# How Big Does a "Big Push" Need to Be? Evidence from Randomizing Asset Transfer Size

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**Abstract**

Poverty is persistent but the evidence regarding the existence of poverty traps is mixed. We provide the first experimental test of asset-threshold poverty traps, by randomizing households in Egypt into a standard asset transfer, a half transfer, or control. Both transfer groups show large, sustained gains in assets and consumption 40 months later. By comparing how assets grow after the transfer for households at different points in the baseline asset distribution, we find no support for the presence of a poverty trap. These results suggest that scarce anti-poverty resources need not be concentrated in large asset transfers to be effective.

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# 1 Introduction

Over 800 million people live in extreme poverty (World Bank, 2025). Despite tens of billions of dollars spent on social protection programs each year (ILO, 2024), progress in reducing that number has ground to a near halt, with the 2020s on track to be a "lost decade" for poverty reduction (World Bank, 2024b). A prominent view for why this challenge persists is the idea of an *asset-threshold poverty trap* (Banerjee and Newman, 1993; Galor and Zeira, 1993). It suggests that households below a critical level of assets can get stuck at a low-wealth equilibrium, whereas the same households, given a larger endowment, would grow. This idea offers a clear and hopeful policy prescription: a sufficiently large one-off transfer can push households past the threshold and onto a path out of poverty for good. This logic has been highly influential. Graduation-style programs, which bundle a large asset transfer with training, coaching, and savings support, have been hugely successful and now operate in 88 countries, reaching over 70 million beneficiaries (Banerjee et al., 2015; Arévalo-Sánchez et al., 2024).

But the poverty trap view carries an underappreciated cost: because transfers must be large enough to clear the threshold, programs are expensive per household and so currently reach less than 10 percent of the extreme poor (Arévalo-Sánchez et al., 2024). If the trap does not bind, the same budget could help far more people through smaller, broader transfers. Whether poverty traps exist is therefore not just a long-standing theoretical question, it is also a first-order policy question about how to allocate scarce anti-poverty resources. Yet after decades of research, the question remains unsettled (Kraay and McKenzie, 2014; Banerjee et al., 2019; Balboni et al., 2022; Barker et al., 2024; Karlan et al., 2026; Bandiera et al., 2026). Testing the poverty trap hypothesis is extremely challenging (Karlan et al., 2026; Bandiera et al., 2026). What would be needed is to compare the asset accumulation trajectories of households who differ *only* in the value of their assets, but no study has yet been able to test this hypothesis using experimental variation.

In this paper, we provide the most direct experimental test of asset-threshold poverty traps to date. We carry out a cluster randomized trial of a graduation program in rural southern Egypt in which treated households are randomly assigned to receive either the full asset transfer (≈USD 700) or one that is only half that size– well below what most graduation programs provide.[1] Because the transfer size is randomized, households with similar baseline assets and other predetermined characteristics end up with sharply different post-transfer asset levels. This allows us to test for poverty trap thresholds at different points of the baseline asset distribution.[2] We find that

[1]The price of a cow in our setting is about five times larger than the value of the half transfer.

[2]More specifically, this allows us to test for a poverty trap threshold in the portion of the asset distribution bounded by the asset wealth of the half- and full-transfer beneficiaries immediately post-transfer. And we do so for each decile of the beneficiaries' baseline asset distribution.

both treatment groups show large, sustained improvements in assets and consumption over 40 months, and post-transfer asset growth is, if anything, slightly larger for the half-transfer group. This suggests that, in our context, there is no poverty trap, on average, for each decile of the distribution of baseline assets. If poverty traps do not bind more broadly, as our results suggest, then for the same budget, policymakers can choose between concentrating resources in larger transfers to fewer households or spreading them across many more households, without reducing aggregate gains in assets, income or consumption.

More specifically, we work with 3,469 poor households in southern Egypt across 435 agglomerations. In line with the standard poverty graduation model, our "full-intensity" intervention provides households a bundle that includes a large asset transfer, training, consumption support (cash) for the first 8 months of the program, as well as further support such as bi-weekly household visits by a counselor, bi-weekly coaching/savings group meetings and access to a health emergency fund. In our half-intensity intervention arm, we provide all the same components, but only half the quantity for most of the components.[3]

We find that both the full- and half-transfer interventions improved asset ownership and consumption outcomes at endline (40-month post-asset transfer). The Average Treatment Effects (ATE) of the half-transfer intervention are at least half those of the full-transfer intervention in magnitude. In particular, we find that total asset ownership increases by EGP 13,161 in the full-transfer arm and EGP 8,156 in the half-transfer arm compared to a control mean of EGP 6,654. Monthly household consumption increases by EGP 240 and 276 in the full- and half- transfer arms, respectively, compared to a control mean of EGP 5,514.

When comparing the asset accumulation of households with the same baseline asset ownership, but who exogenously differ in the value of assets due to treatment, we find no support for the presence of asset-threshold poverty traps. Indeed, we do not find that *post-transfer* asset accumulation (i.e. how asset values changed after the treatment and until the endline survey) is larger among the full-transfer group relative to the half-transfer group. In fact, for 90% of the baseline asset distribution, the point estimate for the average treatment effect of the full-transfer treatment on *post-transfer* asset growth is *smaller* than for the half-transfer treatment—consistent with a concave transition function between assets today and assets tomorrow.

Our main results rule out asset-threshold poverty traps on average and for the average household in each decile of the baseline asset distribution. But they do not rule out the possibility that poverty traps bind for specific subgroups (e.g., entrepreneurial

[3] Although most components of the intervention were cut in half in the half-intensity arm, not just the transfer, for conciseness we will subsequently refer to the two arms as the full-transfer and half-transfer arms.

types as in Banerjee et al., 2019). To address this possibility, we explore heterogeneity in the effect on post-transfer asset accumulation of the full- vs. half-transfer treatment using machine learning (specifically, the approach developed by Chernozhukov et al., 2025). We find that 40% of the sample would experience more post-transfer asset accumulation with the full- than the half-transfer treatment, on average. But even in the top quintile of households with the largest relative gains, the average additional accumulation is no larger than the additional cost of the full transfer. In other words, even in this group, there is no evidence of higher per-dollar investment with the full-transfer intervention than with the half-transfer intervention.

It's worth noting that if we used cross-sectional variation in baseline asset-ownership and a single transfer size our results would be consistent with the existence of a poverty trap. First, the impact of either transfer on post-transfer growth tends to increase with baseline assets. Second, in our machine-learning heterogeneity analysis, we find that being better-off at baseline (e.g., owning more livestock) is a strong predictor of treatment effects on post-transfer asset growth (and strong predictor of being more likely to benefit from larger transfers). The random variation in asset ownership created by our alternative treatments is therefore essential in ruling out that interpretation.

We then investigate mechanisms. Both arms lead households to engage in more economic activities than control. But they diverge in what kinds. While both full- and half-transfer groups add market activities equally, the full-transfer arm engages in even more subsistence activities. Since market activities are what typically drive post-transfer asset growth, seeing both groups doing similar amounts helps explain why the full-transfer arm doesn't outpace the half-transfer arm: the extra capital goes into diversification, not into additional market work. As an additional test of whether full-transfer beneficiaries enjoy less uncertain post-transfer asset growth as a result of higher diversification—i.e., whether they trade-off lower average growth for lower risk, we compute the coefficient of variation across total assets, livestock, income and consumption and find them to be slightly lower in the full-transfer arm relative to the half-transfer arm.

We contribute to three strands of literature. First, we contribute to the literature on poverty traps. Despite decades of research, the evidence on whether asset-threshold poverty traps exist remains mixed.[4] Recent evidence based on randomized trials has

[4]Descriptive tests have yielded contradictory conclusions across settings. Jalan and Ravallion (2004) and Naschold (2013) find no evidence of poverty traps in China, Pakistan, and Ethiopia, while Barrett et al. (2006) and Adato et al. (2006) find supporting evidence in rural Kenya, Madagascar, and South Africa. Kraay and McKenzie (2014) review this evidence and conclude that asset-threshold poverty traps are unlikely outside extremely remote markets or cases where the poor face severe behavioral constraints. Jee (2025) revisits this evidence by harmonizing data from 27 randomized transfer experiments and finds trap dynamics in 60% of studies on average but only 25% of households trapped within a given study. His identification still requires comparing households at different asset levels, an assumption our experimental variation shows can distort conclusions.

not settled the question. Banerjee et al. (2021) find growing positive effects of a graduation program ten years after the asset transfer in India, consistent with a big push helping households to escape a poverty trap. Banerjee et al. (2019) find effects of micro-credit consistent with a poverty trap, although only among a subset of talented entrepreneurs. Using data from a randomized graduation program in Bangladesh, Balboni et al. (2022) interpret divergence of households trajectories around an asset threshold just above the value of the transfer as evidence of a poverty trap. Karlan et al. (2026) reanalyze the same data and argue that geography, more than household asset levels, drives the observed divergence. Barker et al. (2024) also conclude that there is no poverty trap in Ethiopia as their control group catches up to treatment over time. A fundamental challenge is that existing tests rely on observational variation in asset holdings or indirect comparisons across households, making it difficult to separate threshold dynamics from other factors that shape household trajectories (Karlan et al., 2026).

A few experiments have varied the size of one-off *cash* transfers (Haushofer and Shapiro, 2016, 2018; Aggarwal et al., 2024), but these compare average effects across transfer sizes rather than testing for the non-linear dynamics that the poverty trap hypothesis implies, such as whether returns differ across the asset distribution or whether a threshold exists above which asset growth accelerates.[5] We contribute to the poverty-trap literature by providing the first direct experimental test of the asset-threshold poverty trap hypothesis, using randomly assigned differences in transfer size to generate exogenous variation in post-transfer asset ownership across otherwise identical households, and testing for threshold dynamics at different points of the baseline asset distribution. Using experimental variation in asset ownership proves to be important. Our results show that patterns suggestive of threshold effects in observational comparisons do not survive experimental variation. For a given treatment, we tend to find larger impacts on post-transfer asset accumulation for households with larger (endogenous) baseline assets, but we do not find larger impacts for larger transfers which exogenously shift households on the asset distribution.

Second, we contribute to the literature on poverty graduation programs and, more broadly, to the literature on the design of social protection programs (see Banerjee et al., 2024, for a thorough recent review). Graduation programs have proven effective across a wide range of settings (e.g., Banerjee et al. 2015; Bandiera et al. 2017), and recent work has begun to unbundle the multi-component model to understand which elements drive impacts, testing the importance of coaching (Beam et al., 2025), asset transfers alone (Banerjee et al., 2022), complementary support with no asset transfer

[5] Unlike in-kind asset transfers, cash transfers are more easily subjected to social "taxes" and self-control issues, which can prevent larger transfers from generating more than proportional impacts even when non-convexities exist in the production set.

(Leight et al., 2023), or simplified designs (Sedlmayr et al., 2020).[6] In the broader social protection literature, experimental evidence has compared transfer modalities (Cunha, 2014; Crépon et al., 2024), conditionality (Baird et al., 2011; Benhassine et al., 2015), and targeting approaches (De Walque and Valente, 2023; Hanna and Olken, 2018). Yet nearly all of this work varies *what* is given or *how* it is structured. The few experiments that do vary transfer size study unconditional cash, not multi-component programs.[7] We contribute to these literatures by varying the intensity of the full graduation bundle rather than removing individual components, holding the structure of support fixed while scaling its size. We show that a proportionally scaled-down program generates per-dollar impacts at least as large as the full program, implying that the same budget could reach substantially more households without sacrificing long-run effectiveness. This finding is consistent with recent evidence that broader, less intensive livelihood programs can generate large impacts (Mahmud and Riley, 2025) and that widespread transfers can produce large multiplier effects (Egger et al., 2022; Walker et al., 2024).

Third, we contribute to the literature on returns to capital in low-income settings. That literature has largely studied small cash grants or loans to micro-entrepreneurs and has documented high average returns in some settings, alongside substantial heterogeneity across recipients.[8] More recent work points to the importance of larger, lumpier productive investments, whether through larger enterprise loans (Bryan et al., 2024) or asset-based microfinance contracts (Bari et al., 2024; Haggag and Osman, 2026). But much less is known about how returns vary with the scale of large productive transfers among households at the very bottom of the asset distribution, where poverty-trap logic is most often a concern. Our findings are consistent with returns being concave rather than convex over this range, pointing toward diminishing rather than increasing marginal returns to capital.

# 2 Context and Experimental Design

## 2.1 Context

Egypt is a lower middle income country with a population of more than 100 million people. As of 2021, Egypt had a poverty headcount ratio of 33.5% based on the national poverty line and 7.1% based on the $4.20 a day in 2021 PPP international poverty line (World Bank, 2024a). Much of this poverty is concentrated in the rural areas of

[6]Another strand of literature has instead tried to add additional facets to the standard graduation program to tackle further constraints (e.g., Bossuroy et al., 2022).

[7]Kondylis and Loeser (2021) review this evidence via meta-analysis, finding that increasing cash transfer size does not improve the persistence of impacts.

[8]See, for example, de Mel et al. (2008), de Mel et al. (2012), Fafchamps et al. (2014), and Hussam et al. (2022).

southern Egypt. We work in Assiut and Sohag, the two poorest governorates in Egypt, with poverty rates estimated at 66.7% and 59.6% respectively in 2018 according to the national poverty line (MPED, 2021).

We worked with the Sawiris Foundation for Social Development (SFSD) and BRAC to design localized versions of the well-known "Graduation Approach", a comprehensive "big push" intervention called "Bab Amal" or "Door of Hope".[9] The primary intervention, which lasted 18 months for each household, provides households a bundle that includes the transfer of an asset (chosen from a list by participants), training on how to make the most of that asset, consumption support (cash) for the first 8 months of the program, bi-weekly household visits by a counselor, and bi-weekly coaching/ savings group meetings. Households also received some simple financial education, access to an emergency health fund, and support in linking them to any government support they were eligible for that they did not yet avail themselves of.

## 2.2 Interventions

We sought to test the impacts of the standard BRAC program relative to a control group who did not receive support. We also designed a "Half-Transfer" intervention. The half-transfer arm provides households with about half of the support that is provided by the "Full-Transfer" arm. Whereas the full-transfer arm provides households with a list of assets to choose from that together were valued at 11,000 EGP at the time of the transfer (approx US$700 at prevailing exchange rates), the half-transfer arm provided households with a list of assets valued at 5,500 EGP. Similarly, the consumption support of 400 EGP a month was reduced to 300 EGP and bi-weekly household visits were provided monthly instead. Group counseling and savings groups meetings were kept on a bi-weekly basis in the half-transfer arm.

We randomly split our full-transfer arm into two subgroups. A group where the household was free to choose any member to receive the asset and training (the "unrestricted" group), and a group where the household was only allowed to choose a woman to receive the asset and training (the "restricted" group). We initially thought that many households would inefficiently choose men to take the lead on the asset and training. Instead we found that 88% of households in the *unrestricted* group chose a woman anyway. In this paper, which focuses on differences in the size of the transfers across experimental arms, we pool the two full-transfer arms for our analysis and show that our main conclusions hold when focusing only on the unrestricted full-transfer arm (Table A9). The gender of the recipient of the transfer was unrestricted in the half-transfer arm.

