02)		(0.03)	
Low aspirations (%)	1918	0.26	946	0.24	0.22
		(0.01)		(0.01)	
Self-efficacy index	1918	0.14	946	0.17	0.28
		(0.02)		(0.03)	
Low self-efficacy (%)	1918	0.21	946	0.21	0.91
		(0.01)		(0.01)	
Internality score	1918	17.81	946	17.95	0.39
		(0.09)		(0.13)	
Low internality (%)	1918	0.19	946	0.18	0.32
		(0.01)		(0.01)	
HH in rural area (%)	1918	0.60	946	0.66	0.01***
		(0.01)		(0.02)	
Number of HH members	1918	4.25	946	4.35	0.13
		(0.04)		(0.05)	
Homeownership (%)	1918	0.76	946	0.77	0.46
		(0.01)		(0.01)	
Respondent is married (%)	1918	0.60	946	0.61	0.75
		(0.01)		(0.02)	

Notes: Income, assets, and savings are presented in USD (the current exchange rate is 1 USD = 7,692 Paraguayan guaranis). Monthtly income per capita, business assets, and savings are winsorized at the 95% percentile. F-stat for F-test of joint significance is 1.48 (p-value of 0.11). Cutoffs for depressed and low internality status are 10 and 15 points, respectively. The value displayed for t-tests are p-values. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

3 Results

First, I present the empirical results of the study using a standard approach that does not consider spillover effects. In Section 3.1, I discuss the average and conditional quantile treatment effects for economic outcomes. Next, in Section 3.2, I examine psychological factors as a potential source of impact heterogeneity in economic outcomes. Section 3.3 then analyzes the psychological measures themselves as outcomes of *Tenondera*, presenting average treatment effects once more. Finally, in Section 3.4, I show empirical results that do account for spillovers, based on the variation in saturation intensity across communities, for both economic and psychological variables.

3.1 Average and Conditional Quantile 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} is the outcome variable of interest at midline for household h in district d, y_{hd}^0 is the baseline value of that same variable, and $Treat_{hd}$ is an indicator for assignment to *Tenondera* as part of the Early Treatment group. The error term ε_{hd} is clustered at the community level, and γ_d represents district fixed effects in my preferred specification.[4] Following this specification, the control group consists of households eligible for *Tenondera* assigned to either Late Treatment or Control. The parameter β identifies the ITT impact of the program under the assumption of no spillovers between treatment and control households. I will relax this assumption in Section 3.4.

[4]The district fixed effects capture, among other aspects, variations in state capacity across districts. Some districts are covered by more *Tenondera* promoters than others, meaning that the workload the promoters face, and thus their availability to support beneficiaries, might vary district to district.

Table 4: Treatment Effects for Economic Outcomes (USD)

Variable	N	(1) ATEs (OLS)	(2) Q10	(3) Q25	(4) Q50	(5) Q75	(6) Q90
			Conditional Quantile Treatment Effects				
Monthly per capita income	2584	4.00*	-1.11	-0.32	4.41*	8.21**	10.79**
		(2.39)	(1.02)	(1.76)	(2.64)	(4.14)	(4.97)
Household business assets	2584	255.71***	4.52	48.38***	173.30***	324.18***	463.63***
		(33.93)	(6.49)	(14.13)	(26.25)	(48.91)	(77.94)
Household savings	2584	3.06*					
		(1.72)					
District FEs				✓			

Notes: Baseline mean values of monthly per capita income, household business assets, and household savings are 58.75, 427.60, and 9.49, respectively. For each regression, only the ITT impact parameter is reported. Regressions include baseline levels of the dependent variable. Standard errors in parentheses are clustered at the community level. Conditional quantile treatment effect estimates on savings are not reported due to a lack of variation in the dependent variable. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

For key economic outcomes, Column (1) of Table 4 reports the OLS estimates of the ITT impact parameter from Equation 1. At midline, 10 months into *Tenondera* out of a total program duration of 24 months, beneficiary households report an average increase in monthly per capita income of USD 4, in business assets of USD 255.71, and in savings of USD 3.06. These estimates are statistically significant at the 10%, 1%, and 10% levels, respectively, and represent gains of 6.8%, 59.8%, and 32.2% relative to the baseline mean levels.

