Micro-enterprise saturation: Critical mass or overcrowding?*

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Abstract

Should entrepreneurship support programs to extremely poor people be scaled by saturating a few villages, or maintaining low saturation and enrolling more villages? On the one hand, saturation may create a *critical mass* of entrepreneurs whose income generation and associated spending multipliers make each other viable. On the other hand, encouraging the start-up of many small firms in close proximity may be a recipe for *overcrowding* and artificially fierce competition in a small village economy. In a large randomized control trial in Malawi, I exogenously vary the saturation of an entrepreneurship support program to assess whether business performance increases or decreases in the share of treated neighbors. I also test a simple intervention to mediate overcrowding by encouraging entrepreneurs-to-be to coordinate which sectors they enter. One year after the start of the intervention, the number of successful businesses scales linearly with the number of supported entrepreneurs to be, but the overall earnings do not appear to grow as saturation increases. Instead, earnings are split among the larger number of supported entrepreneurs. The coordination intervention is not affecting business choice.

KEYWORDS: Entrepreneurship, poverty alleviation, general equilibrium, randomized control trial, saturation design, spillovers, coordination

JEL CLASSIFICATION: O12, O14, O25, D7, D51, D58

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1 Introduction

Poverty alleviation in the absence of stable employment opportunities from the private or public sector relies on individual entrepreneurship by poor people themselves (Blattman and Ralston, 2015; Banerjee et al., 2025). Policies that support entrepreneurship in developing countries are growing in popularity, but economic theory does not offer a clear-cut prediction of their impact at scale. In particular, the reason we are seeing few firms in contexts of extreme poverty might be that firms are not individually profitable as they depend on the weak demand of other similarly poor people. On the one hand, it is possible that a large-scale support program would create a critical mass of entrepreneurs whose income generation and associated spending multipliers make each other viable. The logic of a big-push individual-level intervention (Balboni et al., 2022) would then carry over to the aggregate (local) economy (Rosenstein-Rodan, 1957). On the other hand, encouraging the start-up of many small firms in close proximity may be a recipe for overcrowding and artificially fierce competition in a small village economy (Hardy and Kagy, 2020). Failure rates for micro-enterprises are high, especially soon after launch (De Mel et al., 2012; Balboni et al., 2022; Misha et al., 2019) and anecdotally too many UPG-beneficiary households start similar businesses in close proximity (Bauchet et al., 2015). This would suggest tight upper bounds to how far these programs can be scaled – and more broadly to the approach of alleviating poverty bottom-up from the household level (Goldberg and Reed, 2023).

These two scenarios (critical mass versus overcrowding) motivate my study. They imply contrasting policy prescriptions for whether entrepreneurship should be fostered by saturating a few locations or scaling over a larger geographic area at low saturation. In a large randomized control trial in Malawi, I exogenously vary the program saturation of an entrepreneurship support training to assess whether business performance increases or decreases in the share of treated neighbors. I also test a simple coordination intervention encouraging households to coordinate with each other when choosing their business venture, rather than duplicating existing businesses. The entrepreneurship support program under investigation is the graduation model, first popularized by the Bangladeshi NGO BRAC, which combines monthly cash transfers, productive assets or business start-up grants, entrepreneurship training, formation of savings groups, and one-on-one lifeskills coaching.

Ultra-poor graduation programs (UPGs), aimed at promoting entrepreneurial activity by people in extreme poverty, are the closest that development economics has come to a silver bullet for sustainably alleviating extreme poverty (Sulaiman et al., 2016; Wydick, 2020). While in-kind, conditional and unconditional cash transfers have been shown to provide effective poverty relief and invigorate micro-enterprises in the short run (Sulaiman et al., 2016; Baird et al., 2011; Blattman et al., 2014; Haushofer and Shapiro, 2016; Cahyadi et al., 2020; Brooks et al., 2022; Egger et al., 2022), in-kind transfers or strong-handed conditionalities may induce distortionary effects (Bryan et al., 2021), and the evidence on long-term effects is mixed at best and favors settings where households are above the international extreme poverty line to begin with (Gertler et al., 2012; De Mel et al., 2012; Aizer et al., 2016; Ikegami et al., 2017; Haushofer and Shapiro, 2018; Araujo et al., 2019;

Millán et al., 2020; Aggarwal et al., 2022; Altındağ and O'Connell, 2023; Fiala et al., 2022). Skills training, coaching, or access to loans or savings facilities by themselves show few if any impacts (Blattman and Ralston, 2015; Berge et al., 2015; Valdivia, 2015; Bauchet et al., 2015; Chowdhury et al., 2017; Brooks et al., 2018; Sedlmayr et al., 2020; Giné and Mansuri, 2021; Leight et al., 2021, 2022). On the other hand, UPGs following the BRAC model (studied here) seem to be standing the test of time while providing results that are larger than the sum of their parts in response to the highly complex challenge that multidimensional poverty traps pose (Sulaiman et al., 2016; Banerjee et al., 2015; Bandiera et al., 2017; Banerjee et al., 2021; Balboni et al., 2022; Chowdhury et al., 2017; Sedlmayr et al., 2020; Bedoya et al., 2019). If the first Sustainable Development Goal of ending extreme poverty by 2030 is to be achieved, the development community needs to better understand how UPGs scale.

Spillovers are a common concern in poverty alleviation programming. While Egger et al. (2022) find almost no inflationary pressure from cash transfers in Western Kenya, Filmer et al. (2021) argue that this may be linked to relatively low saturation in the study area while in their own study they find important price increases when cash transfer saturation is high and markets remote and Cunha et al. (2019) find differential price effects by village wealth. In the context of business training interventions, McKenzie and Puerto (2021) find no evidence of negative side effects, while Drexler et al. (2014) show that in their context, at least part of the benefits to the treated come from business stealing from the control group. Such spillovers may also extend to programs that support new entrepreneurial activity, the focus of this study.