[9] The program's implementation was managed by two NGOs: the Giving Without Limit Association in Assiut and the Human Development Egyptian Association in Sohag. Both organizations have extensive experience in poverty alleviation and community support within their regions.

After the target households were identified, but before the randomization, we implemented an "information session" where we met with the household and explained the intent of the experiment and how the different arms worked. This included explaining that some people would receive 11,000 EGP worth of assets, others would receive 5,500 EGP and still others would receive 0. We then asked each household to decide what asset bundle they would choose if they were randomly placed in the full-transfer arm as well as what their selection would be if they were placed in the half-transfer arm.[10]

Participants could choose from a diverse menu of assets: several types of livestock (goats, sheep, chicken, rabbits), a sewing machine with equipment, inventory for retail business, skilled trade training with tools, and other options. The full-transfer budget was typically enough to purchase 4-5 sheep, though this varied by animal type, age, sex, and market conditions. Even the full-transfer amount was insufficient to purchase a cow or buffalo, so no households received large livestock. In practice, most households chose medium-sized livestock: 85% in the full-transfer arm selected goats or sheep and 63% in the half-transfer arm; the remainder chose non-livestock options.

## 2.3 Targeting, Sampling & Randomization

Since the intervention was designed to reach ultra-poor households, several measures were taken to ensure that the poorest were targeted. A 2013 poverty map for Egypt was used to select the three poorest districts in each of the two governorates. Villages in these districts were then ranked according to their poverty headcount. Thirty seven "mother villages" across the six selected districts in Sohag and Assiut governorates were selected, giving priority to villages above the median poverty headcount.[11] Using Google Maps, these villages were then sub-divided into agglomerations of roughly 70 households each. Out of these 470 agglomerations were selected, prioritizing the ones deemed poorest according to the subjective judgment of NGO field staff.

Using data from the 2015 Egyptian Household Income, Expenditure and Consumption Survey, a model was developed to predict absolute poverty status as a function of easily observable characteristics.[12] A full-count enumeration survey was then

[10] We also had them identify who would get the training and the asset in the full-transfer unrestricted arm as well as who would get it in the restricted (to women) arm. We also asked them for their asset allocations across both full-transfer arms. In addition to selecting a female recipient 88% of the time in the unrestricted arm, the choice of asset bundle was near identical across both arms (85% choosing livestock and 15% choosing non-livestock under both scenarios), making the "restricted arm" very similar in practice to the "unrestricted" arm.

[11] A mother village in Egypt is defined as a village and its associated hamlets.

[12] According to this survey, the absolute poverty headcount ratio was 17% in Assiut and 19% in Sohag. The explanatory variables included in the model were the gender of their head of household, whether they were under 25 or over 65 years of age, together with gender interactions, marital status of head, household composition, such as proportion of household members in various age groups, illiteracy

conducted in the 470 selected agglomerations to collect data on these observable characteristics (25,942 households). The data were used to estimate a proxy means score for each household in the larger sample. In each agglomeration, the 5-10 poorest households, as indicated by these scores, were selected for inclusion. The list was further refined by community leaders to exclude obviously misclassified households.

Randomization into the four treatment arms was carried out at the agglomeration level instead of at the household level to reduce the potential for negative psychological spillovers that may occur when one neighbor ends up in treatment while another equally poor neighbor ends up in control (Banerjee et al., 2015). Neighboring agglomerations were grouped into groups of four to form "quartets" and the agglomerations in each quartet were randomized into the four treatment arms (full-transfer unrestricted, full-transfer restricted to female recipients, half-transfer and control). In some villages, the number of agglomerations was not divisible by four, so we grouped some agglomerations into quintets instead. The additional agglomeration was randomly assigned to either control or full-transfer restricted or unrestricted.

We enrolled the entire sample in 2019 and began implementation in batches of quartets (our randomization strata) towards the second half of that year. In early 2020 our implementation was paused due to the lockdown associated with the COVID-19 pandemic. By the time we paused our work we had provided assets to about half of the sample. We then resumed implementation in the second half of 2020. Including randomization strata fixed effects in the regressions also controls for the timing of implementation and data collection since these vary across but not within strata.

The final sample is comprised of 3,469 households across 435 agglomerations. We ended up with 1687 households receiving the full-transfer treatment (in 218 agglomerations), 773 receiving the half-transfer treatment (in 86 agglomerations), and 1009 households in the control group (in 131 agglomerations).

# 3 Data and Methods

## 3.1 Data

We collected data at four different points: (1) an enumeration survey, (2) a baseline survey, (3) a 20-month follow-up survey, and (4) a 40-month follow-up survey.

The enumeration survey was conducted between March and August 2019, covering all households (25,942) in the agglomerations included in the experiment. The

status of head or spouse, employment state of head, and whether head is employed in agriculture, craft, or elementary occupation, whether they work in or out of establishments, typically an indicator or casual employment, proportion of male and female earners in household, some dwelling characteristics and access to amenities, and ownership of basic appliances. The model had a pseudo-$R^2$ of 0.41.

objective was to collect information on basic observable characteristics to construct the proxy means score used to identify eligible ultra-poor households as described above.

The baseline survey was conducted between June and August 2019, targeting the households that were identified as ultra-poor in the enumeration survey. It was implemented before the information sessions were held and before the randomization of the agglomerations. The baseline survey included a household questionnaire, typically answered by the head of household or their spouse, and two individual surveys administered to two adult members of the household. The household survey collected information on demographics, economic activities and income sources, labor market participation, asset ownership (including livestock and land), consumption, food security, among other topics. The adult survey focused mostly on time use, women's agency, and mental health.

The 20-month and 40-month follow-up surveys were conducted between August 2021 and July 2022 and between February 2023 and March 2024, respectively, reflecting the staggered implementation of the Bab Amal program. The household survey essentially replicated the questions asked at baseline and was administered to the head of the household. The adult survey was also similar to the baseline version but one of the adults had to be the individual designated as the program beneficiary during the information session (the designee). [13]

Appendix Tables A1 and A2 report information on baseline characteristics and balance tests across the different treatment arms on a set of outcomes related to household composition, aid and transfers received, consumption, and baseline main outcomes. Appendix Table A2 reports the p-values associated with the omnibus tests of equality between pairwise comparisons (full-transfer versus control, half-transfer versus control, and full-transfer versus half-transfer) for the full sample and for the post-attrition sample, computed jointly across the variables in both balance tables. Only 6 to 7 % of our baseline sample was not interviewed again at the endline survey (40 months after the asset transfer). In both samples, experimental arms are well-balanced, as expected under random assignment and given the sufficiently large number of observations.

### 3.2 Average Treatment Effects

When estimating the average treatment effects of the program, regressions take the following form:

$$Y_i = \beta_0 + \beta_H * Half\ Transfer_j + \beta_F * Full\ Transfer_j + \sum \delta_q + \varepsilon_{ij} \quad (1)$$

[13] A subjective expectations module was added to the adult survey to assess women's beliefs regarding their hypothetical participation in various economic activities—specifically their perceived skills and aptitudes to engage in those activities, as well as expectations regarding potential investments and returns. But we do not use the beliefs data in this paper.

where $i$ represents the household, $j$ the agglomeration and $q$ the quartet of agglomerations. $Half\ Transfer_j$ and $Full\ Transfer_j$ are dummy variables for the assignment of the agglomeration to either the half-transfer or the full-transfer intervention. $\delta_q$ represents a set of randomization strata fixed effects (agglomeration quartet). We cluster our standard errors at the agglomeration level, matching our level of randomization. To understand the relative impact of the half-transfer versus the full-transfer interventions, we test for the equality between $\beta_H$ and $\beta_F$.

## 3.3 Testing for Poverty Traps

### 3.3.1 Prior Tests of Asset-Threshold Poverty Traps

In Figure 1, we depict asset dynamics in the presence of an asset-threshold poverty trap. The x-axis represents the amount of assets the household has in the pre-period, while the y-axis represents the amount of assets they have in the post-period. When the curve is above the 45-degree line, tomorrow's assets exceed today's assets—i.e., assets grow. But when the curve is below the 45-degree line, assets tomorrow are *less* than assets today, so that households revert to poverty.

These dynamics give rise to two stable equilibria such that the value of assets is constant over time: one low-asset equilibrium A* and one high-asset equilibrium C*, as well as one unstable equilibrium B* such that a household who has just under B* assets today will revert back to A* whereas a household who has just over B* assets today will converge to the high-level equilibrium C*.[14] In other words, there is an asset poverty trap threshold at B*.

Without experimental variation in the value of assets, a typical test for the presence of a poverty trap would be to compare the asset dynamics experienced by households who start at different levels of assets, and check whether the dynamics of those who start with assets above a certain threshold are more favorable than the dynamics of those who start below that. But this comparison is likely endogenous, since the amount of assets in the pre-period can be associated with many other things that will affect asset accumulation.

Balboni et al. (2022) implement a test in this spirit in a context with two neat features: (1) the baseline distribution of assets in their sample of households is bimodal, with a low-density region between a low- and high-mode, which suggests the presence of an unstable equilibrium in the low-density region and (2) treated households, who receive the one-off transfer of a cow, turn out to be split between the left- and right-hand side neighborhood of the candidate unstable equilibrium. Even though the split between left- and right-hand side is not generated experimentally, this appears to

[14] In our illustration, A* is strictly positive, but the same argument follows if A* is equal to zero.

be the most reasonable scenario under which to test for the presence of a poverty trap using variation in baseline assets, since they are able to compare the asset dynamics of households who do not differ much in baseline assets but end up on different sides of the candidate critical threshold to escape a trap. Although, as Karlan et al. (2026) show, this comparison suffers from some drawbacks, including that households on the left (right) side of the threshold are concentrated in geographic locations where asset prices are low (high), which is in itself likely to be correlated with different asset dynamics.

#### 3.3.2 Novel Test of Asset-Threshold Poverty Traps

With our experimental variation in the size of the asset transfer we can implement a cleaner test. In particular we will compare households who, prior to the intervention, owned similar values of assets, but who, after the one-off transfers, owned either an additional EGP 5,821 (half transfer valued at endline prices) or an additional EGP 11,642 (full transfer valued at endline prices) worth of assets due to the random assignment. This allows us to experimentally test for the presence of a poverty trap threshold between X+EGP 5,821 and X+EGP 11,642, where X stands for the value of baseline (i.e., pre-transfer) assets, and to repeat this test at different values of X.

To illustrate the new test, consider a household who owned $X = 0$ assets at baseline in Figure 1 and that there is a poverty trap ending in the region that we test (i.e. between X+5,821 & X+11,642). Shortly after the start of implementation of our graduation program, households in the full-transfer arm would experience positive asset growth, while households assigned to the half-transfer arm would experience negative asset growth. This assumes both that there is a poverty trap threshold between X+5,821 & X+11,642 and that the large transfer is not so large that it pushes the household past the high-equilibrium C* and into another region where there is negative asset growth.

Under these two conditions, the treatment effects on *post-transfer asset growth* should be larger for the full-transfer arm compared to the half-transfer arm for households with (pre-transfer) baseline assets equal to X EGP.[15]

Our test therefore proceeds as follows:

1. Compute the difference between the value of the productive assets owned by the household at the 40-month follow up ($A_{40\ month}$) and immediately after assets were transferred, holding prices constant over time. In other words compute

[15] If we were in a stable equilibrium prior to the intervention (hence with no asset growth in the control group), these conditions would imply not only that the full-transfer treatment effect on asset accumulation should be larger than that of the half-transfer, it would imply that the treatment effects have different signs. I.e., in this case, the half-transfer treatment effect on post-transfer asset accumulation would be negative and the full-transfer treatment effect would be positive.

$\Delta A = A_{40\ month} - (A_{baseline} + A_{transfer})$, where $A_{baseline}$ is the value of productive assets at baseline and $A_{transfer}$ equals $11,642$ for households in the full-transfer arm, $5,821$ in the half-transfer arm, and 0 for the control group.

2. Split the sample into bins of pre-transfer baseline asset values ($A_{baseline}$)

3. In each bin, estimate the average treatment effect (ATE) on $\Delta A$ of the half-transfer and full-transfer arms relative to the control group

4. In each bin, test the necessary condition that the full-transfer ATE weakly exceeds the half-transfer ATE. We implement this as a one-sided test of the boundary null $H_0 : \beta_{full} = \beta_{half}$ against $H_1 : \beta_{full} < \beta_{half}$. Rejection implies rejection of the null that $\beta_{full}$ weakly exceeds $\beta_{half}$, and therefore rejects a necessary condition for the presence of a poverty trap threshold between $EGPX_i + 5{,}821$ and $EGPX_i + 11{,}642$, where $i = 1, 2, \ldots, N_i$ and $N_i$ is the total number of bins.

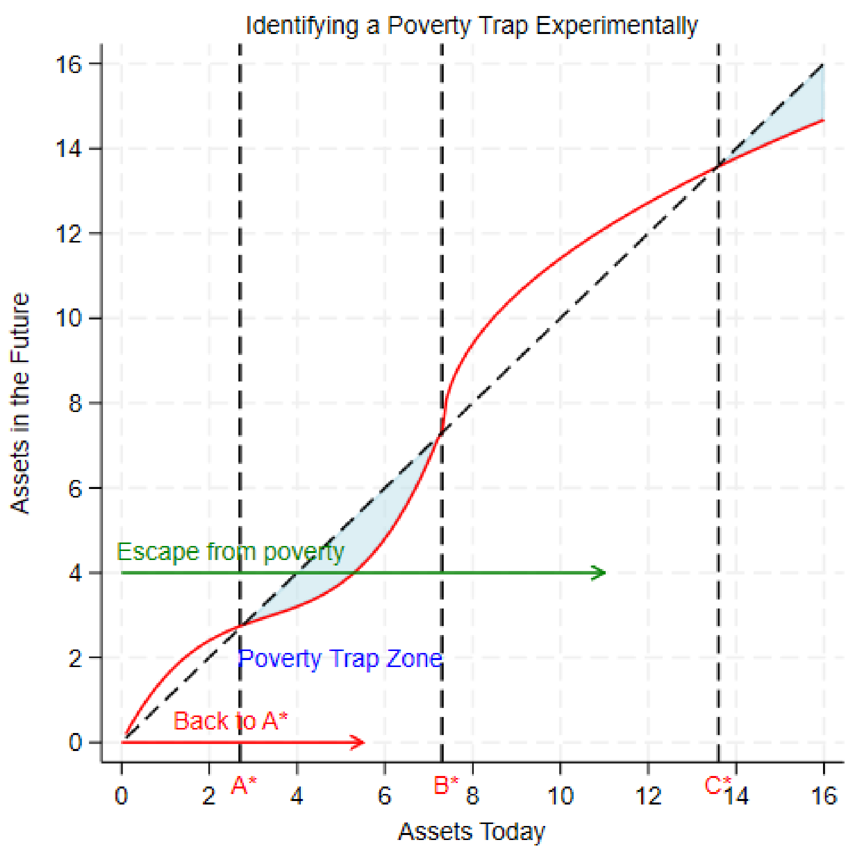
It is also important to consider heterogeneity because poverty trap thresholds may vary with household characteristics, or only exist for households with some characteristics (Banerjee et al., 2015; Balboni et al., 2022). Our decile-by-decile test allows for heterogeneity along two dimensions. First, by testing at different points in the baseline asset distribution (i.e., for different values of X), we allow for poverty trap thresholds that vary with initial wealth. Second, we test for a range rather than a point threshold (X+EGP5,821 to X+EGP11,642), which accommodates heterogeneity in thresholds across households with similar baseline assets but different characteristics. Finally, we complement this approach with a machine learning strategy that identifies subgroups predicted to benefit differentially from the full versus half transfer, which we discuss in Section 5.3.

