Seventeen-Year Effects of the Targeting the Ultra Poor Program *

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September 10, 2026

Abstract

This paper examines the seventeen-year effects of the Targeting the Ultra Poor (TUP) Program in West Bengal, India through a randomized controlled trial with 978 households. The TUP program led to large and persistent gains in consumption (1.27 SD), income (0.3 SD), food security (0.2 SD), asset ownership (0.49 SD), physical health (0.24 SD), and mental health (0.23 SD) even seventeen years after the initial transfer of assets. Importantly, treatment effects on all economic outcomes grew since the ten-year endline. A main channel of growth is higher microenterprise income among treated households, where the treatment effect grew by 345% compared to the ten-year effect. Treated businesses are both larger and more productive. Our evidence points to an important intergenerational mechanism driving higher productivity in treated businesses: young adults (17-35 years) who were children at baseline are substantially more likely to manage and devote more time to household businesses.

*Andrés Orduña Correcher, Antoine de Mascarel, and Rahul Paul provided excellent research assistance. We further thank Tamal Majhi, Getsie Immanuel, and Arkodeep Bandhopadhyay for logistical support at J-PAL. Finally, we are exceedingly grateful to Ramaprasad Mohanta, Sanjay Das, Arijit Dutta, Sanjib Kumar Das, and everyone else at Bandhan for their long-term commitment to this experiment. We gratefully acknowledge funding from the J-PAL Social Protection (SPI) Initiative (GR-000002769). We received IRB approval from MIT (#: 2405001308) and IFMR in Chennai. Main analysis results were pre-registered on the American Economic Association's RCT Registry (AEARCTR-0015528).

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1 Introduction

The persistence of the impacts of anti-poverty interventions is a central question of social policy. If the beneficial effects persist after the intervention itself finishes, all else being equal, the cost-benefit calculations will (obviously) be more favorable. Moreover, interventions that generate long-term benefits after a short-lived treatment may also be easier to implement well.

Persistence is obviously related to the nature of the intervention—human capital improvements are usually assumed to last a lifetime and even feed into the next generation. On the other hand, the presumption is that small short-term cash transfers will be spent quickly without leaving much of a long-term imprint, in part because the poor face many constraints to saving enough to invest in a productive asset (Dupas and Robinson (2013); Carranza et al. (2025)).

Larger cash transfers might be different because they can be invested in a productive asset that generates future earnings or a household asset (a metal roof, for example) that saves on recurrent future expenses, both of which tend to be lumpy and require a minimum investment. A direct asset transfer, like the Targeting the Ultra-poor or TUP program we have been studying for many years, could have a similar effect. If the income gains from the asset are large enough, they may spur further investments and further income gains—this is the idea that a large enough transfer might spring a household from a poverty trap and launch a virtuous circle. Alternatively, the receipt of a transfer that is large enough to open up new life possibilities might change households' attitudes toward labor supply, savings, and investment. Recipients might start to believe that they can change their lives and that might encourage them to try to do so, again unlocking a virtuous cycle.[1]

On the other hand, there are forces that work against persistence. Shocks to health and market conditions are one. Generational transitions are another—the next generation may not have the same skills, work habits, or aspirations as the parent.[2] Both types of shocks have the potential to undo the accumulated gains of the previous many years.

Based on the 10-year follow-up of an RCT of the TUP program in India, our previous work (Banerjee et al. 2021) finds strong evidence of persistence in its impacts. The ten-year impacts on consumption are indistinguishable from the 7-year impacts and larger than the impacts at 18 months or 3 years. This is also the evidence from the long-term follow-up of the Bangladesh TUP program by Balboni et al. (2022) who use these results to make the case that the intervention helped households exit a poverty trap.

Here we report on the 17-year follow-up. Apart from the fact that seventeen-year follow-ups are extremely rare—with important recent exceptions including Duflo et al. (2026) and Hamory et al. (2021)—but crucial for studying persistence, the 17-year gap is also important because it

[1] For evidence related to changes in labor supply see Banerjee et al. 2025

[2] Thomas Mann's great novel Buddenbrooks, is exactly about the problem of generational transmission of attitudes in a German bourgeois family.

covers both kinds of shocks that provide potential reasons why programs impact may not persist. First, there was substantial intergenerational turnover: those who were children in 2007 are now well into adulthood and working. Second, there was a major shock to both health and market conditions: the Covid-19 pandemic and the resulting lockdown, which led to a 25 percent drop in Indian annual GDP and especially hurt households like those in our sample, who largely work in the informal sector (World Bank 2020).

Strikingly, we find persistence at seventeen years. The economic impacts of the TUP program are larger in magnitude at year seventeen compared to year ten for the main pre-registered outcomes, including consumption, income, food security, and asset ownership. At year seventeen, the treatment effect on consumption is 1.27 SD, income is 0.32 SD, food security is 0.22 SD, asset ownership is 0.49 SD, physical health is 0.24 SD, and mental health is 0.23 SD. By way of magnitude, these seventeen-year economic impacts of the TUP program are higher than at year ten, with the consumption treatment effect doubling relative to year ten, asset ownership rising to 1.4 times, income to 1.19 times, and food security to 1.7 times its year-ten magnitude, with stable effects on physical and mental health. Treated households consumed 28% more per capita and earned 55% more than the control group ($940 versus $605 in monthly household income).

In a second important result, we find that, for key economic outcomes, gains extend throughout the distribution and are not confined to households that started with higher initial wealth. For consumption, income, and asset wealth, the distribution in the treated group first-order stochastically dominates that in the control group. While impacts are larger among households that started with more assets, we find positive effects throughout the distribution. The finding of a rightward shift in the treated distribution provides important insight into the ongoing debate on poverty traps (e.g., Jee (2025); Karlan et al. (2026)). While our finding does not rule out a poverty trap—the program may, for example, have pushed nearly all households across the relevant threshold—it nonetheless answers a distinct, policy-relevant question: did the long-run benefits of TUP extend throughout the wealth distribution? The answer is yes.[3]

Third, unlike in standard models of poverty traps, where there is a single new source of income that represents the exit from the trap, the source of additional income for the treated households evolves over time. While at year three treated households earned more predominantly through the transferred livestock asset, by year ten the main source of higher income became more lucrative wage employment and migration opportunities that arose naturally in the economy. By year seventeen, the treatment effect on wage income remains similar to year ten, but microenterprise income emerges as a major new source of the overall income gain. Monthly microenterprise earnings among treated households grew from a $46 treatment effect at year ten to $254 at year seventeen, a

[3]New results more formally establish gains across treated households by estimating village-level treatment-versus-control effects, available upon request.

128% increase over the control group that accounts for 60% of the total treatment effect on income (compared to only 23% at year ten).

Why do treated businesses earn more relative to the control group now than at year ten? We explore three possibilities: that gains are concentrated in a narrow set of industries, driven by shifts into more lucrative business activities, or driven by a small number of highly successful businesses. The evidence points against all three explanations and instead points toward treated households earning more across all businesses categories. Treatment effects on business income shift the entire business income distribution right relative to the control group, rather than being concentrated in outlier businesses. Effects are not concentrated in a particular industry, and arise both among households that continue operating existing businesses and those that transition into new businesses. Treated households earn substantially more both when maintaining existing businesses (166% higher revenues) and when switching into new ones (160% higher revenues). Together these facts imply that all categories of treated businesses (both new and old, and across industries) are systematically doing something different: investing more, using more inputs, or raising productivity.

Treated businesses are indeed larger, but they are also more productive. Treated businesses employ 2.77 more labor days per month (16% increase), but do not use more productive capital or increase investment. This relatively minor increase in labor inputs coupled with no detectable change in capital or inputs cannot account for the dramatic increase in treatment business revenues (128%), suggesting an important role for productivity. To estimate the treatment effect on productivity, we impose a standard Cobb-Douglass production function in capital, labor, and materials with a Hicks-neutral productivity shock. We back out TFP given measured revenue, labor, capital, materials, and calibrated input elasticities using comparable microenterprise estimates from Sri Lanka and Ghana (Janes et al. 2025). The TUP program raises TFP by approximately 25%. Even absent structural assumptions, we show that treated households generate substantially more revenue from observably similar inputs in observably similar businesses compared to the control group, implying higher productivity.

Our evidence points to an important intergenerational mechanism driving higher productivity in treated businesses. Younger household members increasingly participate in family enterprises and devote more time to them. Young adults aged 17–30 (aged 0-18 at baseline) spend 27.6 additional minutes per day working in household businesses, with no comparable increase among older household members or returned migrants. Among households operating businesses, younger household members spend approximately 50 additional minutes per day in business activity and 70 additional minutes per day in paid labor. In fact, greater participation by young adults in household businesses is the only observable difference in inputs between treated and control businesses. These findings show that treated businesses are not merely larger but also more productive, at least

in part because the next generation contributes more effectively to them.

Turning next to shocks that could alter the impact of these programs: we first examine the intergenerational transmission of gains (or not) of the TUP program. We examine the economic outcomes, education, and life satisfaction of household members who were children at baseline. Seventeen years later, children in treated households earn $148 more per month (2023 PPP), driven almost entirely by higher household business income. However, we find no evidence that treated children attain more education, whether among the original cohort of children observed over time or among contemporaneous children observed at each endline. Finally, consistent with their higher earnings, treated children exhibit higher life satisfaction than the control group. We measure how children's lives compare against their aspirations. Children in treated households are 22 percentage points more likely to report that their occupation exceeded their aspirations at age fifteen and 13 percentage points less likely to report that their occupation fell short of aspirations.

A final question we address is whether large negative shocks can slide households back into poverty, eroding the long-run gains of the program. One such shock was the Covid-19 pandemic, which struck India between our ten- and seventeen-year endlines. We find substantial economic losses for both treated and control households, but also remarkable recovery to pre-pandemic levels by mid-to-late 2022, with no treatment effects in either losses or recovery. During the pandemic, roughly half of households had a member lose a job, 23% of farm businesses and 62% of non-farm businesses temporarily closed, and 81% of households with a migrant worker reported migrant job loss. Yet recovery was also remarkably strong. Nearly all households had a member return to work within six months (91%), most businesses eventually reopened, and households primarily smoothed consumption through borrowing and other coping mechanisms rather than asset sales. Thus, while the pandemic temporarily lowered outcomes for both treated and control households, the treatment effects of the TUP program remained intact.