The seed capital transfer of *Tenondera* amounts to USD 390 per household. When comparing this figure to the estimated treatment effects, I find that there was on average some depletion of the purchased assets across households, as households were able to maintain about two thirds of the value of the assets they received. At the same time, the estimated increases in income and savings indicate that there was a transfer of these assets into these categories.

Columns (2)–(6) of Table 4 display the conditional quantile treatment effects on income and assets. These figures document the sizable heterogeneity in *Tenondera* impacts, which I will further examine in the following subsections. Plots for these treatment effects can be found in Appendix B. The estimates on savings are not reported, as there is little variation in the dependent variable in that case.

There are two main implications of these treatment effects with consequences for household inequality and program cost-effectiveness. First, the program does not transform the economic prospects of all households. For roughly a tenth of beneficiaries, there are no statistically significant asset gains, despite the large asset purchase embedded within the program. For roughly half of beneficiaries, there is no statistically significant increase in income as a result of the program. While these results might reflect the fact that households are only midway through the program, even at this point, clear disparities in outcomes are evident.

Second, even among households that do see improvements in their material conditions, the dispersion is substantial. In terms of income, the effect identified at the 90th percentile of the conditional distribution is 2.4 times larger than that identified at the median. For assets, the effect identified at the 75th percentile of the conditional distribution is 6.7 times greater than that identified at the 25th percentile. Additionally, a striking observation is that, given their assignment to treatment and conditioned on baseline assets level, households in the 90th percentile and above in the total business assets distribution experience asset accumulation exceeding the size of the seed capital transfer.

3.2 Impact Heterogeneity by Baseline Psychological State

Theory suggests three likely sources of impact heterogeneity for 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 an important source of impact heterogeneity for graduation programs. In this study, using midline data, I examine whether and how differences in baseline levels of depression, internality (a locus of control subscale), aspirations, and self-efficacy lead to varying *Tenondera* treatment effects. Given that a key component is the transfer of intangible psychological assets, individuals with different psychological states may experience disparate outcomes from the program. Specifically, a poor psychological state at baseline might limit the ability of a households to benefit. Baseline values of these variables are plotted in Appendix C, demonstrating substantial variation in these measures prior to program implementation.

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 before the survey, aiming to capture 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 apply a commonly used cutoff of 10, above which respondents are considered to have symptoms consistent with clinical depression. Using this threshold, I find that 20.67% of respondents experienced depression at baseline.

The locus of control construct comprises three subscales (Levenson (1981)): *internality* measures a person's confidence in their abilities and capacity to control their life; *powerful others* captures the extent to which someone feels their life is controlled by people with advantages over them; and *chance* assesses how much a person attributes their life situations to luck. In the empirical analysis below, I focus on the internality subscale. In the baseline sample, internality scores range from 0 to 24, with higher scores reflecting a stronger belief in one's capacity to control their life. I apply a cutoff of 15, below which respondents are considered to have low internality, representing 18.58% of respondents at baseline.

Aspirations are measured using an index constructed from a series of statements about a person's satisfaction with their current situation and their intentions for business growth or improvement. The aspirations survey questions are drawn from Lybbert and Wydick (2019). Lastly, self-efficacy is quantified through an index based on statements about a person's perceived capabilities and confidence in reaching their goals (adapted from the New General Self-efficacy Scale featured in Chen et al. (2001)). Based on the index values, I set natural cutoffs and find that, at baseline, 25.52% of respondents showed low aspirations and 20.84% experienced low self-efficacy.

To measure the impact of these psychological measures on *Tenondera* outcomes, I run a modified version of Equation 1 that includes an interaction between an indicator for low psychological state at baseline and treatment:

$$y_{hd} = \alpha_0 + \alpha_1 y_{hd}^0 + \beta Treat_{hd} + P_{hd}^0 \times [\delta_0 + \delta_1 Treat_{hd}] + \gamma_d + \varepsilon_{hd}, \tag{2}$$

where the new binary indicator P_{hd}^0 is a time-invariant variable that switches on for respondents with a low level of a given psychological variable at baseline. This specification enables identification of differential effects across two sub-populations based on their baseline psychological state. For instance, when comparing beneficiaries who were depressed and not depressed at baseline, the treatment effect on those depressed is $\beta + \delta_1$, while for those not depressed, it is β. It follows that the impact gap due to poor psychological state at baseline is δ_1.