#### 3.3.3 Limitations in Theory and in Practice

Our strategy tests a necessary, but not sufficient, condition for the presence of a poverty trap. If households who receive the smaller transfer go on to accumulate assets more quickly than otherwise similar households who receive the larger transfer, then the smaller transfer cannot have left them stuck below a relevant poverty-trap threshold (unless households in the full-transfer arm are pushed beyond the higher stable equilibrium). At the same time, finding that post-transfer accumulation is higher in the full-transfer arm than in the half-transfer arm does not, on its own, establish the presence of a poverty trap. The same pattern could arise if households with more assets tend to accumulate assets faster over this range, even if all households remain on a path of positive growth and there is no trap.

With sufficient data and variation in the size of transfers, the test we propose could, in principle, test exhaustively for the presence of poverty traps (e.g., using a grid

**Figure 1: Experimental Test of Poverty Traps**



search approach). In practice, however, our test has three main limitations. First, we test for the presence of a trap between X+EGP5,821 and X+EGP11,642, so that we cannot test for the presence of a trap below 5,821 EGP. Second, we have limited statistical power at higher deciles of the asset distribution, where we have a limited number of observations. Third, our design is better suited to detecting threshold effects that operate over the range between the half- and full-transfer treatments. If the relevant non-linearity is concentrated in a much narrower region, our test may have limited power to detect it cleanly.

Despite these limitations we argue that our test is informative because we are considering policy-relevant ranges of possible transfers. At the lower end of the distribution, 5,821 EGP is much less than the typical poverty graduation asset transfer: for instance, the median price of a cow in our context is EGP27,500. So if we learn that 5,821 EGP is sufficient to escape the poverty trap, if there is one, this has important policy implications irrespective of the possible presence of a poverty trap zone at lower asset levels. At the higher end of the distribution, asset thresholds beyond $EGPX_{N_i} + 11,642$ are unlikely to be addressed by large-scale interventions given that $EGP11,642 \approx USD700$.

# 4 Impact Evaluation Results

In this section, we present the effects of the full-transfer and half-transfer interventions on a set of primary outcomes related to the economic well-being of beneficiaries, measured 40 months after program implementation and the asset transfer.[16]

Results of the regression described in Section 3.2 are reported in Table 1 which shows the estimated $\beta_H$ and $\beta_F$ associated with the half-transfer and full-transfer treatments, their associated standard errors, the p-value of a test of equality of the two coefficients, and the p-value of a test of the null that the full transfer effect is twice as large as that of the half transfer.

The first two columns report the effects of Bab Amal on the value of livestock assets and on the value of total assets, which are key outcomes given that a central component of the intervention was the transfer of livestock (primarily goats and sheep). The half-transfer and full-transfer arms see an increase of, respectively, EGP 5,549 and 9,106 of their value of livestock compared to the control group (representing a 210 and

[16]Here we discuss findings based on Equation 1, reported in Table 1. For ANCOVA results, see Table A7. While ANCOVA coefficients should be more precisely estimated and therefore generally preferable (McKenzie, 2012), one limitation of our ANCOVA specification is that we have a number of missing values at baseline for some outcome variables, especially for the consumption and income dependent variables. In those cases we include a binary for the missing cases and replace the missing value with the median. Reassuringly, however, estimates are very similar in Tables 1 and A7.

344 % increase).[17] The magnitude of the effects is even larger when we consider all other productive assets including land, carts, etc. with increases of EGP 8,156 and 13,161 relative to the control for the half-transfer and full-transfer, respectively (corresponding to a 120 and 200% increase relative to the control group mean). These results suggest that more than three years after receiving the asset transfer, households in both treatment arms still have a large advantage in asset ownership relative to the control group. This may reflect continued ownership of a sizable share of the initial transfer and/or specialization and development of livestock activities. For both livestock and total assets value, the effects are significantly larger for the full-transfer than the half-transfer arm but the point estimate for the half-transfer impact is more than half of the magnitude of the impact of the full-transfer arm (although we generally cannot reject the null that $\beta_F = 2\beta_H$). The greater impact on total assets relative to livestock assets may suggest that the program induced households to invest in complementary productive assets.

We now turn to the effects of the program on household income. A key aspect of the theory of change underpinning graduation programs is that bundling a large asset transfer with other components such as training, coaching and group savings may enable households to develop economic activities and generate more income. Given that the vast majority of beneficiaries chose livestock assets (85% in the full-transfer arms and 63% in the half-transfer arm), we would expect revenues linked to livestock to be most strongly impacted. We find sizeable but imprecisely estimated effects on total monthly income (4 to 8%, see column 3). Appendix Table A3 provides a breakdown of income by source. As expected, we find significant positive effects of both half and full-transfer arms on livestock revenues, with large increases compared to the control mean (EGP 118 to 185, or 84 to 132%, for the half- and full- transfer arms, respectively). And again, the half-transfer arm has an impact per dollar transferred that is more than proportional to that of the full-transfer arm. The treatment effects on livestock revenues are very similar in magnitude to the treatment effects on total income. The key difference is that standard errors are four times as large in the total income estimates. Indeed, households in these poor rural areas derive most of their income from casual wage work (control group mean: EGP 1,334), which remains unaffected by the interventions. The program did not induce any changes either in income derived from crop production or non-agricultural businesses, both of which are very low in the control group, as only a small share of households produce and sell crops or own a small business. Taken together, the results on total income and income components show large, statistically significant impacts on livestock revenues that are not

[17] Interestingly, while we did not transfer large livestock (cows or buffaloes), both half- and full-transfer households significantly increase their ownership of large livestock, and these effects are large relative to the control group mean. This shows that, in both treatment arms, some beneficiaries use the asset transfers as a stepping stone to large livestock rearing.

compensated by decreases in other sources of income.

For consumption (column 4), we observe that both the half-transfer and full-transfer treatments have positive effects and that, despite substantial differences in the initial intensity of transfers, the impacts are very similar in magnitude (with a large p-value associated to the test of equality between treatment effects). Monthly consumption increases by EGP 276 and 240 (or 4-5%) relative to the control group.

The intervention could have encouraged households to invest in migration by enabling some members to find some employment outside the village by reducing the cost and risk associated with migration. But we find no effect on the probability that members in the household migrate (column 5). We also do not find effects on school enrollment among children aged 7-18 at the time of our endline survey (column 6), showing that households neither increased nor reduced investments in human capital during the period of the study.

Finally, in column 7, we summarize our six primary outcomes in an index (summing the z-score of each variable). The results confirm that both interventions improved households' economic well-being and that the effects of the half-transfer are about half the magnitude of those of the full-transfer intervention overall. The proportionality in program impacts across the different intensity of the initial transfer would suggest, *prima facie*, that there is no widespread poverty trap in our sample. We specifically explore this question in the next section.

## Table 1: 40 Month Impacts on Primary Outcomes

| | (1) Value of Livestock | (2) Total Assets | (3) Income (Monthly) | (4) Consumption (Monthly) | (5) Migration Indicator | (6) Share Enrolled | (7) Summary Index |
|---|---|---|---|---|---|---|---|
| Half Transfer | 5549*** | 8156*** | 107 | 276** | 0.01 | -0.01 | 0.22*** |
| | (669) | (1288) | (148) | (109) | (0.02) | (0.02) | (0.03) |
| Full Transfer | 9106*** | 13161*** | 206* | 240*** | 0.03 | -0.01 | 0.37*** |
| | (684) | (1282) | (116) | (79) | (0.02) | (0.02) | (0.03) |
| Control Mean | 2646 | 6654 | 2456 | 5514 | 0.42 | 0.72 | -0.00 |
| p-value H=F | 0.000 | 0.000 | 0.450 | 0.744 | 0.351 | 0.899 | 0.000 |
| p-value F=2H | 0.095 | 0.179 | 0.975 | 0.125 | 0.709 | 0.711 | 0.176 |
| Obs | 3,247 | 3,247 | 2,861 | 2,941 | 3,253 | 3,254 | 3,254 |

*Notes:* **Value of Livestock:** Valued using 40-month HH reported prices at the district level, winsorized at 5%. **Total Assets:** Includes Livestock, Land, Tractors, Ploughs, Shears, and Carts valued using 40-month HH reported prices (district median prices for livestock and overall sample median prices for other assets). **Monthly Income:** HH reported income winsorized at 5%; includes wage income, livestock revenues, government and non-government assistance, non-agricultural activities, remittances, and net crop revenues. **Monthly Consumption:** Includes food and non-food consumption, winsorized at 5%. **Migration:** Binary indicator for whether any household member migrated in the previous 12 months (2 or more consecutive nights away for work). **Education:** Proportion of children aged 7–18 enrolled in school; households with no children in this age range are coded as zero. **Summary Index:** Following Kling et al. (2007), the index is the equally weighted average of the six standardized components. Each component is standardized by subtracting the control group mean and dividing by the control group standard deviation. For households missing any component, we impute the missing value with the treatment-arm mean of that standardized component before averaging. **p-value F=2H:** Test that the Full Transfer effect equals twice the Half Transfer effect. The model uses quintet fixed effects; SEs clustered at the agglomeration level. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

As described in Section 3.1, we also collected a 20-month follow-up survey, about two months after the non-monetary components of the graduation bundle stopped. However, in line with the BRAC poverty graduation model, beneficiaries were asked not to sell the assets received as part of the program for the first 18 months. This first follow-up survey is therefore likely to reflect the result of a constrained optimization so is therefore harder to interpret.[18] For completeness, Appendix Table A5 mirrors Table 1 using the 20-month follow-up data. The treatment effects on consumption are remarkably similar in magnitude to the 40-month results, with all groups increasing by 4-6% across the two time periods. Average treatment effects on total assets are also very similar at midline and endline for the half-transfer households (approximately 8.4K at midline and 8.2K at endline). And the ratio of livestock ownership impacts in the half- vs. full-transfer arms is also relatively stable.

However, full-transfer recipients have much higher impacts on the total value of assets owned at midline than at endline, which is largely driven by higher ownership of livestock assets.[19] Income impacts are substantially larger at midline than endline, and largely proportional to transfer size. Looking at the breakdown of income sources at midline (Table A6), we see that the increase in income at the 20-month follow-up comes from livestock revenues and, to a lesser extent, from non-agricultural/non husbandry sales of about the same magnitude as a reduction in wage work. Overall, these results suggest that, for as long as beneficiaries receive regular support from program staff and feel compelled to hold on to the assets transferred to them, they experience large income gains and accumulate assets fast (rather than increasing their consumption proportionately), while reducing wage work. But after program staff visits end and constraints on asset sales are lifted, the beneficiaries of *full* transfers draw down some of their additional assets while maintaining consumption gains similar to those of the half-transfer arm. As we explore in Section 6, this appears to reflect a shift towards greater diversification of economic activities in the full-transfer arm, a strategy that reduces variance at the cost of lower average returns.

# 5 Poverty Trap Analysis

In this section, we ask whether our data supports the hypothesis that a poverty trap exists in the relevant asset range. We start with a descriptive graphical analysis before implementing the experimental test described in Section 3.3.2.

[18] Similarly, Balboni et al. (2022) focus on outcomes observed four years post-transfer onward despite collecting data two years post-transfer as the four-year follow-up is "the first time [they] observe the beneficiaries after they are free to sell the assets provided by BRAC".

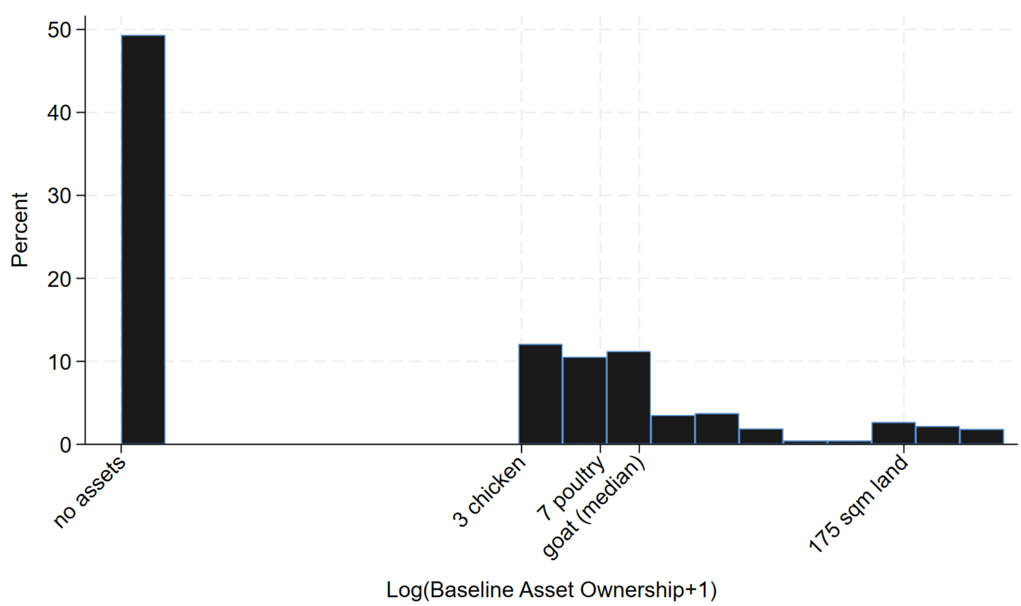
[19] At midline, the half transfer impact on total asset ownership is lower than that on livestock, suggesting that half transfer recipients may have initially liquidated some of their assets or foregone other investments in order to invest in livestock.

## 5.1 Descriptive Analysis

We construct an asset ownership aggregate equal to the value of all livestock, land, and agricultural equipment (cart, tractor, shears, and ploughs) owned at baseline, valued at endline prices.

A bimodal distribution of baseline assets could constitute *prima facie* evidence of a poverty trap, with each mode corresponding to the low- and high- stable equilibria depicted in Figure 1—$A^*$ and $C^*$, respectively. Figure 2 shows that, in our setting, the baseline distribution of assets is unimodal, with most households concentrated at very low levels of productive assets and the rest of the households spread largely evenly across a long tail.[20]

**Figure 2: Baseline Asset Distribution**




This finding alone does not imply that there is no poverty trap. Indeed, an asset distribution having more than one peak is not a necessary condition for a threshold-based poverty trap. For example, a poverty trap could exist without a second mode if the only high-returns technology required migrating away, in line with the idea of geographical poverty traps, and hence households would not be in our sample. Similarly if the stable equilibria faced by different households are very heterogeneous there would be no bunching of observations around a single (steady-state) asset ownership value.

[20]This is the case both in our experimental sample of households who were deemed eligible to receive the intervention based on a means test, and in the overall population from which we identified eligible households, so that the absence of a high-asset mode is not because of our sample selection criteria.

Next, we split the households in our experimental sample by decile of the baseline distribution of asset ownership. Since about half of the sample owned no assets at all at baseline, the bottom five deciles are lumped together.