One year after the start of the intervention, the saturation results are nuanced: Looking purely at the number of successful businesses, their number doubles when the number of supported households doubles. In terms of earnings, the businesses in high and low saturation clusters initially track each other's performance closely over time, but start to diverge soon after, with low saturation cluster entrepreneurs earning about twice as much. This suggests that the pie of added earnings has not grown, it has just been sliced into twice as many pieces. The coordination intervention does not meaningfully affect sector choices or performance.

This paper proceeds as follows. Section 2 describes the setting, research design, and intervention. Section 3 discusses the analysis, while results can be found in Section 4.

2 Experimental design

Setting and census This study takes place in Mangochi district in the South of Malawi, one of the poorest districts in the country. To determine program eligibility and convey an accurate picture of the local economy, I conducted a household census of all 29,000 households in the area, covering a population of almost 140,000 individuals (Appendix Table A1). The majority of the population are subsistence or pre-commercial farmers (Appendix Figure A1). Outside the harvest season, few have any income sources other than ad hoc daily labor ("ganyu"). About 40% of aggregate production in the study area is consumed without being traded in a market. Three quarters of households

report at least one member having skipped a meal in the past week (Appendix Table A3). This is in line with the fact that agricultural yields are low. The 2018 harvest lasted fewer than 20% of farmers longer than six months and four months after the 2019 harvest only half of registered households had any food reserves at all. It is six months after harvest that investments in the next planting, both in terms of seeds and fertilizers as well as in terms of labor need to be made, but when a majority of the rural poor are running precariously low on food and money, which they compensate for with casual labor, drawing them away from their own fields. Existing businesses are overwhelmingly one-person enterprises and a sizable share depend on demand within their own village.

Sample selection and treatment assignment Using the Simple Poverty Score Card and self-reported income, I identified about 40% of households as ultra-poor and eligible.[1] Using k-means geographic clustering, these roughly 11,000 households were assigned to 271 clusters. To determine the sample, I deployed a custom algorithm to select 72 clusters into the study in such a way as to maximize the distance between clusters and minimize the potential for spillovers with buffer clusters in between that are not part of the study.[2] The minimum distance between study clusters corresponds to the distance beyond which Egger et al. (2022) no longer find spillover effects of cash transfers.[3] In my analysis, I will therefore allow for spillovers within but not across clusters. Though this assumption may not exactly hold in practice (for example information is likely to "travel further" than cash), the assumption is that spillovers are substantially greater between neighbors within a cluster than they are across clusters. Clusters were grouped into sets of three that were assigned to a program implementer as well as being randomly assigned to one of three cohorts to be launched a few months apart from each other (early 2024, mid 2024, pending, see Figure 1).

Within each implementer's area, the three clusters were assigned at random to either high treatment saturation (everybody treated), low treatment saturation (half treated, randomly selected at the individual level) or pure control group (nobody treated). The saturation levels were chosen following Baird et al. (2018).[4] Assigning each implementer one of each type of cluster is tantamount

[1] The implementing partner excluded households from participation that have no female; have no children under the age of 5 as of August 1, 2023; did not appear ultra-poor based on either their stated income or the Simple Poverty Scorecard; in which the female was unable to carry out physical labor; whose data appeared implausible to the research team or enumerator; or who were not willing to be registered or could not be found during several (mop up) visits. The latter two criteria were very rare.

[2] After cluster selection and baseline survey, but before the start of the intervention, the local government designated one of our clusters as a flood-prone area and ordered it to be vacated. As our study may have encouraged people to stay in an unsafe area, we shifted to a neighboring cluster in this one instance.

[3] If anything, my study area's road network is substantially less developed and personal ownership of bicycles or even motorbikes is lower in Southern Malawi than in Western Kenya and the terrain is sliced up by seasonal rivers that make effective distances larger than straight-line distance.

[4] I am designing my study to be optimally powered for spillovers among treated households. Modifying equation (6) in Baird et al. (2018) to omit the standard error of the spillover effect on the control group, I restate their Theorem 2 to conclude that the optimal high saturation is 100% and the optimal low saturation would have been below 30% and dependent on intra-cluster correlation of outcomes. However, in conversation with the implementing partner, we decided to increase the lower saturation to 50% which would represent a more policy-relevant comparison. Less

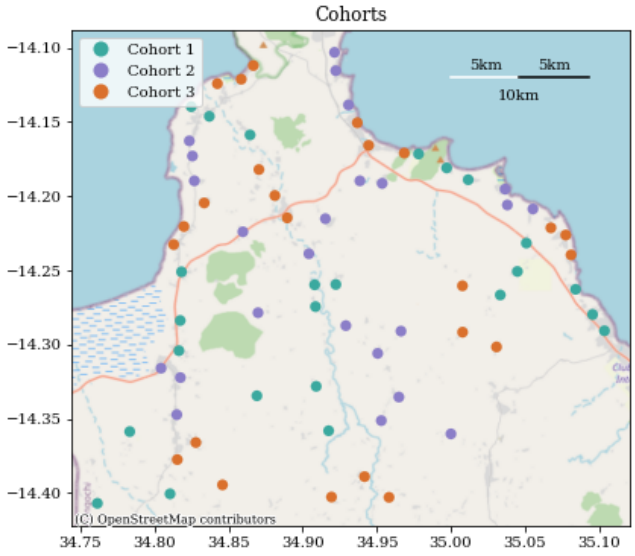


Figure 1: **Clusters** by cohort

to geographic and implementer stratification (Bruhn and McKenzie, 2009).