For the following analysis, I run the specification in Equation 2 separately for each psychological factor. Alternatively, the four measures could be combined into a single mental health index, and Equation 2 could be estimated by defining P_{hd}^0 using baseline values for this index. I do not present the results from this approach are here, as they are qualitatively similar to those featured in Table 5.

Based on Equation 2, the estimated ITT impact parameter β for beneficiaries with a regular psychological state at baseline and the differential impact parameter δ_1 for those with a low psychological state are reported for each measure in Table 5. Overall, these coefficients are estimated with noise, providing no conclusive evidence of baseline psychological state being a source of impact heterogeneity in this study. I will revisit this analysis at endline to determine if the precision in the estimation of these results improves.

Notably, some point estimates for the differential impact parameter are large relative to the main ITT impacts, with most indicating that participants with a poorer psychological state face

Table 5: 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	$\hat{\beta}$	5.00*	(2.78)	266.75***	(36.93)
	$\hat{\delta}_1$	-4.32	(4.36)	-43.52	(55.40)
Self-efficacy	$\hat{\beta}$	4.05	(2.77)	236.79***	(33.34)
	$\hat{\delta}_1$	-0.25	(5.09)	88.96	(69.76)
Internality	$\hat{\beta}$	4.04*	(2.43)	256.72***	(35.10)
	$\hat{\delta}_1$	0.08	(5.61)	-6.23	(66.42)
Observations		2584		2584	
District FEs		✓		✓	

Notes: Each pair of rows displays estimates from a separate regression. Regressions include 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 critical level.

greater challenges in benefiting from *Tenondera*. For instance, beneficiaries with depression or low aspirations at baseline experience substantially lower income gains—51% and 86% less, respectively—compared to those with regular baseline psychological states. Similarly, beneficiaries with low aspirations experience a 16% smaller increase in business assets. Interestingly, those with low self-efficacy experience a 38% greater asset gain than those with regular self-efficacy at baseline, indicating that this variable may play a more complex role in 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. If these trends are confirmed at endline with more precise estimates, they may carry important implications for cost-effective program design. One possible approach could be to intensify the coaching component of the program specifically for participants with lower psychological assets to support more robust economic gains within this sub-population. Alternatively, targeting strategies could be refined to ensure that beneficiaries with poorer psychological states receive additional support prior to receiving the program or are identified for different interventions. Such adjustments could prevent potential inefficiencies in program delivery and help ensure that resources are allocated to maximize outcomes across diverse participant profiles.

3.3 Average Treatment Effects for Psychological Outcomes

Having explored the potential role of baseline psychological state as a source of impact heterogeneity in economic outcomes, I now shift focus to examine these psychological variables themselves as direct outcomes of the program. *Tenondera* aims 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. If this component achieves its intended effect, one would anticipate observable improvements in these psychological measures.

Table 6 presents the average treatment effects for depression, aspirations, self-efficacy, and internality, following the specification in Equation 1. Panel A shows raw scores, while Panel B categorizes participants using the thresholds for low psychological state defined above in Section 3.2. Contrary to expectations based on program design, the findings reveal a consistent trend: on average, participants experience a worsening in their psychological states at this stage of the program. Specifically, the proportions of beneficiaries classified as depressed, having low aspirations, low self-efficacy, and low internality increased by 6%, 5%, 8%, and 7%, respectively. Therefore, while the program's economic benefits are evident, its impact on psychological well-being is more complex and warrants further investigation.