Together, these findings show that the Graduation approach placed households on a persistently higher economic trajectory. Treatment effects grew between years ten and seventeen, remained intact through the Covid-19 pandemic, and generated benefits for the next generation. These sustained and growing long-run effects are consistent with the program unlocking a poverty trap for treated households, with benefits persisting through the COVID-19 shock and extending to the next generation.

This paper contributes new seventeen-year evidence to a literature studying the medium-to-long-term impacts of graduation programs, which find positive effects four years (Banerjee et al 2015), seven years (Bandiera et al 2017), and ten years (Balboni et al. 2021, Banerjee et al 2021) after the asset transfer. Studies in South Asia find positive effects, while a long-run evaluation in Ethiopia finds that the control group has caught up (Barker et al. 2024). We are the first to report effects after seventeen years, and are the only setting able to do so since the control group has

remained a "pure control" over time. The seventeen year evaluation also helps us provide unique insight on the intergenerational impact of the TUP program, and the response of households to negative shocks induced by the Covid-19 pandemic.

2 Experimental Design and Data

The TUP intervention was implemented by the NGO Bandhan in Murshidabad district of West Bengal, India starting in 2007, following the model pioneered by BRAC in Bangladesh. Bandhan began by identifying the poorest households across 120 hamlets in two steps. First, a participatory appraisal exercise by village residents ranked households into five wealth quintiles. Eligible households were then drawn from the bottom wealth quintile and had to satisfy at least five of seven criteria (including mandatory criteria i and ii): (i) an able-bodied female member to manage the asset, (ii) no credit access, (iii) landholding below 0.2 acres, (iv) no ownership of productive assets, (v) no able-bodied male member in the household, (vi) presence of school-aged children working instead of attending school, and (vii) primary source of household income being informal labor or begging. The 978 households deemed eligible were stratified by hamlet and randomly assigned to the treated group (514) or control group (464), and 266 treated households ultimately accepted treatment. Given that only a subset of treated households received treatment, all results report intent-to-treat effects of the program.

The TUP intervention bundled several components. The first was an asset transfer intended to support income-generation among households. Treated households chose a productive asset from a menu of options: 2 cows, 4 goats, 1 cow and 2 goats, or inventory for a non-farm microenterprise. About 82% of households chose livestock as their asset, with 52% choosing goats, 30% choosing cows, and 11% choosing to receive microenterprise inventory. Assets were always transferred to a female member of the household. Second, treated households received weekly consumption support for 30 to 40 weeks[4]. This weekly allowance of 90 rupees ($11.18 in 2023 US dollar PPP) was designed to prevent households from selling their assets to finance consumption. Finally, beneficiaries received access to savings accounts and weekly visits from Bandhan staff for 18 months. The NGO visits trained households on how to generate income from their chosen asset, and also delivered life skills coaching and health information. Bandhan had virtually no contact with beneficiary households starting 18 months after the transfer of assets, except for tracking during follow-up surveys.[5]

Our data comprise six waves of household surveys conducted from 2007 to 2024. The base-

[4] 30 weeks for goat recipients and 40 for cow recipients.

[5] Our 2024 data collection revealed that 41 control households or roughly 9% of the control group had received the TUP treatment several years after the initial intervention. Asset transfers to control households would lead us to underestimate the benefits of the program.

line survey, administered prior to asset distribution in 2007–2008, recorded both household-level outcomes, including demographics, consumption, food security, asset ownership, income, and financial inclusion, as well as adult-level outcomes including time use, physical well-being, and mental well-being. Treated and control households were balanced on all characteristics and outcomes at baseline (Banerjee et al. 2021). We then track the same outcomes in five follow-up waves administered 18 months, 3 years, 7 years, 10 years, and 17 years after the transfer of assets. The seventeen-year endline also tracked the impact of the COVID-19 pandemic on households between March 2020 and December 2021 via a new module covering these households' consumption, job loss, business loss, and migration.

The key contribution of this paper is adding the seventeen-year endline survey collected between September 2024 and January 2025. The survey provides insight on both original and new household members, and captures the program's intergenerational impact. Given the long time elapsed between baseline and the present, we invested substantial effort in tracking the original study households. We surveyed all surviving household members and interviewed the remaining household members if the original beneficiary was deceased.

This fifth endline survey tracks 85% (835) of the original 978 households. By way of comparison the 18 month endline tracked 83% of study households, three-year tracked 89%, seven-year tracked 88%, and ten-year survey tracked 90% of original study households. Attrition at year seventeen is balanced by treatment status and treatment interacted with baseline characteristics (Appendix Table **??**). Our inability to interview the remaining 143 households (15%) was driven by permanent migration out of the study area (38%), refusal (40%), and inability to contact the household (22%).

Appendix Table A.1 describes summary statistics at baseline and shows balance in baseline characteristics. All values are reported in 2023 USD adjusted for PPP. Treated households consumed on average $49.4 per capita per month, while control households consumed $49.9. Both treated and control households had respectively monthly incomes of: $32.0 and $20.8 from nonfarm microenteprises, $107.6 and $99.2 from wage income, and $0.1 and $-0.1 from agricultural profits. Treated households had 3.9 members on average in 2007, including 1.6 children under 16, 1.9 working-age members (17–55), and 0.43 elderly members (56+); control households had 3.8 members on average in 2007, including 1.5 children under 16, 1.9 working-age members (17–55), and 0.36 elderly members (56+). Appendix Table A.4 confirms that treated and control households remained balanced on age and gender at the seventeen-year endline. By 2024, the average household had 3.8 members, with far fewer children (0.6) and more working-age (2.3) and elderly (0.9) members, reflecting the aging of the original sample.

3 Results

3.1 Seventeen-Year Effects of the TUP Program

Empirical strategy To maintain continuity and comparability with earlier evaluations of the TUP program, we replicate the analysis from the ten-year endline in Banerjee et al. (2021) and extend it to seventeen years. The main outcomes include five household-level indices capturing the financial wellbeing of households (asset ownership, per capita consumption, food security, income, financial inclusion) and four adult-level indices (physical health, mental health, productive time use, and political involvement) constructed identically to prior waves (Banerjee et al. 2015, Banerjee et al. 2021). Specific variables included in each index are described alongside results below.

We construct each index by first constructing z-scores of each component variable, that is, subtracting the baseline mean and dividing by the baseline standard deviation; then averaging across variables and once again standardizing the resulting index to baseline value. A index value of zero thus indicates the household is at the baseline average and treatment effects are reported in baseline standard deviation units. The income and revenue index, for which some components lack baseline data, is standardized to the control mean and reported in control-group standard deviation units. All monetary outcomes are reported in 2023 USD adjusted for purchasing power parity.

The following regression estimates the intent-to-treat effects of the TUP program:

$$Y_{iht} = \alpha_t + \beta_t \,\text{Treat}_i + \kappa_t Y_{ih,0} + \gamma_{ht} + \varepsilon_{iht} \tag{1}$$

where Y_{iht} is the outcome for household i in hamlet h at survey wave t, Treat_i is an indicator for treatment assignment, γ_{ht} are hamlet fixed effects to account for stratification at the hamlet-level, and $Y_{ih,0}$ is the baseline outcome when available. Standard errors are heteroskedasticity-robust for household-level outcomes and clustered at the household level for adult outcomes.

Persistence and growth of effects between years ten and seventeen. Figure 1 and Table 1 track treatment effects of the TUP program over time, at 18 months, 3 years, 7 years, 10 years, and 17 years after the asset transfer. Figure 1 extends the ten-year evaluation in Banerjee et al. (2021) to seventeen years, while Table 1 extends the three- and ten-year evaluations over time (Banerjee et al. 2015, Banerjee et al. 2021).

Our main finding is that the TUP program had large effects on all measures of economic wellbeing, health, and mental health even seventeen years after treatment. A previous evaluation showed that treatment effects grew between eighteen months and seven years and persisted through year ten (Banerjee et al. 2021). We find that the gains of year ten grew further until year seventeen, such

that treatment effects at year seventeen are larger than all prior waves.

Consumption and Income Over Time Figure 1, Panel A begins by plotting average monthly per capita consumption over time. Seventeen years after the TUP asset transfer, treated households consumed on average $154 (2023 USD PPP) per capita per month compared to $120 in the control group (a 28% treatment effect). These consumption levels rose from ten-year monthly per capita consumption of $117 for treated households and $98 for the control group (a 19.3% effect at year ten). The treated-control gap at seventeen years thus exceeds the gap found in all previous waves in both dollar terms and percentages (15% effect at eighteen months, 10.7% at three years, and 20% at seven years). The seventeen-year levels of consumption indicate that both treated and control households escaped poverty relative to the World Bank's international poverty line of $3.00/day (a feat they had also managed to achieve by year ten). However, only the treated group exceeded the more demanding poverty threshold for lower- and middle-income countries ($4.72/day).[6]

Figure 1, Panel B tracks the evolution of a second important marker of poverty, the share of households where every member gets enough to eat every day. Both groups improved since year ten, with the control group improving from 70% to 79% and treated group from 80% to 89% by year seventeen. The treatment effect remained statistically significant but unchanged in magnitude relative to at ten years.

Figure 1, Panel C depicts monthly household income. Control households' income stayed flat between years ten and seventeen (roughly $605/month), while treated households increased income from $829 to $940. The income advantage of treated households thus widened further over this period. The control group's ability to increase consumption despite stagnant income is partially explained by a 2023 reform that made staple grains completely free under India's National Food Security Act, which almost all sample households participate in (790 out of 835).[7]

Household outcomes Table 1, Columns 1–5 report the treatment effect of the TUP program on five measures of household economic wellbeing: asset ownership, per capita consumption, food security, income, and financial inclusion. We find positive effects on all five margins at seventeen years, with effects exceeding those at ten years.

Column 1 constructs an asset index via principal component analysis over livestock, productive assets (including bicycles or sewing machines), and household durable goods (such as refrigerators and fans). There is a treatment effect of 0.49 SD ($p < 0.01$) on asset ownership at seventeen years, growing from 0.35 SD at year ten. Column 2 reports effects on per capita consumption. The

[6]The World Bank defined the typical poverty line in lower-middle-income countries as $4.20/day in 2021 USD PPP ($4.72 in 2023 PPP). The $3.00/day "international poverty line" reflects the typical national poverty line in the poorest countries, i.e. the absolute poorest globally. Source: `https://datatopics.worldbank.org/world-development-indicators/themes/poverty-and-inequality.html`

[7]790 out of 835 (95%) of sample households are enrolled in India's Below Poverty Line ration system for essential commodities.

treatment effect on per capita consumption grew from 0.64 SD at year ten ($p < 0.01$) to over 1.27 SD by year seventeen ($p < 0.01$), a substantial increase over all prior waves. Column 3 combines six measures of food security: that everyone in the household ate enough food every day, no household member went without food for a day in the last year, no one skipped a meal in the last year, no child skipped a meal, no adult skipped a meal, and everyone in the household regularly ate two meals a day. Treated households are 0.22 SD more food secure at year seventeen ($p < 0.01$) compared to 0.13 SD at year ten.