In order to balance the sample on a number of key outcome and mechanism variables (see Table 1), I first run the treatment assignment algorithm 100,000 times (seeding each with the counter of its iteration). I then estimate baseline balance under each theoretical treatment assignment and discard assignments that lead to a statistically significant imbalance in any of the key mechanism variables at the 10% significance threshold (Morgan and Rubin, 2012). In other words, I enforce a "visually clean" balance table. I then randomly pick one of the permissible treatment assignments. As pointed out in both Bruhn and McKenzie (2009) and Morgan and Rubin (2012), classical inference that does not take this rerandomization into account would now on average (though not in every single case) be too conservative, leading to confidence intervals on the average treatment effect with overly wide coverage and too few rejections of null hypotheses. A Fisher (1935) exact test has the correct coverage. Traditionally, researchers using rerandomization methods have done so somewhat *ad hoc*, rerandomizing until the balance table was satisfactory. My approach is more transparent. Given R simulated assignments and k baseline balance table variables, the likelihood of discarding an assignment at the 10% significance level is $q = 1 - (1 - 0.1)^k$ and I can therefore use the remaining $R \times (1 - q)$ simulated assignments for the randomization inference (Young, 2019).

I leverage the exogenous variation between high and low saturation to answer whether increased saturation leads to excess competition and overcrowding of small village economies or to critical mass and spending multipliers through which the budding entrepreneurs make each other viable.

saturated clusters would also make program components like savings groups difficult to implement.

Intervention The intervention is a standard BRAC-style UPG (Banerjee et al., 2015; Bandiera et al., 2017) with two key modifications. First, instead of livestock and related training, households receive a one-off labeled[5] cash grant of $300 intended for starting a business. Flexibility is given to participants for leveraging their own informational advantage over ability and preferences and they receive four months of weekly general-purpose business training covering financial literacy, market analysis, budgeting, record keeping, funding sources, and the development of business ideas. Second, program participants (mothers of children under five) receive an additional four-month training course on early childhood development. Otherwise, the program is comparable to BRAC's and other UPGs in the literature: Treated households receive six monthly cash transfers of $20 (labeled for investments in child health and wellbeing as well as business experimentation prior to the large grant). The structure of cash transfers is closely aligned with evidence on how poor households themselves prefer to receive such funds, combining a regular buffer with a large lump-sum that arrives after sufficient time for deliberation (Kansikas et al., 2023).

All households in the study receive fortnightly visits from program implementers which in the original program design are intended for tracking training progress and answering questions the participant may have. During the program evaluation, we additionally use these meetings for brief surveys on consumption and investment choices, livelihood activities, as well as shocks and coping behavior. Both the treatment and control group receive a basic phone for scheduling of surveys and for receipt of cash transfers or survey incentives through mobile money. At the community level, the formation of savings and loan groups is encouraged. Local childcare centers are supported financially and with the same ECD training. Both treatment and control group are expected to benefit from this last aspect.

In half of the treated clusters, implementers do not guide which kinds of businesses participants start start, while the other clusters receive a business selection guidance treatment. In these clusters, participants first listen to a story of three women in a similar position as them, having participated in an entrepreneurship training program and ready to receive their cash grant when they discover that they all three had the same business idea. The women in the story come to the realization that there would probably not be enough demand for all three of them to pursue the same business and they instead choose different businesses. After reading the story, the implementers ask participants to share with each other what kinds of businesses they would like to start (as a ranked preference list). Participants are then asked to consider what would happen to their profitability if they all started similar businesses and encouraged (but not forced) to coordinate so that they do not all start the same businesses as one another. The goal is to first widen participants' pool of potential business ideas and second to prime them to think about the general equilibrium consequences of their business choice. The coordination intervention is motivated by the fact that when asked why they would like to pursue a particular research idea, the majority of participants mentioned that

[5]The grant is largely unconditional, but the NGO encourages that it be used for starting a business or supporting an existing one. Receipt requires attending the training and being able to adequately verbalize a concrete business plan, but participants can miss sessions without penalty and are coached until their plan is satisfactory and the actual implementation of said plan is not afterwards enforced.

they had seen other people earn good money this way, which I interpret as a potential neglect of general equilibrium effects if many entrants think this way (Figure 2).