Several potential explanations may account for this phenomenon. One possibility is that the structure of the program places burdensome expectations on participants. The emphasis on households transforming their livelihoods through business success could create a pressure-filled environment, leading participants to feel frustrated or distressed if they perceive a gap between their efforts and economic outcomes. This may be particularly challenging as par-

Table 6: Average Treatment Effects for Psychological Outcomes

Variable	ATE
Panel A	
CES-D 10 score	0.48**
	(0.23)
Aspirations	-0.14**
	(0.06)
Self-efficacy	-0.21***
	(0.06)
Internality	-1.07***
	(0.32)
Panel B	
Depressed (percentage points)	5.71**
	(2.20)
Low aspirations (percentage points)	5.23**
	(2.56)
Low self-efficacy (percentage points)	8.47***
	(2.77)
Low internality (percentage points)	7.17**
	(2.89)
Observations	2046
District FEs	✓

Notes: Regressions are run only on the subsample where the same respondent was surveyed in both rounds. For each regression, only the ITT impact parameter is reported. Regressions include 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 critical level.

ticipants are in the early stages of applying new technical and life skills, which may not yield immediate results. Mid-program, when participants are actively working to meet the transformative goals of the program but may not yet see substantial returns, this misalignment between effort and outcome could intensify feelings of stress. Another factor could be issues with program implementation, particularly in the delivery of life skills training. Finally, anticipation effects among non-beneficiaries could play a role. Non-participants may feel optimistic about the future as they approach potential program inclusion, leading to improved psychological states. In this case, the negative treatment effects observed may partially reflect this pattern. Newly added questions for the endline round will enable a more detailed analysis of these aspects.

3.4 Spillover Analysis

I leverage the saturation design of the study to investigate the presence and magnitude of spillover effects. Existing spillovers could bias estimates of program effects when using standard impact evaluation techniques. This design allows for the estimation of spillovers by comparing outcomes 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 intervention at midline. 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 saturation rate of zero.

The imposed saturation intensities (outlined in Tables 1 and 2) were designed to yield a limited set of values for the saturation rate at midline. However, these rates served as targets, and practical implementation details introduced greater variability in the actual saturation rates observed across communities. For example, in Scheme B, while the target saturation rate at midline was 0.8, the average saturation rate among study households in these communities is 0.81, with a standard deviation of 0.09. The other schemes behave similarly. Consequently, the saturation rate functions as a continuous variable, ranging from 0 to 1 (with a mean value 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 examining spillover effects across different saturation levels.[5] While a similar analysis could instead be conducted using saturation rate as a continuous variable, there are advantages to the categorical approach.

[5] Using the distribution of midline saturation rates (see Appendix D), I set cutoff points at 0.6 to differentiate low from medium saturation and 0.75 to separate medium from high saturation, resulting in 51% of sampled households in the zero group, 9% in the low group, 22% in the medium group, and 18% in the high group. The results presented in this section are robust to the choice of alternative, similar cutoffs.

Table 7: Impacts and Spillovers at Different Saturation Categories for Economic Outcomes (USD)

	Monthly per capita income		Household business assets	
Treated	-1.72	(4.94)	203.52**	(98.75)
Low saturation	3.17	(5.26)	105.31*	(56.37)
Medium saturation	4.84	(4.32)	-72.01*	(42.34)
High saturation	-10.57*	(6.25)	-36.33	(58.34)
Medium saturation × Treated	5.47	(6.98)	133.86	(112.16)
High saturation × Treated	14.86*	(7.63)	114.63	(122.79)
Baseline level of outcome	0.28***	(0.02)	0.38***	(0.06)
Constant	43.58***	(2.14)	219.11***	(35.26)
Observations	2573		2573	
District FEs	✓		✓	

Notes: 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 critical level.

The categorical variable helps reveal potential nonlinearities in spillover effects and allows for a more straightforward interpretation of the estimated effects 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 adaptation of Equation 1, following Baird et al. (2018):

$$y_{hd} = \alpha_0 + \alpha_1 y^0_{hd} + \beta Treat_{hd} + \Sigma_{s\in(Zero,Low,Medium,High)} S_s \times [\theta_{0,s} + \theta_{1,s} Treat_{hd}] + \gamma_d + \varepsilon_{hd}, \quad (3)$$

where S_s denotes a set of indicator variables that turn on when household h is in a community within a specific saturation group. The estimated spillover effect for a household in the control group within a community assigned to saturation category s is represented as $\theta_{0,s}$. For a household assigned to treatment within the same community, the estimated direct impact is given by $\beta + \theta_{1,s}$.

