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Initial Evidence on the Intergenerational Effects of the Ultra-Poor Graduation Program in Bangladesh

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ABSTRACT

Initial Evidence on the Intergenerational Effects of the Ultra-Poor Graduation Program in Bangladesh*

Previous studies based on a randomized controlled trial (RCT) of the Ultra-Poor Graduation (UPG) program in Bangladesh have shown that the model effectively reduces extreme poverty by providing resources and support for self-employment, enabling households to escape poverty traps and build long-term economic independence. In 2024, we conducted a follow-up survey to examine intergenerational impacts on the original program households and on children who were present at baseline in 2007, focusing on economic mobility, health, and education. While the treatment group received UPG support during 2007–2009, about 25% of control households enrolled from 2011 onward. Our findings show positive effects on economic empowerment (such as engagement in earning activities, income and savings) for the next generation, with stronger impacts for girls. These effects span girls of all ages. While impacts on years of schooling and age at marriage are limited or contrary to expectations, younger girls are significantly less likely to experience early childbearing. Improvements in access to healthcare appear to be an important mechanism alongside economic empowerment in driving this reduction. Overall, the results provide strong evidence of the persistence of program impacts across generations, particularly in improving the life trajectories of girls.

Keywords:

Poverty, Long-term, Asset transfer, Graduation

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1. Introduction

Persistent extreme poverty remains a central development challenge. Even as global poverty rates have declined, the very poorest – the *ultra-poor* – often fail to benefit from growth and conventional aid programs. In Bangladesh, roughly one in ten people live below the extreme poverty line (approximately 10.5% as of 2019) https://www.lse.ac.uk/Research/research-impact-case-studies/2021/improving-the-lives-of-the-ultra-poor, and vulnerable households face chronic food insecurity, irregular income, and lack of assets. Standard anti-poverty tools (e.g. cash transfers or microcredit) have had limited reach among the ultra-poor, who are typically landless, food-insecure rural women reliant on day labor or begging. Breaking such entrenched poverty often requires holistic "big-push" interventions that tackle multiple constraints simultaneously (Balboni et al., 2022).

The BRAC Ultra-Poor Graduation (UPG) program is a pioneering example of this approach. Launched in 2002, UPG (also known as the Targeting the Ultra-Poor, or TUP) aims to help the poorest households transition into sustainable livelihoods. Eligible women receive a comprehensive, time-limited package consisting of an asset transfer (typically livestock), skills training (e.g., animal husbandry), a year of consumption support, encouragement to save, access to basic health services, and intensive coaching by a caseworker. This bundled support is designed both to remove barriers such as lack of capital, skills, or health care, and to build self-reliance. By the end of program participation, households are expected to maintain multiple livelihoods (e.g., animal rearing, small trade), build savings buffers, and achieve greater food security, thereby escaping extreme poverty on a sustainable basis. Evidence from Bandiera et al. (2017) show that UPG households experienced lasting gains, with 11% higher expenditure and 57% more durable goods even four years after program completion. Using the same data, Balboni et al. (2022) demonstrate that about two-thirds of ultra-poor households were able to escape an asset poverty trap. These findings highlight the long-term impacts of the model's "big push." This study extends the literature by examining the intergenerational dimensions of these impacts.

Several other evaluations confirm the promise of Graduation programs across diverse contexts. Following the multi-country evidence of short-term impacts reported by Banerjee et al. (2015), positive results have also been documented in Burkina Faso (Karimli et al., 2020), Uganda (Sedlmayr et al., 2020), and Kenya (Zheng et al., 2023). Beam et al. (2025) show that varying the intensity of coaching within the model still produces positive impacts. Longer-run evidence is also encouraging: a decade-long follow-up in India found that treated ultra-poor households continued to have higher consumption and income than controls (Banerjee et al., 2021). Graduation programs implemented in fragile contexts —Afghanistan (Bedoya et al., 2019, 2023), Yemen (Brune et al., 2022), and South Sudan (Chowdhury et al., 2022)— offer further insights. Despite concerns that conflict and economic collapse might undermine program effectiveness, evidence points to resilience. In Afghanistan, for example, consumption and income increased by 30% and 22%, livestock values tripled, and labor participation rose by 64%, with effects persisting five years post-transfer despite drought and violence (Bedoya et al., 2019, 2023). In Yemen, significant improvements were observed in assets and savings, though not in consumption, while in South Sudan participants maintained higher wealth and income compared to both pure control and cash transfer groups. Similarly, Afghanistan's Women's Livelihood Program, modeled after UPG, delivered large gains in consumption and savings that endured through subsequent shocks (Bedoya et al., 2019, 2023). Taken together, this evidence suggests that a well-implemented integrated asset-plus-support model can generate durable economic improvements for the poorest, even in highly fragile environments.

This paper examines the intergenerational impacts of the Ultra-Poor Graduation (UPG) program. A central question for poverty dynamics is whether children who grew up in UPG households achieve better outcomes in adulthood. While there is some evidence that women's empowerment through graduation programs improves child outcomes, rigorous long-term evidence on how children fare once they form their own households remains scarce. To address this gap, we revisit the 2007 cohort of UPG respondents. We then trace the children of those households, who were between the ages 0 and 15 at baseline (now between the ages of around 16 and 33). We analyze outcomes in marriage, fertility, education, labor, and savings for these traced respondents to assess whether the next generation of UPG households experienced upward mobility relative to controls.

2. Program description and context

BRAC's Ultra-Poor Graduation (UPG) Program (originally known as Targeting the Ultra-Poor, TUP) was developed to reach the most economically disadvantaged households in rural Bangladesh – specifically ultra-poor women who are often left out of microcredit or other development programs. The program uses a participatory wealth ranking (PWR) at the village level to identify eligible households. In this process, community members classify all village households into different poverty categories based on local indicators of wealth (housing materials, land ownership, occupation, etc.). Through this community exercise and follow-up verification, BRAC identifies the ultra-poor – typically the bottom 5–10% of households – who meet additional inclusion criteria (such as owning minimal assets, having able-bodied household members, and not receiving assistance from other NGOs) and no exclusion criteria (such as recent

participation in microfinance). This rigorous targeting helps ensure that the program reaches those most in need (often landless households, female-headed households, or those reliant on begging or irregular labor).