The income and revenues index (Column 4) captures earnings from livestock, microenterprise, and wage- and self-employment, including remittances. The value of in-kind payments is treated as wage income. The treatment effect on income rose from 0.26 SD at year ten to 0.32 SD ($p < 0.01$) at year seventeen. Financial inclusion (Column 5, monthly loans and savings) shows a treatment effect of 0.28 SD, which is not statistically significant, consistent with findings at years 7 and 10.

To assess whether the TUP program disproportionately impacted certain types of consumption, such as food versus non-food consumption or durable versus non-durable goods, Table 2 disaggregates effects on consumption and food security into their individual components. We find positive effects on all categories of consumption: treated households spent 27% more on food consumption, 35% more on non-food, and 57% more on durable consumption (Columns 3-5). All measures of food security improved (Columns 6-8), with effects exceeding or comparable to effects at year ten.

Adult outcomes Table 1, Columns 6–8 report effects on outcomes for individual adult members: productive time use, physical health, and mental health. We find positive effects on all dimensions.

The physical health index combines individuals' perceived health, whether the individual missed a day of work in the past month due to poor health, and a score based on the individual's ability to complete daily activities. Physical health (Column 6) is 0.24 SD higher in treated households ($p < 0.01$), a modest increase from 0.19 SD at year 10. The mental health index aggregates life satisfaction, feelings of sadness, and periods of worry. Mental health (Column 7) is 0.23 SD higher ($p < 0.05$), up from 0.20 SD. Productive time use (Column 8), captured by minutes per day spent on income-generating activities, shows a 0.96 SD treatment effect ($p < 0.05$). We find no treatment effect on political involvement consistent with all prior waves.

The evolution of income sources A recurring theme across prior study waves has been the evolution of treated households' income-generating activities. At 18 months, gains came primarily from the transferred livestock asset; by year three, treated households had diversified into non-farm microenterprises; by years seven and ten, wage income, especially migrant remittances, became the largest source of income gains for treated households.

Year seventeen marks another shift in the composition of income sources for treated house-

holds. Between years ten and seventeen, treated households widened their earnings advantage over control households primarily through rising microenterprise income, while wage income grew more modestly.

Table 3 reports treatment effects on disaggregated income sources across all waves. Panel E focuses on year seventeen. All values are reported in 2023 USD adjusted for PPP. Monthly microenterprise income (Column 2) grew from a $46 treatment effect at year ten to $254 at year seventeen, or a 128% advantage relative to the control group up from 38% at ten years. This increase in microenterprise income accounts for 60% of the total treatment effect on income at seventeen years compared to only 22% of the effect at year ten. By contrast, wage income now accounts for 24% of the increase compared to 32% at ten years.

In addition to accounting for a larger *share* of the treatment effect on income, the surge in microenterprise activity also accounts for the *level* increase in effects between years ten and seventeen. While treated households earned on average $63 more microenterprise income than the control group at year ten, this grows to $211 by year seventeen. The increase in microenterprise income is driven primarily by three categories of enterprises: *bidi* making (cigarette-rolling), small shops and vending, and service-sector businesses such as barbering and rickshaw pulling (accounting for 96% of enterprises among the control group and 95% among treated households). Section 3.2 investigates two mechanisms underlying the rise in business income among treated households: in microenterprises by treated households versus the higher productivity of treated microenterprises.

Wages (Columns 5–6) remain the single largest income source for all households, accounting for 32% of total income among treated and 45% of total income among control households. Household wages are the sum of members' earnings from agricultural labor, casual labor, and salaried employment. We measure all household members' wages in a typical month, and use this to calculate total wages of all household members living at home. To this, we must add wages earned by household members who have migrated for work in the last year. Calculating this is not trivial for two reasons. First, we do not observe the migrant's full wage when he/she is gone for work, and instead only observe the cumulative amount that they sent back home in the last 12 months. Second, migration is often short-term or seasonal, so migrants work and earn at home part of the year. Following Banerjee et al. (2021), we bound migrant wage income under two assumptions. Column 5 treats annualized remittances as the migrant's full away-wage — a lower bound on migrant wages, since migrants may keep some earnings for personal expenses. Alternatively, Column 6 uses the migrant's self-reported typical-month earnings, which is an upper bound on the migrant wage since a "typical month" averages over both time at home and time away and migrant wages are typically higher than home wage. Robust to different assumptions about migrant remittance shares, the wage treatment effect has modestly surpassed seven-year levels: treated households

earn \$75 (Column 5) or \$89 more (Column 6) per month from wages at seventeen years ($p < 0.01$ for both).[8]

On other minor income sources, we find that treated households earn \$24 (133%) more monthly from livestock (Column 1). The productive asset index (Column 4) shows a smaller treatment effect than its year-seven peak, driven more by control-group fluctuations than a decrease in asset ownership among the treated group. Remittance income has remained roughly flat at \$43/month for control households since year seven, and the treatment effect on remittances is no longer significant at year seventeen (Column 7). Finally, treated households rate their economic satisfaction 1.5 points higher on a scale from 1 to 10 ($p < 0.01$, Column 8).

In sum, the households assigned to receive the TUP program have maintained or increased their earning advantage over the control group across all income sources: microenterprise income, wage employment, and livestock ownership.. In year seventeen, treated households earned more predominantly as a result of more lucrative microenterprises (a 128% treatment effect, accounting for 60% of the full treatment effect on income), followed by wage employment (a 21% effect, accounting for 24% of the treatment effect on income). While wages remained the largest income source for all households, household microenterprises grew to become the second-largest and fastest-growing income source. Below we explore the factors driving the increase in business income among treated households.

Test for differential impact by gender. Table A.17 tests for any differential impact of the TUP program by gender on the nine main indices tracked across all endline surveys.

We use the following specification to test for gender differences in treatment effects:

$$Y_{piht} = \alpha_t + \beta_t^1 \,\text{Female}_p + \beta_t^2 \,\text{Treat}_i + \beta_t^3 \,(\text{Treat}_i \times \text{Female}_p) + \kappa_t Y_{ih,0} + \gamma_{ht} + \varepsilon_{iht} \qquad (2)$$

where Y_{piht} is an adult-level outcome from the main replication results (see Table 1 Columns 6-9) for person p in household i in hamlet h at survey wave t. These are indices for physical health, mental health, productive time use, and political involvement. The household-level indices aggre-

[8]For household members present at the time of the survey, wages are measured as total earnings in a typical month summed across all salaried or wage activities. Column 5 assumes that average monthly remittances represent the migrant's entire monthly away-wage. This is a lower bound: it excludes any earnings the migrant spent on personal expenses while away rather than remitting home. The migrant's wage contribution is calculated as annual remittances divided by 12, multiplied by the fraction of the last month during which he/she was away. Column 6 instead relies on a separate survey question asking how much the migrant earns in a typical month. Because migrants are often home for part of the year, this "typical month" blends earnings from time at home with earnings from time away; multiplying by the fraction of the last month away therefore overstates the pure away-wage, making Column 6 an upper bound. We follow the same strategy regardless of whether the person who migrated in the last year is at home or away at the time of the survey.

gate across all members and cannot be measured separately by gender. Equation 2 is the same as the main specification Equation 1, with the additional control for whether the adult is female (Female_p) and the interaction term $(\text{Treat}_i \times \text{Female}_p)$. β_t^3 is the coefficient of interest and captures any difference in how the TUP program impacts women compared to men.

The results find no significant gender difference in the TUP program's effects at seventeen years (Table A.17). Although it initially appeared that women in the treated group were reaping fewer physical health gains from the program compared to their male counterparts, this difference disappears by the seven-year endline. There are, however, large gender differences in the overall level of the outcomes. 18 months after asset transfer, women had 0.18 SD worse physical health, 0.10 SD worse mental health, 0.37 SD lower productive time use, and 0.32 SD lower political involvement. Despite women starting at a lower baseline, women and men appear to gain equally from the TUP program, , i.e., the magnitude of the treatment effect is the same.

Multiple hypothesis testing. Following the same method as in Banerjee et al. (2015, 2021), we account for the fact that with a large number of outcomes, some regressions might appear to show statistically significant results by chance. First, we aggregate outcomes into nine (pre-registered) indices presented in Table 1. Second, we calculate and report adjusted p-values to control for the expected proportion of rejections of the null that are Type I errors (i.e. erroneous findings of statistical significance) across the nine indices. Following the Benjamini-Hochberg step-up method from Anderson (2008), we calculate q-values for each outcome. A q-value of 0.01 signifies that if we reject the null for this particular outcome, then we expect we are erroneously rejecting the null in 1% of outcomes. Thus, a smaller q-value signals greater confidence that a rejection of the null is not erroneous, and we interpret q-value as an adjusted p-value.

Cost-benefit analysis. The TUP intervention is expensive to deliver. See Appendix A for detailed methodology of cost-benefit analysis, summarized here. Accounting for inflation, the total cost of program delivery in 2007 was $1,610 (2023 USD PPP), 56% of which was transferred directly to beneficiary households. After adjusting to net present value at 2024 for money spent in 2007 with a standard 5% social discount rate, we estimate the total cost per beneficiary household at the equivalent of $3,691 (2023 USD PPP).

We calculate program benefit as the annual household non-asset consumption at each endline, we multiply the average treatment effect on per-capita consumption from Table 2 Column 2 by the average household size (3.8) and 12 (months per year). We then similarly inflate the value of past benefits by year 17 by a social discount rate of 5%. To interpolate program benefits in years between endline surveys, we assume benefits persist at the same level as the most recent survey wave. We separately estimate the program's impact on asset ownership, but the program's impact

on asset ownership has always been negligibly small ($7 by year 17), and thus the TUP program's benefits are driven entirely by consumption gains. By year 17, the average household assigned to treatment consumes $1,741 more (USD 2023 PPP) than a control household.