Table 7 displays the estimated direct treatment and spillover effects for key economic variables based on the full study sample.[6] In this table, the mean for the super-control group is represented by the constant term, with all other coefficients indicating differences relative to this group. Specifically, the row labeled "Treated" corresponds to the main treatment effect, β, while the rows for low, medium, and high saturation represent the spillover effects for non-treated households in these saturation categories, $\theta_{0,s}$. The rows for medium and high saturation interacted with treatment rows capture the differential spillover effects for treated households in these saturation categories, $\theta_{1,s}$. Notably, there is no interaction term for zero saturation with treatment; thus, the treated row reflects outcomes for treated households in low-saturation communities (relative to the super-control group).

Among the monthly per capita income estimates, two are statistically significant at the 10% level, both corresponding to high-saturation communities. Households in these high-saturation communities, where many neighbors participate in *Tenondera*, experience divergent outcomes depending on their own treatment status. Non-treated households in high-saturation communities show significantly lower income compared to super-control households, and notably, they fare worse than non-treated households in communities with lower saturation levels. In contrast, treated households in high-saturation communities achieve a significantly higher income than super-control households, with outcomes surpassing those of treated households in communities with lower saturation levels. The results for business assets follow a similar pattern. Overall, higher community saturation leads to better economic outcomes for treated households but appears to negatively impact non-treated households in those same communities.

An asset-building anti-poverty program can generate spillovers of this kind through various mechanisms. Spillovers may be pecuniary, where an increase in program beneficiaries affects the returns others receive from their economic activities. These effects can be negative, such as through congestion or competition, or positive, with agglomeration benefits arising when

[6] Results using only the saturation design subsample are available in Appendix E; however, these results are less precise and informative due to the smaller sample size.

economic activities cluster and complement one another. Additionally, spillovers can have a psychosocial dimension, as psychological assets like confidence and aspirations are non-rival and can be shared within communities.

While market forces may influence the spillovers observed in this section, certain types of effects are more likely than others. The program does not seem to generate a broad agglomeration effect that lifts all households equally within the community. Instead, a substantial source of spillovers may be psychosocial.

When a greater number of neighbors participate in the program, the shared experience could reinforce the motivation and soft skills imparted by the program, leading to improved outcomes for fellow participants. This supportive environment might create a network of resource-sharing and encouragement that aligns with the goals of the program, helping beneficiaries feel less isolated and more resilient in their efforts.

This social diffusion of psychological assets may or may not extend beyond program participants. High-saturation communities could potentially benefit non-participants by fostering a sense of collective advancement and optimism. In some cases, however, they may lead to adverse effects among non-beneficiaries, such as social comparison or perceived exclusion, which could undermine their well-being.

The hypothesis that psychosocial factors are a source of spillovers finds support when examining spillover effects on psychological outcomes. Table 8 provides suggestive evidence that high-saturation communities improve psychological well-being for treated households but not for non-treated ones. Results show that treated households in high-saturation communities report lower depression scores than super-control households, while non-treated households exhibit higher scores. This outcome supports the notion that potential psychosocial spillovers are concentrated among active program participants.

At this time, I cannot definitively identify the sources of these spillovers. However, I am conducting further analysis to explore whether the introduction of the program into communities

Table 8: Impacts and Spillovers at Different Saturation Categories for Psychological Outcomes

	CES-D 10 score	
Treated	0.26	(0.75)
Low saturation	0.29	(0.46)
Medium saturation	0.64**	(0.32)
High saturation	0.74	(0.66)
Medium saturation × Treated	-0.56	(0.86)
High saturation × Treated	-0.07	(1.04)
Baseline level of outcome	0.19***	(0.02)
Constant	5.01***	(0.20)
Observations	2036	
District FEs	✓	

Notes: 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 critical level.

led to shifts in market composition. For instance, the program may have promoted diversification, with households investing in ventures uncommon in their communities, or alternatively, increased concentration, with beneficiaries investing in more familiar enterprises. Isolating these effects will help clarify the specific roles of congestion, competition, and agglomeration relative to psychosocial spillovers. This analysis will provide a clearer understanding of how different types of spillovers contribute to the overall impact of *Tenondera*.