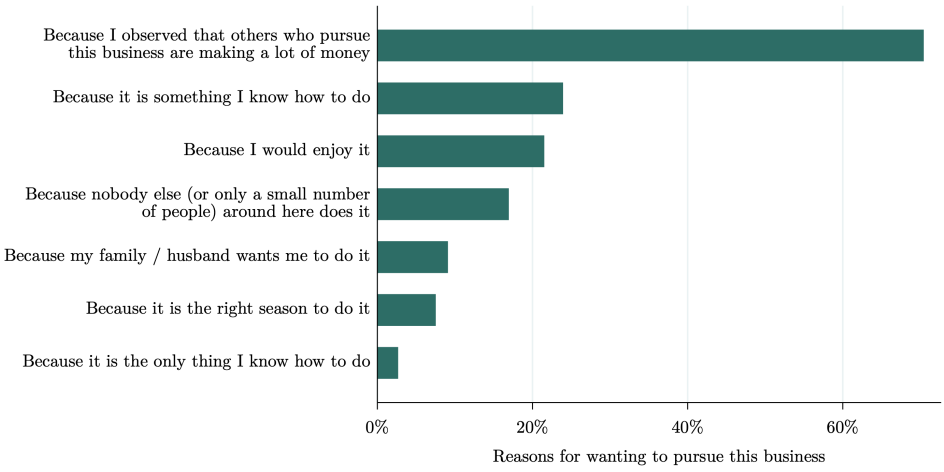


Figure 2: Evidence of general equilibrium neglect

3 Empirical strategy

There are two natural benchmarks for program performance. One is to ask, whether more or fewer businesses become successful relative to how many were supported (and how well they perform) as more people are treated (Figure 3 left). So for example, if five out of thirteen supported businesses are successful in a low saturation cluster, then if the program scales with constant returns to saturation, we would expect ten to succeed when 26 are supported. Under increasing returns, it would be more than ten and vice versa. But in practice, some losses in program effectiveness are permissible considering the important economies scale in implementation cost (Figure 3 right). Given my intimate familiarity with the cost structure of the program I evaluate, I am able to investigate not just whether there are *any* adverse saturation effects (which will be of interest to academic economists), but also how they relate to the cost savings (which will be of interest to policy makers and practitioners) when the goal is to maximize the number of beneficiaries with viable businesses *given* a fixed budget. The yellow line in the right panel, shows the program cost increasing less than one-for-one between the low and high-saturation scenario. The gap between the blue dashed and the yellow solid line represents the case where the program has decreasing returns to saturation, but they are outweighed by the economies of scale.

As pre-specified, I estimate the following linear regression:

$$\begin{aligned} y_i = &\beta_0 + \beta_T \text{Treatment} + \beta_H \text{High saturation} + \beta_C \text{Coordination Intervention} \\ &+ \beta_{H\times C} \text{High saturation} \times \text{Coordination Intervention} + \beta_{\text{BL}} y_i^{\text{BL}} + \varepsilon_i \end{aligned} \tag{1}$$

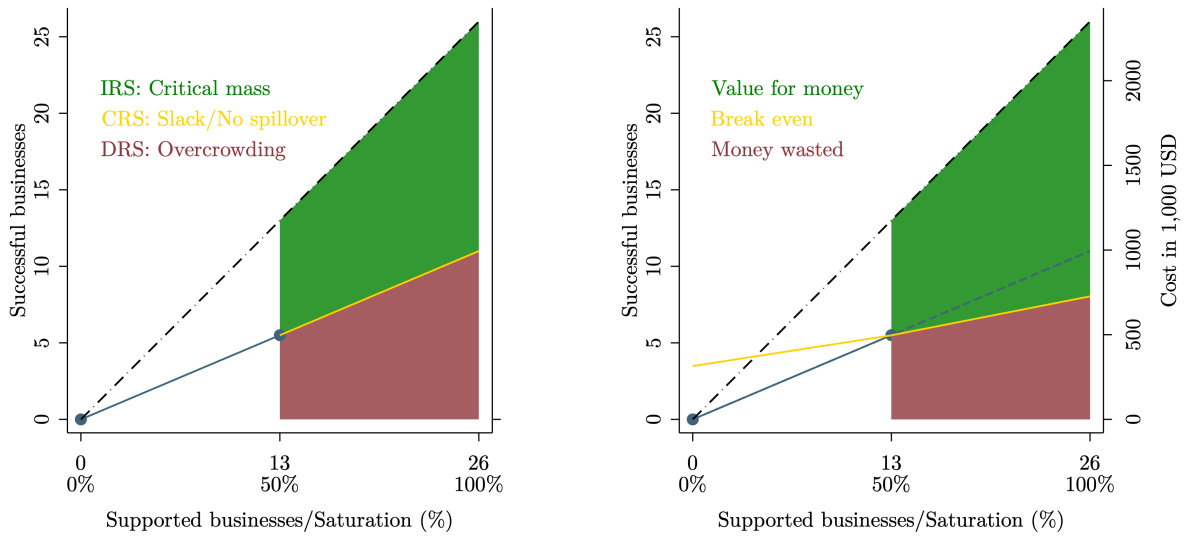


Figure 3: Conceptual framework

Standard errors are clustered at the household cluster level and I am controlling for baseline levels of all outcome variables. I investigate two key outcome variables, business survival and profits.[6] An important mechanism variable will be the participant post-training assessments to understand whether differences in performance were mediated through teaching quality.

The sign of β_H in equation (1) is the main parameter of interest in this study, theoretically ambiguous, and merits discussion. If it is relatively close to zero for both outcomes, there would be no evidence of spillovers among the treated for business performance. This would indicate room in the local economy for more firms. If the coefficient is positive for either outcome, this would speak in favor of the spending multipliers and critical mass hypothesis. If either one or both of the coefficients are negative, this would support overcrowding. If one were positive and the other negative (e.g. more surviving firms but with lower profits), then the local economy may be at capacity, but the constraint operates only along one margin. Finally, the interaction term $\beta_{H\times C}$ estimates whether the bottleneck is a dearth of variety in business ideas by testing whether increasing business diversity compensates for overcrowding in high-saturation clusters.

4 Results and mechanisms

4.1 Results after one year

In Table 1, I show sample balance. As discussed above, the clean balance table follows mechanically from the randomization approach. Attrition at the one-year follow-up (midline) survey is 2% and

[6] In what follows, I will use the term "profit" somewhat loosely. To be precise, I will focus on the gross margin, defined as revenue minus all ongoing expenses and abstract away from any amortization of a potential asset that households may already own or buy from intervention cash grant.

