Check for updates

PREPRINT

Not Peer Reviewed

RESEARCH ARTICLE

Graduation through Credit? Long-Run Impacts of a Hybrid Ultra-Poor Graduation Programme in Bangladesh

[version 1]

Shaila Ahmed[1], Marjan Hossain[1], Muhammad Towkir Hossain[1,2], Atiya Rahman[1,3], Munshi Sulaiman[1]

[1]BRAC Institute of Governance and Development, Dhaka, Dhaka Division, 1205, Bangladesh
[2]Schar School of Policy and Government, George Mason University, Fairfax, Virginia, USA
[3]University of East Anglia, Norwich, England, UK

v1 **First published:** 04 Nov 2025, **2**:362
https://doi.org/10.12688/verixiv.2196.1
Latest published: 04 Nov 2025, **2**:362
https://doi.org/10.12688/verixiv.2196.1

Abstract

This paper evaluates the long-term impacts of BRAC's Other Targeting the Ultra Poor (OTUP) model in Bangladesh, a multifaceted intervention that combines subsidized credit with a consumption allowance, enterprise training, and social and health support. Using a randomized controlled trial (RCT) of 8,973 households across 88 branch areas, we assessed outcomes seven years after program implementation. Our findings indicate that the program has persistent, positive effects on household asset accumulation and savings. Furthermore, it enhances human capital investments, leading to increased healthcare utilization among treatment women and improved educational outcomes, particularly for girls' secondary school enrollment. However, these household-level gains do not translate into significant impacts on women's individual earnings or empowerment, suggesting a limited conversion of household resources into personal agency. The high benefit-cost ratio from our analysis suggests that the credit-based graduation model could be an effective poverty reduction strategy for the marginally better-off ultra-poor. This trial was prospectively registered in the AEA RCT Registry (AEARCTR-0013689).

Keywords

poverty, graduation, randomized control trial, health, women empowerment, Bangladesh

This article is included in the Gates Foundation gateway.

Corresponding author: Atiya Rahman (atiya.rahman@bracu.ac.bd)

Author roles: Ahmed S: Writing – Review & Editing; **Hossain M**: Writing – Original Draft Preparation, Writing – Review & Editing; **Hossain MT**: Formal Analysis, Investigation, Project Administration; **Rahman A**: Conceptualization, Data Curation, Formal Analysis, Investigation, Methodology, Project Administration, Supervision, Writing – Review & Editing; **Sulaiman M**: Supervision, Writing – Review & Editing

Competing interests: No competing interests were disclosed.

Grant information: This research was supported by a grant from the Bill & Melinda Gates Foundation

Copyright: © 2025 Ahmed S *et al*. This is an open access article distributed under the terms of the Creative Commons Attribution License , which permits unrestricted use, distribution, and reproduction in any medium, provided the original work is properly cited.

How to cite this article: Ahmed S, Hossain M, Hossain MT *et al.* **Graduation through Credit? Long-Run Impacts of a Hybrid Ultra-Poor Graduation Programme in Bangladesh [version 1]** VeriXiv 2025, **2**:362 https://doi.org/10.12688/verixiv.2196.1

First published: 04 Nov 2025, **2**:362 https://doi.org/10.12688/verixiv.2196.1

1. Introduction

Microfinance and graduation programs are two extensively studied anti-poverty interventions that aim to address poverty through different strategies and target distinct population groups. Microfinance extends small loans to entrepreneurially capable poor households, enabling productive investment and business expansion (Banerjee, Karlan and Zinman, 2015) at low unit costs, which systematically excludes the ultra-poor. Micro-credit programs, despite increasing business investment, profits, and enterprise activity (Banerjee et al., 2015*b*; Cr´epon, El Komi and Osman, 2024; Attanasio et al., 2015), fail to translate these enterprise- level gains into broader household welfare improvements (Banerjee et al., 2015*b*; Angelucci, Karlan and Zinman, 2015; Tarozzi, Desai and Johnson, 2015; Karlan and Zinman, 2011; Augsburg et al., 2015; Bandiera et al., 2022). However, modifications to standard contracts, such as repayment grace periods (Field et al., 2013), alternative selection mechanisms (Maitra et al., 2017), or changing borrower demographics (Fiala, 2018), have occasionally improved outcomes, but often at the cost of sustainability or scalability. In contrast, graduation pro- grams[1] reach the ultra-poor through an intensive, multi-component package designed to facilitate their transition out of poverty (Heil et al., 2024). However, high per-beneficiary costs constrain coverage to only a subset of eligible households. The evidence base for graduation programs is generally more favourable than for micro-credit in improving welfare outcomes. Graduation programs have been shown to generate positive long-term impacts on income, consumption, savings, and productive assets among ultra-poor households (Banerjee, Karlan and Zinman, 2015; Banerjee, Duflo and Sharma, 2021; Bandiera et al., 2017), with several studies documenting impacts that persist even in fragile and conflict-affected contexts (Bedoya et al., 2019; Brune et al., 2022; Chowdhury et al., 2017). The targeting strategies of microfinance and graduation create a "missing middle" among ultra-poor households, where they are too poor for microfinance yet insufficiently destitute to qualify for graduation pro- grams. We examine whether hybrid interventions can cost-effectively serve this group of population by studying BRAC's Other Targeting the Ultra Poor (OTUP) model (hence- forth referred to as the hybrid model), which combines subsidized credit with components of graduation programmes. In particular, we assess whether the effects observed in the short run (Rahman, Bhattacharjee and Das, 2021) persist seven years after implementation and extend beyond economic outcomes to include health and women's empowerment.

We evaluate the programme using data from a randomized controlled trial (RCT) implemented across 88 BRAC branch areas in 11 districts of Bangladesh. Branches were randomly assigned within districts, with two of the eight branches designated as control (22 total) and the remaining six as treatment (66 total). In each branch, BRAC selected 7–8 villages and applied standard eligibility criteria to identify approximately 14 ultra-poor households per village, resulting in around 100 households per branch. The baseline survey, conducted in 2016, interviewed 8,973 households (7,042 treatment; 1,931 control). In the first follow-up, conducted in 2019, budget constraints limited data collection to all control households and a randomly selected two-thirds of treated households. This round successfully re-interviewed 5,951 households (4,212 treatment; 1,739 control). The second follow-up in 2024, targeted all baseline households, but managed to reach around 87% of both treatment (6,185) and control (1682) households.

We find that seven years post-intervention, treated households remain more likely than controls to engage in livestock rearing. Treated households own more valuable livestock, hold more land, and report significantly higher savings than controls. Simultaneously, treated households reduced their holdings of non-productive assets, suggesting that the intervention may have influenced a shift in household investment behaviour away from less productive to more productive assets. In addition to borrowing from BRAC, treated households are more likely to obtain credit from other formal institutions, a pattern that emerges only in the long run. Treated households also spend more on food and achieve higher dietary diversity. Health outcomes reveal a pattern of improved service uptake rather than direct improvements in physical well-being. Treated individuals report a greater incidence of illness in the past two weeks, primarily due to increased reporting of non-infectious diseases. Health-seeking behaviour improves among beneficiaries: women are significantly more likely to consult a healthcare provider or a licensed physician, and average out-of-pocket medical spending is higher as well. The programme also positively affects maternal health care, as women in treatment areas are significantly more likely to receive at least four antenatal care visits during their period of pregnancy.[2] The intervention does not significantly affect contraceptive use or reproductive health outcomes. We find no statistically significant effects on women's empowerment based on our constructed index. We find some programme effects on the children residing in treated households- school enrollment increases among adolescents in treated households, particularly for girls. However, impacts on child health and nutrition remain limited. No significant effects are observed on anthropometric indicators overall, though gender-disaggregated analysis suggests a modest improvement in weight- for-age among male children in treated households, with no comparable effect for females. Measuring both short- and long-run impacts, we

[1]These programmes typically feature a multifaceted support package, including an asset transfer, a consumption allowance, and health and social services (Heil et al., 2024).
[2]We collected data from women who were pregnant or had younger child below 2 years.

find that while the intervention's initial income gains dissipate over time, its positive effects on assets and savings persist, albeit at a reduced magnitude compared to the short run (Rahman, Bhattacharjee and Das, 2021).