4 Conclusion

This study provides insights into the economic and psychosocial impacts of the Paraguayan graduation program, *Tenondera*. Through an RCT, I observe 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. Baseline psychological characteristics emerge as a possible explanation for these variation, though the results at midline are inconclusive due to estimation noise.

While the program demonstrates positive economic impacts, 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 psychological measures, with increased rates of depression, low aspirations, low self-efficacy, and low internality. Possible explanations include challenges in implementing the life skills component or heightened expectations that may lead to frustration 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 play a role. Newly added survey questions 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 through psychosocial channels such as shared learning and support from other beneficiaries. In contrast, non-treated households in high-saturation communities experience a negative income effect, potentially due to exclusion from these psychosocial benefits and possible competition or congestion-related pecuniary effects. This divergence highlights the nuanced ways in which exposure to neighbors participating in the program can produce varied outcomes based on own treatment status.

These findings raise important considerations for program design, particularly around targeting and support for households with low baseline psychological assets. If endline results confirm these patterns, it may be valuable to refine the program to better serve those who may be less prepared to capitalize on its resources. Options could include intensifying coaching for households with lower psychological assets or adjusting targeting mechanisms to reach individuals most likely to benefit. Such adjustments could improve the overall cost-effectiveness of *Tenondera* by better aligning support with household needs.

Moreover, the findings on saturation suggest that governments should be cautious when implementing the program widely within a community while excluding some households. High saturation appears to benefit program participants but may lead to unintended consequences for those left out, such as worsened economic or psychological outcomes. Policymakers might consider targeting entire communities or ensuring that excluded households receive alternative support to counteract negative spillovers. These strategies could help optimize program impacts while minimizing the risk of adverse effects for non-beneficiaries.

In conclusion, *Tenondera* demonstrates the potential of asset-building anti-poverty programs to improve economic prospects, yet its impacts are more complex when factoring in psychological well-being and social dynamics within communities. Understanding the interplay between economic assistance, psychosocial factors, and community context is essential for enhancing the effectiveness of such programs in lifting individuals out of poverty and building resilience to shocks.

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A Attrition Checks

Table A1: Attrition: Dependent Variable: Completed Survey, OLS

Panel A	
Treatment status	−0.0145
	(0.0118)
Observations	2864
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	2864
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	2864
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

Notes: ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

B Quantile Treatment Effect Plots

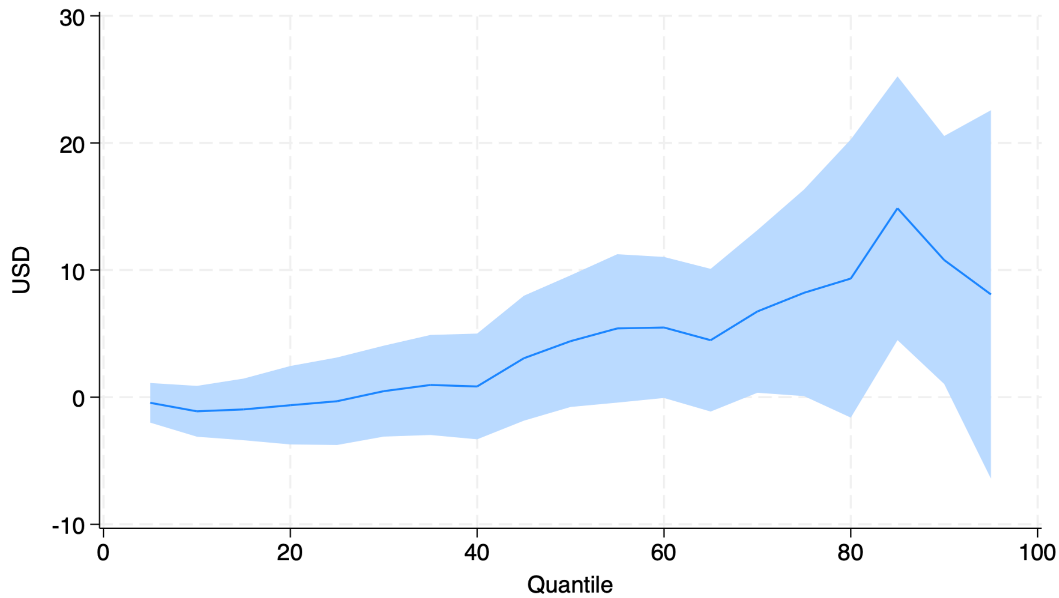

Figure 3: Conditional quantile treatment effects of assignment to treatment for monthly per capita income