Table 1: Balance

	(1) Control Mean (SD)	(2) Treatment (SE)
Household and participant characteristics		
Household size	5.4	-.09054
	(1.9)	(.1347)
Age	31	-.5112
	(8.4)	(.5391)
Years of education	5.5	.01522
	(3)	(.3018)
Literate	.66	.05529
	(.47)	(.03539)
Livelihood and assets		
Hours worked (7 days)	23	-.9046
	(16)	(1.041)
HH income (12 months, MWK)	397,551	-10,340
	(342,106)	(26,829)
Total savings, assets, livestock, minus debt (MWK)	197,717	-24,421
	(292,008)	(22,688)
Largest purchase (12 months, MWK)	27,550	-3,130
	(61,344)	(4,508)
Poverty and food security		
Thatch / tarp / no roof	.76	.01683
	(.43)	(.03463)
Food insecurity score (0-8)	7.5	.09215
	(1.4)	(.0868)
Health and non-health shocks		
Health episodes (12 months)	16	-.9687
	(13)	(1.202)
Non-health shocks (12 months)	21	-.4215
	(17)	(1.065)
Observations		1,040
F		1.04
$p(F = 0)$		.41

This table describes the baseline balance. The main panel reports the control group means and coefficients of the treatment indicator in a regression predicting each baseline outcome variable. At the bottom of the table, an F statistic is reported for a regression in which all outcomes jointly predict the treatment. Standard errors are clustered at the household cluster level and reported in parentheses. Significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

balanced between treatment and control group.

Preliminary results indicate that the coordination lesson did not meaningfully increase the number of distinct or unique businesses in a cluster. If anything, it slightly increased the Herfindahl-Hirschman index for industry concentration among low saturation clusters. It appears to have compelled program participants to propose non-retail businesses ahead of receiving the cash grant (Table 2), but it did not meaningfully affect the actual sectors they are operating in after receiving the cash grant (Table 3).

Figure 4 shows signs of a divergence in earnings between the two cluster saturation levels, but the difference is not presently statistically significant (Table 4). This is because, similar to many programs of its class, there is heterogeneity in how much different studied intervention appears to benefit only a subset of entrepreneurs: In Figure 5, I split the sample by top, medium, and bottom tercile of earners, revealing that two thirds of the treated households do not earn more than their control group counterparts. Among the top earners, low saturation households earn about double what high saturation households earn. I estimate that on average 5.5 out of the 13 households in the low saturation group earn twice what the control group earns. In the high saturation clusters, this rises to 11, but the number of supported household also doubles to 26. Taken together, this suggests that the overall earning gains have not in fact grown, they are just split among more people. It will be important to track how this evolves beyond the first year.

Table 2: Business choices (planned)

	(1) Distinct businesses	(2) Unique businesses	(3) Cluster HHI	(4) Non-retail
High saturation	2.460***	0.635	-0.0396***	0.0847*
	(0.668)	(0.980)	(0.00210)	(0.0468)
Coordination treatment	1.071	1.524	0.00583***	0.120**
	(0.686)	(0.980)	(0.00210)	(0.0486)
High sat × Coordination	0.897	0.413	-0.00597*	-0.0985
	(0.958)	(1.386)	(0.00297)	(0.0607)
Low sat × non-coord mean	6.4286	3.1429	0.0828	0.0575
Observations	31	32	32	589

Notes: Where available, regressions control for baseline levels of the outcome. Standard errors are clustered at the household cluster level. Significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Business sector choices (actual)

	(1) Control mean (SD)	(2) Treatment Effect	(3) High Saturation	(4) Coordination Intervention	(5) High Sat × Coordination	(6) N (BL)
Charity & Remittances	.04639	.07348	-.1003	-.06288	.1559	495
	(.2109)	(.06481)	(.06259)	(.07825)	(.09691)	✓
Farming, Fishing & Forestry	.799	-.1578	-.07576	.007496	.06202	495
	(.4018)	(.1899)	(.2254)	(.2077)	(.3033)	✓
Manufacturing	.02577	.0527	-.01029	-.03934	.001135	495
	(.1589)	(.03228)	(.03464)	(.04644)	(.0501)	✓
Non-retail Services	.07732	.04577	.01015	-.02739	.08419	495
	(.2678)	(.08396)	(.08704)	(.09626)	(.1219)	✓
Retail	.05155	.3584**	-.04736	-.01936	-.08149	495
	(.2217)	(.1547)	(.1561)	(.185)	(.1941)	✓

Notes: Where available, regressions control for baseline levels of the outcome. Standard errors are clustered at the household cluster level. Significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Business performance

	(1) Control mean (SD)	(2) Treatment Effect	(3) High Saturation	(4) Coordination Intervention	(5) High Sat × Coordination	(6) N (BL)
Does business	.3683	.4004***	.09634	.07778	-.1889	493
	(.3733)	(.1133)	(.1073)	(.09505)	(.1291)	×
Investment in inventory	69,737	841,383**	-3,168	44,027	-306,990	493
	(228,640)	(388,910)	(458,410)	(547,655)	(634,768)	×
∼ productive assets	11,611	26,801	20,533	-10,585	-5,696	493
	(136,693)	(19,657)	(41,465)	(18,014)	(44,947)	×
Hours worked	135	5.465	6.92	8.059	-22.49	493
	(98.45)	(56.71)	(57.57)	(57.86)	(77.04)	×
Earnings	177,134	1,007,418	-472,988	-433,219	433,041	493
	(225,354)	(663,686)	(711,472)	(737,525)	(861,919)	×
Gross margin	107,397	166,035	-469,820	-477,246	740,030*	493
	(168,977)	(303,658)	(359,614)	(317,003)	(420,005)	×
Profitable	.8608	-.3608*	1.267e-14	-.09615	.1219	493
	(.347)	(.2103)	(.2561)	(.255)	(.364)	×

Notes: Where available, regressions control for baseline levels of the outcome. Standard errors are clustered at the household cluster level. Significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