When assessing the intervention's scalability, the hybrid model demonstrates a comparatively high benefit-cost ratio (BCR) of 8.47 at one year post-intervention (Rahman, Bhattacharjee and Das, 2021), and 5.3 after seven years, remaining robust under all sensitivity scenarios. Although the hybrid model appears more efficient than the grant-only graduation model,[3] partly because its design allows for cost recovery, the two interventions serve distinct segments of the ultra-poor. Different target groups mean efficiency metrics alone cannot capture the equity trade-offs inherent in programme design, since welfare gains depend not only on average returns per unit of cost but also on who benefits. Grant-based models, while less cost-efficient, may generate larger welfare gains for the most deprived by fostering transformative livelihood shifts that hybrid models are not designed for. Ultimately, programme selection involve a trade-off between efficiency and equity, as different models cater to distinct dimensions of poverty.

This paper contributes to the literature on poverty-alleviation strategies by providing evidence on a credit-based model of the graduation programme, assessing both the persistence of its impacts and whether the benefits justify the costs. It further extends the analysis to an underexplored area of this literature by assessing the hybrid model's effects on health outcomes for both direct beneficiaries and other household members.

The remainder of the paper is organized as follows. Section 2 provides background on BRAC's OTUP programme. Section 3 describes the experimental design, data collection, and outcome measures. Section 4 outlines the empirical strategy, and Section 5 presents the main results on household- and individual-level outcomes. Section 6 reports short- and long-run impacts of the programme. Section 7 presents the cost-benefit analysis and Section 8 concludes the paper.

2. BRAC's Other Targeted Ultra-Poor (OTUP) programme

BRAC's OTUP programme, a credit-based graduation model of the Targeting the Ultra Poor (TUP) programme,[4] was introduced in 2007 to extend coverage to ultra-poor households not eligible for the original grant-based model.[5]

Both models target the ultra-poor population, with the original model focusing on the most marginalized women. In contrast, the hybrid model serves households that are slightly better off yet still categorized as ultra-poor. Household selection into the hybrid model follows a standardized process identical to that of the grant-based model. First, sub-districts are selected using food security maps developed by the World Food programme (Bandiera et al., 2017). Within selected sub-districts, the poorest communities (referred to as "spots", each comprising roughly 100–150 households) are identified. Participatory rural appraisal (PRA) exercises are then conducted to rank households by community-defined wealth categories. Households in the lowest tiers are surveyed and screened against programme-specific inclusion and exclusion criteria (Rahman, Bhattacharjee and Das, 2021). Based on proximity to the poverty threshold, households are allocated to either grant-based or hybrid programme. Those meeting the criteria of the grant-based model[6] are enrolled in the programme while the latter are tested against the criteria for the hybrid model. The hybrid intervention targets households just above the eligibility threshold of the grant-based model: while still excluded from formal financial services and social protection programmes, these households typically hold slightly greater baseline assets. Eligibility for the hybrid model requires meeting at least three of five criteria: (i) reliance on irregular or seasonal income sources; (ii) ownership of no more than 30 decimals of land; (iii) a record of unsuccessful credit use; (iv) child labor among school-aged children (ages 6–14); and (v) food insecurity, proxied by the absence of protein consumption in the preceding three days. Households are excluded if they lack an adult female earner or if any member participates in micro-credit or similar development/anti- poverty programmes. Consistent with the grant based model, the designated beneficiary is the adult female earner.

The hybrid intervention delivers a bundled package centered on a subsidized loan, supplemented by periodic consumption stipends and health care support, which lasts for the duration of the intervention period. While the overall programme architecture mirrors TUP, the frequency of staff visits is lower in the hybrid model, as this group is considered slightly more self sufficient than TUP households.

---

[3]Bandiera et al. (2017) find the BCR of their model to be 3.21.
[4]Since 2017, TUP programme is known as the Ultra-Poor Graduation (UPG) programme.
[5]The original grant-based program is known as Specially Targeting the Ultra poor (STUP) (Rahman, Bhattacharjee and Das, 2021).
[6]Eligibility in this case requires satisfying at least three of five inclusion criteria and none of the exclusion criteria (Rahman, Bhattacharjee and Das, 2021). Inclusion criteria include reliance on irregular income sources (such as female day labour or begging), ownership of no more than 10 decimals of land, absence of productive assets (e.g., livestock), presence of school-aged children (ages 6–14) engaged in labour, and absence of an adult male income earner. Households are excluded if they lack a female earner or if any member participates in microfinance or other anti-poverty programmes.

- Enterprise development training
- Bi-weekly coaching and follow-up
- Temporary consumption support
- Basic health care assistance and referrals
- Access to savings facilities
- Awareness building on social and health issues
- Community resource mobilization

Following the selection of a productive asset, participants receive a three-day classroom training course designed to help them build the skills required to maintain, scale, and generate income from the chosen enterprise. The curriculum is tailored to a specific enterprise (e.g., livestock rearing, poultry, or agriculture), which is chosen jointly by the participant and programme staff. This selection is based on the participant's prior experience, health, and local economic opportunities. Following the asset transfer, participants attend a seven-day refresher course to reinforce the lessons from the initial training sessions. Training focuses on topics around asset maintenance, income generation, and enterprise scaling. Loan disbursement (BDT 15,000) typically occurs within three weeks of completing the training. Loans carried a 20% annual interest rate, which was below 27%[7] typical for micro-credit and included a two-month grace period. Assets must be purchased under staff supervision within one month of the loan agreement. Repayment terms mirror those of BRAC's mainstream micro-credit operations (weekly or bi-weekly), and successful repayment qualifies participants for future micro-credit access.

Following asset acquisition, beneficiaries receive bi-weekly home visits over a period of 18 months. These visits provide coaching on enterprise management, financial literacy, asset diversification, and household well-being. Staff also discuss social topics such as child marriage, dowry, and trafficking. To ease consumption shortfalls during the transition period, participants receive a weekly stipend of BDT 210. Duration varies by enterprise type: 24 weeks for small businesses, 36 weeks for livestock, and 52 weeks for agriculture. The amount is calibrated to ensure households can meet minimum food needs (typically equivalent to purchasing 1 kg of rice per day) based on regional prices, household size, and national consumption survey benchmarks. Participants also receive subsidized treatment for critical illnesses and are referred to local facilities for less severe conditions. Each programme village forms a Village Social Solidarity Committee (formerly Village Poverty Reduction Committee) comprising local leaders who support programme implementation, mediate conflicts, and reinforce informal social protection. Finally, beneficiaries are encouraged to save small weekly amounts (BDT 10–20), with caps in place to avoid compromising early-stage consumption.

3. Design, data, and attrition

3.1 Experimental design

We evaluate the hybrid model through a randomized controlled trial (RCT), with randomization implemented at the level of BRAC branch offices. The study sample comprises 88 branch offices across 11 districts of Bangladesh, with eight branches selected per district. In each district, two branches were randomly assigned to the control group (22 in total), while the remaining six were allocated to the treatment group (66 in total). Eligible ultra-poor households were identified for the hybrid intervention using BRAC's standardized targeting protocol. Within each branch catchment area, 7–8 villages were selected, and approximately 14 eligible households were identified per village, resulting in around 100 households per branch office.