In Figure 3, we plot, for each of these deciles, the average value of asset ownership (i) post-transfer (i.e., baseline assets plus the value of the assets received as part of the graduation program, if any) and (ii) at the 40-month follow-up (endline). A clear dashed bar (endline) above the filled bars (post-transfer) therefore implies asset growth post-transfer, on average. Starting with the control group, we note that, on average, there is an increase in asset ownership over time for all but the top decile, contrary to what would be expected in the presence of an asset threshold-based poverty trap.[21]

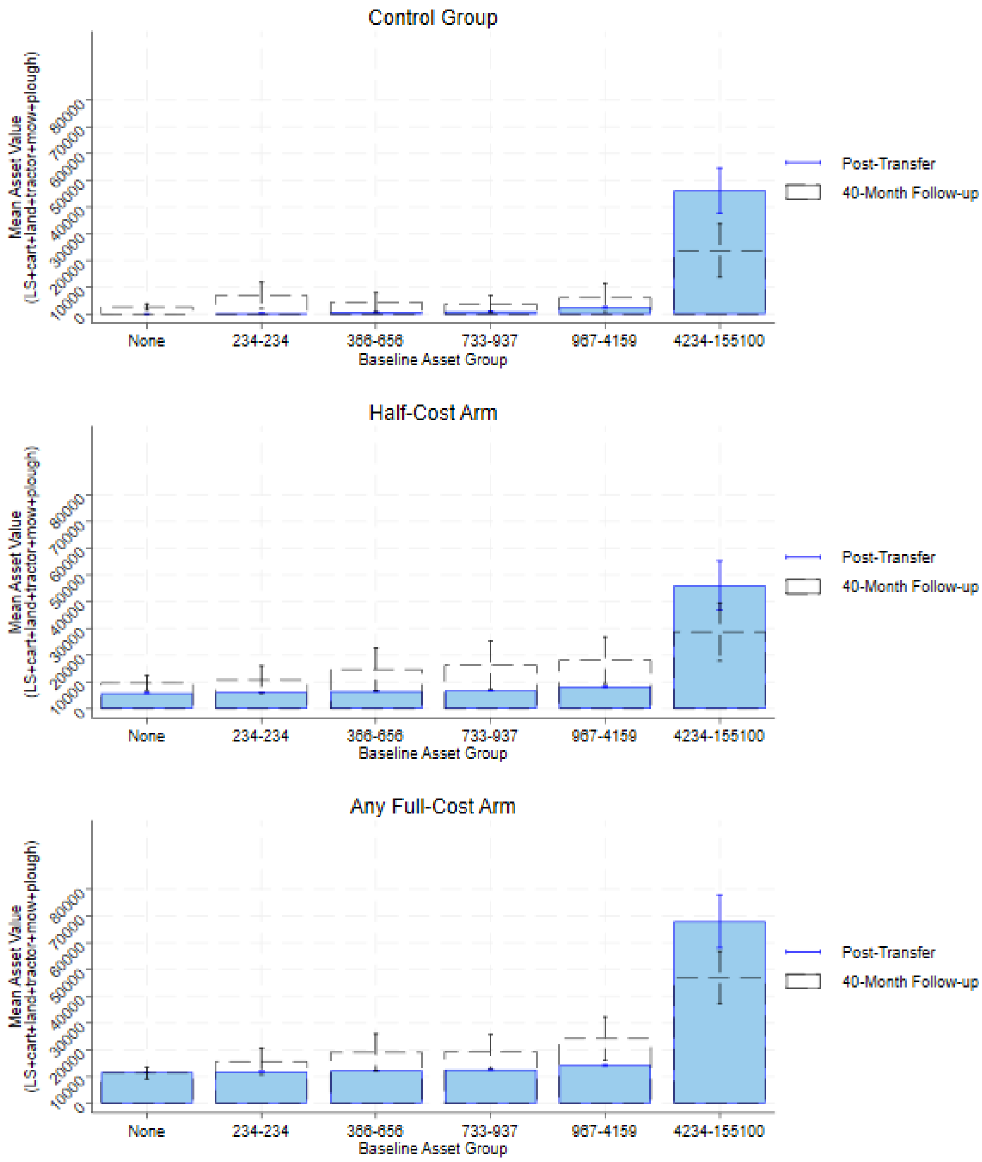
Yet comparing asset growth between households who have different levels of baseline assets cannot be interpreted as causal evidence. Instead, we use the randomization of households into the half- and full-transfer arms to test for differences in asset accumulation. Consider for instance the sample of households who had no assets at all at baseline (first set of bars in each graph). Immediately after the asset transfers, treated households in this group would either have owned EGP5,821 (11,642) in assets, if randomly assigned to the half-transfer (full-transfer) arm—valued at endline prices. If there was a poverty trap between EGP5,821 and EGP11,642, we would see a drop in assets for the half-transfer arm and an increase in the full-transfer arm. But the figure shows positive asset growth in the half-transfer arm and no growth in the full-transfer arm. Looking across each baseline asset ownership decile in turn, there is no sign that, on average, for a given baseline asset ownership decile, a larger asset transfer leads to larger post-transfer growth. We will test this more formally in the next section.

## 5.2 Experimental Analysis

To formally test for the presence of a poverty trap we regress the change in post-transfer assets on indicators for the half-transfer and full-transfer arms separately by decile of baseline assets (as outlined in Section 3.3.2). Table 2 shows that nearly 50% of eligible households have no productive assets at baseline, where productive assets are defined as livestock, land, carts, tractors, shears, and ploughs. Therefore, a split by deciles of baseline asset ownership leads to six bins. Descriptively, the second bin (51-60 percentile in baseline pre-transfer assets) includes households with around 1 to 4 heads of poultry, households in the seventh decile have the equivalent of around one goat or 5-9 head of poultry, the next group has around 2 goats or 10 head of poultry, or one donkey, the ninth decile group has the equivalent of 2 goats and a donkey,

[21] We observe a reduction in asset ownership in the top decile across experimental arms, suggesting that some of the asset ownership captured at baseline may have reflected transitory ownership (e.g., of large cattle bought for immediate resale), or suffer measurement error despite winsorizing at 5%.

# Figure 3: Post-Transfer Asset Accumulation




Overall sample sizes: **None:**1548, **234-234:**381, **366-656:**336, **733-937:**330, **967-4159:**243, **4234-155100:**315. Vertical lines: 95% CIs of means.

while the richest group typically owns some land and/or cows along with other small and medium livestock. Although we did not stratify the sample by baseline asset ownership, the share of households in each bin is similar across experimental groups (Table 2). When estimating Equation 1 using each decile indicator in turn as dependent variable, only one (two) out of eighteen differences between arms are statistically significant at the 5% (10%) statistical significance level.

**Table 2: Baseline Asset Decile Shares by Experimental Group**

| Full Distribution Percentile | (1)<br>Control | (2)<br>Half Transfer | (3)<br>Full Transfer |
|---|---|---|---|
| 0-50 (No assets at baseline) | 0.47 | 0.50 | 0.50 |
| 50-60 | 0.13 | 0.12 | 0.12 |
| 60-70 | 0.12 | 0.10 | 0.10 |
| 70-80 | 0.10 | 0.10 | 0.11 |
| 80-90 | 0.08 | 0.10 | 0.07 |
| 90-100 | 0.10 | 0.09 | 0.10 |
| Observations | 925 | 696 | 1532 |

*Notes:* Shares are reported by treatment arm. The transfer treatment arms also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm.

Table 3 reports the results from regressing the change in post-transfer assets on indicators for the half-transfer and full-transfer arms. In Panel A we find that, in all bins but one, the ATE of the full-transfer is (much) smaller in magnitude than that of the half-transfer arm. The only exception is the 60-70th percentile group, in which the ATE is slightly larger in the full-transfer group (EGP3,872 compared to EGP3,678 in the half-transfer arm). While in some arms we lack power to reject equality even in the presence of meaningful differences, we reject the poverty trap necessary condition ($\beta_{full} \geq \beta_{half}$) at the 10% level or less for all but 30 percent of the sample (60th to 90th percentiles). Indeed, except in these three deciles, we reject equality in favor of the one-sided alternative that the effect of the half-transfer on post-transfer growth is *larger* than the effect of the full transfer. In Panel B, we increase statistical power by starting from the poorest group at baseline, and progressively adding the next decile to the sample. Across all columns we find that post-transfer asset accumulation is *larger* in the half-transfer arm relative to the full-transfer arm, with p-values less than 0.01 throughout. We therefore find no support for the presence of a poverty trap.

These results contrast sharply with what a simple observational analysis would suggest. If we compared asset growth *within* treatment arm we would have seen increasing asset growth for households who (endogenously) owned more at baseline. In other words, treatment effects are typically larger for those who were richer at baseline, with the poorest part of our sample experiencing the lowest amount of asset accumulation. If taken at face value, this would lead to the spurious conclusion that the larger amount of assets caused the larger growth, which is in stark contrast with the results obtained when using experimental variation.

**Robustness Checks**

In this subsection, we confirm the robustness of our conclusions to valuing assets at a single price vector across all villages, using log- or binary transformations of post-transfer asset accumulation as dependent variable, and to changing two sample selection choices we make in the main analysis.

As discussed in Karlan et al. (2026), asset prices may be correlated with other characteristics of the household's place of residence, which may themselves be correlated with asset growth. In Table A13, we replicate the poverty trap test of Table 3, but this time valuing each asset at a single price (namely, the median unit price as reported by households across the whole sample), instead of using geography-specific medians, as in our main analysis. Our conclusions are unchanged.

In the main analysis, we focus on post-transfer accumulation in levels rather than logs because it is transparent and does not rely on choices regarding the treatment of the many households owning zero assets (see Karlan et al., 2026, for a comprehensive discussion). Using an Inverse Hyperbolic Sine transformation, we reach similar conclusions: the impact of the full transfer treatment on the post-transfer asset growth rate is *less* than that of the half transfer treatment for all but the two top deciles of the baseline asset distribution, and significantly so for the 50% of households who have no assets at baseline (Panel B Table A14).[22]. As suggested in Karlan et al. (2026), we also estimate impacts on a binary indicator equal to 1 for positive growth and 0 otherwise. Again, there is no evidence that full transfer households are more likely to have positive asset growth (Panel A Table A14)

We also test the robustness of our conclusions regarding poverty traps to imputing missing values instead of dropping observations for whom some components of asset ownership are missing at baseline or endline. To do so, we replace missing values with the experimental arm mean. This leads to a total sample of 3,469 households instead of 3,153. Results are reported in Appendix Table A8, showing that our conclusions are not sensitive to the choice of imputing or not missing values.

[22]More specifically, the average impact on the growth *rate* relative to the control group is less negative for half- than full-transfer beneficiaries. This sign is not surprising when considering that control households have no or very few assets at baseline, creating a floor effect in asset growth, while treated households have several thousand EGP worth of transfer post-transfer. Consider for instance a household owning EGP234, i.e., the lowest non-zero value of assets owned in our sample at baseline. Then an increase by EGP 300 (4 chickens) would imply a 128% increase in assets for this household if assigned to the control group, whereas the same asset accumulation would only translate into a 5% (2.5%) growth in assets for a household owning the same baseline assets but who would have received the full (half) transfer.

## Table 3: Test of Poverty Traps

| | | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|---|
| Panel A | Post Transfer Total Asset Growth at 40 Months by decile group | | | | | | |
| | baseline asset decile | 0-50 | 50-60 | 60-70 | 70-80 | 80-90 | 90-100 |
| | Half Transfer | 911 | 1,563 | 3,678 | 7,842* | 5,522 | 10,511 |
| | | (1,276) | (3,468) | (4,061) | (4,487) | (6,071) | (8,723) |
| | Full Transfer | -3,098*** | -3,228 | 3,872 | 7,126* | 1,715 | -2,720 |
| | | (993) | (2,719) | (3,552) | (3,892) | (5,218) | (8,041) |
| | Control Mean | 2,692 | 6,784 | 3,771 | 2,869 | 3,576 | -22,386 |
| p-values: | Ha: H>0 | 0.238 | 0.326 | 0.183 | 0.041 | 0.182 | 0.115 |
| | Ha: F<0 | 0.001 | 0.118 | 0.862 | 0.966 | 0.629 | 0.368 |
| | Ha: H>F | 0.003 | 0.075 | 0.519 | 0.439 | 0.255 | 0.058 |
| | Ha: F>H | 0.997 | 0.925 | 0.481 | 0.561 | 0.745 | 0.942 |
| | Obs | 1,548 | 381 | 336 | 330 | 243 | 315 |
| Panel B | Post Transfer Total Asset Growth at 40 Months by cumulative decile | | | | | | |
| | | (1) | (2) | (3) | (4) | (5) | (6) |
| | baseline asset threshold | 0-50 | 0-60 | 0-70 | 0-80 | 0-90 | 0-100 |
| | Half Transfer | 911 | 296 | 1,055 | 1,715 | 1,761* | 2,306* |
| | | (1,276) | (1,185) | (1,072) | (1,048) | (1,025) | (1,206) |
| | Full Transfer | -3,098*** | -3,286*** | -2,078** | -1,433 | -1,071 | -1,070 |
| | | (993) | (987) | (979) | (995) | (987) | (1,112) |
| | Control Mean | 2,692 | 2,692 | 3,584 | 3,496 | 3,503 | 1,454 |
| p-values: | Ha: H>0 | 0.238 | 0.402 | 0.163 | 0.051 | 0.043 | 0.028 |
| | Ha: F<0 | 0.001 | 0.000 | 0.017 | 0.075 | 0.139 | 0.168 |
| | Ha: H>F | 0.003 | 0.003 | 0.004 | 0.004 | 0.008 | 0.004 |
| | Ha: F>H | 0.997 | 0.997 | 0.996 | 0.996 | 0.992 | 0.996 |
| | Obs | 1,548 | 1,929 | 2,265 | 2,595 | 2,838 | 3,153 |

Notes: Baseline asset deciles and thresholds based on total assets including Livestock, Land, Tractors, Ploughs, Shears, and Carts valued in 40 months prices. Asset measure values are winsorized at 5%. The model uses quartet fixed effects and SE are clustered on the agglomeration level. H and F denote the Half Transfer and Full Transfer treatments, respectively. The transfer treatment arms also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm. Ha states the alternative hypothesis against the null hypothesis of equality. *** p<0.01, ** p<0.05, * p<0.10

Finally, we drop the full-intensity experimental arm in which we imposed the condition that the recipient of the transfer should be a woman. As explained in Section 2.2, we had two "full" intensity interventions, which differed by whether we imposed or not the restriction that the beneficiary had to be female, but in practice this constraint did not have much bite as 88% of beneficiaries in the unrestricted full-transfer arm were female anyway. In the main analysis, we pool the two full-transfer groups to maximize power. In Appendix Table A9, we show that the results of our poverty trap tests are largely unchanged when focusing only on the unrestricted full-transfer arm.

### 5.3 Heterogeneity Analysis

Our analysis above shows no evidence of an asset-threshold poverty trap at any point of the asset distribution, on average. But this may hide subgroups who may be stuck in a poverty trap and therefore need the larger transfer to sustain higher asset growth after receiving the transfer, as is the case for those with high entrepreneurial skills in Banerjee et al. (2019). We turn to recently developed machine learning strategies to provide a more flexible test.