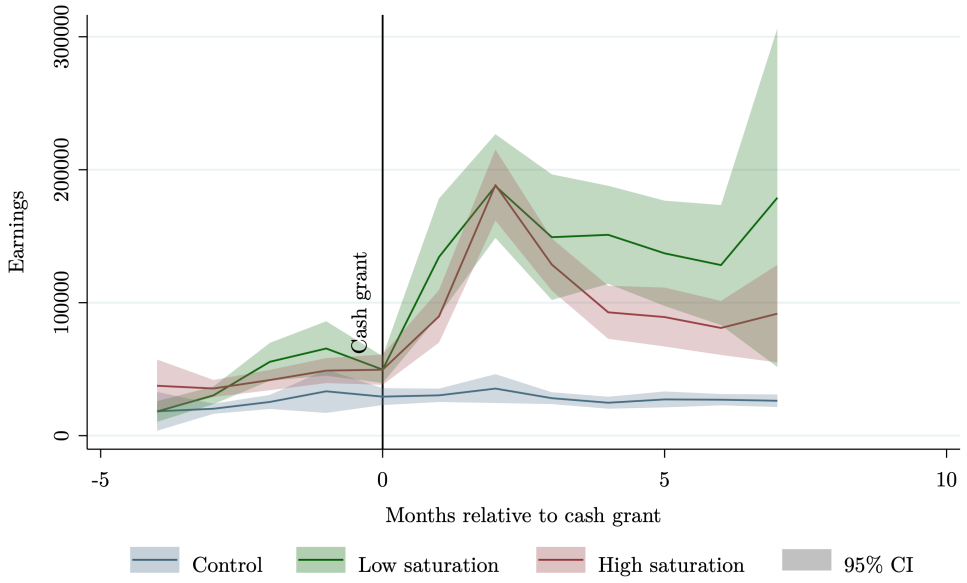


Figure 4: Earnings by saturation

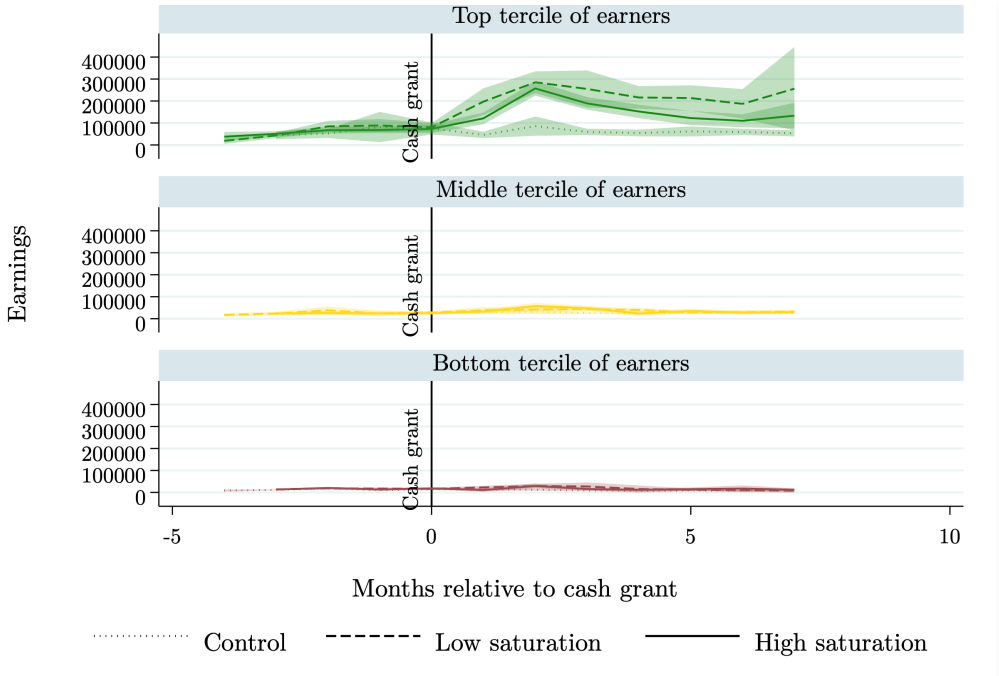


Figure 5: Earnings by saturation

4.2 Mechanisms

The section discusses potential channels through which overcrowding or slack might operate or manifest themselves. I do not find any evidence that entrepreneurs in the different types of clusters spend more or less time working (Figure 6) or invest differentially in assets (Table 5). While all treated participants are more likely than the control group to *ever* sell goods at another town's market, only those in low saturation clusters are more likely to sell locally, as well as those in high saturation clusters that received the coordination intervention (Table 6). This may be a mediating response to increased competition in high saturation clusters and would explain why the spillover effects are small. I find no effects on further away trips, longer travel, or permanent relocation in response to the treatment. The most common place for businesses to sell on a day-to-day basis, however, remains nearby (Figure 7).