3.2 Data collection

The baseline survey conducted between April and July 2016 covered 8,973 households, including 7,042 in treatment areas and 1,931 in control areas. The first follow-up conducted in December 2018-January 2019, re-interviewed all control households and a randomly selected two-thirds of treated households due to budget constraints, resulting in a sample of 5,951 households (4,212 treatment; 1,739 control). The second follow-up, conducted between July and October 2024, attempted to revisit the full baseline sample and successfully tracked approximately 87% of households,

[7]This figure represents the prevailing market interest rate for micro-credit at the time of the intervention's implementation (Rahman, Bhattacharjee and Das, 2021).

reaching 6,185 in treatment areas and 1,682 in control areas. Both follow-up surveys collected data on household demographics, labour market outcomes, asset ownership, loan behaviour, coping strategies, health, and women's empowerment.

3.2.1 Ethical approval, process of consent, and clinical registration

Before conducting our second follow-up survey, we obtained the Institutional Review Board of the BRAC James P. Grant School of Public Health (IRB-2024-ES-19). The full research protocol was presented to the review board in early June 2024. Following this review and presentation, the survey work commenced in late June 2024. Due to the IRB's internal administrative and processing timelines, the final approval letter was formally issued in August 2024. The survey was conducted after receiving feedback in the presentation and also followed the procedure that was approved by the panel. However, the baseline in 2016 and first endline 2019 surveys were conducted when the research team was part of the Research and Evaluation Division (RED) at BRAC. At that time, in accordance with the organization's standard procedures, ethical oversight was provided by an internal ethical review board/team within RED. These surveys received approval from this internal body before data collection began. Formal documentation from an external IRB, as is now the standard, is not available for these earlier phases. We note that RED merged with the BRAC Institute of Governance and Development (BIGD) in 2019, and institutional procedures have since been updated to the current standard, which includes the external IRB approval sought for the 2024 survey.

Both verbal and written consent were obtained from all participants prior to their enrollment in the study. The enumerators read the consent form aloud to each participant, detailing the study's objectives, procedures, potential risks and benefits, and the voluntary nature of participation, including their right to withdraw at any time. Participants were given the opportunity to ask questions to ensure their understanding. After all questions were answered and the participant verbally agreed to participate, their consent was formally documented by obtaining their digital signature on the data collection tablet. For participants who were unable to provide a digital signature, a family member present with the participant signed the form to attest that the full consent process was conducted correctly and that the participant had voluntarily given their consent.

The study protocol was prospectively registered in the American Economic Association's (AEA) RCT Registry on 26 May 2024 and published on 30, May 2024 (Trial ID: AEARCTR-0013689) and the corresponding survey began in June 2024. The registry is publicly accessible at https://www.socialscienceregistry.org/trials/13689. This study only observes the long run outcomes of the program, the survey of which was conducted in June 2024. Since our working paper builds on this round of survey, we have referred to the registry that was done before this round of survey. We have not registered the baseline and the first round of the endline survey before.

3.3 Attrition

We examine differential attrition across three sub-samples: (i) households (Online Appendix Table A.1), (ii) treated respondents (Online Appendix Table A.2), and (iii) children (Online Appendix Table A.3). Each table reports estimates from the following specifications. Column 1 reports whether treatment assignment alone predicts attrition. Column 2 examines whether the relationship holds when conditioned on household and individual characteristics, while Column 3 tests whether these characteristics interact with treatment status to differentially predict attrition. We test whether attrition is systematically related to treatment status or other baseline characteristics.[8] We define attrition as a binary indicator equal to 1 if the household, respondent, or eligible child under the age of five was not successfully re-interviewed in 2024.

Our findings indicate that attrition is unlikely to bias our estimates and is likely random. Children under the age of five experienced the highest attrition, with approximately 48% of the child sample absent during the survey period (Online Appendix Table A.3).[9] Household- and respondent- level attrition was substantially lower and comparable, at around 13% in each group. Across all subgroups and specifications, there is no evidence that treatment assignment predicts attrition. Baseline characteristics, including household income, savings, household size, number of rooms, use of sanitary latrines, outstanding loan status, and whether the household had borrowed rice, do not differentially predict attrition between treatment and control groups.

[8]Rahman, Bhattacharjee and Das (2021) report no evidence of differential attrition in 2019 and find that, aside from homestead land ownership, no household or individual characteristics significantly predicted attrition.

[9]The child sample includes not only respondents' own children but also children of other family members within surveyed households. Attrition is primarily attributable to children being at school during survey visits (around 59%), with the remainder absent because they were playing outside or visiting relatives and thus unavailable at the time of interview.

4. Method and outcomes

4.1 Empirical strategy

Our primary results report intent-to-treat (ITT) effects of the intervention at both household and individual levels using the specification below:

$$Y_{ibd} = \beta_0 + \alpha_d + \beta_1 T_{bd} + X'_{ibd}\gamma + \epsilon_{ibd} \quad (1)$$

where Y_{ibd} denotes the outcome of interest for individual or household i in branch b in district d, and T_{bd} is an indicator equal to 1 if the household was assigned to receive the intervention and 0 otherwise. The coefficient β_1 measures the ITT effect of the intervention. We control for time-invariant differences across districts using district fixed effects (denoted by α_d), and for baseline respondent and household characteristics (denoted by the vector X'_{ibd}). Standard errors (ϵ_{ibd}) are clustered at the branch level.

4.1.1 Short- and long-run impacts on household economic outcomes

To assess short-and-long term effects of the intervention, we estimate a difference-in- differences (DiD) model on selected household economic outcomes. We focus on key economic outcomes—annual consumption expenditure, monthly savings, productive and non- productive assets, and per capita monthly income—to provide a clearer assessment of how treatment effects evolve over time at the household level. We then incorporate these estimated impacts into the cost-benefit analysis.

The DiD estimates are obtained from the following specification:

$$Y_{ibd} = \beta_0 + \alpha_d + \beta_1 (T_{bd} \times \text{Year1}_t) + \beta_2 (T_{bd} \times \text{Year2}_t) + \beta_3 Year1_t + \beta_4 Year2_t + X'_{ibd}\gamma + \epsilon_{ibdt} \quad (2)$$

Where Y_{ibd} denotes the outcome of interest for household i in branch in district d, α_d captures district-level fixed effects. The indicator T_{bd} is a binary indicator which equals to 1 if the assigned household was in a treatment district or otherwise. $Year1_t$ and $Year2_t$ are time dummies, indicating the first and second follow-up surveys, respectively. The interaction terms $T_{bd}Year1_t$ and $T_{bd}Year2_t$ identify the short-run (β_1) and long-run (β_2) treatment effects, respectively. We also control other baseline characteristics of the respondent and household (denoted by vector X'_{ibd}).[10]

4.2 Outcomes of interest

We measure intervention impacts at both household and individual levels across a broad set of economic and health outcomes. Household outcomes include measures of income, consumption, assets, savings, food security, loan behaviour, and medical expenditure. Individual- level outcomes include measures of income, health status, medical expenditure, fertility, and maternal health. We also examine the programme's impact on child outcomes, including enrollment status (for children ages five and above) and the prevalence of healthy weight among children under the age of five, based on age-adjusted body mass index (BMI).

5. Results

5.1 Household-level impacts

5.1.1 Income-generating activities and earnings

Table 1 presents the ITT impacts on household labour market engagement and earnings at the second follow-up. Column (1) shows that treated households engage in a significantly greater number of income-generating activities than control households, which is 4% higher than the control mean of 7.05. Treated households are 4 percentage points (pp) more likely to engage in livestock rearing than control households (Column (2)), representing a 5% increase relative to the control mean of 0.79. However, the observed increase in livestock rearing does not translate into measurable gains in household income, as the intervention yields no statistically significant effect on monthly per capita earnings (Column (3)).