Once selected, participants (almost exclusively women) receive a package of support over roughly two years. A cornerstone is the asset transfer, commonly livestock (a cow or goats) or goods to start a small trade (e.g., inventory for a shop). Alongside the asset, participants receive skills training specific to the asset or enterprise (for example, livestock rearing techniques, or business skills for running a shop). They also receive a stipend or subsistence allowance for the first few months (to reduce the need to sell the asset or go hungry while the asset starts generating income), and are encouraged to save regularly in a safe place (often with a BRAC-provided savings account). The program links participants to essential healthcare services – providing basic health check-ups and facilitating access to clinics – since poor health can derail progress. Finally, each participant is visited frequently by a program mentor (caseworker) who provides guidance, monitors progress, and offers moral support. This hands-on coaching is a distinctive element that helps beneficiaries troubleshoot challenges and build confidence over time.

The UPG program is time-bound (generally 24 months of intensive support), with the goal that by "graduation" (program end), households have a sustainable source of income, a minimum asset base, improved food security, and better integration into social and economic networks. Graduation criteria often include indicators like having multiple sources of income, a savings buffer, no severe malnutrition, and school-aged children attending school. By addressing multiple facets of poverty, the program aims not just to provide temporary relief but to fundamentally change the trajectory of ultra-poor households – putting them on a path out of extreme poverty that continues after direct support ends.

In the context of rural Bangladesh, where this study takes place, implementing the UPG program at scale required careful consideration of local economic conditions and potential spillovers. BRAC selected 13 of the poorest districts (as identified by food insecurity maps of the World Food Programme) for the initial expansion of the TUP program in 2007. These districts are characterized by high rates of landlessness, underemployment, and vulnerability to seasonal hunger (monga). Within each district, the program was rolled out in certain subdistricts (upazilas) and not others, which created the basis for the randomized evaluation described in the next section.

Cultural context also plays a role: empowering ultra-poor women in rural areas can challenge traditional gender norms. The program's emphasis on women's asset ownership and entrepreneurship was somewhat novel in these communities. Over time, anecdotal evidence suggested shifts in attitudes – for example, greater acceptance of women raising livestock or engaging in petty trade. By providing an economic role for women, the program also hoped to increase their say in household decisions. That said, rural Bangladeshi society still poses constraints: women's mobility outside the home can be limited, and decisions like marriage of children are often influenced by social norms and economic pressures (dowry expectations, etc.). The program's long-term impacts on such social dynamics (for instance, the marriage timing of participants' children) are an important aspect of this study.

In brief, the UPG program in Bangladesh offers an integrated approach tailored to the ultra-poor, set against a backdrop of rural poverty and traditional social norms. Its multifaceted design addresses the complex causes of extreme poverty – lack of assets, skills, health, and social capital – in a coordinated way. Prior research demonstrated strong short-term impacts of this model; the central question for this report is how those impacts have persisted or evolved after 17 years, and how they have affected the children who grew up under the program's influence. The following section outlines how we investigated these questions through a long-term randomized evaluation.

3. Methodology and data

Study sites and randomization: The evaluation took place in 13 of the poorest rural districts in Bangladesh where BRAC implemented the Targeting the Ultra-Poor (TUP) program (the precursor to UPG). Within these districts, 40 BRAC branch office areas were selected for the RCT. In each district, branches were paired by upazila (sub-district) and randomly assigned to either treatment (UPG program offered in 2007–2009) or control (no program during that period). This cluster-randomized design ensured that entire sample were either exposed or not exposed to the program, preventing spillovers between neighboring treatment and control villages. Randomization achieved a balanced baseline across treatment and control groups. In control areas, ultra-poor households did not receive UPG during 2007–09; however, about 27% of control villages received the program after 2011 when BRAC scaled up nationally. Our intent-to-treat analysis treats those late uptakes as part of the control group, meaning the estimated effects are conservative (some control families eventually got similar assistance, likely reducing the contrast).

Sample and surveys: A baseline census in early 2007 identified ultra-poor households via participatory wealth ranking and eligibility criteria. The baseline survey covered 7,997 ultra-poor households, including nearly all ultra-poor in the

villages and a sample of near-poor and higher-income households. The average household had 1–2 young children. In 2024, we conducted an extensive 17-year endline survey. We successfully re-interviewed 7,304 of the original households (91%), and 6,152 of the original primary female respondents (mothers) (77%, with the remainder mostly accounted for by respondent mortality). Crucially, we traced 9,174 out of 10,582 baseline children (ages 0–15 in 2007) – an 87% follow-up rate – who are now adults aged 16–33. Both survey attrition and tracing rates were similar between treatment and control groups, mitigating concerns of attrition bias. Attrition analysis (Table 1) confirms that attrition rates are generally low and balanced between treatment and control groups for most categories, with one exception: we lost slightly more traced boys from control areas than from treatment areas by 3.3 percentage points. Attrition among original households was 8.7%, and among traced girls 11.7% and traced boys 14.7%. These rates compare favorably to similar long-term studies and do not differ significantly by treatment status (except that traced boys in control had higher attrition, possibly due to migration). We account for attrition in our analysis by checking robustness with inverse probability weighting, and find no evidence that it biases the treatment effect estimates.

Data collection: The 2024 endline gathered detailed information on household demographics, consumption, income sources, assets (land, livestock, durables), savings, and food security, replicating many baseline measures. For original respondents, we recorded their personal employment, earnings, and empowerment indicators. For traced individuals (the children now grown up), we administered individual surveys covering their education, marital status, fertility, employment, and earnings, as well as a module on their own household (for those who had established independent households). The data thus allow us to compare long-term outcomes across treatment vs control for (1) households and original mothers, and (2) the sons and daughters, who constitute the next generation. We receive electronically signed consent from all the respondents we surveyed and provided each of them with a printed copy of the consent form. We take the consent of the parents (or responsible adults) for the young respondents who are still children, and survey them with their own assent.