Accumulating all measured program benefits since the experiment began, the cumulative program benefit at year 17 is $21,715 (USD 2023 PPP). If treatment effects persist at the same level for another 15 years then become zero, cumulative program benefit would rise to a net present value of $34,277. If the treatment effects persisted indefinitely–if the TUP program propelled treatment households to a permanently higher steady state–then total benefits would be $54,877 at net present value. In Banerjee et al (2021), we reported that the program had broken even by year 4 and paid for itself 2.8 times over by year 10. We now find the cost-benefit ratio is 588% at year 17 and would be 1,487% if current benefit levels are sustained in perpetuity. The TUP program has thus paid for itself 4.9 times over, and remains highly cost-effective.

3.2 Channels of persistent treatment effects

Household businesses Section 3.1 shows that the seventeen-year effects of the TUP program on household income and revenue are primarily driven by business income. We find that treated households are both more likely to operate businesses and earn more from them. On the extensive margin, treated households are 9.9 percentage points (29.7%) more likely than the control group to have a business at year seventeen ($p < 0.01$; Appendix Table A.6). On the intensive margin, we find that the program shifted the entire distribution of business income to the right for treated households relative to the control group (Figure 2). To evaluate statistical significance across the distribution, we estimate treatment effects at selected quantiles of the business income distribution—10th, 25th 50th, 75th, and 90th percentiles. We find positive effects at each quantile (Appendix Table A.7 and Table A.8). Thus, the treatment effect on business income was broadly distributed across businesses rather than driven by growth in a few outlier businesses.

We next probe whether higher business income among treated households reflects differences in business activity. First, treated households could have different types of businesses at baseline; second, they may systematically change their business composition; finally, gains in business income may be concentrated in certain types of business activities. First, treated households could have different types of businesses compared to the control group; for example, they may operate in different industries at EL4, and some industries may have grown more between EL4 and EL5 regardless of treatment status. Second, treated households may systematically change their business composition, such that treatment effects are driven by changing or new businesses rather than expanding existing ones. Finally, treatment effects could be concentrated within specific business categories. Even conditional on baseline business type (e.g., shopkeeping or *bidi* or cigarette-rolling), treated households may especially succeed in expanding certain businesses, for instance

because they were better able to capitalize on growth opportunities in those sectors (e.g., obtain cheaper credit for shops).

First, baseline differences in business composition do not drive results: we find overlapping and statistically indistinguishable distributions of EL4 industries between treated and control households. Second, on changing business composition, the increase in business income is driven by both existing and changing businesses (Table 5). Treated households earn significantly higher revenues when maintaining the same business over time (166% increase) and when transitioning to a new business (160% increase), suggesting that the result is not driven solely by business persistence or switching. Finally, Table 4 examines if gains are concentrated in particular business categories. There is no evidence of concentration of gains and instead we find positive treatment effects in all major categories (Table 4). The three main business activities in West Bengal villages are *bidi* making (cigarette-rolling), shops and vending, and service businesses such as barbering, tutoring, and rickshaw pulling (18%, 19%, 20% of households respectively). Table 4 shows positive treatment effects on income across all three business activities. Among households operating *bidi* businesses, treated households earn $301 more per month ($p < 0.01$), while treated service businesses earn $560 more ($p < 0.01$), roughly 2.5 times the revenue of comparable control businesses. Treated shop businesses earn $106 less than the control group, albeit this effect is not statistically distinguishable from zero. The evidence also reveals that both treated and control businesses became more lucrative between years ten and seventeen, with the control group earning 45% more in this period (Table 3, Column 2). However, revenue among treated households grew substantially faster than the control group.

Together, the findings so far show that higher business income in the treated group is not driven by differences in original business composition, shifts in business composition, or gains concentrated in a narrow set of business (as we see all business types grow and the full distribution of business income shift rightward). Thus, treated households appear to be doing something different across all businesses categories: either investing more, using more inputs, or raising productivity.

What drives the treatment effect on business income across all business categories? Treated businesses could be larger or more productive. We find a role for both explanations, with productivity gains being a larger driver of higher business income. Appendix Table A.10 explores business size: inputs and output. Treated businesses utilize 2.77 more labor days per month, a 16% increase relative to the control mean of 17.04 days ($p < 0.05$). However, treated businesses have neither significantly more productive assets nor more valuable productive assets.[9] Treated businesses are thus larger in labor but not capital inputs.

However, this extra labor used by treated businesses cannot entirely drive the increase in busi-

[9]Productive assets are defined as equipment and machinery, furniture, small or large tools, large and small vehicles, and other durable goods, for example sewing machines, fridges, carts and cooking pots

ness revenue, and treated businesses are also more productive compared to the control group. In particular, while treated businesses generate 128% higher revenues compared to the control group, their labor inputs are only 16% higher and productive capital is not statistically significantly (or economically meaningfully) higher than the control group. The 16% increase in labor days is thus too small to plausibly account for the larger (128%) increase in revenue, indicating higher productivity.

To estimate the treatment effect on business productivity, we impose a standard Cobb-Douglas production function and back out productivity given measured inputs and revenue (Technical Appendix B). We assume a standard production function, $Y_{it} = A_{it} \cdot K_{it}^{\beta_k} \cdot L_{it}^{\beta_l} \cdot M_{it}^{\beta_m}$, where output is a function of capital, labor, material inputs, and total factor productivity A_{it}. Because our data do not allow us to estimate factor elasticities β_k, β_l, and β_m given no instrument for (KLM), we calibrate them to Janes et al. (2025), who estimate these parameters among household microenterprises in low-income settings (Sri Lanka and Ghana) using similar surveys (methodology discussed in Technical Appendix B)[10]. Reassuringly, the authors find similar elasticities across the two countries. We assume that the production function and elasticities remain constant across treatment groups and between years ten and seventeen; thus the only household-level variation in output comes from differences in inputs (K_{it}, L_{it}, M_{it}) and productivity (A_{it}).[11,12] Using observed inputs and revenues, we recover implied total factor productivity as a residual. Appendix B describes the procedure in detail.

Treated households are more productive than the control group. The analysis finds a 28.5% treatment effect on productivity for the highest-earning business ($p < 0.05$) and 24.3% treatment effect across all household businesses ($p < 0.10$) (Appendix Table A.13). These results show that treated businesses have become markedly more productive over time, even relative to endline four. In Table A.14 we see that using the range of elasticities reported in Janes et al. (2025), TFP increases by 21 to 32% in treated businesses across specifications.

Why are treated businesses more productive than the control group? Our evidence points to an important role for the next generation, whereby younger household members devote more time

[10] Janes et al. (2025) estimate a Cobb-Douglas production function for microenterprises in Sri Lanka and Ghana using two standard methods from industrial organization: Blundell and Bond (1998) and Gandhi, Navarro, and Rivers (2020) (GNR). Their sample covers small, owner-operated firms across a range of industries — approximately 30% small-scale manufacturing, 30% retail, 15% services, and the remainder in specialty trade — with capital stocks below $1,000 and no paid employees. The size, labor use, and industrial composition of their samples in Sri Lanka and Ghana are very similar to our sample in West Bengal.

[11] Assuming similar production functions by treatment status is not unreasonable given the overlapping and nearly identical distributions of business activities between treated and control households. The production function may differ by business type, e.g., different for shops versus service businesses. The Appendix shows robustness to restricting the analysis to shop businesses, which most closely resemble microenterprises in Janes et al. (2025).

[12] Assuming similar production functions between years ten and seventeen is only necessary to compare treatment effects on productivity at EL4 relative to EL5. Below we argue that the Janes et al. (2025) procedure estimates β in a setting very similar to our study sample in 2025, and provide reliable treatment effects on productivity at EL5.

to household businesses. Young adults aged 17–30 in treated households spend 27.6 more minutes per day working in household businesses ($p < 0.10$), with no comparable increase among older age groups or former migrants who have returned in the wake of COVID-19 (Table 6).[13] Among households with active businesses, we find a treatment effect of 50 minutes per day for young adults' time spent in business activity and 70 minutes per day on time spent in paid labor. Combined with the fact that treated households do not run different types of businesses, and do not invest more in businesses, these findings suggest that treated businesses are more productive in part because younger household members devote more productive time to them.

In sum, treated households earn greater business revenue compared to the control group seventeen years after the transfer of TUP assets, driven both by greater labor inputs and higher productivity. The higher productivity of treated businesses is driven by the greater involvement of the younger generation in running businesses.

Robustness We rule out several competing explanations that would lead the productivity result above to reflect a spurious conclusion rather than a treatment effect on "A". Specifically, we show that even if any concern about misspecified production functions or mismeasurement prevent us from obtaining an unbiased structural estimate of "A", the evidence strongly shows treated households generating substantially more revenue from observably similar inputs in observably similar businesses compared to the control group.

First, the micro-enterprise production function may have changed over time, leading to mismeasured input elasticities of output (βs). Changes across endlines within our settings are not a threat, as our main argument for a positive treatment effect on TFP relies only on treatment-control differences at the 17 year endline in 2024 (Appendix Table A.13). However, our β parameters from Janes et al. (2026), who use data from 2005 (Sri Lanka) and 2008 (Ghana), would lead to mismeasurement if micro-enterprise production functions changed systematically between 2005 and 2024. Importantly, this temporal drift will bias the ATE on TFP only if there is a treatment effect on input quantities.[14] The only detectable treatment effect on inputs is a 16% (0.148 log point) increase in labor hours (Appendix Table A.10). In order for mis-measured β_l to fully explain 24-28.5% TFP effect, it would require a true labor elasticity $\beta_l \in [1.8, 2]$[15]. Such an elasticity would

[13] There is no role for former migrant members in driving higher business revenues—an extremely small fraction of businesses employ migrant members ($< 18\%$) and there is no treatment effect on migrant member use for treated businesses (Table 6).

[14] Let $\beta_k^*, \beta_l^*, \beta_m^*$ denote the true 2024 West Bengal elasticities, which may differ from the calibrated $\beta_k, \beta_l, \beta_m$. Let $\alpha^{\hat{a}_{it}}$ denote the measured ATE on TFP, and $\alpha^{a^*_{it}}$ denote the true ATE of TFP. Our recovered residual is $\hat{a}_{it} = a^*_{it} + (\beta_k^* - \beta_k)k_{it} + (\beta_l^* - \beta_l)l_{it} + (\beta_m^* - \beta_m)m_{it}$, so $\alpha^{\hat{a}_{it}} = \alpha^{a^*_{it}} + \sum_{x \in \{k,l,m\}} (\beta_x^* - \beta_x)\,\Delta x$, where Δx is the treatment effect on log input x. Bias from an outdated β_x can therefore only enter through inputs that treatment actually moves.