Although the intervention initially increased earnings by 15% relative to the control group (Rahman, Bhattacharjee and Das, 2021), this effect was not sustained; the estimated long-run impact of a BDT 110.1 earnings gain is statistically insignificant. These findings imply that even though the intervention may have influenced the types of income-generating activities households engage in, the associated income gains do not sustain over time.

[10]Since our analysis relies on three survey rounds, we do not condition on baseline values of the outcome variables.

Table 1. **Treatment effects on household labor market engagement and earnings.**

	(1)	(2)	(3)
	No. of IGAs engaged in (Avg.)	**Livestock rearing (Yes = 1)**	**Monthly income (Per Capita)**
Treatment	0.28*	0.04**	110.10
	(0.158)	(0.020)	(97.967)
Observations	7867	7867	7867
$_R2$	0.072	0.043	0.089
Control mean	7.05	0.79	3429.90

Notes: The table presents treatment effects on the following set of outcomes. The first outcome captures the total number of income-generating activities undertaken by the household in the last 12 months. Livestock rearing is defined as a binary indicator equal to 1 if the household reported earning income from rearing any type of livestock—including cattle, goats, and poultry—within the past year. Household per capita monthly income is adjusted for inflation to reflect 2024 prices. All regressions include district fixed effects and control for baseline characteristics, including respondent age, and binary indicators for livestock ownership, land ownership, and outstanding loan status. Standard errors are reported in parentheses and are clustered at branch level.
*$p < 0.10$,
**$p < 0.05$,
***$p < 0.01$.

Table 2. **Treatment effects on household assets and savings.**

	(1)	(2)	(3)	(4)	(5)
	Livestock value (BDT)	**Land (Decimal)**	**Other productive assets (BDT)**	**Non-productive assets (BDT)**	**Savings (BDT)**
Treatment	9899***	1.45*	2495***	-2516.30**	1641.50**
	(1933.152)	(0.712)	(723.954)	(820.054)	(551.650)
Observations	7867	7867	7867	7867	7867
$_R2$	0.077	0.075	0.043	0.035	0.026
Control mean	24538.30	8.57	12818	10800.20	6948.50

Notes: The table presents treatment effects on the following outcomes. The first outcome measures the total value of livestock owned by the household, including cattle or buffalo, goats or sheep, and poultry (chickens or ducks). The second captures total landholdings—measured in decimals—and aggregates all types of land ownership, including homestead, cultivable plots, ponds, as well as land rented or mortgaged out. Other productive assets refer to income-generating equipment such as ploughs, tractors, sprayers, engine-powered vehicles, rickshaws or vans, boats, fishing nets, and trees, provided each item is valued at BDT 500 or above. Non-productive assets include household durables and consumer items such as radios, televisions, fans, refrigerators, sewing machines, bicycles, motorcycles, basic furniture (e.g., chairs, tables, bed frames), mosquito nets, and clothing. The final outcome measures total household savings, inclusive of deposits held with formal institutions (e.g., BRAC, banks, NGOs) and informal channels (e.g., cash stored at home or with family members). All monetary values for livestock and savings are expressed in 2024 BDT, adjusted for inflation. All regressions include district fixed effects and control for baseline characteristics, including respondent age, and binary indicators for livestock ownership, land ownership, and outstanding loan status. Standard errors are reported in parentheses and are clustered at the branch level.
*$p < 0.10$,
**$p < 0.05$,
***$p < 0.01$.

5.1.2 Assets and savings

We estimate the intervention's effects on household asset accumulation, focusing on live- stock, land, other productive assets, and non-productive assets (Table 2). Column (1) shows that beneficiary households hold livestock assets valued at BDT 9,899 higher than control households, corresponding to a 40% increase over the control mean. Although the effect size has attenuated from the 142% gain observed during the first follow-up (Rahman, Bhat- tacharjee and Das, 2021), the presence of an impact seven years post-intervention suggests that households were able to maintain, if not expand, their livestock holdings. The intervention also generates significant and persistent gains in household savings, land ownership, and other productive assets.

As shown in Column (2), treated households own an additional 1.45 decimals of land on average, which represents a 17% increase relative to the control mean. Similarly, they hold other productive assets valued at BDT 2,495 more than control

Table 3. Treatment effects on land amount and ownership.

	(1)	(2)	(3)	(4)
	Homestead land		Cultivable land	
	Owns (Yes = 1)	Amount (decimal)	Owns (Yes = 1)	Amount (decimal)
Treatment	0.03* (0.020)	0.002 (0.306)	0.04*** (0.011)	1.11*** (0.390)
Observations	7867	7867	7867	7867
$_R2$	0.080	0.081	0.054	0.056
Control mean	0.61	5.05	0.07	1.77

Notes: The table presents treatment effects on the following outcomes. Land ownership for both homestead and cultivable plots is recorded as a binary indicator equal to 1 if the household owns it, and 0 otherwise. The corresponding land area is measured in decimals and reported separately for homestead and cultivable holdings. All regressions include district fixed effects and control for baseline characteristics, including respondent age, and binary indicators for livestock ownership, land ownership, and outstanding loan status. Standard errors are reported in parentheses and are clustered at branch level.
*$p < 0.10$,
**$p < 0.05$,
***$p < 0.01$.

households, reflecting a 20% increase over the control mean (Column (3)). Treated households also report holding BDT 1,641 more in accumulated savings, a 24% increase over the control mean (Column (5)). Although all estimates remain statistically significant, effect sizes are notably smaller than those recorded in the first follow-up. Treated households hold BDT 2,516 less in non- productive assets— 23% less relative to the control mean (Table 2, Column (4)). This reallocation suggests that, in the long run, the intervention may have influenced households' investment behaviour, shifting resources away from consumption-oriented goods and toward productive asset accumulation.

Table 3 shows the intervention's impact on household land ownership, disaggregated by homestead and cultivable land. Treated households are 3 pp more likely to own homestead land and 4 pp more likely to own cultivable land than control households (Columns (1) and (3), respectively). However, the observed gain in total landholdings (as shown in Column (2) of Table 2) appears to be driven primarily by an increase in cultivable land rather than homestead land. Treated households hold an additional 1.11 decimals of cultivable land—equivalent to a 62.7% increase over the control mean (Column (4)). These results suggest that the intervention enabled treated households to expand land ownership along the extensive margin, as evidenced by the increased likelihood of owning both homestead and cultivable land. In contrast, gains along the intensive margin, reflected in the amount of land owned, were concentrated on cultivable holdings.

5.1.3 Expenditure and food security

Table 4 reports intervention impacts on household food consumption, dietary diversity (measured by the food consumption score (FCS)), food security (self-reported), and medical expenditures. Column (1) shows that treated households spend significantly more on food, with daily per capita expenditure being higher by BDT 7.23, which is equivalent to a 7.9% increase over the control mean. For a household of four members, this would correspond to an additional BDT 923 in monthly food expenditure. Increased spending is accompanied by gains in dietary quality, as treated households score three points higher on the FCS (Column (2)) relative to control households. Treated households also incur significantly greater medical expenditures, with annual per capita expenses increasing by BDT 610.6, a 15.9% higher than control mean (Column (5)). Despite improvements in food expenditure and dietary diversity, there is no significant impact on their food security.[11] Although the evidence points to sustained improvements in both the quantity and nutritional quality of food consumption, additional analysis is needed to better understand how these gains translate into broader food security outcomes.

5.1.4 Loans and credit market engagement

Table 5 presents the impacts on household borrowing behaviour. Treated households are more likely to have borrowed from both BRAC (5 pp) and other NGOs (5 pp) in the last 18 months. The relative effect is greater for credit uptake from

[11]We construct a binary indicator of household food security based on responses to five questions. House- holds receive one point if each of the following conditions are satisfied: (i) the ability to afford at least two meals per day most of the time in the past year; (ii) the ability to afford nutritious food at least twice over the past year; (iii) reporting no income or food deficit (or reporting a surplus); (iv) not experiencing any episode in the past month when household members could not eat; and (v) not having to rely only on rice for meals in the past month. Households scoring five out of five are coded as food secure (1) and others as food insecure (0).