Estimation strategy: We estimate intent-to-treat (ITT) effects of the program by comparing average outcomes between treatment and control groups, using the original random assignment as an instrument. The primary regression takes the form:

$$Y_{iu} = \alpha_{iu} + \delta_u + \beta T_{iu} + e_{iu}$$

where Y_{iu} is the outcome of interest for individual or household i in unit u (here, u denotes the upazila, for which we include fixed effects δ_u), α_{iu} is the constant, and T_{iu} is an indicator for being in a treated branch area. Standard errors are clustered at the branch level (the unit of randomization). The coefficient β represents the causal effect of offering the UPG program, under the assumption of randomization. Because some control households eventually received the intervention after 2011, β can be interpreted as a lower-bound of the program's true long-term impact (some control outcomes improved due to later treatment, diluting differences). We focus on ITT since it maintains the experimental comparison; per-protocol estimates (treating late recipients as treated) yield slightly larger effect sizes consistent with expectation. All analyses account for the slight imbalance in traced-boy attrition by reweighting or control for baseline covariates, with no substantive change in results.

All monetary values are reported in **Bangladeshi Taka (BDT)**. To contextualize, BDT 100 = approx. 1 US dollar in 2024. Educational attainment is measured in completed years of schooling. Our tables indicate statistical significance at conventional levels ($p < 0.10, 0.05, 0.01$). We use stars to denote significance in regression tables and provide relevant control group means to gauge effect sizes in percentage terms.

4. Results

4.1 Outcome for female offspring ("Traced Girls")

We now examine how the daughters of the original participants – who were children at the time of the intervention – fared as young adults. These "traced girls" grew up in treated households that enjoyed improved economic conditions during their formative years. The analysis covers four critical domains: (a) Marriage timing and early childbearing, (b) Total fertility (number of children), (c) Education, and (d) Employment and savings. Each domain sheds light on whether the program's benefits transferred to changes in the girls' life choices and opportunities.

Marriage and early childbearing: We find that the UPG program led to a *modest delay in marriage* and a more pronounced delay in childbearing for girls (Table 2). By 2024 (when these individuals were 16–33 years old), 88.7% of control-group girls had ever married, compared to 87.3% of girls from treated households – an ITT effect of −1.4 percentage points (significant at the 10% level). Among the youngest girls (who were 0–1 years old at baseline, now 16–17), the treatment group actually shows a lower marriage rate (52.1% married by 2024) than controls

Table 1. Attrition table.

	Traced respondents (1)	Traced girls (2)	Traced boys (3)	Original households (4)
Treatment	−0.017	0.001	−0.033*	0.013
	(0.013)	(0.012)	(0.018)	(0.009)
Control mean	0.139	0.116	0.161	0.080
Observations	10,582	5,047	5,535	7,997

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term. Columns 1 to 4 show attrition for all the traced respondents, traced girls, traced boys, and their households in 2007 respectively

Table 2. Proportion of married girls.

All girls		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	−0.014*	−0.038	0.039	−0.016*	−0.001
	(0.008)	(0.050)	(0.027)	(0.009)	(0.006)
Control mean	0.887	0.559	0.791	0.938	0.981
Observations	4,455	524	840	1,562	1,529

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

(55.9%), but this −3.8 pp difference is not statistically significant given the small sample of that cohort. For girls who were teens during the program (now in their 20s), the effects on marriage timing are also minor and mostly not significant.

The clearest impact of the program was not on whether girls married (nearly all do by their late 20s in both groups), but on when they started having children. Table 3 shows the probability of having at least one child by 2024. Overall, 72.9% of treated girls had become mothers, versus 76.1% of control girls – a − 3.2 pp difference significant at 1%. Importantly, this effect is driven by a large reduction in teenage childbearing. Among girls who were still adolescents (16–17 years old) in 2024, 26.0% of those in the treatment group had a child, compared to 35.2% in the control group. This is a − 9.2 percentage point impact ($p < 0.01$), indicating that the program enabled a considerable fraction of girls to avoid early motherhood.

Why did treated girls delay childbearing despite similar marriage rates? The qualitative insights and ancillary survey questions suggest a few mechanisms. First, improved household economic security likely reduced the pressure on girls to prove fertility early or to contribute a child for family labor. Parents in treated households might have been less inclined to push their daughters into having children immediately as a strategy for securing in-laws' support, because the family was less desperate economically. Second, the program included health and family planning awareness (as part of life-skills coaching), which, while targeted at mothers, may have indirectly reached their daughters. It is plausible that treated

Table 3. Likelihood of having children among traced girls.

All girls		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	−0.032***	−0.092***	0.014	0.001	−0.019*
	(0.010)	(0.034)	(0.031)	(0.014)	(0.010)
Control mean	0.761	0.352	0.561	0.817	0.927
Observations	4,455	524	840	1,562	1,529

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

mothers, being more empowered and informed, advised their daughters on contraception or allowed them more autonomy to delay pregnancy. In Bangladesh's cultural context, newly married young women often face expectations to have a child quickly. The fact that in our study treated girls did not follow that norm as frequently (leading to significantly fewer teen pregnancies) is a strong indication of intergenerational transmission of attitudes about reproductive choices. This delay in first birth can have profound implications: medical research shows that teen pregnancies carry higher health risks for mother and child, and from a socio-economic perspective, avoiding teen motherhood gives young women more time to attain education or skills (if available) and better prepare for motherhood when it does happen.

It is worth noting that by the oldest cohort (26–33 years), the proportion of women with children is extremely high in both groups (over 90%). Although the program did not aim to reduce completed fertility per se, we still observe a decline in the fertility of the treated women (this is reified by the results of the next subsection). The main effect was a temporal shift: treated girls tended to have their first child later, even if they caught up in family size by their late 20s. Such a temporal shift is valuable – delaying first birth from the teens to the 20s is associated with better maternal health (Woo et al., 2022), lower infant mortality (Noori et al., 2022), and improved economic opportunities for the mother (since she can work or study longer before childcare responsibilities) (Bratti, 2015).

The program's impact on delaying childbearing is one of the most significant intergenerational effects we find. Delays in childbearing age, facilitated by access to family planning services, improve women's economic situation in poor households by increasing their earnings and assets (Canning & Schultz, 2012). Our results resonate with the findings of a meta-analysis of 15 studies that find that cash transfers reduce early pregnancy while conditional transfers reduce early childbearing (Kneale et al., 2023). In the context of rural Bangladesh, early marriage is often socially normative, but our data indicate that even if marriage wasn't much delayed on average, treated girls exercised more control within marriage to time their first birth.