Chernozhukov et al. (2025) develop a data-driven method for assessing the extent of heterogeneous treatment effects in randomized experiments. It uses modern machine learning techniques to identify subgroups of the population that are predicted to benefit most from the intervention and subgroups that are predicted to benefit least. It does so by building a non-parametric model that predicts a unit's outcome in the treated state and another model that predicts a unit's outcome in the control state and subtracts them to generate a predicted individual treatment effect. It then sorts units by this value, generating groups who are predicted to be most and least affected. Critically it generates these predictions using only half of the sample and then tests it on the other half of the sample. This method, along with specific well-defined steps for bootstrapping confidence intervals, provides an "honest" yet flexible data-driven strategy for identifying heterogeneity.

We implement this procedure using the data from our experiment. We compare those in the full-transfer arm to those in the half-transfer arm—as here we want to identify households who would be predicted to benefit more in the full- than half-transfer state. Table 4 reports the results. We split the sample into five quintiles based on the predicted difference in treatment effects on post-transfer asset growth. In Panel A we report the average additional post-transfer growth in the full- relative to the half-transfer arm. The Bottom Quintile corresponds to households who are predicted to benefit least in the full-transfer arm relative to the half-transfer arm, while the Top Quintile are those predicted to benefit the most. In the first column we show treatment effects on post-transfer growth in *total* assets while in the second column we look at post-transfer growth of *livestock* assets.

In the bottom quintile, we find that, if assigned to the full transfer, households would decrease their asset accumulation by over EGP 7,500 on average relative to if they were assigned to the half-transfer group. Asset accumulation also decreases for those in the next two lowest quintiles if assigned to the full-transfer treatment compared to the half-transfer treatment. We see increases in asset accumulation for those in the top two quintiles if assigned to the full-transfer arm relative to the half-transfer arm. In light of the asset based poverty-trap hypothesis, this suggests that for at least 60% of our sample, asset accumulation does not happen faster if getting the larger transfer relative to getting the smaller transfer. If the poverty trap was binding on these groups we would expect that asset accumulation would increase with the larger transfer relative to the smaller one. If we consider only growth in livestock assets we find that four of the five quintiles are predicted to have lower post-transfer asset accumulation in the full-transfer treatment.

For the top two quintiles we do see greater post-transfer total asset accumulation in the full-transfer arm relative to the half-transfer arm. Although this does not imply that these households are subject to a poverty trap, we cannot reject a necessary condition for an asset threshold poverty trap for these groups. However, even among these households, the additional post-transfer asset accumulation in the full- relative to the half-transfer arm does not exceed the additional capital provided upfront. These estimates are consistent with some heterogeneity in returns, but not with the disproportionate gains from the larger transfer that would make threshold dynamics especially compelling over our study horizon.[23]

In Panel B we compare the baseline characteristics of those in the top and bottom quintiles. We identify the 8 baseline characteristics that are most predictive of the heterogeneity.[24] We find that those who are most likely to be in the top group—i.e., are

[23]If we found that the full transfer produces larger asset growth than the half transfer, it would be on its own unsurprising since it would hold under any positive return to capital. A stricter test is whether the impact of the full transfer relative to control exceeds twice the impact of the half transfer, i.e., whether $F - C > 2(H - C)$, or equivalently $F - 2H + C > 0$. This tests whether marginal returns to capital are increasing over the range of assets induced by our transfers which is a necessary condition for a poverty trap threshold in this range. This type of test is in the spirit of what Karlan et al. (2026) argue is needed to credibly assess poverty traps- direct evidence on the shape of returns to capital, identified experimentally. We extend the Chernozhukov et al. (2025) framework to accommodate our three experimental arms and rank households by their predicted value of $F - 2H + C$. The resulting estimates provide little evidence of increasing returns. Although the top quintile has the most positive point estimate, it is small and imprecise (19 EGP, s.e. 243), and the best linear predictor coefficient is modest (0.18), consistent with the lower precision inherent in combining noise from three treatment arms rather than two. Overall, this exercise reinforces the conclusion that even when using a flexible, data-driven approach to search for subgroups with increasing marginal returns, we find no evidence that larger transfers generate disproportionate gains in our setting.

[24]We do this following the strategy used in Bryan et al. (2024). We recover the predicted treatment effect and run a feature selection algorithm on all the baseline covariates to find the ones that are most

predicted to benefit the most from a larger transfer in terms of post-transfer growth—are generally richer than those in the bottom group. They are more likely to already own livestock, less likely to be classified as among the poorest households, and have more working age household members.

Taken together with our decile-by-decile results in Section 5.2, these patterns illustrate why experimental variation is essential to testing for poverty traps. Within treatment arms, post-transfer asset growth rises with baseline assets, and our machine-learning analysis shows that baseline wealth, especially livestock ownership, strongly predicts who benefits most from the larger transfer. A researcher relying on observational variation alone would read both patterns as evidence of an asset threshold that households must cross before larger transfers generate higher returns. The experimental comparison rules this out. Holding baseline assets fixed and comparing households who were randomly given larger or smaller transfers, we find no evidence that doubling the transfer generates faster post-transfer accumulation, even in the top quintile where the point estimate is positive but small relative to the additional capital provided. The observational patterns instead reflect unobserved differences across households that correlate with baseline wealth, not causal returns to asset levels.

# 6 Mechanisms

Why do households who receive twice as much not accumulate more assets post-transfer? We investigate two broad types of explanations: whether the two treatments are beneficial to different households (i.e., they operate at different margins), or whether they induce different economic strategies that lead to similar asset growth, on average.

We first test whether households who benefit the most from the half-intensity and full-intensity transfers are different—i.e., whether the two interventions work through a different "margin". We do this in two ways. First we study the correlation in the predicted individual treatment effects (ITEs) obtained under half- and full-intensity treatments, estimated using the methodology described in Section 5.3 (now applied separately to full vs. control and half vs. control, rather than full vs. half). If the correlation is high, then it suggests that both treatments are helping the same types of households. Figure 4 presents scatterplots of the relationship between predicted individual treatment effects in the Full- (x-axis) and the Half-transfer (y-axis) arms for three outcomes: post-transfer growth in livestock, post-transfer growth in total assets, and consumption. The solid line shows the fitted line and the dashed line represents the 45-degree line. The correlation between the household ITEs is positive throughout,

predictive of treatment effects.

## Table 4: Heterogeneity Results and Baseline CLAN

**Panel A: Group Average Treatment Effects for Full vs Half Transfer Arms**

| | Post Transfer Growth in Total Assets | Post Transfer Growth in Livestock |
|---|---|---|
| Bottom Quintile | -7578 | -5253 |
| | (5622) | (2838) |
| 2nd Quintile | -4442 | -3825 |
| | (5476) | (2885) |
| 3rd Quintile | -1771 | -2501 |
| | (5566) | (2922) |
| 4th Quintile | 1366 | -1255 |
| | (5983) | (2942) |
| Top Quintile | 5576 | 232 |
| | (6619) | (3088) |
| Top vs Bottom p-value | 0.075* | 0.117 |
| Beta 2 | 0.560 | 0.300 |
| Beta 2 p-value | 0.093* | 0.327 |
| Obs | 2204 | 2204 |

**Panel B: Difference in Baseline Covariates for Top and Bottom Groups (CLAN)**

| | Bottom Quintile Mean | Difference with Top Quintile | | Bottom Quintile Mean | Difference with Top Quintile |
|---|---|---|---|---|---|
| Household Financial Status (0-10) | 1.701 | 0.946*** | Preferred Asset is Livestock (Half Arm) | 0.475 | 0.443*** |
| | | (0.169) | | | (0.045) |
| No Coping Strategy for Medical Expenses | 0.308 | -0.068 | Livestock Assets (EGP) | 229 | 7455*** |
| | | (0.050) | | | (982) |
| Owns Sickle | 0.005 | 0.118*** | Beneficiary Unable to Work (Past 30 Days) | 0.656 | -0.204*** |
| | | (0.027) | | | (0.055) |
| Owns Any Livestock | 0.258 | 0.566*** | Number of Small Livestock | 1.629 | 3.514*** |
| | | (0.046) | | | (0.394) |
| Number of Medium Livestock | 0.023 | 0.624*** | Number of Large Livestock | 0.005 | 0.498*** |
| | | (0.110) | | | (0.071) |
| Number of Large Livestock | 0.005 | 0.507*** | Household Size | 6.197 | 1.120*** |
| | | (0.072) | | | (0.267) |
| Number of Elderly Household Members | 0.045 | 0.249*** | Unauthorized Electricity Connection | 0.068 | -0.054** |
| | | (0.171) | | | (0.022) |
| Number of Working-Age Household Members | 2.285 | 1.269*** | Beneficiary Input in some Enterprise Decisions | 0.036 | 0.136*** |
| | | (0.047) | | | (0.034) |

$* p < 0.10$, $** \ p < 0.05$, $*** p < 0.01$. Outcomes are winsorized at 5% (w05) within each treatment arm. The left and right blocks in Panel B show the top eight baseline covariates for each outcome in Panel A. Baseline covariates were selected using LASSO stability selection. In the left block, the top six covariates and in the right block, the top three covariates are of equal importance; the rest follow in descending order of selection priority. The transfer treatment arms also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm.

indicating that those households who are predicted to do well with the full-transfer are also predicted to do well with the half-transfer. The r-squared for post-transfer total asset growth is 0.33, for livestock is 0.51 and for consumption is 0.73.

We also assess how the baseline characteristics of households who benefit most from each treatment compare to each other. Appendix Tables A10, A11, and A12 show the top predictors for each treatment and outcome. We see that, for example, one of the main predictors of treatment effects on post-transfer livestock accumulation in the full-transfer arm is the value of baseline livestock assets, which is also one of the strongest predictors of treatment effects in the half-transfer arm. Many other baseline covariates move in the same direction, suggesting again a similar profile of households that benefit in each of the treatment arms.

While there is substantial positive correlation between predicted ITEs in the half- and full transfer arms, the more limited correlation observed for asset growth than consumption suggests that the way in which half- vs full-intensity treatments are successful at improving their living standards over time may differ. To test this hypothesis, we investigate whether the beneficiaries of the half- and full-intensity interventions engage in different economic activities. To do so, we use our survey data on the extent to which the designated transfer beneficiary engaged in a range of economic activities in the previous seven days.[25] These questions focus on economic activities that are linked to the type of assets which beneficiaries could choose from, namely: livestock and "non-agricultural/non-husbandry" market activities such as retail and sewing. For livestock activities, we asked about each type of livestock (small (poultry), medium (sheep/goats) and large (cattle/buffaloes)), and, for each, we asked about engagement in two types of activities: subsistence and market-oriented.[26] Prior to asking these questions, interviewers explained carefully what we meant by "subsistence" and "market-oriented" activities, using locally grounded classifications. "Subsistence" activities were defined as activities carried out "mainly for the purpose of own consumption, although there may be some sales" and market-oriented activities were defined as activities carried out "mainly for the purpose of selling, although there may be some own consumption."

Table 5 reports treatment effects on economic activities carried out by female "designees" (i.e., women who were nominated by the household to receive transfers prior to randomization). Panel B reports estimates from a regression of a dummy equal to one if the respondent engaged in each subsistence activity in the previous 7 days. Panel C reports estimates for market-oriented activities. Panel A reports on the num-

[25]We asked households to designate a potential beneficiary prior to randomization, so we have comparable "designees" in the control and treatment arms.

[26]In our context, very few households engage in growing crops (so that the control group mean crop revenue is only EGP 22 per month, for instance). Thus we did not ask specifically about participation in agricultural activities.

ber of subsistence activities (column 1), the number of market-oriented self-employment activities (column 2), and the total number of subsistence or market activities (column 4). In addition, Panel A reports results for a dummy for "working for others" for a wage (column 3) and a dummy equal to 1 if the respondent said she engaged in no economic activity at all (column 5).

We find that the beneficiaries of both half- and full-intensity transfers are significantly more likely to take part in economic activities involving livestock and other assets, and less likely to engage in no economic activity at all. Importantly, there is no significant difference between the half- and full-transfer arms on the number of market activities they engage in.[27]. However, we find that full-transfer respondents are significantly more likely to have engaged in subsistence activities in the past 7 days (p-value of 0.001 for number of subsistence activities (Panel A) and between 0.000 and 0.120 for each subsistence activity (Panel B)). Full-transfer beneficiaries also engage in more economic activities overall (an additional 0.57 activities in the full-intensity group compared to 0.40 more activities in the half-transfer group, significantly different at 1%). These results suggest that the key difference in the impact of the half- and full-transfer arms is that the recipients of full transfers are more likely to diversify their economic activity rather than maximizing their participation in market-oriented activities. This may explain why they do not accumulate more assets post-transfer than half-transfer households, on average.[28,29]

Finally, we test whether this diversification strategy pays off in terms of mitigating risk. To do so, we compare the coefficient of variation (CV) in outcomes between the treatment groups. We find that across four primary outcomes the CV for the full-transfer arms is *slightly* lower than the half-transfer arm, although these differences are not statistically significant. We find this pattern for total assets (1.54 Full vs 1.70 Half), livestock (1.36 vs 1.43), income (0.84 vs 0.87) and consumption (0.31 vs 0.32).

# 7 Conclusion

We use experimental variation in the size of asset transfers to provide the most direct test of asset-threshold poverty traps to date. Both the full- and half-intensity treat-

[27]This rules out an alternative mechanism where higher transfers induce changes towards less asset intensive occupations such as small-scale commerce, which could increase income without necessarily increasing assets. This is corroborated by the results on income in Appendix Table A3, where we see no differential effect on non-agricultural income at the household level

[28]We also find a small but statistically significant reduction in wage work in the half-transfer arm, but results may not be representative as there is high non-response for this question due to the stigmatization of wage work for women in Southern Egypt.

[29]As these questions were only asked female beneficiaries and, by construction, we imposed the beneficiary to be female in the "restricted" full-intensity transfer arm, we repeat this exercise in a sample excluding this arm and reach similar conclusions (see Appendix Table A4).