Table 4. **Treatment effects on consumption, health expenditure, and food security.**

	(1)	(2)	(3)	(4)	(5)
	Food expenditure (BDT)	**Food sonsumption score**	**Share of HHs with FCS (≥51)**	**Food secure (Yes = 1)**	**Medical expenses (BDT)**
Treatment	7.23***	2.99***	0.06**	0.01	610.6***
	(2.148)	(1.025)	(0.027)	(0.013)	(226.391)
Observations	5399	5408	5408	5408	7867
$_R2$	0.039	0.071	0.056	0.028	0.026
Control mean	91.63	48.30	0.39	0.88	3844.2

Notes: The table presents treatment effects on the following outcomes. The outcomes are in chronological order. In the second follow-up, data on food expenditure and food security were collected only for households included in the first follow-up survey. Per capita daily food expenditure is reported for the adult sample (individuals aged 18 and above), expressed in 2024 constant prices. The FCS is derived from a seven-day household dietary recall and ranges from 0 to 112, with scores above 51 indicating high dietary diversity and frequency. [a]Annual medical expenditure is calculated as total household health spending divided by the number of current household members. All regressions include district fixed effects and control for baseline characteristics, including respondent age, and binary indicators for livestock ownership, land ownership, and outstanding loan status. Standard errors are reported in parentheses and are clustered at the branch level.
*$p < 0.10$,
**$p < 0.05$,
***$p < 0.01$
[a]Details on how the FCS index is constructed can be found here: https://resources.vam. wfp.org/data-analysis/quantitative/food-security/food-consumption-score.

Table 5. **Treatment effects on household borrowing and credit behavior.**

	(1)	(2)	(3)
	Outstanding loan (Yes = 1)	**BRAC loan (Yes = 1)**	**Non-BRAC loan (Yes = 1)**
Treatment	0.05***	0.05*	0.05*
	(0.015)	(0.027)	(0.027)
Observations	7867	7867	7867
$_R2$	0.052	0.026	0.047
Control mean	0.72	0.22	0.52

Notes: The table presents treatment effects on the following outcomes. The first outcome is a binary indicator equal to 1 if any household member currently holds an outstanding loan—whether in cash or kind—and 0 otherwise. Two additional binary capture credit uptake, one of which equals 1 if the household has taken a loan from BRAC within the past 18 months, and the other equals 1 if the household has borrowed from any non-BRAC source over the same period. All regressions include district fixed effects and control for baseline characteristics, including respondent age, and binary indicators for livestock ownership, land ownership, and outstanding loan status. Standard errors are reported in parentheses and are clustered at the branch level.
*$p < 0.10$,
**$p < 0.05$,
***$p < 0.01$.

BRAC (23% more than the control mean) than from other NGOs (10% more than the control mean) (Columns (2) and (3), respectively). They are also 5 pp more likely to hold an outstanding loan, corresponding to a 7.4% increase relative to the control mean (Column 1). In the first follow-up, there was no evidence of an intervention impact on credit uptake from other NGOs (Rahman, Bhat-tacharjee and Das, 2021). By the second follow-up, however, treated households exhibited a significant increase in borrowing from non-BRAC institutions, suggesting that diversification or expansion into other formal credit sources may emerge only over a longer time horizon.

5.2 Individual level impacts

5.2.1 Labour market and empowerment

Table 6 presents intervention impacts on beneficiaries' economic engagement and empowerment outcomes. Although the point estimates indicate a marginal increase in their participation in income-generating activities (Column 1), average daily work hours (Column 2), and earnings for beneficiaries (Column 5), these effects on women's labor supply and

Table 6. Treatment effects on individual labor market and empowerment outcomes.

	(1)	(2)	(3)	(4)	(5)	(6)
	Engaged in at least one IGA (Yes = 1)	**Avg. daily work hours**	**No. of IGAs engaged in**	**Livestock rearing (Yes = 1)**	**Monthly income (BDT)**	**Share of *empowered* women**
Treatment	0.01	0.02	-0.13	0.04**	112.30	-0.03
	(0.019)	(0.073)	(0.192)	(0.020)	(116.076)	(0.022)
Observations	7770	7770	7770	7770	7770	7770
$_R2$	0.075	0.107	0.054	0.047	0.091	0.098
Control mean	0.18	2.84	5.60	0.78	1696.10	0.47

Notes: The table presents treatment effects on the following outcomes. Engagement in income- generating activities is measured using a binary indicator equal to 1 if the respondent reports participation in at least one income-generating activity, and 0 otherwise. Daily hours spent captures the average number of hours per day allocated to such activities. Income diversification is represented by the number of distinct income-generating activities reported by the respondent. We define empowerment by constructing a binary index equal to 1 if the female respondent re- ports not requiring spousal permission across all seven domains: starting a new income-generating activity, visiting friends' homes alone, purchasing clothing for herself or her children, selling live- stock or poultry, selling small goods, and managing loans independently. The indicator takes the value 0 otherwise. All regressions include district fixed effects and control for baseline character- istics, including respondent age, and binary indicators for livestock ownership, land ownership, outstanding loan status. Standard errors are reported in parentheses and are clustered at the branch level.
*$p < 0.10$,
**$p < 0.05$,
***$p < 0.01$.

economic outcomes are statistically insignificant. Similar to our household-level results, treated beneficiaries are 4 pp more likely to be involved in livestock rearing (corresponding to a 5.3% increase relative to the control group) (Column 4).

Fewer treatment beneficiaries than control beneficiaries are classified as empowered under our index, although the difference is not statistically significant (Column 6). The index defines autonomy as the ability to make major decisions independently of one's spouse, which may not fully capture more nuanced forms of agency in contexts where decisions are made jointly or primarily by the husband. The intervention does not significantly enhance women's empowerment, consistent with its limited impact on income. Over time, treatment effects on women appear to dissipate, highlighting the challenge of translating household-level benefits into individual empowerment (Roy et al., 2019).

5.2.2 Morbidity and health-care

Table 7 presents intervention impacts on self-reported morbidity and health-seeking behaviour among beneficiaries themselves (Panel A) and their family members (Panel B).

Column (1) of Panel A shows that beneficiaries are 6 pp more likely to report illness within the past 15 days, (14.2% higher than control mean). This rise is largely driven by a 7 pp increase (33% higher than control mean) in reported cases of non-infectious diseases among beneficiaries. Consistent with this, there is an increase in demand for formal healthcare services. Specifically, beneficiaries are 4 pp more likely to consult any health-care professional (a 35% increase relative to the control mean) and 3 pp more likely to seek care from a licensed physician (a 43% increase relative to the control mean). These patterns are reflected in significantly higher out-of-pocket medical expenditures, which are 23% greater than the control mean. We find no statistically significant effect on illness reporting among other family members in treatment households, although their health-care spending is 21% higher than that of control household members.