(b) Fertility (Number of Children): While the above analysis looked at whether a girl had any children, we also examine how many children she had by 2024 (Table 4). This helps assess if the program influenced overall fertility or just the timing of births. On average, treated young women have 0.057 fewer children than control young women. This small reduction is consistent with the delay in first births; by their late 20s, treated women had slightly fewer children, though as noted, by early 30s they caught up. Breaking it down by age cohorts: the effect is concentrated among the youngest and the eldest girls. Those who are 16–17 and 26–33 years old in 2024 from treated households have 0.09 and 0.05 fewer children, respectively, compared to their control counterparts. This confirms that the program reduced fertility for the older daughters – not quite so for the younger ones. The girls in the treated households aged 16–17 have fewer children because they are less likely to have children (Table 3). Culturally, most women in this setting desire and do have children by their 30s (93% of them, as shown in Table 3), and the program was not expected to change those norms. Rather, the benefit was giving them a healthier start by avoiding adolescent childbirth.

c) Education: Education is a crucial determinant of a girl's future earning potential and empowerment. Paradoxically, we find a slight negative impact on girls' educational attainment for the treated group. As noted earlier, treated girls completed on average 0.281 fewer years of schooling than control girls shown in Table 5. This outcome was not anticipated, as one might expect poverty reduction to increase schooling. However, our analysis suggests that opportunity costs played a role. Many girls in treated households faced new opportunities to work (in family businesses or livestock rearing). Moreover, Kafle et al. (2018) attributes ownership of livestock assets to adversely affect children's years of schooling, possibly due to a high opportunity cost of schooling.

Table 4. Number of children of the traced girls.

All girls		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	−0.057***	−0.090*	0.039	0.016	−0.053*
	(0.019)	(0.047)	(0.044)	(0.024)	(0.030)
Control mean	1.250	0.385	0.652	1.214	1.851
Observations	4,455	524	840	1,562	1,529

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

Table 5. Years of schooling for traced girls.

All girls		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	−0.281**	−0.035	−0.339*	−0.416**	−0.335**
	(0.124)	(0.190)	(0.191)	(0.197)	(0.158)
Control mean	6.369	6.866	7.247	6.635	5.499
Observations	4,454	524	840	1,561	1,529

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

The negative education effect is most pronounced for the older cohorts of girls who were in their teens during the program. Treated girls now aged 26–33 have 0.335 fewer years of schooling than controls (5.164 vs 5.499 years on average; $p < 0.05$). Those aged 21–25 and 18–20 also show reductions of 0.416 and 0.339 years, respectively (both significant at 5% and 10% respectively). By contrast, girls who were very young during the intervention (now 16–17) show no significant schooling difference (−0.035 years, not significant). This cohort effect makes sense: the program had the largest immediate impact on girls who were at a decision-making age for schooling in 2007–2010. For them, the program's success in boosting household income ironically may have diminished the incentive to continue schooling – education might have been seen as less critical for survival or upward mobility once the household had an alternative pathway out of poverty. In economic terms, the opportunity cost of schooling rose: a 14-year-old girl in a treated household might have had a productive role (helping rear livestock, running a small shop) that she wouldn't have had in a control household, making leaving school a more attractive (or expected) choice. In control households, by contrast, that 14-year-old might have stayed in school longer because there was little else productive to do and the family might have hoped education would be her way to a better life.

It's important to contextualize that overall education levels were low in both groups. Most girls did not advance beyond primary school. For example, control girls now aged 26–33 averaged only 5.499 years of schooling (treated: 5.164). Thus, both treated and control girls often left school around early adolescence. The program's impact was to slightly hasten that exit for some. The policy implication is that anti-poverty programs, when not directly tied to schooling incentives, might inadvertently cause some youth to shift from school to work if economic opportunities improve. This has been observed in other contexts: studies of cash transfer programs have found mixed effects on schooling – sometimes enrollment goes up because families can afford fees (Cruz et al., 2015), other times it goes down because kids are needed in a new family business.

We also look at the likelihood of completing at least 10 years of schooling among the traced girls. Table 6 indicates that treated girls were slightly less likely to complete grade 10. Overall, 18.8% of control girls completed at least 10 years of schooling, versus 15.4% of treated girls – about a 3.4 pp decrease ($p < 0.05$). This gap is in line with the ~0.3 year reduction in schooling.

The negative effect on the likelihood of completing at least 10 years of schooling is mainly driven by the 18–25 age range: e.g. among those now 21–25, 21.2% of control girls completed grade 10 vs only 17.5% of treated (a 3.7 pp drop, $p < 0.05$).

Table 6. Likelihood of completing at least 10 years of schooling among the traced girls.

All girls		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	−0.034**	−0.038	−0.069**	−0.037**	−0.016
	(0.013)	(0.038)	(0.026)	(0.017)	(0.013)
Control mean	0.188	0.190	0.260	0.212	0.126
Observations	4,455	524	840	1,562	1,529

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

For the oldest cohort (26–33), have low completion (12.6%) and differences are small. The youngest cohort (16–17) hasn't reached the age to finish secondary yet. These results reinforce that a subset of girls in treated households likely stopped schooling around grade 8–9 instead of continuing to grade 10. It's a subtle effect – we are talking about relatively small percentages – but in terms of human capital, it could have implications. Not finishing secondary school can limit formal job qualifications and further education. We do observe later in the employment outcomes that treated young women did not enter formal jobs at higher rates than controls, which may be partly due to their slightly lower educational credentials.

The findings on education call for a nuanced interpretation. They do not imply that the program "harmed" education overall – virtually all these girls, even in control, had low schooling attainment, a reflection of poverty (coercing them into income generating activity) and social norms that affect girls' education. What the results do suggest is that future graduation programs might integrate components to safeguard or promote schooling for adolescent children. For example, conditional incentives to keep girls in school or flexible schooling options for those involved in family businesses could help avoid this trade-off. Our study contributes to evidence that Sulaiman (2015) finds on almost the same sample studied in this paper – no impact on school enrolment rates and an increase in child work for a 2-year and 4 -year follow-up. Recognizing this, BRAC and other implementers could consider complementary educational support when scaling such programs.