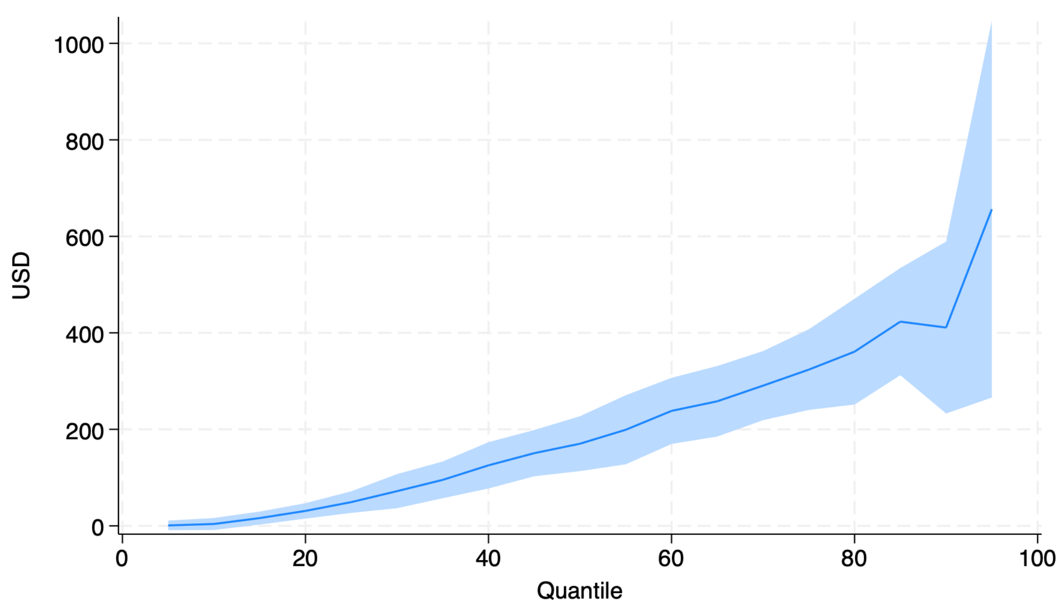

Figure 4: Conditional quantile treatment effects of assignment to treatment for business assets