We further analyze health-seeking behaviour among individuals who reported experiencing an illness within the last 15 days (Table 8). Column (1) of Panel A shows that beneficiaries are 8 pp less likely to report infectious conditions—16% lower than the control mean—and equally more likely to report non-infectious conditions. The intervention also increases the likelihood of care-seeking if they're ill: women are also 6 pp more likely to consult any healthcare provider (16.7% higher than control mean) and 5 pp more likely to consult a licensed physician (25% higher than control mean). Among pregnant beneficiaries, the probability of receiving at least four antenatal care visits rises by 6 pp, which is an 86% increase over the control mean (Column (6)). On being ill, treatment beneficiaries do not report significantly different health expenditures than control respondents. However, family members from beneficiary households spend an additional BDT 109.3 (15.7% more than the control mean) on health care compared to members in control households

Table 7. **Treatment effects on individual morbidity and health-care outcomes.**

	Reported illness in the last 15 days			*Sought treatment from*		*Costs*
	(1)	(2)	(3)	(4)	(5)	(6)
	Any Illness (Yes = 1)	**Infectious Disease (Yes = 1)**	**Non-infectious Disease (Yes = 1)**	**Health Care Professional (Yes = 1)**	**MBBS Doctor (Yes = 1)**	**Medical Expenses (BDT)**
Panel A: Treatment (All Respondents)	0.06*** (0.016)	-0.01 (0.012)	0.07*** (0.010)	0.04*** (0.010)	0.03*** (0.007)	78.70** (30.677)
Observations	7770	7770	7770	7770	7770	7770
R^2	0.058	0.022	0.043	0.020	0.026	0.034
Control mean	0.39	0.20	0.20	0.13	0.08	335.30
Panel B: Treatment (Other Members)	0.01 (0.009)	0.01 (0.008)	0.003 (0.004)	0.01 (0.007)	0.01 (0.005)	38.19** (14.852)
Observations	26145	26145	26145	26145	26145	26145
R^2	0.018	0.015	0.008	0.012	0.009	0.013
Control mean	0.26	0.18	0.09	0.10	0.05	182.70

Notes: The table presents treatment effects on the following outcomes. With the exception of medical expenditures, which are reported in Bangladeshi Taka (BDT), all outcomes are binary indicators. These include whether the respondent reported any illness in the 15 days before the survey: a transmissible or infectious disease (e.g., fever, cough/cold, diarrhea, chickenpox, jaundice, dysentery, pneumonia, skin conditions, measles, malaria, tuberculosis, or typhoid), or a non-transmissible disease (e.g., heart disease, respiratory issues, diabetes, kidney problems, or reproductive health conditions). Other outcomes include whether the respondent sought care from any healthcare provider, and, specifically, from a licensed physician (with MBBS certification). Medical expenditures reflect total out-of-pocket costs incurred over the past 15 days, encompassing consultation fees, medicines, diagnostic tests, and transportation. All regressions include district fixed effects and control for baseline characteristics, including respondent age, and binary indicators for livestock ownership, land ownership, and outstanding loan status. Standard errors are reported in parentheses and are clustered at the branch level.
*$p < 0.10$,
**$p < 0.05$,
***$p < 0.01$.

VeriXiv 2025, 2:362 Last updated: 04 NOV 2025

Table 8. **Treatment effects on morbidity and health-care outcomes** ***(Among individuals who reported illness in the last 15 days).***

	Suffering from		*Sought treatment from*		*Costs*	*Maternal care*
	(1)	(2)	(3)	(4)	(5)	(6)
	Infectious disease (Yes = 1)	**Non-Infectious disease (Yes = 1)**	**Health care professional (Yes = 1)**	**MBBS Doctor (Yes = 1)**	**Medical expenses (BDT)**	**No. of ANC Visits (≥4) (Yes = 1)**
Panel A: Treatment (Respondents)	-0.08***	0.08***	0.06**	0.05**	73.29	0.06**
	(0.020)	(0.020)	(0.026)	(0.021)	(77.343)	(0.026)
Observations	3241	3241	3241	3241	3241	460
R^2	0.031	0.031	0.026	0.046 0.027	0.044	
Control Mean	0.51	0.51	0.33	0.20	868.10	0.07
Panel B: Treatment (Other Members)	0.003	-0.01	0.02	0.02	109.30*	
	(0.015)	(0.014)	(0.024)	(0.020)	(57.651)	
Observations	7081	7081	7081	7081	7081	
$_R2$	0.025	0.024	0.025	0.031	0.032	
Control Mean	0.67	0.33	0.37	0.19	692.60	

Notes: The table presents treatment effects for a sub-sample of respondents who reported being ill in the last 15 days. With the exception of medical expenditures—which are reported in (BDT)—all outcomes are binary indicators. These include whether the respondent suffered from a transmissible or infectious disease (e.g., fever, cough/cold, diarrhea, chickenpox, jaundice, dysentery, pneumonia, skin conditions, measles, malaria, tuberculosis, or typhoid); or a non-transmissible/infectious disease (e.g., heart disease, respiratory issues, diabetes, kidney problems, or reproductive health conditions) in the last 15 days. Other outcomes include whether the respondent sought care from any healthcare provider, and specifically from a licensed physician (with MBBS certification). Medical expenditures reflect total out-of-pocket costs incurred over the past 15 days, encompassing consultation fees, medicines, diagnostic tests, and transportation. A value of 1 would mean that the individual had received antenatal care checkups more than four times during the course of pregnancy and 0 otherwise. All regressions include district fixed effects and control for baseline characteristics, including respondent age, and binary indicators for livestock ownership, land ownership, and outstanding loan status. Standard errors are reported in parentheses and are clustered at the branch level.
*$p < 0.10$,
**$p < 0.05$,
***$p < 0.01$.

(Panel B, Column 5). Although findings suggest improved awareness of and access to care among treated respondents, they also point to a greater burden of chronic illness among this group.

5.2.3 Reproductive and maternal health outcomes

We examine the intervention's effects on women's reproductive and maternal health out- comes, including contraceptive use, menstrual hygiene, and antenatal care. Online Appendix Table A.4 presents findings on contraceptive use. We find no significant impact of the intervention on contraceptive use among beneficiaries. Online Appendix Table A.5 reports results on menstrual hygiene and health-seeking behaviour. Although women in treated households are more likely to report using sanitary pads and less likely to self-medicate without professional consultation, these estimates are not statistically significant. Maternity-related outcomes show some effects: pregnant women in beneficiary households are 19.3% more likely to receive antenatal care relative to the control group (Online Appendix Table A.6). Overall, the intervention's influence on reproductive and maternal health outcomes appears limited, with some improvement in prenatal service utilization.

5.3 Treatment effects on children's education and health

5.3.1 Education enrollment

The intervention significantly increases school enrollment among adolescents (Panel A, Column 3) in treated households by 8 pp, or 18.8% over the control group mean. (Online Appendix Table A.7). Among male adolescents (Panel B,

Column 3), the intervention raises enrollment by 7 pp—corresponding to a 16.8% increase over the control mean. The effect is even more pronounced among female adolescents (Panel C, Column 3), who experience a 9 pp increase, corresponding to a 21% gain relative to the control mean. These results suggest that the intervention may have a positive impact on adolescent school enrollment, where the risk of dropout is often higher. This finding is notable, as prior evaluations of the Ultra-Poor Graduation (UPG) programme documented no significant impacts on child enrollment. The observed educational gains thus warrant further investigation to understand the mechanisms driving them.

5.3.2 Children anthropometry

We also assess the programme's impact on a specific anthropometric outcome: the prevalence of healthy weight among children under the age of five, measured using age-adjusted BMI thresholds (Online Appendix Table A.8). Overall, the programme does not significantly affect child nutritional status. However, gender-disaggregated analysis reveals heterogeneous impacts: male children in treated households are 7 pp more likely to meet healthy weight standards relative to their counterparts in control households, corresponding to a 17.8% increase over the control mean. No effect is observed for female children. These results indicate that the programme's nutritional impacts, if any, may not be evenly distributed across gender.