## Table 5: Economic Activities of Female Designees at 40 Months

**Panel A: Impact on Aggregated Activity Types**

| | (1) # Subsistence Activities | (2) # Market Activities | (3) Wage Work | (4) # Activities | (5) No Activities |
|---|---|---|---|---|---|
| Half Transfer | 0.27*** | 0.14*** | -0.02** | 0.40*** | -0.07*** |
| | (0.04) | (0.03) | (0.01) | (0.06) | (0.02) |
| Full Transfer | 0.40*** | 0.18*** | -0.00 | 0.57*** | -0.10*** |
| | (0.04) | (0.03) | (0.01) | (0.05) | (0.02) |
| Control Mean | 0.94 | 0.24 | 0.02 | 1.19 | 0.23 |
| p-value H=F | 0.001 | 0.175 | 0.118 | 0.003 | 0.055 |
| Obs | 3,170 | 3,170 | 1,657 | 3,170 | 3,170 |

**Panel B: Impact on Each Subsistence Activity**

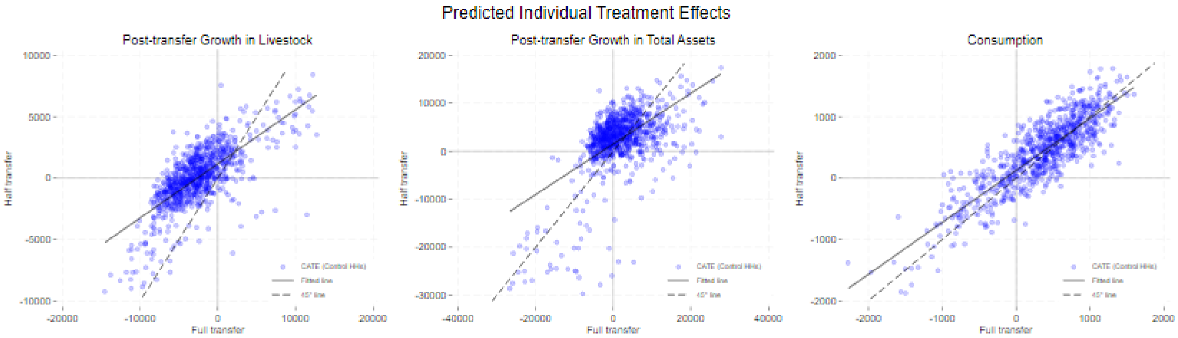
| | (6) Small LS | (7) Med LS | (8) Large LS |
|---|---|---|---|
| Half Transfer | 0.04* | 0.09*** | 0.14*** |
| | (0.02) | (0.02) | (0.02) |
| Full Transfer | 0.07*** | 0.15*** | 0.17*** |
| | (0.02) | (0.02) | (0.02) |
| Control Mean | 0.73 | 0.13 | 0.09 |
| p-value H=F | 0.064 | 0.000 | 0.120 |
| Obs | 3,170 | 3,170 | 3,170 |

**Panel C: Impact on Each Market Activity**

| | (9) Small LS | (10) Med LS | (11) Large LS | (12) Non-agri |
|---|---|---|---|---|
| Half Transfer | 0.02 | 0.04*** | 0.03*** | 0.05*** |
| | (0.02) | (0.01) | (0.01) | (0.02) |
| Full Transfer | 0.04*** | 0.04*** | 0.04*** | 0.06*** |
| | (0.01) | (0.01) | (0.01) | (0.01) |
| Control Mean | 0.11 | 0.03 | 0.02 | 0.08 |
| p-value H=F | 0.224 | 0.854 | 0.397 | 0.535 |
| Obs | 3,170 | 3,170 | 3,170 | 3,170 |

*Notes:* Dependent variables in Panel A are as follows. Column 1 reports the total number of subsistence activities in which the respondent participated in the last 7 days (out of 3). Column 2 reports the total number of self-employed market activities in which the respondent participated in the last 7 days (out of 4). Column 3 is a dummy equal to 1 if the respondent reports having worked for others for a wage in the last 7 days, and 0 otherwise. This column has a reduced sample because many female respondents regard working for others as demeaning and do not answer the question. Column 4 reports the total number of activities other than wage work in which the respondent participated in the last 7 days (out of 7). Column 5 is a dummy equal to 1 if the respondent did not engage in any economic activity in the last 7 days, and 0 otherwise. Panel B reports participation indicators for each listed subsistence activity. Panel C reports participation indicators for each listed market activity. **Small LS:** small livestock (poultry). **Medium LS:** medium livestock (sheep and goats). **Large LS:** large livestock (cows and buffaloes). **Non-agri:** self-employment in neither agriculture nor husbandry. **Half Transfer** and **Full Transfer** refer to the Half Transfer arm and either of the two Full Transfer arms, respectively. All regressions include quartet (stratification) fixed effects; standard errors are clustered at the agglomeration level (the randomization unit). The sample is restricted to female designees, that is, women whom households selected as transfer beneficiaries prior to randomization. The transfer treatment arms also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

# Figure 4: ITE Correlation in the Full- vs Half-Intensity Treatments




*Notes:* The scatter plots show predicted individual treatment effects (ITEs) from the pairwise comparisons (Full vs Control on the x-axis and Half vs Control on the y-axis). The solid line is the fitted line and the dashed line is the 45-degree line. The reported $R^2$ is the squared correlation between the Half- and Full-transfer averaged ITEs: $R^2 = 0.514$ for post-transfer growth in livestock, 0.327 for post-transfer growth in total assets, and 0.730 for consumption.

ments produce large, sustained gains in assets and consumption 40 months after the initial transfer. Yet post-transfer asset growth is not larger for households who received the full transfer relative to those who received half. This is true on average, as well as when looking across different levels of baseline asset values. This evidence is consistent with a concave transition equation, not the S-shaped dynamics that a poverty trap would imply.

Without having two treatments with varying levels of asset transfers we would have had to rely on potentially endogenous variation across households in baseline assets. If we only looked across households with different levels of baseline assets within a treatment arm the pattern observed in our data would appear to support the hypothesis that richer households grow their assets faster post-transfer. However, our experimental variation shows this is not the case.

These findings have direct implications for the design of anti-poverty policy. If poverty traps are not as stark as previously thought, then many more households could be reached with no loss in aggregate effectiveness. Recent evidence shows that widespread transfers can produce large multiplier effects in economies (Walker et al., 2024) and that universal livelihood programs can generate large impacts at lower per-household cost (Mahmud and Riley, 2025). Taken together this suggests that expanding the reach of graduation programs could be a viable strategy for policymakers.

Testing for the presence of poverty traps is fraught with difficulties (Karlan et al., 2026; Bandiera et al., 2026) and our test has important limitations. We can identify poverty traps only in the range of asset ownership bounded by the half- and full-transfer values; traps could exist below the half-transfer, though this threshold is itself only about one-fifth of the standard graduation transfer (a cow), and at the very low end of graduation programs more generally. Our findings come from one context, rural Upper Egypt, where most beneficiaries chose livestock assets, and the dynamics of asset accumulation may differ in settings with different market structures or asset types. Finally, we rule out poverty traps on average, for whole deciles of baseline assets, and our machine learning analysis of treatment effect heterogeneity finds no evidence of subgroups for whom the full transfer generates higher returns per dollar. But this is not to say that all households manage to durably escape poverty, even when they receive large asset transfers and complementary support. This remains an area where further investigation is warranted.

A growing body of work is already unbundling the graduation model, testing which components drive impacts and how program design can be optimized. Our contribution to this agenda is to show that the scale of the transfer, long treated as a binding constraint, may not be one. Future research may consider how transfer size interacts with the complementary components of graduation programs, such as coaching, savings support, and training. Embedding variation in both transfer size and program components into future evaluations, something that is feasible given the scale at which graduation programs now operate, would allow researchers and policymakers to better allocate scarce resources where they generate the highest returns.

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# A-1 Appendix Tables

## Table A1: Baseline Balance Table, Part 1

| | Full Sample | | | BL & EL Sample | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Balance | Control Mean | Half Cost | Full Cost | Control Mean | Half Cost | Full Cost |
| Household size | 6.80 | 0.08 | -0.02 | 6.83 | 0.05 | -0.05 |
| | [2.25] | (0.11) | (0.09) | [2.26] | (0.11) | (0.09) |
| Has latrine | 0.96 | -0.01 | -0.01 | 0.96 | -0.02 | -0.01 |
| | [0.20] | (0.01) | (0.01) | [0.19] | (0.01) | (0.01) |
| Worried about food | 0.67 | 0.01 | -0.02 | 0.67 | 0.01 | -0.02 |
| | [0.47] | (0.03) | (0.02) | [0.47] | (0.03) | (0.02) |
| Receives remittances | 0.09 | 0.02 | 0.01 | 0.09 | 0.01 | 0.01 |
| | [0.29] | (0.02) | (0.01) | [0.29] | (0.02) | (0.01) |
| Remittances amount | 166 | -21.08 | 19.05 | 167 | -55.44 | 24.78 |
| | [1562] | (71.21) | (61.47) | [1602] | (69.46) | (65.27) |
| Receives Takaful | 0.47 | 0.02 | -0.02 | 0.46 | 0.03 | -0.01 |
| | [0.50] | (0.02) | (0.02) | [0.50] | (0.02) | (0.02) |
| Takaful amount | 224 | 9.92 | 3.49 | 225 | 12.58 | 5.67 |
| | [306] | (16.16) | (13.81) | [310] | (17.00) | (14.23) |
| Receives NGO aid | 0.04 | -0.00 | -0.00 | 0.04 | 0.00 | -0.00 |
| | [0.21] | (0.01) | (0.01) | [0.21] | (0.01) | (0.01) |
| NGO aid amount | 54.25 | -5.26 | -19.30 | 57.50 | -4.90 | -23.74 |
| | [514] | (21.96) | (15.40) | [530] | (22.87) | (16.05) |
| Food consumption per capita | 43.28 | -1.69 | -0.71 | 43.06 | -1.67 | -0.68 |
| | [23.62] | (1.05) | (0.80) | [23.36] | (1.14) | (0.84) |
| Clothing consumption per capita | 98.55 | 2.37 | -0.92 | 96.98 | 3.49 | 0.28 |
| | [107] | (5.61) | (4.56) | [106] | (5.83) | (4.60) |
| Obs (in each arm) | 1002 | 771 | 1682 | 942 | 715 | 1585 |

*Notes:* Control-group means are reported with standard deviations in brackets. Half and Full columns report differences relative to the control group from regressions with randomization-strata fixed effects and agglomeration-clustered standard errors in parentheses. Consumption per capita winsorized at 5%. The transfer treatment arms also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm.

## Table A2: Baseline Balance Table, Part 2

| | Full Sample | | | BL & EL Sample | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Balance | Control Mean | Half Cost | Full Cost | Control Mean | Half Cost | Full Cost |
| HH head education attainment | 4.74 | 0.05 | 0.06 | 4.73 | 0.03 | 0.07 |
| | [1.33] | (0.06) | (0.05) | [1.33] | (0.07) | (0.05) |
| Small livestock (count) | 1.90 | -0.00 | 0.05* | 1.91 | 0.00 | 0.04 |
| | [0.66] | (0.03) | (0.03) | [0.67] | (0.04) | (0.03) |
| Medium livestock (count) | 0.19 | -0.00 | 0.01 | 0.19 | 0.01 | 0.01 |
| | [0.68] | (0.03) | (0.03) | [0.68] | (0.03) | (0.03) |
| Large livestock (count) | 0.11 | -0.01 | 0.03 | 0.12 | -0.01 | 0.03 |
| | [0.47] | (0.02) | (0.02) | [0.48] | (0.02) | (0.02) |
| Baseline livestock assets | 1885 | -255 | 523 | 1914 | -198 | 581 |
| | [7735] | (413) | (372) | [7723] | (421) | (378) |
| Baseline total assets | 8496 | -1127 | 2669 | 8682 | -1125 | 2192 |
| | [40053] | (1750) | (1887) | [40138] | (1735) | (1884) |
| Baseline land value | 6467 | -862 | 2170 | 6624 | -925 | 1632 |
| | [37713] | (1602) | (1780) | [38110] | (1615) | (1786) |
| Baseline monthly consumption | 684 | -12.82 | -2.55 | 687 | -11.18 | -0.92 |
| | [292] | (15.61) | (12.11) | [301] | (16.74) | (12.92) |
| Baseline monthly income proxy | 463 | -11.54 | 2.20 | 464 | -9.87 | 3.77 |
| | [453] | (17.73) | (15.55) | [467] | (18.95) | (16.34) |
| Baseline school enrollment share | 0.83 | -0.00 | -0.00 | 0.82 | -0.00 | -0.00 |
| | [0.30] | (0.02) | (0.01) | [0.31] | (0.02) | (0.01) |
| Obs (in each arm) | 1002 | 771 | 1682 | 942 | 715 | 1585 |
| p-value Joint test | F v C | H vs C | F v H | F v C | H vs C | F v H |
| | 0.580 | 0.850 | 0.894 | 0.670 | 0.882 | 0.915 |
| Obs (in each test) | 2684 | 1773 | 2453 | 2527 | 1657 | 2300 |

*Notes:* Control-group means are reported with standard deviations in brackets. Half and Full columns report differences relative to the control group from regressions with randomization-strata fixed effects and agglomeration-clustered standard errors in parentheses. The reported joint-test p-values are randomization-inference p-values for pairwise omnibus balance tests run on the full combined set of variables reported across Part 1 and Part 2 of the balance table. Monetary asset variables are winsorized at 5%. The livestock-asset and total-asset rows are reconstructed from winsorized baseline component asset values. The school-enrollment row reports the baseline share of school-age household members enrolled in school. The transfer treatment arms also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm.

## Table A3: Impact on household income

| | (1) Total income | (2) Work Wages | (3) Livestock revenues | (4) Assistance | (5) Non-agri sales | (6) Crops net revenues | (7) Remittances |
|---|---|---|---|---|---|---|---|
| Half Transfer | 107 | -77 | 118*** | -1 | 29 | 7 | 5 |
| | (148) | (87) | (33) | (25) | (87) | (6) | (12) |
| Full Transfer | 206* | 7 | 185*** | 16 | -21 | 14** | -4 |
| | (116) | (70) | (28) | (21) | (64) | (6) | (10) |
| Control Mean | 2456 | 1334 | 140 | 579 | 309 | 22 | 43 |
| p-value H=F | 0.450 | 0.286 | 0.054 | 0.514 | 0.503 | 0.200 | 0.356 |
| p-value F=2H | 0.975 | 0.284 | 0.401 | 0.711 | 0.595 | 0.917 | 0.465 |
| Obs | 2,861 | 3,021 | 3,223 | 3,218 | 3,207 | 3,203 | 3,234 |

*Notes:* All income components are winsorized at 5%. Total income is HH reported monthly income winsorized at 5% and consists of HH members' work wages, livestock revenues, government and non-government assistance, non-agricultural activity sales, remittances, and crops' net revenues. The model uses quartet fixed effects and standard errors clustered at the agglomeration level. Wage work is all household members' work wages. Livestock revenues are revenues from selling livestock and net revenues from selling subsistence products. Assistance is assistance to households from government (Takaful & Karama, pensions, and assistance from NGOs). Non-agri sales are net revenues from non-agricultural activities by household members. Crops net revenues are net revenues from crop sales and revenues from renting out agricultural land. Remittances are transfers from anyone who is not part of the household. The transfer treatment arms also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm.