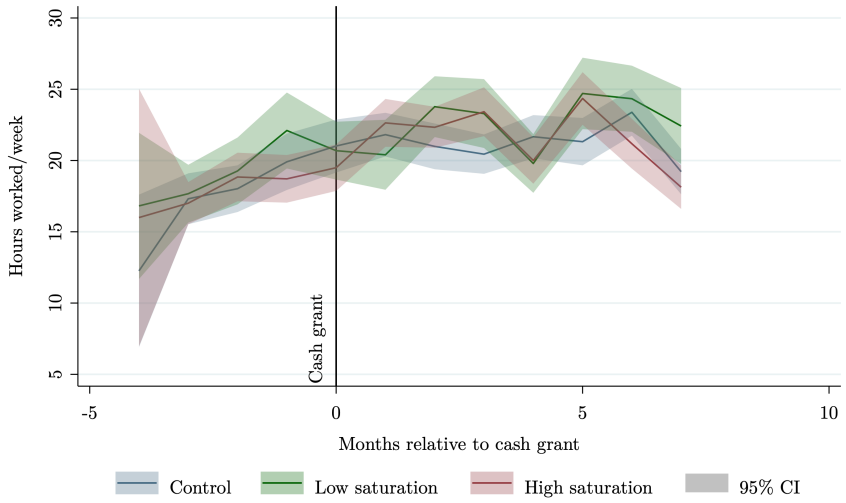


Figure 6: Hours worked by saturation

It does not appear as though the larger cluster size deteriorated training quality significantly. Table 7 shows that the high-saturation clusters score consistently (but not statistically significantly) lower than their low-saturation counterparts, though only by 1-3 points out of 100. I also verify whether the coordination intervention is reflected in the test scores by investigating whether coordination-related themes are mentioned by program participants. Those who received the coordination intervention were indeed more likely to mention being aware of one's competitors as factors that contribute to business success, though they are no more likely to say that one should find one's own niche and there is no difference in whether they highlight that one should improve on competitor weaknesses.

4.3 Discussion

High and low saturation clusters track each other very closely in business inputs and the number of successful businesses. The coordination intervention appears to have shifted some participants

Table 5: Assets owned

	(1) Control mean (SD)	(2) Treatment Effect	(3) High Saturation	(4) Coordination Intervention	(5) High Sat × Coordination	(6) N (BL)
Total assets (MWK)	357,394	205,664**	63,656	-26,236	24,854	610
	(885,452)	(74,004)	(176,807)	(53,875)	(184,055)	✓
Business assets	57,219	71,973	99,117	16,143	11,333	610
	(718,369)	(52,552)	(95,120)	(38,331)	(126,644)	✓
Agricultural Assets	13,936	11,640***	-7,078	-1,866	-1,639	610
	(18,655)	(3,794)	(5,110)	(3,822)	(5,215)	✓
Personal assets	286,239	137,852***	-39,583	-35,357	-21,663	610
	(436,897)	(46,807)	(56,355)	(45,972)	(62,149)	✓

Notes: Where available, regressions control for baseline levels of the outcome. Standard errors are clustered at the household cluster level. Significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Mobility

	(1) Control mean (SD)	(2) Treatment Effect	(3) High Saturation	(4) Coordination Intervention	(5) High Sat × Coordination	(6) N (BL)
Sell at local market	.2277	.2385**	-.239*	-.09579	.1958*	610
	(.4201)	(.09338)	(.1194)	(.07136)	(.1043)	✓
Sell elsewhere	.1914	.2421***	-.06051	-.03673	-.01556	610
	(.3941)	(.0682)	(.07651)	(.07176)	(.0815)	✓
Minutes to market	54.88	46.09	-44.67	-30.66	21.74	610
	(50.47)	(38.52)	(40.56)	(24.25)	(24.67)	✓
Times left district	.5116	1.054	-.6721	.8038	-.9883	610
	(1.764)	(.9169)	(.7952)	(.8723)	(.9008)	✓
Traveled to Lilongwe	.1056	.04786	.05555	-.05335	-.0791	610
	(.6526)	(.07095)	(.1495)	(.0771)	(.1355)	✓
∼ Blantyre	.1749	-.1231	.01549	-.0646	.01766	610
	(1.77)	(.09143)	(.07721)	(.09426)	(.1103)	✓
∼ Mangochi town	.3663	.3004	.3912	.1102	-.2275	610
	(1.125)	(.2188)	(.3767)	(.1779)	(.3904)	✓
Adults left HH	.2673	-.0188	.07519	-.05273	.01843	610
	(.618)	(.07218)	(.09667)	(.06031)	(.1041)	✓

Notes: Where available, regressions control for baseline levels of the outcome. Standard errors are clustered at the household cluster level. Significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

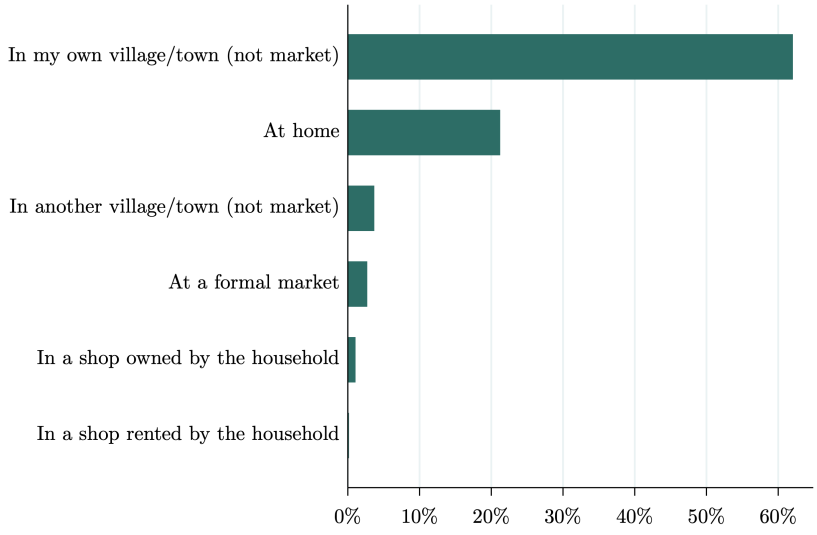


Figure 7: Location of most business sales

Table 7: Post-training evaluation results

	(1) Business knowledge	(2) ECD knowledge	(3) Preventive health knowledge
High saturation	-0.0110	-0.0108	-0.0117
	(0.0185)	(0.0121)	(0.0216)
Constant	0.403***	0.498***	0.542***
	(0.0177)	(0.0143)	(0.0228)
BL mean	0.2217	0.2577	0.2826
Observations	619	619	619

Notes: Where available, regressions control for baseline levels of the outcome. Standard errors are clustered at the household cluster level. Significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: "Name factors that contribute to business success."