6. Short- and long-run impacts on household economic outcomes

In the first follow-up (2019), treated households in the study experienced significant gains relative to the control group in monthly income (BDT 506.02), savings (BDT 3,787.80), productive assets (BDT 30,232.04), non-productive assets (BDT 2,814.85), and annual consumption (BDT 13,336.36) (Table 9). By the second follow-up (2024), significant impacts remained only for savings (BDT 2,762.44) and productive assets (BDT 7,459.72), with effect sizes substantially smaller than in the short run. By the second follow-up (2024), most of these gains had attenuated. Intervention impacts could only be observed on household savings (BDT 2,762.44) and productive assets (BDT 7,459.72) in the long run, with

Table 9. Short- and long-term impacts on economic outcomes

	(1) Per Capita Monthly Income (BDT)	(2) Household Savings (BDT)	(3) Productive Assets (BDT)	(4) Non-Productive Assets (BDT)	(5) Household Annual Consumption (BDT)
Impact at first follow-up	506.02***	3787.80**	30232.04***	2814.85**	13336.36*
	(176.830)	(1447.435)	(3223.486)	(1288.917)	(7454.918)
Impact at second follow-up	68.89	2762.44**	7459.72**	1129.11	6651.55
	(135.310)	(1072.936)	(2955.771)	(1665.150)	(6003.750)
Treatment	9.60	-331.24	881.03	1624.38*	8069.59**
	(89.377)	(609.547)	(1320.579)	(955.390)	(3156.514)
First follow-up	208.17	3591.67***	21411.94***	3172.67***	45188.75***
	(157.431)	(1125.707)	(2313.252)	(989.824)	(6317.965)
Second follow-up	26.48	5933.63***	21206.87***	8614.30***	75296.72***
	(111.789)	(720.512)	(2413.731)	(1383.110)	(4920.733)
N	16227	16227	16227	16227	16227
Adjusted R^2	0.069	0.024	0.211	0.065	0.151
F-statistic	5.62	0.55	43.60	1.39	0.74
P-value	0.02	0.46	0.000	0.24	0.39

Notes: This table reports results from a DiD specification estimating the causal impact of the inter- vention across two post-treatment periods. All monetary values are expressed in BDT. All regressions include district fixed effects and control for baseline characteristics, including respondent age, and bin- ary indicators for livestock ownership, land ownership, outstanding loan status, and food expenditure. Each column reports the p-value for the null hypothesis of equal impacts across the two follow ups. Standard errors are reported in parentheses and are clustered at the branch level.
*$p < 0.10$,
**$p < 0.05$,
***$p < 0.01$.

both effect sizes substantially smaller than in the short run. Pre-intervention differences in non-productive assets and annual consumption indicate a minor baseline imbalance, which we address by including baseline covariates in all specifications. Overall, the results point to large, short-run improvements in income, assets, and consumption, followed by substantial attenuation over time. Overall, the results point to sizable, short-run improvements in income, assets, and consumption, followed by substantial attenuation over time. The persistence of gains in savings and productive assets suggests lasting improvements in household resilience, though the fading of income and consumption effects indicates that these gains may not fully translate into sustained welfare improvements.

7. Cost-benefit analysis

Table 10 reports the cost–benefit analysis (CBA) of the intervention, bench-marked against intervention costs and expressed in 2024 prices. Following the approach of Banerjee et al. (2015*a*), Bandiera et al. (2017), and Rahman, Bhattacharjee and Das (2021), we use changes in household consumption expenditure as the primary measure of intervention benefits, under the assumption that gains from financial and productive assets are fully reflected in

Table 10. Cost-benefit analysis.

Panel A: External parameters	
Cost (BDT) per household at year 2016	15,925
Cost (BDT) per household discounted at year 2024 at 6.60% annual inflation rate	26,530
Social discount rate = 5%	
Panel B: Estimated consumption benefits	
1 Change in household consumption expenditure at year 2017	7,735
2 Change in household consumption expenditure at year 2018 [a]	13,336
3 Change in household consumption expenditure at year 2019	12,222
4 Change in household consumption expenditure at year 2020	11,108
5 Change in household consumption expenditure at year 2021	9,994
6 Change in household consumption expenditure at year 2022	8,880
7 Change in household consumption expenditure at year 2023	7,766
8 Change in household consumption expenditure at year 2024 [b]	6,652
9 NPV change in household consumption expenditure from year 2017 for 20 years	58,954
10 Change in household assets at year 2018 [c]	2,815
11 Change in household assets at year 2024 [d]	1,129
12 Total benefits (1 + 2 + 3 + 4 + 5 + 6 + 7 + 8 + 9 + 10 +11)	1,40,591
13 Benefit-cost ratio (BCR, assuming benefits last 20 years from transfer date)	5.30
Panel C: Sensitivity to different discount rate/time horizons	
Benefit-cost ratio (BCR, assuming benefits for the last 30 years from transfer date)	6.38
Social discount rate = 10%	
Benefit-cost ratio (assuming benefits last 5 years from transfer date)	2.20
Benefit-cost ratio (assuming benefits last 15 years from transfer date)	4.30
Benefit-cost ratio (assuming benefits last 25 years from transfer date)	5.09
Social discount rate = 12%	
Benefit-cost ratio (assuming benefits last 30 years from transfer date)	4.75
Benefit-cost ratio (assuming benefits last 25 years from transfer date)	4.86
Benefit-cost ratio (assuming benefits last 20 years from transfer date)	4.63
Benefit-cost ratio (assuming benefits last 15 years from transfer date)	4.22

Note: NPV stands for Net Present Value.
[a]We have used the 2018 impact of household annual consumption value from Table 9.
[b]We have used the 2024 impact of household annual consumption value from Table 9.
[c]We have used the 2018 impact of household non-productive assets value from Table 9.
[d]We have used the 2024 impact of household non-productive assets value from Table 9.

consumption flows.[12] In addition, we separately include the value of non-productive assets in 2019 and 2024 as a one-time stock benefit which are added to consumption-based benefits under the assumption that they are not already captured in consumption.

Panel A presents intervention costs. The intervention incurred an average cost of BDT 15,925 per household in 2016, which, adjusted for an annual inflation rate of 6.6%, equals BDT 26,530 in 2024 prices. Consistent with standard practice (Bandiera et al., 2017), we initially use an annual social discount rate of 5%, and subsequently conduct sensitivity analyses using higher rates. Panel B details benefit estimation. Observed impacts on consumption from 2019 and 2024 follow-ups are applied to corresponding years. For 2017, we assume an effect size equal to 58% of the 2019 impact, following Bandiera et al. (2017). For 2019–2023, we linearly interpolate between 2019 and 2024 impacts, noting this as a simplifying assumption that may not capture potential non-linear decay or persistence. The ninth row reports the present net value (NPV) of consumption benefits, estimated under the assumption that the 2024 impact holds for an additional 12 years (i.e. a total of 20 years from the transfer date). Non-productive asset gains (BDT 2,815 in 2019 and BDT 1,129 in 2024) are then added to total benefits. Under these assumptions, the intervention's total estimated benefits equal BDT 140,591, yielding a benefit–cost ratio (BCR) of 5.30 for a 20- year horizon. If benefits persist for 30 years,[13] the BCR rises to 6.38. Sensitivity tests (Panel C), which vary the horizon and discount rate, confirm that BCR estimates are sensitive to both assumptions: with a 10% rate, the BCR ranges from 2.20 (5 years) to 5.09 (25 years); with a 12% rate, it ranges from 4.22 (15 years) to 4.75 (30 years). Across all scenarios, BCR estimates remain above one, indicating at the intervention's potential as a sustainable and scalable poverty alleviation strategy.

Comparing models, the hybrid UPG component yields a BCR of 8.47 at the first follow-up (Rahman, Bhattacharjee and Das, 2021), declining to 5.30 after six years, while the grant- only UPG programme achieves a BCR of 3.21 four years post-intervention (Bandiera et al., 2017). The hybrid model appears more efficient in terms of average returns per unit of expenditure, partly because a share of programme costs is recovered, contributing to its higher BCR. However, the two interventions serve distinct segments of the ultra-poor population: the hybrid model serves households just above the extreme poor threshold, while the grant- based model targets the most deprived and asset-poor households.[14] Hence, considering average efficiency metrics in isolation risks overlooking important equity considerations.