C Baseline Psychological State Histograms

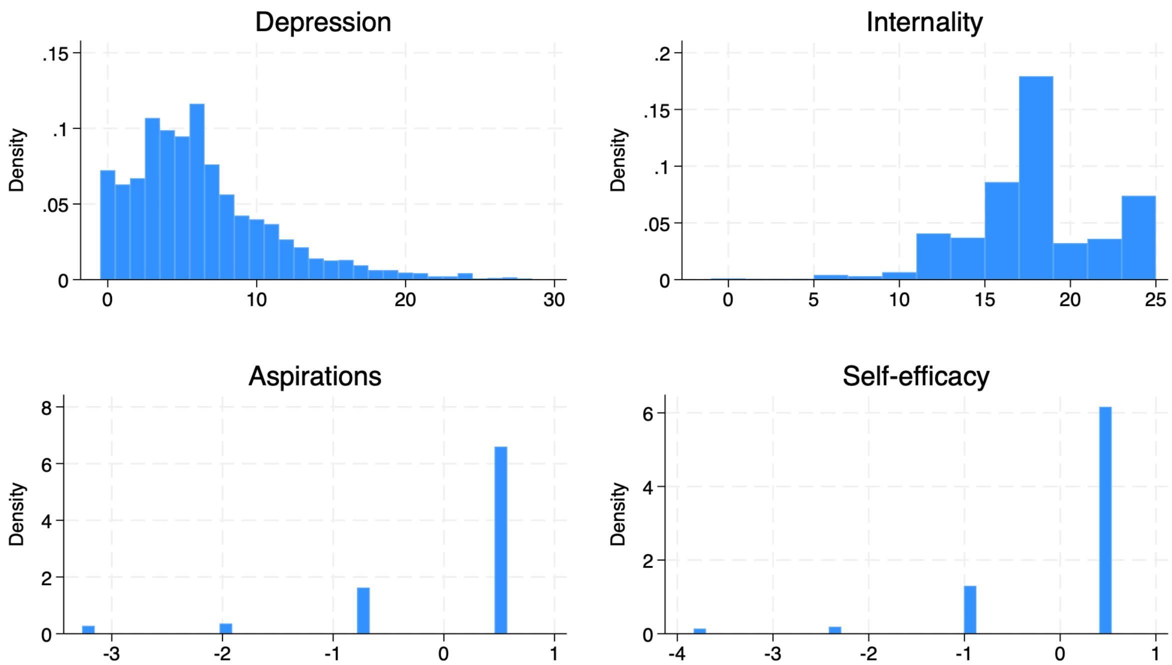

Figure 5: Histograms of baseline values for key psychological variables

D Saturation Intensity Histogram

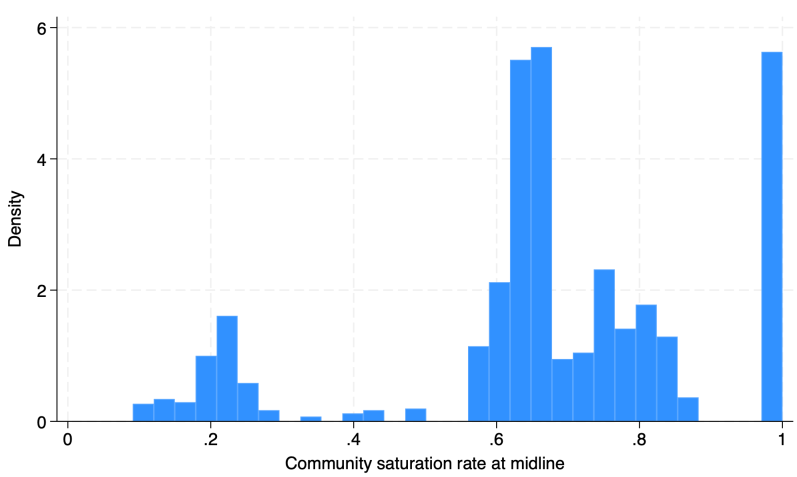

Figure 6: Saturation intensity at midline (excluding zero)

E Spillover Analysis for the Saturation Design Subsample

Table A2: Impacts and Spillovers at Different Saturation Categories for Economic Outcomes (USD), Saturation Design Subsample

	Monthly per capita income		Household business assets	
Treated	5.45	(6.04)	251.65**	(119.10)
Low saturation	-2.17	(4.34)	56.25	(57.21)
Medium saturation	14.49	(34.12)	-244.33***	(90.76)
High saturation	-8.39	(7.37)	-138.49*	(69.89)
Medium saturation × Treated	31.45	(56.46)	-464.37***	(173.15)
High saturation × Treated	9.07	(9.15)	132.31	(139.31)
Baseline level of outcome	0.33***	(0.03)	0.47***	(0.04)
Constant	36.28***	(2.59)	247.36***	(35.23)
Observations	1187		1187	
District FEs	✓		✓	

Notes: 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 community level. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.

Table A3: Impacts and Spillovers at Different Saturation Categories for Psychological Outcomes, Saturation Design Subsample

	CES-D 10 score	
Treated	1.02	(0.87)
Low saturation	0.03	(0.51)
Medium saturation	3.77	(2.71)
High saturation	0.43	(0.80)
Medium saturation × Treated	-3.61	(2.19)
High saturation × Treated	-0.49	(1.22)
Baseline level of outcome	0.15***	(0.03)
Constant	5.19***	(0.30)
Observations	931	
District FEs	✓	

Notes: 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 community level. ***, **, and * indicate significance at the 1, 5, and 10 percent critical level.