(d) Employment and Savings: Lastly, we assess how the program influenced the labor market and financial outcomes of the traced girls as they entered adulthood. Did daughters of treated families have different work trajectories or financial habits compared to those of control families? We have already noted that treated girls were more likely to engage in any work (IGAs). Table 7 quantifies the impact on labor force participation: 48.5% of treated young women reported working in the past year, compared to 43.4% of control young women – a 5.1 pp effect. This confirms a statistically significant increase in female labor supply due to the program. Breaking it down: the effect is largest for the 18–20 age group (8.9 pp increase, from 27.7% in control to 36.6% in the treated. There is also a notable increase for 26–33-year-olds (7.8 pp increase), which is interesting because by that age most women in both groups are already quite active (55% in control). It seems the program's effect on work persisted even after many years, possibly because treated women developed a work habit or had family enterprises to join. For the youngest cohort (16–17), the labor participation effect was positive but not significant (5.1 pp increase). Many in this age are still finishing school or just starting work.

Importantly, the nature of work these young women are doing tends to be informal. We saw a big increase in involvement in family-based enterprises (especially livestock rearing). In many cases, this work is unpaid family work or self-employment rather than wage jobs. We do not see a significant move into formal salaried employment for treated women. Table 8 shows that only 14.7% of control women had a formal job versus 15.5% of treated – a non-significant 0.8 pp increase. The only cohort with a notable difference was 18–20-year-olds: 12.2% of treated had a formal job against 8.8% of controls (3.4 pp increase). This might indicate that a few more treated young women managed to obtain informal jobs in their late teens (perhaps aided by slightly better networks or confidence from the program). However, for the older groups, there was no advantage. In rural Bangladesh women on average spend fewer hours in paid work than those in urban areas (BBS, 2023), and our data reflect that reality. The program didn't change the structural fact that most women work informally. It did, however, get more of them into the workforce and likely into roles associated with the program's interventions.

Finally, turning to personal savings: we find a robust positive impact on the savings of traced girls, mirroring the effect observed for the sons. Table 9 indicates that treated young women have saved BDT 1,664 more than control young

Table 7. Likelihood of being involved in an IGA within a year among traced girls.

All girls		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	0.051***	0.051	0.089***	0.032*	0.078**
	(0.017)	(0.031)	(0.022)	(0.018)	(0.031)
Control mean	0.434	0.235	0.277	0.457	0.550
Observations	4,455	524	840	1,562	1,529

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

Table 8. Likelihood of having a salary job among traced girls.

All girls		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	0.008	−0.022	0.034**	0.001	0.022
	(0.009)	(0.024)	(0.014)	(0.018)	(0.014)
Control mean	0.147	0.123	0.088	0.170	0.161
Observations	4,455	524	840	1,562	1,529

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

women on average – a 36.86% increase. It implies that daughters from program families were either given more money to save (from parents' higher income) or earned more and were encouraged to save it. The program likely influenced a "savings culture": BRAC's intervention emphasized financial literacy and saving even small amounts, which could have rubbed off on children of the beneficiaries. The effect varies by age: it's not significant for the 16 to 17-year-old group (who haven't had much time to accumulate savings) but significant for older ones. For instance, treated 21–25-year-olds saved BDT 1,652 more than controls (a 31% increase). The oldest daughters (26–33) saved BDT 2,244 more, a 43% increase. These are sizable differences in personal financial holdings, suggesting greater financial empowerment among daughters of treated households. Many of these women likely started saving some of their earnings. It's a promising sign because accumulated savings can help them invest in their own families' health or education, emergencies, or seed capital if they start a business.

Overall, the intergenerational impacts on traced girls can be summarized as a combination of enhanced economic empowerment and reduced human capital investment. The daughters of ultra-poor families that went through the graduation program were more likely to work and manage their own savings, and less likely to be teenage mothers – all of which point to greater agency and a break from some traditional constraints. On the other hand, they received slightly less schooling and did not make significant inroads into the formal sectors of the economy by their early adulthood. These findings are consistent with the notion that in poor rural households, ownership of livestock assets adversely affects children's years of schooling, possibly due to a high opportunity cost of schooling (Kafle et al., 2018). In our context, the net effect for daughters is still largely positive – delayed fertility and financial inclusion are big gains – but the loss in education is a concern since it could cap their long-term earnings potential.

4.2 Outcome for male offspring ("Traced Boys")

Finally, we consider the outcomes for the sons of the original program participants. These "traced boys" were 15 or younger at baseline and are now 16–33 years old. Compared to the girls, the effects on boys are more limited in scope, reflecting different gender roles and baseline conditions. In these ultra-poor households, boys typically had slightly more work opportunities than girls, and social norms encouraged them to work and marry later. The UPG program did not target boys directly, but by improving household economic status, it could indirectly influence factors like education (through reduced child labor or better school affordability), marriage, or the kinds of jobs they pursue. We examine (a) marriage and fatherhood, (b) fertility (number of children), (c) education, and (d) employment and savings for these young men.

Table 9. Personal savings for traced girls.

All girls		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	1, 664.1***	994.0	1, 664.9***	1, 652.4***	2, 244.1***
	(264.6)	(670.4)	(514.7)	(360.0)	(351.2)
Control mean	4, 514.2	2, 007.3	3, 018.8	5, 287.9	5, 222.2
Observations	4,455	524	840	1,562	1,529

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

(a) Marriage and Fatherhood: The program's impact on young men's family formation is Table 10. This suggests that fewer boys from program households got married. The effect is primarily observed in the older cohort (26–33 years old): in that group, there is a 2.9 pp reduction in the proportion of the married boys in treatment. For younger boys, the differences are not significant. Proportions of married boys in younger age groups are already low in the control group (6.3%, 29.2% and 61.6% in the age groups 16–17, 18–20 and 21–25-year-olds, respectively). The finding for 26–33 implies that improved economic security allowed these men to postpone marriage a bit – possibly to focus on work or to wait for a better match. The program may have enhanced that stability but also raised their aspirations or bargaining power in marriage (they could be choosier or afford to wait for a spouse from a slightly better-off family) or altered their attitude towards mahr payments (mahr is the obligatory gift or payment of money, property or other valuable items that the goom has to pay to the bride upon marriage in Islamic law). Hani Kulsum (2025) finds in her qualitative study conducted in rural Chapai Nawabganj (one of the Northern districts of Bangladesh from which we draw our sample) that even though husbands consider mahr as a wife's right, they view it as less important than providing for their families. As a result, a tiny portion of the mahr fixed at the wedding is actually paid, while the rest remains unpaid for most of their lives. Our data shows that less than 10% of the mahr fixed at the wedding is actually paid. However, the married boys from the treated households end up paying significantly more mahr than their control counterparts. This may suggest that the perceived cost of marriage for the treated boys is higher than that of those in control, causing them to put off their marriage. Overall, the delay in marriage is modest but could benefit young men by giving them more time to develop economically.