## Table A4: Economic Activities of Female Designees at 40 Months (Unrestricted Arms Only)

**Panel A: Impact on Aggregated Activity Types**

| | (1) # Subsistence Activities | (2) # Market Activities | (3) Wage Work | (4) # Activities | (5) None |
|---|---|---|---|---|---|
| Half Transfer | 0.28*** | 0.14*** | -0.01* | 0.42*** | -0.07*** |
| | (0.04) | (0.03) | (0.01) | (0.06) | (0.02) |
| Full Transfer | 0.41*** | 0.18*** | -0.01* | 0.59*** | -0.11*** |
| | (0.04) | (0.03) | (0.01) | (0.06) | (0.02) |
| Control Mean | 0.94 | 0.24 | 0.02 | 1.19 | 0.23 |
| p-value H=F | 0.002 | 0.242 | 0.771 | 0.009 | 0.019 |
| Obs | 2,401 | 2,401 | 1,249 | 2,401 | 2,401 |

**Panel B: Impact on Each Subsistence Activity**

| | (6) Small LS | (7) Med LS | (8) Large LS |
|---|---|---|---|
| Half Transfer | 0.04** | 0.09*** | 0.15*** |
| | (0.02) | (0.02) | (0.02) |
| Full Transfer | 0.08*** | 0.15*** | 0.17*** |
| | (0.02) | (0.02) | (0.02) |
| Control Mean | 0.73 | 0.13 | 0.09 |
| p-value H=F | 0.018 | 0.001 | 0.254 |
| Obs | 2,401 | 2,401 | 2,401 |

**Panel C: Impact on Each Market Activity**

| | (9) Small LS | (10) Med LS | (11) Large LS | (12) Non-agri |
|---|---|---|---|---|
| Half Transfer | 0.02 | 0.04*** | 0.03*** | 0.05*** |
| | (0.02) | (0.01) | (0.01) | (0.01) |
| Full Transfer | 0.05*** | 0.04*** | 0.03*** | 0.06*** |
| | (0.02) | (0.01) | (0.01) | (0.01) |
| Control Mean | 0.11 | 0.03 | 0.02 | 0.08 |
| p-value H=F | 0.120 | 0.945 | 0.779 | 0.653 |
| Obs | 2,401 | 2,401 | 2,401 | 2,401 |

*Notes:* Dependent variables in Panel A are as follows. Column 1 reports the total number of subsistence activities in which the respondent participated in the last 7 days (out of 3). Column 2 reports the total number of self-employed market activities in which the respondent participated in the last 7 days (out of 4). Column 3 is a dummy equal to 1 if the respondent reports having worked for others for a wage in the last 7 days, and 0 otherwise. This column has a reduced sample because many female respondents regard working for others as demeaning and do not answer the question. Column 4 reports the total number of activities other than wage work in which the respondent participated in the last 7 days (out of 7). Column 5 is a dummy equal to 1 if the respondent did not engage in any economic activity in the last 7 days, and 0 otherwise. Panel B reports participation indicators for each listed subsistence activity. Panel C reports participation indicators for each listed market activity. **Small LS:** small livestock (poultry). **Medium LS:** medium livestock (sheep and goats). **Large LS:** large livestock (cows and buffaloes). **Non-agri:** self-employment in neither agriculture nor husbandry. **Half Transfer** and **Full Transfer** refer to the Half Transfer arm and the Full Transfer unrestricted arm, respectively. All regressions include quartet (stratification) fixed effects; standard errors are clustered at the agglomeration level (the randomization unit). The sample is restricted to female designees in either the Half Transfer arm or the Full Transfer unrestricted arm, that is, women whom households selected as transfer beneficiaries prior to randomization. The transfer treatment arms also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm. *** p<0.01, ** p<0.05, * p<0.10.

## Table A5: 20 Month Impacts on Primary Outcomes

| | (1) Livestock | (2) Total Assets | (3) Consumption | (4) Income | (5) Index |
|---|---|---|---|---|---|
| Half Transfer | 10448*** | 8376*** | 259** | 384*** | 0.58*** |
| | (816) | (2015) | (117) | (112) | (0.05) |
| Full Transfer | 17289*** | 18440*** | 259*** | 790*** | 0.97*** |
| | (767) | (1769) | (93) | (98) | (0.04) |
| Control Mean | 2103 | 10983 | 5379 | 2540 | 0.00 |
| p-value H=F | 0.000 | 0.000 | 0.996 | 0.000 | 0.000 |
| p-value F=2H | 0.011 | 0.643 | 0.219 | 0.909 | 0.033 |
| Obs | 3,287 | 3,285 | 2,949 | 2,868 | 3,297 |

*Notes:* Treatment effects at 20 months (midline) without baseline controls. Migration and education were not collected at midline. **Summary Index** follows Kling et al. (2007): equally weighted average of the four standardized components (livestock value, total assets, consumption, income), each standardized by subtracting the control group mean and dividing by the control group standard deviation. For households missing any component, the missing value is imputed with the treatment-arm mean of that standardized component before averaging. Quartet FE; SEs clustered at agglomeration level. The transfer treatment arms also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm. **p-value F=2H:** Test that the Full Transfer effect equals twice the Half Transfer effect. *** p<0.01, ** p<0.05, * p<0.10.

## Table A6: 20-Month Impact on household income

| | (1) Total income | (2) Work Wages | (3) Livestock revenues | (4) Assistance | (5) Non-agri sales | (6) Crops net revenues | (7) Remittances |
|---|---|---|---|---|---|---|---|
| Half Transfer | 384*** | -210*** | 397*** | -26 | 141*** | 22*** | 64*** |
| | (112) | (80) | (50) | (30) | (45) | (7) | (14) |
| Full Transfer | 790*** | -155** | 655*** | 12 | 183*** | 22*** | 5 |
| | (98) | (71) | (40) | (25) | (41) | (5) | (7) |
| Control Mean | 2540 | 1448 | 165 | 716 | 169 | 16 | 33 |
| p-value H=F | 0.000 | 0.459 | 0.000 | 0.165 | 0.391 | 0.994 | 0.000 |
| p-value F=2H | 0.909 | 0.055 | 0.145 | 0.209 | 0.240 | 0.093 | 0.000 |
| Obs | 2,868 | 3,009 | 3,289 | 3,249 | 3,221 | 3,238 | 3,277 |

*Notes:* All income components are winsorized at 5%. The model uses quartet fixed effects and standard errors clustered at the agglomeration level. Wage work is all household members' work wages. Livestock revenues are revenues from selling livestock and net revenues from selling subsistence products. Assistance is assistance to households from government (Takaful & Karama, pensions, and assistance from NGOs). Non-agri sales are net revenues from non-agricultural activities by household members. Crops net revenues are net revenues from crop sales and revenues from renting out agricultural land. Remittances are transfers from anyone who is not part of the household. The transfer treatment arms also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm.

## Table A7: 40 Month Impacts on Primary Outcomes (ANCOVA)

| | (1) Livestock | (2) Total Assets | (3) Consumption | (4) Income | (5) Migration | (6) Education | (7) Index |
|---|---|---|---|---|---|---|---|
| Half Transfer | 5512*** | 8278*** | 332*** | 109 | 0.01 | -0.01 | 0.23*** |
| | (657) | (1164) | (103) | (149) | (0.02) | (0.02) | (0.03) |
| Full Transfer | 8839*** | 12290*** | 260*** | 196* | 0.03 | -0.01 | 0.38*** |
| | (661) | (1116) | (78) | (117) | (0.02) | (0.02) | (0.03) |
| Control Mean | 2646 | 6654 | 5514 | 2456 | 0.42 | 0.72 | -0.00 |
| p-value H=F | 0.000 | 0.001 | 0.490 | 0.502 | 0.351 | 0.899 | 0.000 |
| p-value F=2H | 0.055 | 0.043 | 0.036 | 0.932 | 0.709 | 0.711 | 0.140 |
| Obs | 3,247 | 3,247 | 2,941 | 2,861 | 3,253 | 3,254 | 3,254 |

*Notes:* Treatment effects at 40 months (endline). All regressions control for the baseline value of the outcome where available (Livestock, Total Assets, Consumption, Income); where the baseline value is missing, it is imputed with the treatment-arm mean and a missing indicator is included. Migration, Education, and Summary Index are estimated without baseline controls. **Summary Index** follows Anderson (2008): equally weighted average of available standardized outcomes; households with no children aged 7–18 are coded as zero for the education component. Quartet FE; SEs clustered at agglomeration level. The transfer treatment arms also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm. **p-value F=2H:** Test that the Full Transfer effect equals twice the Half Transfer effect. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

## Table A8: Test of Poverty Traps: Missing Values Imputed

| | | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|---|
| Panel A | Post Transfer Total Asset Growth at 40 Months | | | | | | |
| | baseline asset decile | 0-50 | 50-60 | 60-70 | 70-80 | 80-90 | 90-100 |
| | Half Transfer | 159 | 1,266 | 5,446 | 6,704* | 2,667 | 17,039* |
| | | (1,213) | (2,909) | (3,887) | (3,870) | (5,365) | (8,859) |
| | Full Transfer | -3,387*** | -2,769 | 3,545 | 4,635 | 1,720 | 4,602 |
| | | (997) | (2,476) | (3,490) | (3,339) | (4,453) | (8,526) |
| | Control Mean | 2,768 | 5,681 | 3,155 | 3,339 | 4,059 | -26,200 |
| p-values: | Ha: H>0 | 0.448 | 0.332 | 0.081 | 0.042 | 0.310 | 0.028 |
| | Ha: F<0 | 0.000 | 0.132 | 0.845 | 0.917 | 0.650 | 0.705 |
| | Ha: H>F | 0.005 | 0.088 | 0.310 | 0.313 | 0.428 | 0.057 |
| | Ha: F>H | 0.995 | 0.912 | 0.690 | 0.687 | 0.572 | 0.943 |
| | Obs | 1,696 | 409 | 363 | 356 | 299 | 346 |
| Panel B | Post Transfer Total Asset Growth at 40 Months | | | | | | |
| | | (1) | (2) | (3) | (4) | (5) | (6) |
| | baseline asset threshold | 0-50 | 0-60 | 0-70 | 0-80 | 0-90 | 0-100 |
| | Half Transfer | 159 | -320 | 526 | 1,053 | 1,270 | 2,410** |
| | | (1,213) | (1,117) | (990) | (952) | (929) | (1,212) |
| | Full Transfer | -3,387*** | -3,338*** | -2,192** | -1,737* | -1,351 | -1,110 |
| | | (997) | (962) | (927) | (923) | (915) | (1,222) |
| | Control Mean | 2,486 | 2,486 | 3,153 | 3,079 | 2,995 | 997 |
| p-values: | Ha: H>0 | 0.448 | 0.613 | 0.298 | 0.135 | 0.086 | 0.024 |
| | Ha: F<0 | 0.000 | 0.000 | 0.009 | 0.030 | 0.070 | 0.182 |
| | Ha: H>F | 0.005 | 0.008 | 0.008 | 0.005 | 0.007 | 0.002 |
| | Ha: F>H | 0.995 | 0.992 | 0.992 | 0.995 | 0.993 | 0.998 |
| | Obs | 1,696 | 2,105 | 2,468 | 2,824 | 3,123 | 3,469 |

Notes: Baseline asset deciles and thresholds based on total assets including Livestock, Land, Tractors, Ploughs, Shears, and Carts valued in 40 months prices. Missing values imputed at the experimental arm mean. Asset measure values are winsorized at 5%. The model uses quartet fixed effects and SE are clustered on the agglomeration level. H and F denote the Half Transfer and Full Transfer treatments, respectively. The transfer treatment arms also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm. Ha states the alternative hypothesis against the null hypothesis of equality. *** p<0.01, ** p<0.05, * p<0.10

## Table A9: Test of Poverty Traps: Drop Restricted Arm

Full Transfer Unrestricted Only

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Panel A | Post Transfer Total Asset Growth at 40 Months | | | | | | |
| | | (1) | (2) | (3) | (4) | (5) | (6) |
| | baseline asset decile | 0-50 | 50-60 | 60-70 | 70-80 | 80-90 | 90-100 |
| | Half Transfer | 1,258 | 1,355 | 5,548 | 5,819 | 5,493 | 12,533 |
| | | (1,229) | (3,553) | (3,612) | (4,531) | (6,317) | (8,583) |
| | Full Transfer | -3,560*** | -2,555 | -550 | 5,412 | -1,203 | -17,867** |
| | | (1,214) | (3,017) | (3,392) | (4,334) | (6,179) | (7,776) |
| | Control Mean | 2,692 | 6,784 | 3,771 | 2,869 | 3,421 | -22,820 |
| p-values: | Ha: H>0 | 0.153 | 0.352 | 0.063 | 0.101 | 0.193 | 0.073 |
| | Ha: F<0 | 0.002 | 0.199 | 0.436 | 0.893 | 0.423 | 0.012 |
| | Ha: H>F | 0.001 | 0.133 | 0.042 | 0.471 | 0.181 | 0.002 |
| | Ha: F>H | 0.999 | 0.867 | 0.958 | 0.529 | 0.819 | 0.998 |
| | Obs | 1,159 | 295 | 259 | 250 | 201 | 239 |
| Panel B | Post Transfer Total Asset Growth at 40 Months | | | | | | |
| | | (1) | (2) | (3) | (4) | (5) | (6) |
| | baseline asset threshold | 0-50 | 0-60 | 0-70 | 0-80 | 0-90 | 0-100 |
| | Half Transfer | 1,258 | 650 | 1,466 | 1,975* | 1,970* | 2,405** |
| | | (1,229) | (1,170) | (1,065) | (1,036) | (1,022) | (1,158) |
| | Full Transfer | -3,560*** | -3,724*** | -3,198*** | -2,778** | -2,489** | -4,191*** |
| | | (1,214) | (1,148) | (1,085) | (1,114) | (1,149) | (1,205) |
| | Control Mean | 2,692 | 2,692 | 3,584 | 3,496 | 3,489 | 1,454 |
| p-values: | Ha: H>0 | 0.153 | 0.289 | 0.085 | 0.029 | 0.027 | 0.019 |
| | Ha: F<0 | 0.002 | 0.001 | 0.002 | 0.007 | 0.016 | 0.000 |
| | Ha: H>F | 0.001 | 0.001 | 0.000 | 0.000 | 0.001 | 0.000 |
| | Ha: F>H | 0.999 | 0.999 | 1.000 | 1.000 | 0.999 | 1.000 |
| | Obs | 1,159 | 1,454 | 1,713 | 1,963 | 2,164 | 2,403 |

Notes: Baseline asset deciles and thresholds based on total assets including Livestock, Land, Tractors, Ploughs, Shears, and Carts valued in 40 months prices. Asset measure values are winsorized at 5%. The model uses quartet fixed effects and SE are clustered on the agglomeration level. H and F denote the Half Transfer and Full Transfer treatments, respectively. The transfer treatment arms also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm. Ha states the alternative hypothesis against the null hypothesis of equality. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

## Table A10: ML Heterogeneity: Post-Transfer Growth in Total Assets (Full vs Control, Half vs Control)

| Panel A | Full vs Control | Half vs Control |
|---|---|---|
| Bottom quintile | -5071 | -5225 |
| | (4373) | (5497) |
| 2nd quintile | -2233 | -1038 |
| | (4504) | (5397) |
| 3rd quintile | 97.760 | 1111 |
| | (4530) | (5434) |
| 4th quintile | 2940 | 3617 |
| | (4814) | (5532) |
| Top quintile | 10358 | 6823 |
| | (6181) | (5621) |
| Top vs bottom p-value | 0.017** | 0.083* |
| Beta 2 | 0.732 | 0.664 |
| Beta 2 p-value | 0.023** | 0.140 |
| Obs | 2406 | 1584 |

| Panel B: Baseline (CLAN) | Bottom Quintile Mean | Difference with Top Quintile | | Bottom Quintile Mean | Difference with Top Quintile |
|---|---|---|---|---|---|
| Household financial status (0-10) | 1.691 | 0.952*** | Owns other agriculture equipment | 0.038 | -0.038** |
| | | (0.158) | | | (0.019) |
| Owns livestock | 0.212 | 0.664*** | Working-age members (count) | 3.475 | -0.969*** |
| | | (0.040) | | | (0.215) |
| Large livestock (count) | 0.004 | 0.535*** | Attached livestock space | 0.601 | 0.226*** |
| | | (0.071) | | | (0.060) |
| Household head is livestock handler | 0.017 | 0.207*** | Treated households per agglomeration | 8.381 | 3.780*** |
| | | (0.034) | | | (1.148) |
| Any beneficiary would not seek sibling help | 0.813 | -0.203*** | Owns donkey cart | 0.019 | 0.088*** |
| | | (0.047) | | | (0.030) |
| Any beneficiary has input in enterprise decisions | 0.033 | 0.133*** | Water heating by gas stove | 0.818 | -0.192*** |
| | | (0.032) | | | (0.058) |
| Medium livestock (count) | 0.029 | 0.560*** | Medical help from pharmacy (last week) | 0.006 | 0.044** |
| | | (0.098) | | | (0.021) |
| Owns toktok | 0.025 | -0.017 | Owns toktok | 0.025 | -0.019 |
| | | (0.014) | | | (0.014) |

*Notes:* $* p < 0.10$, $** \ p < 0.05$, $*** p < 0.01$. Outcomes are winsorized at 5% within each treatment arm. Panel B reports the top eight baseline covariates for each pairwise comparison in Panel A. Baseline covariates were selected using LASSO stability selection. In the Full vs Control block, the top six covariates are tied in selection importance; in the Half vs Control block, the top two covariates are tied. The treatment arms in these pairwise comparisons also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm.