From a welfare-maximizing perspective, maximizing average returns per dollar is not necessarily the optimal policy goal. As the marginal utility of income is likely higher for the poorest, a dollar transferred to them may yield larger welfare gains than a dollar spent on households relatively better off. Moreover, evidence from Bandiera et al. (2017) shows that ultra-poor households receiving grants can not only close the gap with near-poor households in consumption and assets but surpass them in savings and productive asset accumulation. Thus, while the grant-based model appears less cost-effective in average BCR terms, it may deliver transformative welfare gains for the poorest that are not fully reflected in efficiency metrics. Conversely, the hybrid model's ability to deliver sustained and cost-effective impacts suggests that it can be a promising and more financially efficient alternative to standard microfinance programmes, which have struggled to generate household-level welfare improvements.

8. Conclusion

This paper evaluates the long-run impacts of BRAC's hybrid ultra-poor model (known as OTUP), which combines subsidized credit with elements of the graduation approach to target the "missing middle" of households excluded from both microfinance and grant-based interventions. Seven years after implementation, we observe sustained gains in asset accumulation, savings, landholdings, and food consumption, alongside improvements in health service utilization and adolescent school enrollment. In contrast, the initial income gains observed in the short run dissipate overtime, and the intervention generates only limited effects on women's empowerment and child nutrition.

Despite the attenuation of earnings, the intervention demonstrates strong cost-effectiveness, with BCRs that remain favourable relative to both traditional microfinance and grant-based graduation models. Hybrid interventions can generate long-run gains in household resilience and investment behaviour at lower per-unit costs, making them a promising strategy for improving livelihoods among the ultra-poor who are just above the extreme-poverty threshold. At the same time, efficiency metrics alone should not determine programme choice: grant-based models, though less cost-effective, may yield larger welfare gains for the very poorest by enabling transformative shifts in livelihoods.

Overall, the findings point to the importance of designing poverty-alleviation strategies as a portfolio that accounts for heterogeneity within the poor population and adapts interventions to their specific circumstances. Hybrid programmes offer a cost-effective means of supporting the near-ultra-poor, whereas grant-based graduation remains essential for reaching the most deprived. The evidence points to complementarities rather than substitution across programme types and implies that effective long-run poverty reduction requires balancing efficiency with equity.

Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work, the author(s) used ChatGPT 5, a generative AI tool, for grammatical refinement and rephrasing purposes for the manuscript only. After utilizing this tool, the author(s) thoroughly reviewed and edited the content to ensure accuracy, coherence, and alignment with authors' intentions. The author(s) take full responsibility for the final content of the published article.

Ethical approval

Ethical approval for the study was obtained from the Institutional Review Board of the BRAC James P. Grant School of Public Health (IRB-2024-ES-19).

Supplementary material

An Online Appendix for this preprint can be found at: https://github.com/marjiepan/onlineappendix/blob/main/onlineappendix.pdf.

Acknowledgments

The authors are grateful to Narayan C. Das, PhD, for his valuable comments and feedback on the manuscript.

References

Angelucci M, Karlan D, Zinman J: **Microcredit Impacts: Evidence from a Randomized Microcredit Program Placement Experiment by Compartamos Banco.** *Am. Econ. J. Appl. Econ.* 2015; **7**(1): 151–182.

Attanasio O, Augsburg B, De Haas R, *et al.*: **The Impacts of Microfinance: Evidence from Joint-Liability Lending in Mongolia.** *Am. Econ. J. Appl. Econ.* 2015; **7**(1): 90–122.

Augsburg B, De Haas R, Harmgart H, *et al.*: **The impacts of microcredit: Evidence from Bosnia and Herzegovina.** *American Economic Journal: Applied Economics.* 2015; **7**(1): 183–203.
Publisher Full Text

Bandiera O, Burgess R, Deserranno E, *et al.*: **Microfinance and Diversification.** *Economica.* 2022; **89**(S1): S239–S275. eprint.
Publisher Full Text

Bandiera O, Burgess R, Das N, *et al.*: **Labor markets and poverty in village economies.** *Q. J. Econ.* 2017; **132**(2): 811–870.
Publisher Full Text

Banerjee A, Karlan D, Zinman J: **Six Randomized Evaluations of Microcredit: Introduction and Further Steps.** *Am. Econ. J. Appl. Econ.* 2015; **7**(1): 1–21.

Banerjee A, Duflo E, Sharma G: **Long-Term Effects of the Targeting the Ultra Poor Program.** *American Economic Review: Insights.* 2021; **3**(4): 471–486.
Publisher Full Text

Banerjee A, Duflo E, Goldberg N, *et al.*: **A multifaceted program causes lasting progress for the very poor: Evidence from six countries.** *Science.* 2015a; **348**(6236): 1260799.
Publisher Full Text

Banerjee A, Duflo E, Glennerster R, *et al.*: **The Miracle of Microfinance? Evidence from a Randomized Evaluation.** *American Economic Journal: Applied Economics.* 2015b; **7**(1): 22–53.

Bedoya G, Coville A, Haushofer J, *et al.*: **No Household Left Behind: Afghanistan Targeting the Ultra Poor Impact Evaluation.** 2019.

Brune L, Karlan D, Kurdi S, *et al.*: **Social protection amidst social upheaval: Examining the impact of a multi-faceted program for ultra-poor households in Yemen.** *J. Dev. Econ.* 2022; **155**: 102780.
Publisher Full Text

Chowdhury R, Collins E, Ligon E, *et al.*: **Valuing Assets Provided to Low-Income Households in South Sudan.** *Working Paper.* 2017.

Cr´epon B, El Komi M, Osman A: **Is It Who You Are or What You Get? Comparing the Impacts of Loans and Grants for Microenterprise Development.** *Am. Econ. J. Appl. Econ.* 2024; **16**(1): 286–313.

Fiala N: **Returns to microcredit, cash grants and training for male and female microentrepreneurs in Uganda.** *World Dev.* 2018; **105**: 189–200.
Publisher Full Text

Field E, Pande R, Papp J, *et al.*: **Does the Classic Microfinance Model Discourage Entrepreneurship among the Poor? Experimental Evidence from India.** *Am. Econ. Rev.* 2013; **103**(6): 2196–2226.
Publisher Full Text

Heil A, Sulaiman M, Bandiera O, *et al.*: **Graduation.** *CEPR Discussion Papers.* 2024.

Karlan D, Zinman J: **Microcredit in Theory and Practice: Using Randomized Credit Scoring for Impact Evaluation.** *Science.* 2011; **332**(6035): 1278–1284. Publisher: American Association for the Advancement of Science.
Publisher Full Text

Maitra P, Mitra S, Mookherjee D, *et al.*: **Financing smallholder agriculture: An experiment with agent- intermediated microloans in India.** *J. Dev. Econ.* 2017; **127**: 306–337.
Publisher Full Text

Rahman A, Bhattacharjee A, Das N: **A good mix against ultra-poverty? Evidence from a randomized controlled trial (RCT) in Bangladesh.** *Rev. Dev. Econ.* 2021; **25**(4): 2052–2083.
Publisher Full Text

Roy S, Hidrobo M, Hoddinott J, *et al.*: **Trans- fers, behavior change communication, and intimate partner violence: Postprogram evidence from rural Bangladesh.** *Rev. Econ. Stat.* 2019; **101**(5): 865–877.
Publisher Full Text

Tarozzi A, Desai J, Johnson K: **The Impacts of Microcredit: Evidence from Ethiopia.** *Am. Econ. J. Appl. Econ.* 2015; **7**(1): 54–89.