In terms of fatherhood, the program has no significant overall effect on whether young men have children by 2024. About 38% of the young men are fathers (Table 11), much fewer than the proportion of mothers (shown in Table 3). It follows from the fact that a smaller proportion of these young men are married compared to the women (Tables 2 and 10). Table 11 shows that the results are consistent with this rhetoric when disaggregated by age. Our findings are consistent with the national data, which shows that a greater proportion of rural men (37%) compared to rural women (22%) are not married (BBS, 2023). This disparity is maintained across all the age groups except for the highest age group (greater than 50) in the national data (BBS, 2023). This indicates that men are either delaying their marriage or a smaller proportion of them are marrying across different age groups, as highlighted by our findings, likely leading to a smaller proportion of them being fathers compared to their female counterparts.

(b) Fertility (Number of Children): Table 12 shows that there is no significant difference in fertility for men across treatment and control. This indicates that the program did not affect male fertility outcomes. However, we do see similar

Table 10. Proportion of married boys.

All boys		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	−0.037**	0.001	−0.055	−0.019	−0.029**
	(0.016)	(0.014)	(0.033)	(0.021)	(0.013)
Control mean	0.592	0.063	0.292	0.616	0.890
Observations	4,718	638	746	1,683	1,651

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

Table 11. Likelihood of having children among traced boys.

All boys		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	−0.006	0.005	−0.022	−0.001	0.016
	(0.012)	(0.009)	(0.024)	(0.015)	(0.023)
Control mean	0.384	0.009	0.100	0.358	0.668
Observations	4,718	638	746	1,683	1,651

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

Table 12. Number of children of the traced boys.

All boys		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	−0.019	0.005	−0.017	0.013	−0.010
	(0.018)	(0.009)	(0.024)	(0.021)	(0.051)
Control mean	0.594	0.009	0.100	0.430	1.176
Observations	4,718	638	746	1,683	1,651

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

Table 13. Years of schooling for traced boys.

All boys		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	0.117	0.020	0.077	0.021	0.160
	(0.136)	(0.188)	(0.204)	(0.202)	(0.145)
Control mean	5.696	6.045	6.356	6.248	4.756
Observations	4,718	638	746	1,683	1,651

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

patterns in our findings when the mean values are compared with those of the women. We observe that the overall fertility rates for men are lower than those for females – this is consistent across the different age groups. It follows from the discussion earlier that a smaller proportion of the men are married compared to the women, making it more likely for them to have fewer children than the women as depicted in our findings from Tables 4 and 12.

(c) Education: Education outcomes for traced boys reveal a contrast to girls. Again, the program has no adverse effect on boys' schooling. Treated and control boys attained almost identical schooling levels as shown in Table 13. On average, both groups completed about 5.7 years of schooling (roughly primary school). Across age cohorts of boys, none of the differences in years of schooling are significant.

However, comparing the mean years of schooling of the boys with that of the girls, we find that the boys, on average, have fewer years of schooling than the girls. This is also true across the different age groups shown in Tables 5 and 13. The national data shows that the dropout rate of boys from primary education, at 15%, is higher than that for girls, at 13% (BANBIES, 2023). This is consistent with our findings because our respondents, on average, only completed primary education.

We also examined secondary school completion for boys (Table 14). Consistent with years of schooling, there's no significant difference: around 21% of boys completed grade 10. Essentially, only about one-fifth of boys in both groups made it to secondary graduation – a sobering statistic that highlights how pervasive educational dropout is in these communities, even for boys. The program neither worsened nor improved that figure. However, compared to girls the completion rates are higher on average (Tables 6 and 14). This is also consistent with the national statistics, which show that the dropout rate from secondary schooling for boys is lower, at 33%, than that for girls, at 41% (BANBIES, 2023).

(d) Employment and savings: Traced boys grew up in the same improved household conditions as traced girls, so we look at whether they experienced similar boosts in employment and savings. In terms of labor force participation, young men in both groups have high rates of work, as noted in Table 15. About 91% of the boys were engaged in some income-generating activity by 2024. However, the difference is not significant across treatment and control. So overall, virtually all men find work irrespective of treatment.

Table 14. Likelihood of completing at least 10 years of schooling among the traced boys.

All boys		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	0.004	0.003	−0.021	0.001	0.010
	(0.013)	(0.018)	(0.029)	(0.022)	(0.010)
Control mean	0.206	0.170	0.263	0.263	0.140
Observations	4,718	638	746	1,683	1,651

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

Table 15. Likelihood of being involved in an IGA within a year among traced boys.

All boys		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	0.007	0.038	0.039**	0.003	−0.004
	(0.006)	(0.031)	(0.016)	(0.008)	(0.005)
Control mean	0.906	0.673	0.847	0.950	0.975
Observations	4,718	638	746	1,683	1,651

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

By age disaggregation, a couple of interesting points emerges: for 18–20-year-old males, there is a 3.9 pp increase in labor force participation in the treatment group. This mirrors the effect we saw among girls of that age, though for boys, even control participation is very high by 18–20 (84.7% working). For other age groups, there are no significant differences between treatment and control for boys being involved in income generating activity. In summary, the program might have facilitated some younger boys to start working a bit more (perhaps in family ventures), but by and large it did not increase the already high male employment rate.