## Table A11: ML Heterogeneity: Post-Transfer Growth in Livestock (Full vs Control, Half vs Control)

| Panel A | Full vs Control | Half vs Control |
|---|---|---|
| Bottom quintile | -7102 | -3700 |
| | (2105) | (2308) |
| 2nd quintile | -5465 | -1992 |
| | (2193) | (2471) |
| 3rd quintile | -3894 | -943 |
| | (2284) | (2527) |
| 4th quintile | -1961 | 365 |
| | (2415) | (2527) |
| Top quintile | 5152 | 2644 |
| | (2898) | (2664) |
| Top vs bottom p-value | 0.000*** | 0.028** |
| Beta 2 | 1.080 | 0.760 |
| Beta 2 p-value | 0.000*** | 0.056* |
| Obs | 2406 | 1584 |

| Panel B: Baseline (CLAN) | Bottom Quintile Mean | Difference with Top Quintile | | Bottom Quintile Mean | Difference with Top Quintile |
|---|---|---|---|---|---|
| Asset preference: livestock (half) | 0.207 | 0.716*** | Other water-heating source | 0.088 | 0.264*** |
| | | (0.037) | | | (0.052) |
| Livestock assets (EGP) | 135 | 7847*** | Owns livestock | 0.340 | 0.384*** |
| | | (931) | | | (0.062) |
| Household financial status (0-10) | 1.844 | 0.620*** | Working-age members (count) | 3.308 | -0.733*** |
| | | (0.164) | | | (0.203) |
| Small livestock (count) | 1.204 | 4.374*** | Household-attached livestock space | 0.579 | 0.264*** |
| | | (0.365) | | | (0.058) |
| Medium livestock (count) | 0.021 | 0.593*** | Livestock assets (EGP) | 413 | 4577*** |
| | | (0.102) | | | (965) |
| Large livestock (count) | 0.000 | 0.554*** | Asset preference: livestock (half) | 0.431 | 0.418*** |
| | | (0.073) | | | (0.057) |
| Any disabled or chronically ill member | 0.656 | -0.145*** | Implemented pre-COVID | 0.673 | -0.396*** |
| | | (0.053) | | | (0.061) |
| Any beneficiary would not seek sibling help | 0.776 | -0.170*** | Unauthorized electricity connection | 0.006 | 0.063** |
| | | (0.049) | | | (0.025) |

*Notes:* $* p < 0.10$, $** p < 0.05$, $*** p < 0.01$. Outcomes are winsorized at 5% within each treatment arm. Panel B reports the top eight baseline covariates for each pairwise comparison in Panel A. Baseline covariates were selected using LASSO stability selection. In the Full vs Control block, all eight covariates are tied in selection importance; in the Half vs Control block, the top three covariates are tied. The treatment arms in these pairwise comparisons also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm.

## Table A12: ML Heterogeneity: Consumption (Full vs Control, Half vs Control)

| **Panel A** | **Full vs Control** | **Half vs Control** |
|---|---|---|
| Bottom quintile | -65 | -186 |
| | (363) | (523) |
| 2nd quintile | 160 | 65.612 |
| | (359) | (521) |
| 3rd quintile | 326 | 264 |
| | (365) | (524) |
| 4th quintile | 456 | 489 |
| | (361) | (523) |
| Top quintile | 667 | 771 |
| | (364) | (531) |
| Top vs bottom p-value | 0.115 | 0.142 |
| Beta 2 | 0.125 | 0.062 |
| Beta 2 p-value | 0.753 | 0.679 |
| Obs | 2190 | 1430 |

| **Panel B: Baseline (CLAN)** | **Bottom Quintile Mean** | **Difference with Top Quintile** | | **Bottom Quintile Mean** | **Difference with Top Quintile** |
|---|---|---|---|---|---|
| Treated households per agglomeration | 6.425 | 2.160** | Asset preference: livestock or poultry (half) | 0.545 | 0.322*** |
| | | (0.873) | | | (0.060) |
| Household financial status (0-10) | 1.740 | 0.685*** | Any beneficiary would seek friend help | 0.203 | 0.259*** |
| | | (0.161) | | | (0.064) |
| Work income missing flag | 0.692 | 0.116** | Dependency ratio | 1.528 | 0.417*** |
| | | (0.049) | | | (0.129) |
| Disabled or chronically ill members (ratio) | 0.217 | -0.096*** | Ceiling material: palm leaf | 0.224 | 0.143** |
| | | (0.021) | | | (0.064) |
| Conventional TV | 0.904 | -0.114*** | Any beneficiary would not seek friend help | 0.899 | -0.206*** |
| | | (0.040) | | | (0.054) |
| HH perceives themselves as among poorest | 0.397 | -0.174*** | Distance to water source (minutes) | 1.818 | -1.371 |
| | | (0.052) | | | (1.054) |
| Any beneficiary in perfect health | 0.562 | -0.041 | Flooring type: dirt or sand | 0.493 | 0.122* |
| | | (0.057) | | | (0.068) |
| Any beneficiary independently controls business decisions | 0.137 | -0.064* | Any beneficiary would seek sibling help | 0.406 | 0.098 |
| | | (0.036) | | | (0.070) |

*Notes:* $*p < 0.10$, $**p < 0.05$, $***p < 0.01$. Outcomes are winsorized at 5% within each treatment arm. Panel B reports the top eight baseline covariates for each pairwise comparison in Panel A. Baseline covariates were selected using LASSO stability selection. In the Full vs Control block, the top four covariates are tied in selection importance. The treatment arms in these pairwise comparisons also include complementary services such as training and counseling, with the Half Transfer arm receiving roughly half as much of those services as the Full Transfer arm.

## Table A13: Test of Poverty Traps: Unified Prices Across Sample

| Panel A | Post Transfer Total Asset Growth at 40 Months | | | | | | |
|---|---|---|---|---|---|---|---|
| | baseline asset decile | 0-50 | 50-60 | 60-70 | 70-80 | 80-90 | 90-100 |
| | Half Cost | 1,068 | 2,698 | 3,458 | 8,171* | 6,725 | 8,124 |
| | | (1,297) | (3,525) | (4,031) | (4,488) | (5,733) | (8,834) |
| | Full Cost | -2,914*** | -3,339 | 4,908 | 6,931* | 5,022 | -3,565 |
| | | (1,009) | (2,724) | (3,628) | (3,840) | (5,141) | (7,628) |
| | Control Mean | 2,827 | 6,951 | 4,256 | 3,295 | 3,994 | -21,445 |
| p-values: | Ha: H>0 (one-sided) | 0.205 | 0.222 | 0.196 | 0.035 | 0.121 | 0.179 |
| | Ha: F<0 | 0.002 | 0.111 | 0.911 | 0.964 | 0.835 | 0.320 |
| | Ha: H>F | 0.004 | 0.035 | 0.640 | 0.394 | 0.367 | 0.086 |
| | Ha: F>H | 0.996 | 0.965 | 0.360 | 0.606 | 0.633 | 0.914 |
| | Obs | 1,551 | 382 | 330 | 324 | 260 | 314 |
| Panel B | Post Transfer Total Asset Growth at 40 Months | | | | | | |
| | baseline asset threshold | 0-50 | 0-60 | 0-70 | 0-80 | 0-90 | 0-100 |
| | Half Cost | 1,068 | 613 | 1,232 | 1,903* | 1,821* | 2,306* |
| | | (1,297) | (1,214) | (1,100) | (1,060) | (1,026) | (1,209) |
| | Full Cost | -2,914*** | -3,156*** | -1,903* | -1,321 | -1,000 | -1,175 |
| | | (1,009) | (999) | (989) | (994) | (981) | (1,096) |
| | Control Mean | 2,827 | 3,688 | 3,778 | 3,719 | 3,745 | 1,109 |
| p-values: | Ha: H>0 (one-sided) | 0.205 | 0.307 | 0.132 | 0.037 | 0.038 | 0.029 |
| | Ha: F<0 | 0.002 | 0.001 | 0.028 | 0.092 | 0.154 | 0.142 |
| | Ha: H>F | 0.004 | 0.002 | 0.005 | 0.003 | 0.008 | 0.003 |
| | Ha: F>H | 0.996 | 0.998 | 0.995 | 0.997 | 0.992 | 0.997 |
| | Obs | 1,551 | 1,933 | 2,263 | 2,587 | 2,847 | 3,161 |

*Notes:* This table replicated the analysis by baseline asset decile while using sample-wide median prices (as opposed to district level prices). Transfer values are Full = 11,643, Half = 5,821 (valued in endline EGP). Asset values winsorized at 5% by treatment arm. Quintet FE; SEs clustered at agglomeration level. *** p<0.01, ** p<0.05, * p<0.10

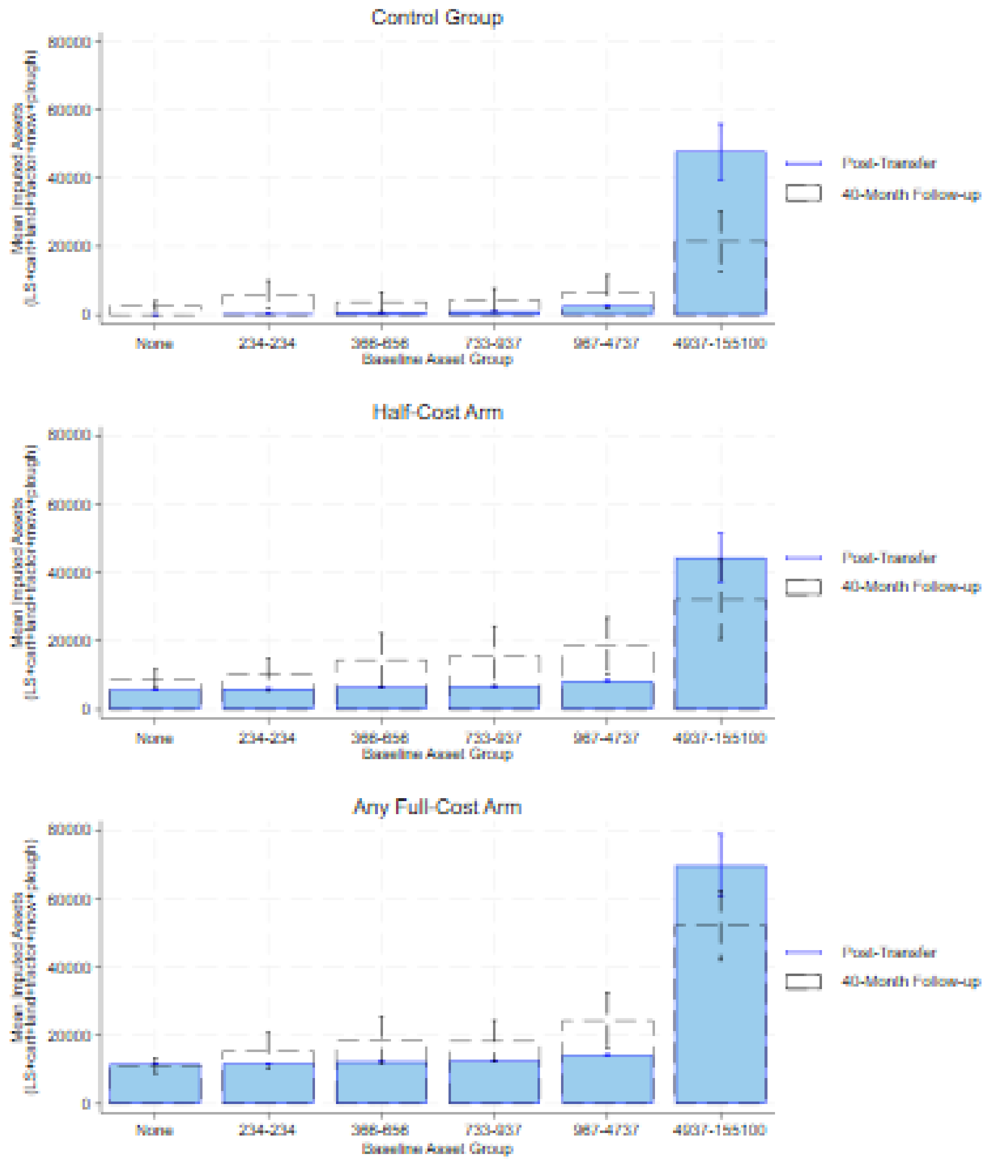
## Table A14: Test of Poverty Traps: Alternative Transformations

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Panel A | Positive asset growth (binary) | | | | | | |
| | baseline asset deciles | 0-50 | 50-60 | 60-70 | 70-80 | 80-90 | 90-100 |
| | Full Transfer | -0.034 | -0.029 | 0.000 | 0.050 | 0.075 | -0.031 |
| | | (0.029) | (0.087) | (0.075) | (0.084) | (0.121) | (0.077) |
| | Obs | 1,048 | 244 | 215 | 216 | 169 | 206 |
| Panel B | Asset growth (IHS) | | | | | | |
| | baseline asset deciles | 0-50 | 50-60 | 60-70 | 70-80 | 80-90 | 90-100 |
| | Full Transfer | -0.884*** | -0.262 | -0.137 | -0.867 | 0.181 | 0.315 |
| | | (0.307) | (0.731) | (0.585) | (0.706) | (0.706) | (0.471) |
| | Obs | 1,048 | 244 | 215 | 216 | 169 | 206 |

*Notes:* The dependent variable in Panel A is a binary indicator equal to one if endline assets are strictly larger than post-transfer assets. The dependent variable in Panel B is the difference between the inverse hyperbolic sine (IHS) of endline assets and the IHS of post-transfer assets, used to approximate log changes in the presence of zero values. Asset measure includes Livestock, Land, Tractors, Ploughs, Shears, and Carts valued in 40 months prices. Asset measure values are winsorized at 5%. The model uses quintet fixed effects and SE are clustered on the agglomeration level. Only half vs full transfer treatment comparison (arm $\neq 0$). *** p<0.01, ** p<0.05, * p<0.10

# A-2 Appendix Figures

### Figure A1: Post-Transfer Asset Accumulation: Imputed Values




Overall sample sizes: **None**:1696, **234-234**:409, **366-656**:363, **733-937**:356, **967-4737**:299, **4937-155100**:346. Vertical lines: 95% CIs of means.