[15] Fully explaining away our $\sim$25% TFP effect through mismeasured β_l would require $\beta_l^* - \beta_l \approx 0.25/0.148 \approx 1.7$, i.e. a true labor elasticity near 1.8–2.0 in 2024 West Bengal.

imply massive increasing returns to labor, and is far outside ranges found in firms large and small (Söderbom and Teal, 2004; Pavcnik, 2002). More intuitively, systematic changes in the production function are unlikely because the micro-enterprises in ours and Janes et al. (2026)'s settings have similar size, industry mix (small shops, handicrafts), and rely on the same very basic production technologies.

Second, bias could arise due to mis-measurement of inputs: capital value K, labor hours L, and input material costs M. Importantly, if inputs are measured equally noisily across treatment and control (classical measurement error), this would not bias our estimated treatment effect on TFP. Only differential measurement error by treatment could lead to an over-estimate[16]. In particular, the treatment group must systematically under-report inputs or over-report revenues. Treatment and control households operate same-industry businesses in the same villages, making differential estimation of capital value (e.g. due to rarer machines or price differences) unlikely. Attrition does not drive the bias because missing rates for input measurements are balanced. We cannot rule out other reasons treatment households could systematically under-report material costs or labor use, but conversely do not see intuitive reasons this would be so.

Finally, because we measure inputs and outputs in monetary rather than unit terms, our positive TFP treatment effect could be driven by the treatment household's ability to buy inputs at lower prices or sell outputs for more. All treatment effects are estimated among households living in the same village–making price differences unlikely–but treatment could in principle provide social network access to markets with different prices. Such a market access story would not contradict our core message that treatment business are able to convert a dollar of inputs into significantly higher revenues. Although not be TFP in a strict sense, treatment businesses would nevertheless be more skillful at revenue-generation.

3.3 Intergenerational Effects

A central open question regarding TUP programs is whether they send households to a permanently higher socioeconomic status. One key measure of "permanence" is whether the next generation—children and grandchildren—also display durable treatment effects. In our original 2007 sample, the median beneficiary was a 44 year-old woman, and the average treatment household had 1.6 children under 14 years-old. Seventeen years after the asset transfer these children have

[16] Write measured (log) inputs as $\tilde{k}_{it} = k_{it} + \varepsilon^k_{it}$, and analogously for l_{it} and m_{it}, where the ε's are reporting/measurement errors. Our recovered TFP residual is $\hat{a}_{it} = y_{it} - \beta_k\tilde{k}_{it} - \beta_l\tilde{l}_{it} - \beta_m\tilde{m}_{it} = a_{it} - \beta_k\varepsilon^k_{it} - \beta_l\varepsilon^l_{it} - \beta_m\varepsilon^m_{it}$. The treatment effect we estimate is

$$E[\hat{a}_{it}|T{=}1] - E[\hat{a}_{it}|T{=}0] = \underbrace{\big(E[a_{it}|T{=}1] - E[a_{it}|T{=}0]\big)}_{\text{true effect}} - \beta_k\Delta\bar{\varepsilon}^k - \beta_l\Delta\bar{\varepsilon}^l - \beta_m\Delta\bar{\varepsilon}^m,$$

where $\Delta\bar{\varepsilon}^x \equiv E[\varepsilon^x|T{=}1] - E[\varepsilon^x|T{=}0]$. Bias requires *differential* measurement error across arms ($\Delta\bar{\varepsilon}^x \neq 0$).

become young adults and we track their economic activities, earnings, and life satisfaction.

To study impacts on the next generation, we define an "intergenerational sample" of individuals, household members who were aged between 0 and 18 years at baseline and who are surveyed at year seventeen. This intergenerational sample comprises 540 individuals aged 17-35 years in 2024. There is no differential attrition of children by treatment status: gender and age among the intergenerational sample is balanced by treatment status at both baseline and endline five (Appendix Table A.4 Panel A).

Table 8 (Panel A) first studies the TUP program's effect on children's economic activities and earnings. Children of treated households are no more likely than the control group to engage in paid employment. However, the program affects children's hours worked, the composition of their economic activities, and earnings. Treated children work 56 minutes more per day. They are 9pp more likely to work in the household business ($p < 0.05$), 11pp more likely to rear livestock ($p < 0.01$), and 10pp more likely to work in agriculture in the previous month than the control group ($p < 0.01$). Importantly, higher participation in these activities does not seem to "crowd out" participation in the most common outside option–wage labor–for the treatment children, who are no less likely to participate in wage work. These additional hours translate into higher earnings: we find a 148 USD per month treatment effect on children's earnings (2023 PPP) ($p < 0.05$). This higher income is driven entirely by household business income (treatment effect of 136.954 USD per month, $p < 0.05$), with no effects on other forms of income, consistent with our main finding that treated households earn more primarily through household businesses.

Second, we evaluate whether treated children receive more education than the control group. Table 9 tests for treatment effects on the extensive margin of school attendance (Panel A) and the intensive margin of grade level (Panel B) over time. The treatment effect regression 1 now includes an additional control for age. We track two samples of individuals: "contemporaneous children" (sample 1) includes a shifting sample of children aged 0-18 at each respective endline, and "original children" (sample 2) includes children aged 0-18 at baseline who are tracked over time. In particular, the "original children" were 0 to 18 years old at baseline and are aged forward over time. A potential selection concern is that the data only captures an individual's current education status rather than their highest grade completed. Treatment effect estimates will thus be biased if there is differential attrition of educated individuals by treatment status. However, we find no evidence of differential attrition by treatment status interacted with education status in the previous wave, gender, or age (Table A.3).

Across samples and endlines, there is no treatment effect on education (Table 9). Treated children are no more likely to attend school or enroll in a higher grade level conditional on age. The average school attendance rate among current children is roughly 60%, so the null result does not reflect near-universal school attendance. Thus, although the children of original beneficiaries are

earning significantly more income, particularly through microenterprises, they are not significantly more educated than the control group.

The final outcome we examine is life satisfaction. An important component of adult well-being is how one's life stacks up against one's aspirations. Table 8 tests whether the next generation is more satisfied with their career path. Consistent with their higher income, children in treated households are 22pp more likely to say that their actual job exceeded their aspirations at 15 years old (Table 8, Panel B ($p < 0.01$)). They are also 13pp less likely to state that their job fell short of their aspirations ($p < 0.05$). In short, the program made the next generation more likely to follow a career path that met or exceeded their aspirations as teenagers.

3.4 Impact of COVID-19

A second open question regarding TUP programs is whether their effects persist in the face of large negative shocks or whether such shocks send households back into poverty. One such shock was the COVID-19 pandemic that struck India between the last two endlines (in 2017 and 2024), resulting in 45 million confirmed cases (the second-highest national count in the world) and over half a million deaths (Our World in Data 2020). For the rural households in our sample, the pandemic directly impacted health outcomes, disrupted economic activity, and sent migrant workers—whose remittances were an important source of income in 2017—back home. At year seventeen, we surveyed households to investigate their resilience to the COVID shock, coping strategies, and recovery from the shock. Throughout the survey, we ask about shocks during the"pandemic period" of March 2020 to September 2021, encompassing the deadliest phase.

These data on the COVID-19 experiences of ultra-poor households make two new contributions to the literature. First, independent of the TUP program, we contribute one of the only detailed household surveys on the experiences of ultra-poor households during the pandemic and their subsequent recovery[17]. The World Bank was gravely concerned that the pandemic plunged 90 million individuals back into extreme poverty (Mahler et al., 2022a). While many papers documented how households coped with the pandemic as it unfolded including through consumption reductions, borrowing, and asset sales (Egger et al., 2021; Josephson et al., 2021; Bundervoet et al., 2022), and some macro-economic work has diagnosed country-level recovery (López-Calva et al., 2026), granular household-level evidence on the recovery of poor households after 2021 remains scarce (Giovanis and Ozdamar, 2024; Lukwa et al., 2025). Second, the impact of COVID-19 on the TUP

[17] An incomplete list of other surveys includes: the World Bank's Panel Rapid Response Phone Survey in Kenya (2020-2022) (Sinha, 2022); The Thailand-Vietnam Socio-Economic Panel 2022 (Grote et al., 2026; Do et al., 2026); the National Income Dynamics Study–Coronavirus Rapid Mobile Survey (NIDS-CRAM 2020-2021) when combined with the National Food and Nutrition Security Survey (NFNSS, 2022) in South Africa (Lukwa et al., 2025). In a large cross-country data collection across 9 low- and middle-income countries, Egger et al. (2021) document livelihood loss and coping strategies in the early pandemic (until July 2020) but do not cover medium-to-long run recovery.

program's effects is of critical policy importance. On one hand, the TUP program may insulate households from negative shocks or speed recovery by providing a greater wealth safety net or better access to government aid programs. Alternatively, the COVID shock could have destroyed treatment households' gains, eroding the long-term durability of the program's benefits.

The main result is that both treated and control households experienced large economic losses (job loss, income loss, and business shutdown) during the COVID-19 pandemic, but also recovered to pre-pandemic levels. This striking pattern of full recovery reveals similar experiences between the treatment and control groups, rather than strong treatment effects. Given this, we report control group estimates, which best capture the magnitude of economic losses and recovery among ultra-poor rural households in our West Bengal context.

Appendix Table A.15 reports the economic losses and coping mechanisms of treated and control households during the pandemic, and the treatment effect between March 2020 and September 2021. We examine the effects of the pandemic on the employment of non-migrant household members (Panel A), business outcomes (Panel B), migrant household members (Panel C), asset sales (Panel D), and consumption smoothing (Panel E). On jobs, roughly half of all households had at least one member lose their job during the pandemic (Panel A). These losses were roughly equally split between temporary and permanent losses. Temporary losses happened where at least one member reported reduced hours, demotion, or being out of work for four or fewer months, but ultimately returned to the same jobs. Permanent losses happened where at least one member never came back to their pre-covid employment. Turning to business outcomes, 23% of control households with a business report a farm business closure and 62% report a non-farm business closure, with no treatment effect on either outcome (Panel B). Similar shares of households report losing business earnings. Among households with a migrant worker, 81% of control-group households report that a migrant member lost their job, and 55% of these losses were permanent (Panel C).