	(1) Competitors	(2) Business niche	(3) Competitor weaknesses
Coordination treatment	0.114**	-0.0422	0.0574
	(0.0447)	(0.0459)	(0.0519)
Constant	0.238***	0.813***	0.480***
	(0.0282)	(0.0371)	(0.0378)
BL mean	0.0921	0.3053	0.1987
Observations	619	619	619

Notes: Where available, regressions control for baseline levels of the outcome. Standard errors are clustered at the household cluster level. Significance is denoted by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

out of the default retail sector only aspirationally, but not in actual business choices. There are noisy first signs of a divergence in earnings a few months after the cash grant, which come into sharper relief when focusing on the highest-achieving entrepreneurs in each group who are half as numerous and earn twice as much in the low saturation clusters. I conclude that in the short run, there is no evidence of critical mass formation when inducing high saturation micro-enterprise creation in an extremely poor context. There appears to be room in the economy for more successful entrepreneurs, but so far this appears to come at the cost of lower earnings per household, though this may change as the sample grows. The higher saturation did not meaningfully affect effort, asset acquisition, location of sale, or the quality of training delivery.

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Appendix

A Descriptive statistics

Table A1: Study population

	Count
HOUSEHOLDS	
Number of surveys conducted	28,853
Female primary adult present	27,684
U5 present	16,882
No children present	3,254
INDIVIDUALS	
Population	138,548
Adults	65,162
Children	73,386
U5s	20,998
Number of villages	162
Number of CBOs	25
Number of ADCs	5

Census of all households in Mangochi district's subdistrict ("Traditional Authority") Nankumba. The urban center Monkey Bay and the tourist destination Cape Maclear were omitted from this census as well as an area in the South-West of Nankumba where another NGO is implementing a livelihood program. Area Development Committees (ADCs) are administrative subdivisions of a subdistrict, while Community-Based Organizations (CBOs) are associations of several villages organizing development initiatives or providing local public goods together.

Table A2: Livelihoods

	Percent
AGRICULTURE	
Crop farming	76.5
Subsistence crop farming	73.5
Marketed crop farming	17.8
Share of yield marketed	29.2
Share of yield marketed (conditional)	69.8
Livestock	3.3
PIECE WORK	
Ganyu	51.7
Exclusively subsistence + ganyu	27.6
BUSINESS	
Business	58.0
Employees > 0	4.1
Local non-ag employment potential	3.4
Cooperates with another business	16.4

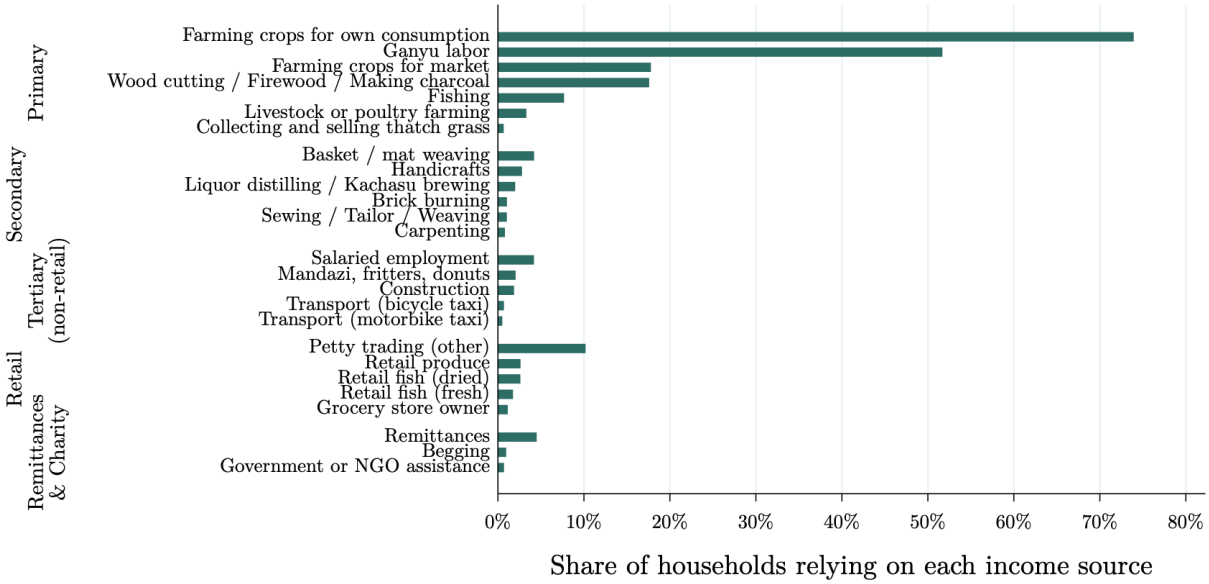


Figure A1: Most common income sources in the study area

Table A3: Poverty

	Percent
Skip meal	76.8
Female illiterate	37.7
Thatch roof	64.5
No furniture	80.3
No phone	51.9
No bedding	15.7
<2.15 PPP 2017 USD (self report)	92.7
<2.15 PPP 2017 USD (PMT)	85.1
Children / adult (cond)	1.5
Number of income sources	1.5