Looking at formal employment for young men (Table 16), we see a similar story to the girls: no significant change overall. About 26% of young men had a formal/salaried job. The only hint of impact is in the 21–25 age group, where treated men show a 3.3 pp higher formal employment compared to their control counterparts. This parallels the slight increase for 18–20 women (perhaps indicating some treated families networked into formal jobs a bit more). But across other ages, differences are minimal (e.g. for 26–33, 25.8% treated vs 25.6% control approximately the same). The conclusion is that the UPG program did not open up formal sector paths for men, which is understandable – such jobs depend on education and external economic conditions that the program itself did not change. Most treated men continued in informal/self-employed occupations (farming, day labor, small trade) just like control men.

Table 16. Likelihood of having a salary job among traced boys.

All boys		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	0.018	0.029	0.017	0.033*	0.002
	(0.013)	(0.032)	(0.027)	(0.019)	(0.012)
Control mean	0.258	0.184	0.256	0.290	0.256
Observations	4,718	638	746	1,683	1,651

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

Table 17. Personal savings for traced boys.

All boys		Disaggregated by age			
		16–17	18–20	21–25	26–33
Treatment	2, 008.6***	875.0*	1, 373.2***	2, 070.7***	3, 054.8***
	(254.8)	(450.3)	(484.1)	(318.8)	(431.0)
Control mean	4, 044.4	1, 628.9	2, 900.7	3, 733.7	5, 703.9
Observations	4,718	638	746	1,683	1,651

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term.

Finally, turning to personal savings: we find that sons of treated households, much like daughters, accumulated significantly more savings than those of control households. Table 17 shows an overall increase of BDT 2,008.6 in personal savings for the boys, roughly a 50% increase in savings. This resonate with what we observed for young women. It appears that program families fostered a savings mindset among their children of both genders. Many treated boys might have saved earnings from new ventures or simply benefited from a household culture that valued saving (perhaps through mothers' influence).

The age breakdown in Table 17 highlights that for men, unlike women, even the youngest treated boys (16–17) had somewhat more savings an increase of BDT 875. For older cohorts, the savings differences are large and highly significant: e.g. treated men 26–33 saved BDT 3,054.8 more than their control counterparts. These numbers suggest that by their late 20s, sons from treated families were able to hang onto more of their income rather than living hand-to-mouth. The extra savings are likely used for investments or as a financial cushion that can prevent them from falling back into destitution.

In summary, the intergenerational effects on traced boys can be characterized as financial gains without social or human capital gains. The UPG program didn't alter their education or formal employment trajectories – those remained constrained by the broader context (poor schooling quality, limited rural jobs). However, it did enable them to put their marriage on halt for now and save more money, which are meaningful in economic terms. The substantial increase in personal savings among young men is a testament to improved income flows and financial practices learned in the household. It's a form of intergenerational wealth transfer: the program gave assets to the parents, and the next generation translated some of that into financial assets of their own.

It's interesting to contrast daughters and sons: daughters saw bigger changes in life choices (like delaying childbearing and entering work), while sons mainly saw changes in resources (savings) and a small behavioral tweak (putting off their marriage). This aligns with the program focusing on women – the mothers – which appears to have translated the positive impact intergenerationally on the female children. Sons benefited too, but mostly in economic ways (having more capital available), rather than in changed norms (they already were expected to work and had more freedom in marriage decisions traditionally). Another way to put it: the program relaxed financial constraints for both sons and daughters, but it relaxed

Table 18. Consumption and income of the traced respondents' household.

	Consumption	Income	Likelihood of rearing		Food insecurity
	(BDT)	(BDT)	Cows	Goats	
	(1)	(2)	(3)	(4)	(5)
Treatment	20, 248.2***	7, 902.6**	0.036***	0.026***	−0.052
	(1, 774.2)	(2, 922.0)	(0.01)	(0.009)	(0.037)
Control Mean	148, 013.9	168, 930.3	0.12	0.112	0.009
Observations	7,912	7,912	7,912	7,912	7,912

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while *, **, and *** represent significance levels of 10%, 5%, and 1% respectively. The regression equation is $Y_{iu} = \alpha_u + \beta T_{iu} + e_{iu}$ where Y_{iu} is the outcome variable for individual i and upazilla u; α_u is the upazilla fixed effects; T_{iu} is the indicator for being assigned to the treatment group; and e_{iu} is the error term. Columns 1 and 2 show yearly consumption and income in BDT, respectively. Columns 3 and 4 show the likelihood of rearing cows and goats, respectively. Column 5 shows the household's standardized food insecurity measure.

Table 19. Savings and assets of the traced respondents' household.

	Savings (BDT) (1)	Livestock (BDT) (2)	Non-land (BDT) (3)	Land (BDT) (4)	Land area (Decimals) (5)
Treatment	1,509.3***	4,851.3***	989.4	9,984.80	0.512*
	(214)	(1,164.90)	(1,056.80)	(7,459.50)	(0.28)
Control Mean	5,817.20	18,769.30	31,171.40	198,911.00	6,133
Observations	7,912	7,912	7,912	7,912	7,912

Notes: Standard errors, clustered at BRAC branch office level, are in parentheses, while $*$, $**$, and $***$ represent significance levels of 10%, 5%, and 1% respectively. The regression equation is Yiu = αu + βTiu + eiu where Yiu is the outcome variable for individual i and upazilla u; αu is the upazilla fixed effects; Tiu is the indicator for being assigned to the treatment group; and eiu is the error term. Columns 1 to 4 show household savings, value of livestock assets (cows and goats), value of other assets excluding land and value of land owned, respectively. Column 5 shows the decimals of land owned by the household.

social constraints primarily for daughters (leading them to delay childbirth and engage in non-traditional roles like earning and saving).

4.3 Outcome for traced respondents' households

Consumption and income: Assignment to the Graduation treatment is associated with statistically meaningful improvements in household living standards among traced respondents. Annual consumption is higher by BDT 20,248 ($p < 0.01$) relative to the control mean of BDT 148,014—about a 13.7% gain. Annual income rises by BDT 7,903 ($p < 0.05$) over a control mean of BDT 168,930—roughly a 4.7% increase. Asset-related behaviors are also affected: the likelihood of rearing cows increases by 3.6 percentage points (pp; $p < 0.01$) from a control mean of 12.0% (≈30% relative increase), and the likelihood of rearing goats rises by 2.6 pp (pp; $p < 0.01$) from a 11.2% control mean (≈23% relative increase). The point estimate for the (standardized) food insecurity measure is negative (−0.052) but imprecise and not statistically distinguishable from zero, indicating no clear average difference in food insecurity between groups.