Next, we explore coping mechanisms via asset sales and consumption smoothing. Households did not sell assets to smooth consumption: the share of households selling livestock was 5% in the control group, while the shares selling land (0%) or durables (1.5%) were negligible. Instead, households smoothed consumption via other behaviors: the average household engaged in 5.3 out of 8 consumption-smoothing behaviors, including consuming goods planned for sale or later use, taking out loans, increasing foraging, drawing down savings, or postponing non-food essential consumption (Panel D).

Appendix Table A.16 examines recovery at the time of endline 5, September 2024, on the same outcomes: non-migrant household members (Panel A), business outcomes (Panel B), migrant household members (Panel C). On jobs, while most households had members regain employment, earnings losses from the pandemic were still substantial. Nearly all households had at least one member return to work (95%), with most returning to the same job (93%). However, the costs

of job loss remained large: the average job-loser spent 4.3 months unemployed, and only 55% recovered their pre-pandemic earnings level (Panel A). Businesses also re-opened: 100% of farms that closed and 91% of non-farm businesses eventually reopened, with average closure durations of 4.4 and 6.1 months, respectively (Panel B). The experiences of migrant household members were similar: nearly all had a job at the time of data collection (95%), despite a slightly longer average unemployment spell of 5.8 months (Panel C). Importantly, the pandemic permanently reduced migration: among households in which a migrant returned home, only 36% later re-migrated.

In sum, the COVID-19 pandemic brought temporary job losses and business closures for the vast majority of households, leading households to reduce consumption and smooth across time rather than sell off valuable assets. Although households did suffer an average 4-6 months of income loss, almost everyone regained their initial occupation and the majority regained the same earnings levels. This pattern of broad-based economic loss coupled with strong recovery has not, to our knowledge, been documented before.

The treatment group neither suffered less nor recovered better from the pandemic. Instead, the pandemic led outcomes to dip for both treated and control households, with both groups subsequently recovering to their pre-pandemic levels. As a result, the treatment effects of the TUP program remained intact despite the shock.

4 Conclusion

We examine the seventeen-year effects of the Targeting the Ultra Poor (TUP) Program in West Bengal, India through a randomized controlled trial with 978 households. The unique contribution of this study is both the long-run evidence, including intergenerational impacts of the program, and the fact that the control group has remained untreated over this unusually long duration. We find that the TUP program led to large and persistent gains in consumption (1.27 SD), income (0.3 SD), food security (0.2 SD), asset ownership (0.49 SD), physical health (0.24 SD), and mental health (0.23 SD) even seventeen years after the initial transfer of assets. Importantly, treatment effects grew substantially since the ten-year endline (for consumption to 2x the effect at ten years, assets to 1.4x, income to 1.19x, and food security to 1.7x), while effects on physical and mental health remained stable relative to the ten-year findings reported in Banerjee et al. (2021). TUP households thus consumed 28% more per capita and earned 55% more than the control group ($940 versus $605).

A recurring pattern across prior study waves has been the evolution of treated households' income sources, a pattern that continued through year seventeen. At year three, gains were predominantly driven by livestock income, at year ten by wage employment, but by the seventeen-year endline the majority of gains (60%) are driven by higher microenterprise income in treated house-

holds. We show that these effects are broad-based across households, rather than driven by outlier businesses, not concentrated in a narrow set of industries, and not driven by shifts into new business activities. Instead, both existing and new treated businesses are larger and more productive than those in the control group, with productivity approximately 25% higher. Our evidence points to an important intergenerational mechanism: younger household members devote more time to family businesses and appear to make them more productive. The sustained and growing long-run effects of the graduation approach in West Bengal are consistent with the program unlocking a poverty trap for treated households, with benefits persisting and expanding across generations.

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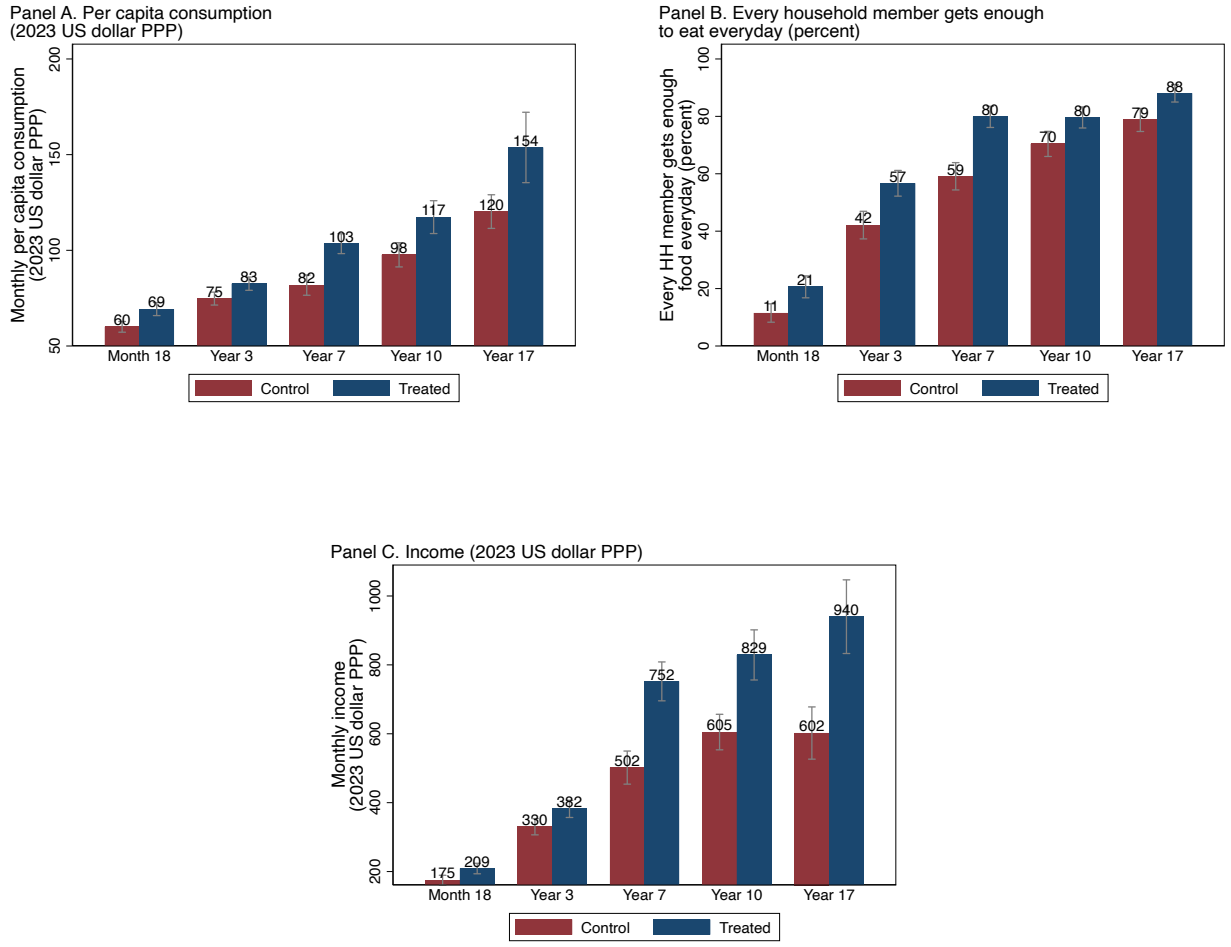
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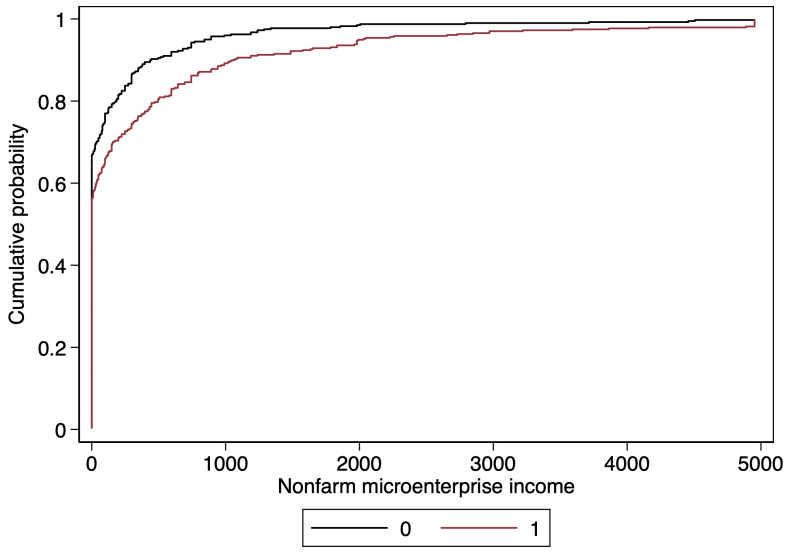
Figures

Figure 1: Consumption and Income over Time

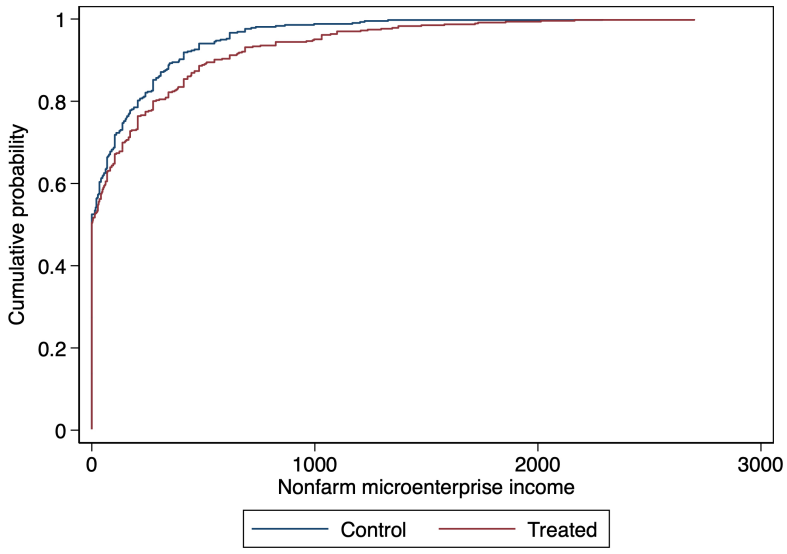


Note: Monthly per capita consumption and monthly household income are reported in 2023 USD corrected for purchasing power parity. Food security is reported in terms of the share of households reporting that every household member gets enough food every day. For per capita consumption, we constructed it using a detailed consumption module asking about household food, non-food, and durable purchases. Income sums over income earned by each individual member from various activities in a typical month, as reported in the household roster, and remittances received by the household. Ninety-five percent confidence intervals are reported.

Figure 2: Cumulative Density Function of Nonfarm Microenterprise Income



Panel A: Endline 5



Panel B: Endline 4

Note: Nonfarm microenterprise income is reported in 2023 USD corrected for purchasing power parity. 95% confidence intervals are reported.

Tables

Table 1: Effect on Household and Adult Outcomes

	Asset Index	Per-capita Consump-tion	Food Security Index	Income and Revenues	Financial Inclusion Index	Physical Health Index	Mental Health Index	Productive Time Use
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A. Endline 1 (18 months)				
Treatment	0.217	0.311	0.184	0.145	-0.004	0.061	0.115	0.285
	(0.111)	(0.076)	(0.048)	(0.075)	(0.042)	(0.028)	(0.029)	(0.049)
q-value	0.041	0.001	0.001	0.041	0.262	0.028	0.001	0.001
Control mean	-0.20	0.35	0.35	0.00	0.14	0.12	0.32	0.23
Baseline mean	-0.00	-0.00	-0.00	.	-0.00	-0.00	-0.00	-0.00
Observations	679	813	812	814	812	1,504	1,502	1,504
				Panel B. Endline 2 (3 years)				
Treatment	0.389	0.292	0.251	0.172	0.192	0.027	0.012	0.102
	(0.103)	(0.079)	(0.059)	(0.071)	(0.062)	(0.027)	(0.037)	(0.044)
q-value	0.001	0.001	0.001	0.015	0.003	0.160	0.334	0.018
Control mean	-0.25	0.85	0.94	-0.00	0.30	0.21	0.75	0.28
Baseline mean	-0.00	-0.00	-0.00	.	-0.00	-0.00	-0.00	-0.00
Observations	875	875	875	875	875	1,757	1,757	1,756
				Panel C. Endline 3 (7 years)				
Treatment	0.814	0.717	0.431	0.334	0.181	0.130	0.249	0.165
	(0.132)	(0.125)	(0.062)	(0.070)	(0.135)	(0.031)	(0.042)	(0.044)
q-value	0.001	0.001	0.001	0.001	0.047	0.001	0.001	0.001
Control mean	-0.46	1.09	1.09	0.00	0.67	0.57	1.09	-0.04
Baseline mean	-0.00	-0.00	-0.00	.	-0.00	-0.00	-0.00	-0.00
Observations	807	867	867	869	867	1,906	1,900	1,915
				Panel D. Endline 4 (10 years)				
Treatment	0.346	0.643	0.127	0.264	0.121	0.187	0.203	0.148
	(0.121)	(0.180)	(0.063)	(0.080)	(0.152)	(0.040)	(0.044)	(0.052)
q-value	0.005	0.002	0.020	0.002	0.105	0.001	0.001	0.005
Control mean	-0.26	1.62	1.21	-0.00	1.08	0.12	0.76	-0.02
Baseline mean	-0.00	-0.00	-0.00	.	-0.00	-0.00	-0.00	-0.00
Observations	885	885	885	885	885	1,229	1,229	1,229
				Panel E. Endline 5 (17 years)				
Treatment	0.485	1.273	0.219	0.315	0.278	0.237	0.234	0.096
	(0.130)	(0.408)	(0.057)	(0.078)	(0.287)	(0.040)	(0.043)	(0.054)
q-value	0.001	0.002	0.001	0.001	0.107	0.001	0.001	0.034
Control mean	-0.29	2.39	1.41	0.00	1.48	0.27	1.07	-0.00
Baseline mean	-0.00	-0.00	-0.00	.	-0.00	-0.00	-0.00	-0.00
Observations	833	835	835	835	835	1,486	1,486	1,486

Notes: Robust standard errors are shown in parentheses; they are clustered by household for adult outcomes. Columns 1–5 report household-level outcomes and columns 6–8 report adult-level outcomes. Section 3.1 describes the construction of each index. The asset index is constructed using principal component analysis and standardized relative to the baseline distribution. The remaining indices are constructed as z-scores of their components and standardized relative to the baseline distribution, except the income and revenues index, which is standardized using the endline control group's standard deviation. Productive time use is measured in minutes. We calculate q-values using the Benjamini-Hochberg step-up procedure across all nine outcome families, including the political involvement index. All regressions include baseline controls and hamlet-level fixed effects.

Table 2: Monthly Consumption and Food Security

	Per capita consumption, excl. migrant	Per capita consumption, hh avg. c for migrant	Per capita food consumption	Per capita non-food consumption	Per capita durable goods consumption	Everyone in HH gets enough food everyday	No one in the HH went a whole day without food	No children skipped meals
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A. Endline 1 (18 months)				
Treatment	9.204	9.985	6.533	2.599	-0.434	0.074	0.128	0.032
	(2.238)	(2.747)	(1.456)	(1.319)	(0.456)	(0.025)	(0.030)	(0.034)
Control mean	60.00	64.29	40.55	19.46	2.78	0.11	0.68	0.75
Baseline mean	49.63		32.60	17.05	1.19	0.11	0.28	0.51
Observations	813	813	813	813	813	812	811	613
				Panel B. Endline 2 (3 years)				
Treatment	8.626	9.051	3.681	4.886	1.074	0.141	0.038	0.085
	(2.330)	(2.975)	(1.402)	(1.388)	(0.469)	(0.034)	(0.023)	(0.025)
Control mean	74.77	81.14	45.73	29.04	2.35	0.42	0.85	0.86
Baseline mean	49.63		32.60	17.05	1.19	0.11	0.28	0.51
Observations	875	875	875	875	875	875	875	636
				Panel C. Endline 3 (7 years)				
Treatment	21.181	25.893	11.913	9.189	3.011	0.205	0.095	0.045
	(3.691)	(4.771)	(2.109)	(2.049)	(0.618)	(0.032)	(0.022)	(0.026)
Control mean	81.82	88.87	46.01	35.82	2.76	0.59	0.83	0.87
Baseline mean	49.63		32.60	17.05	1.19	0.11	0.28	0.51
Observations	867	867	867	867	867	867	867	546
				Panel D. Endline 4 (10 years)				
Treatment	18.997	25.032	10.016	8.998	5.301	0.075	0.029	-0.031
	(5.309)	(6.778)	(2.062)	(4.222)	(2.242)	(0.029)	(0.026)	(0.036)
Control mean	97.66	106.18	47.85	49.89	7.01	0.70	0.78	0.84
Baseline mean	49.63		32.60	17.05	1.19	0.11	0.28	0.51
Observations	885	885	885	885	885	885	884	451
				Panel E. Endline 5 (17 years)				
Treatment	37.618	38.174	14.086	23.472	3.301	0.085	0.055	0.038
	(12.071)	(12.911)	(2.330)	(11.299)	(1.934)	(0.027)	(0.024)	(0.038)
Control mean	120.24	131.73	52.63	67.62	5.78	0.79	0.84	0.85
Baseline mean	49.63		32.60	17.05	1.19	0.11	0.28	0.51
Observations	835	835	835	835	835	835	834	369

Notes: Robust standard errors are shown in parentheses. All values are in 2023 USD adjusted for purchasing power parity. Columns 1 through 5 report monthly per capita consumption. Column 1 excludes migrant workers from the household member count. Column 2 assumes migrant workers consume the household average. Columns 3 through 5 exclude migrant workers. Columns 6 through 8 report food security indicators. All specifications include baseline controls and hamlet-level fixed effects.

Table 3: Monthly Income and Revenue

	Livestock revenue	Nonfarm microenter-prise income	Self-employment (typ month)	Productive Asset Index	Wages: migrants remit 100% of wage	Wages: impute typical migrant earnings	Remittances	Self-Reported Economic Status (1-10)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A. Endline 1 (18 months)				
Treatment	12.498	10.931	23.070	0.444	5.970	7.410	0.000	0.204
	(2.853)	(5.495)	(7.049)	(0.086)	(8.204)	(10.376)	(.)	(0.071)
Control mean	4.06	44.16	56.16	-0.23	117.04	129.70	0.00	2.77
Baseline mean	0.00	16.50	19.87	-0.00			.	1.97
Observations	814	814	814	681	814	814	814	811
				Panel B. Endline 2 (3 years)				
Treatment	9.360	30.524	37.706	0.571	7.679	5.871	4.504	0.297
	(3.232)	(7.615)	(8.400)	(0.072)	(14.204)	(16.654)	(2.887)	(0.080)
Control mean	9.73	60.80	73.85	-0.30	244.98	265.47	15.70	3.36
Baseline mean	0.00	16.50	19.87	-0.00			.	1.97
Observations	875	875	875	875	875	875	875	875
				Panel C. Endline 3 (7 years)				
Treatment	33.216	83.787	131.909	0.795	92.201	108.464	10.809	1.575
	(6.284)	(17.417)	(18.458)	(0.083)	(27.169)	(31.348)	(7.864)	(0.141)
Control mean	11.81	110.25	125.82	-0.40	340.00	367.10	42.49	4.73
Baseline mean	0.00	16.50	19.87	-0.00			.	1.97
Observations	869	869	869	807	869	869	869	867
				Panel D. Endline 4 (10 years)				
Treatment	20.361	46.317	114.554	0.197	46.634	63.743	23.218	0.642
	(10.668)	(17.409)	(25.360)	(0.105)	(29.882)	(36.620)	(9.064)	(0.129)
Control mean	21.69	119.99	175.77	-0.10	395.98	432.39	44.85	4.03
Baseline mean	0.00	16.50	19.87	-0.00			.	1.97
Observations	885	885	885	885	885	885	885	885
				Panel E. Endline 5 (17 years)				
Treatment	24.235	205.282	254.455	0.475	75.344	88.998	7.483	1.500
	(7.877)	(49.273)	(50.837)	(0.085)	(32.469)	(42.882)	(8.105)	(0.175)
Control mean	18.30	173.24	198.58	-0.24	362.17	413.41	43.50	3.72
Baseline mean	0.00	16.50	19.87	-0.00			.	1.97
Observations	835	835	835	833	835	835	835	835

Notes: Robust standard errors are shown in parentheses. All values are in 2023 USD adjusted for purchasing power parity. Column 1 reports revenue from the sale of livestock or livestock products in an average month. Column 2 reports the sum of household members' earnings from microenterprise in a typical month, as reported in the household roster. Column 3 reports self-employment income in a typical month, calculated as the sum of income earned by each individual member from various self-employment activities, as reported in the household roster. Column 4 reports an index of productive assets, constructed by first performing principal component analysis on the constituent components and then creating a z-score relative to the baseline value of the index (that is, subtracting the baseline mean of the index and dividing by its baseline standard deviation); Section 3.1 describes the components of this index in detail. Columns 5 and 6 report two different ways of imputing the earnings of migrant household members, each added to each household member's locally earned wage income (earnings from agricultural labor, casual labor, and salaried employment, as reported in the household roster). Column 5 assumes each migrant remits 100% of their earnings, and uses the migrant's actual reported remittances as their imputed earnings. Column 6 instead assumes each migrant earns the typical local wage they would have earned working in the village, and uses that typical local wage as their imputed earnings. Column 7 reports the monthly average of remittances sent back by migrant household members; we do not collect remittance data at the 18-month survey. Column 8 reports a measure of self-reported economic status on a scale of 1 to 10. All specifications include baseline controls and hamlet-level fixed effects.