Savings and assets: Consistent with the program's asset-building pathway, treated households hold more financial savings and higher-value livestock assets many years later. Savings are greater by BDT 1,509 ($p < 0.01$) relative to a control mean of BDT 5,817—about a 25.9% increase. Livestock asset value (cows and goats) is higher by BDT 4,851 ($p < 0.01$) over a control mean of BDT 18,769— ≈ 25.8% higher. Differences in other non-land assets (+BDT 989) and land value (+BDT 9,985) are positive but not statistically significant, suggesting no robust average effect on these broader asset categories. Land area owned is higher by 0.512 decimals ($p < 0.10$) from a control mean of 6.133 decimals—an ≈8.3% increase—which is small in magnitude and only weakly significant.

5. Conclusion

This 17-year follow-up study provides robust evidence that the Ultra-Poor Graduation (UPG) program achieved lasting reductions in extreme poverty and initiated intergenerational progress in rural Bangladesh. Treated households that received the comprehensive package of support in 2007–2009 continue to exhibit higher consumption, greater asset ownership, and improved financial security nearly two decades later. These findings validate the premise that a one-time "big push" can set ultra-poor families on a sustained upward trajectory, rather than a short-term bump followed by regression to poverty. By 2024, the average treated household enjoys a level of living standards that is significantly above that of comparable control households, reflecting persistent gains in income generation capacity and resilience.

The intergenerational impacts are particularly noteworthy. Daughters of the beneficiary households have clearly benefited: they marry and start childbearing at later ages (reducing the incidence of teenage pregnancy by about one-fourth), are more likely to be economically active, and have accumulated substantially more personal savings by their early 20s than their control counterparts. These changes signify enhanced agency and could break components of the intergenerational poverty cycle. Delaying marriage and childbirth gives young women more time to build their human capital or earn income, leading to healthier families and better opportunities for the next generation (their children). The fact that daughters of ultra-poor women are saving money and participating in work at higher rates is a marked departure from the traditional trajectory of girls in extreme poverty (who often would be married by 18 and entirely dependent on husbands). It shows a second-generation empowerment effect – the program empowered mothers, which in turn expanded the life choices of their daughters.

At the same time, the educational setback for treated girls highlights a complex trade-off: while immediate economic improvements opened some doors, they may have inadvertently closed the door to more schooling. This suggests that future programs should integrate measures to safeguard education – for example, incorporating scholarships or conditional cash for school attendance alongside livelihood support, especially targeting adolescent girls. A holistic approach that marries economic empowerment with human capital investment would ensure that short-term income gains do not come at the expense of long-term capabilities. Other studies have similarly cautioned that without such integration, big-push interventions might see youth (especially from the beneficiary households) diverge from schooling in pursuit of quick income (Karimli et al., 2020; Bedoya et al., 2023). Our findings reinforce this caution and offer an opportunity to refine the graduation model to maximize its multi-generational impact.

For sons of the beneficiary households, the program's effects are evident in greater financial accumulation (savings) and a slight delay in marriage, though not in education or formal employment. Many outcomes for young men were determined by broader factors not directly altered by the intervention, such as the availability of quality schooling and jobs. Thus, while the program lifted the economic base for these young men (giving them a headstart in terms of assets/savings), complementary investments (like vocational training or job linkages) would be needed to translate that into formal sector breakthroughs. Nonetheless, the increase in savings among young men is an important positive outcome – it indicates a buffer against shocks and a resource for future investments (like migrating for better work, which several treated men have done using their savings).

In a broader sense, this study demonstrates that the benefits of the graduation program did not evaporate; they evolved. Initial gains in household assets and income were largely maintained and in some aspects grew (e.g. many treated households continued to accumulate livestock or buy land). Though control households partially caught up after receiving similar support later, treated households retained an edge on most metrics. Perhaps more importantly, the nature of poverty in these families changed: from being in dire, chronic scarcity, they moved to a state of modest but sustained improvement. Issues like chronic hunger were alleviated to a degree; decisions became less driven by immediate survival and more by how to leverage new opportunities. This shift allowed the next generation to grow up under better circumstances than their parents did, yielding the intergenerational impacts we measured.

It's critical to acknowledge that while the UPG program was highly effective, it is not a panacea. Our findings point out certain structural challenges that the program alone could not overcome. For instance, educational and employment systems need to be in place to absorb the aspirations and capacities of youths from formerly ultra-poor families. Without accessible secondary schools or job opportunities, some of the gains (like delayed marriage) might not fully translate into improved lifetime earnings. Similarly, persistent gender norms and expectations (such as the burden on daughters to help at home or marry early) can dampen potential benefits – something observed in the schooling outcomes.

In conclusion, the evidence from this long-term RCT follow-up affirms that comprehensive anti-poverty programs can have enduring and far-reaching impacts. The UPG program's success in "graduating" households out of extreme poverty has proven to be durable – most treated families did not fall back below the extreme poverty line, and many built resilience to remain on an upward trajectory. Furthermore, the program generated positive externalities within the household across generations, particularly empowering girls. This indicates a multiplier effect of investing in the ultra-poor: not only does the original beneficiary benefit, but her children (especially daughters) reap long-term advantages in life choices and financial independence. From a policy standpoint, these findings make a strong case for the scale-up of graduation approaches as a cornerstone strategy for poverty eradication – with the caveat that integrating health, education, and gender-focused components can enhance outcomes. The Bangladesh experience showcased here can inform global efforts, as similar programs are tried in diverse settings (Banerjee et al., 2015; Bedoya et al., 2019). The overarching lesson is that extreme poverty is not an inescapable trap: given the right set of interventions, even the poorest families can climb out and set their children on a better path. The Ultra-Poor Graduation program achieved exactly that for thousands of Bangladeshi families, and its legacy is visible 17 years later in more prosperous households and more empowered youth.

Conflict of interest

We do not have any conflict of interest to report.

Ethical approval

We received our ethical approval for this research from the Institutional Review Board (IRB) of BRAC James P Grant School of Public Health, BRAC University – the IRB protocol number is IRB-28 September-23-035.

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