Table 4: Treatment Effect on Business Income by Business Type

	Nonfarm Microenterprise Income at Endline 4	Nonfarm Microenterprise Income at Endline 5
	(1)	(2)
Treated HH	-16.166	-23.580
	(15.846)	(39.507)
Bidi Business	114.378	108.444
	(27.844)	(76.496)
Treated HH × Bidi Business	45.898	301.326
	(39.858)	(136.397)
Shop Business	141.809	642.837
	(36.877)	(156.412)
Treated HH × Shop Business	54.963	-106.997
	(57.404)	(197.253)
Service Business	168.082	38.103
	(34.956)	(88.557)
Treated HH × Service Business	143.740	559.212
	(53.373)	(162.088)
p-value	0.308	0.551
p-value, Bidi business	0.000	0.157
p-value, Treated HH × Bidi business	0.250	0.027
p-value, Shop business	0.000	0.000
p-value, Treated HH × Shop business	0.339	0.588
p-value , Service business	0.000	0.667
p-value, Treated HH × Service business	0.007	0.001
Control mean	119.992	173.237
N	885	835

Notes: Robust standard errors are shown in parentheses. All values are in 2023 USD adjusted for purchasing power parity. Nonfarm microenterprise income is the sum of household members' earnings from microenterprise in a typical month, as reported in the household roster. Among treated households at endline 5 ($N = 435$), 18.4% own a bidi business, 23.4% own a shop business, 24.1% own a service business, 2.5% own another business (not bidi, shop, or service), and 51% did not have a nonfarm microenterprise. Among control households ($N = 400$), 17.75% own a bidi business, 14.5% own a shop business, 15.75% own a service business, 1.7% own another business (not bidi, shop, or service), and 60% did not have a nonfarm microenterprise. Other businesses include arts and crafts manufacture and shoe-making. All specifications include hamlet-level fixed effects.

Table 5: Treatment Effect on Business Income by Business Change

	Nonfarm Microenterprise Income at Endline 5			
	Business in Previous Wave	Stable Business	Changed Business	New Business
	(1)	(2)	(3)	(4)
Panel A. Treated and Control Restricted by Category				
Treatment	319.044	497.084	736.653	-147.698
	(85.278)	(187.770)	(363.221)	(822.050)
p-value	0.000	0.009	0.045	0.858
Control mean	228.594	298.280	460.508	528.964
Observations	434	141	119	88
Observations, control	201	67	46	31
Panel B. Only Treated Households Restricted by Category				
Treatment	348.425	393.928	660.203	-77.281
	(69.211)	(165.435)	(201.627)	(256.063)
p-value	0.000	0.018	0.001	0.763
Control mean	164.206	400.557	400.557	400.557
Observations	655	233	232	216
Observations, control	466	159	159	159

Notes: Robust standard errors are shown in parentheses. All values are in 2023 USD adjusted for purchasing power parity. Column 1 *(Business in Previous Wave)* corresponds to households who had a business at EL4, setting their EL5 business income to 0 if they had no business in EL5. Column 2 *(Stable Business)* corresponds to households where the highest-revenue business type was the same in both Endline 4 and Endline 5 surveys. Column 3 *(Changed Business)* corresponds to households where the highest-revenue business type changed between EL4 and EL5 surveys, but a business was present in both. Column 4 *(New Business)* corresponds to households who reported no business at EL4 but established a business at EL5. All specifications include hamlet-level fixed effects.

Table 6: Productive Time Use across Age Groups

	Minutes spent in business (17-30)	Minutes spent in business (30-50)	Minutes spent in business (50+)	Minutes spent in paid labor (17-30)	Minutes spent in paid labor (30-50)	Minutes spent in paid labor (50+)	Number of former migrants working in household business
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Endline 5							
Treatment	27.616	4.710	19.545	31.153	8.389	-15.654	0.024
	(15.436)	(14.398)	(13.296)	(25.303)	(21.248)	(20.104)	(0.020)
p-value	0.074	0.744	0.142	0.219	0.693	0.437	0.231
Control mean	49.990	71.448	42.796	119.490	150.159	113.199	0.070
Observations	409	524	431	409	524	431	835
Observations, control	196	252	211	196	252	211	400
Age group	1	2	3	1	2	3	-
Panel B. Endline 4							
Treatment	-1.214	1.082	26.964	-1.468	5.616	5.631	0.005
	(13.919)	(13.886)	(15.328)	(25.646)	(19.184)	(14.807)	(0.014)
p-value	0.931	0.938	0.079	0.954	0.770	0.704	0.709
Control mean	59.315	82.816	51.763	152.945	163.072	66.638	0.036
Observations	456	607	423	456	607	423	885
Observations, control	219	293	207	219	293	207	419
Age group	1	2	3	1	2	3	-

Notes: Robust standard errors are shown in parentheses. Columns 1 to 3 report aggregate time use, in minutes, for each age bin in household business activities over the last 24 hours; within each age bin, we sum work time at the household level and use the household-level total as the outcome. Columns 4 to 6 report the analogous time use spent in paid (wage or salaried) labor. Column 7 reports the number of former migrants working in any household business; we define a former migrant as a household member who had migrated for work in the previous endline wave (endline 4 for Panel A, endline 3 for Panel B) and who is now reported to work in the household business. In columns 1 through 6, observations reports the number of households that reported an adult in the indicated age range during adult survey data collection; in column 7, observations reports the number of households at endline 5. Columns 1 through 6 do not include hamlet-level fixed effects, due to insufficient variation within hamlet and age bin; column 7 includes hamlet-level fixed effects.

Table 7: Productive Time Use across Age Groups, Restricted to Households with an Active Business

	Minutes spent in business (17-30)	Minutes spent in business (30-50)	Minutes spent in business (50+)	Minutes spent in paid labor (17-30)	Minutes spent in paid labor (30-50)	Minutes spent in paid labor (50+)	Number of former migrants working in household business
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Endline 5							
Treatment	50.440	-17.627	30.900	69.901	4.592	-1.647	0.031
	(31.010)	(28.041)	(29.903)	(38.852)	(25.474)	(26.104)	(0.053)
p-value	0.106	0.530	0.303	0.074	0.857	0.950	0.557
Control mean	103.816	160.743	95.714	86.184	96.386	67.063	0.176
Observations	170	239	159	170	239	159	372
Observations, control	76	101	63	76	101	63	159
Age group	1	2	3	1	2	3	-
Panel B. Endline 4							
Treatment	6.765	6.346	53.573	-19.688	5.670	-1.917	0.019
	(24.214)	(24.152)	(30.122)	(32.070)	(21.602)	(15.697)	(0.033)
p-value	0.780	0.793	0.077	0.540	0.793	0.903	0.561
Control mean	104.211	148.427	107.921	116.842	101.608	34.494	0.075
Observations	237	301	186	237	301	186	434
Observations, control	114	143	89	114	143	89	201
Age group	1	2	3	1	2	3	-

Notes: The sample is restricted to households with an active business. Robust standard errors are shown in parentheses. Columns 1 to 3 report aggregate time use, in minutes, for each age bin in household business activities over the last 24 hours; within each age bin, we sum work time at the household level and use the household-level total as the outcome. Columns 4 to 6 report the analogous time use spent in paid (wage or salaried) labor. Column 7 reports the number of former migrants working in any household business; we define a former migrant as a household member who had migrated for work in the previous endline wave (endline 4 for Panel A, endline 3 for Panel B) and who is now reported to work in the household business. Observations reports the number of households. Columns 1 through 6 do not include hamlet-level fixed effects, due to insufficient variation within hamlet and age bin; column 7 includes hamlet-level fixed effects.

Table 8: Intergenerational Effects of the TUP Program on Occupation and Income

	Treatment	Control	Observations	Difference	p-value
Panel A. Employment and Income Indicators					
Productive activity last month	0.752	0.694	540	0.057	0.137
Salaried employment last month	0.034	0.033	540	0.000	0.974
Worked for a wage last month	0.604	0.616	540	-0.012	0.783
Worked on household business last month	0.215	0.128	540	0.087	0.008
Worked on tending livestock last month	0.319	0.207	540	0.112	0.003
Worked on agriculture last month	0.232	0.136	540	0.095	0.005
Total monthly income	470.274	322.063	540	148.211	0.040
Monthly income from salaried employment	16.362	37.110	540	-20.749	0.168
Monthly income from wage work	271.878	264.500	540	7.378	0.785
Monthly income from household business	191.262	54.309	540	136.954	0.040
Monthly income from tending livestock	2.592	0.454	540	2.138	0.156
Monthly income from agriculture	20.533	17.109	540	3.424	0.581
Minutes spent on productive activities in last day	251.880	195.462	339	56.418	0.029
Panel B. Aspiration Indicators					
Exceeded occupation aspirations	0.517	0.297	275	0.220	0.000
Fell short occupation aspirations	0.361	0.492	275	-0.132	0.027
Panel C. Financial Inclusion					
Belong to a microfinance group	0.307	0.237	469	0.070	0.092

Notes: All values are in 2023 USD adjusted for purchasing power parity. The number of observations varies across panels based on survey participation. Panel A (540 observations) includes children from the baseline household roster reported in the endline 5 household survey. Panel B (275 observations) is restricted to baseline children who completed the aspirations module of the endline 5 adult survey. Panel C (469 observations) includes baseline children who completed the adult survey. Income denotes earnings of each individual member from various activities in a typical month, as reported in the household roster. Wage work includes daily labor, salaried employment, and tending animals or doing housework in an outside household. Exceeded and fell short occupation aspirations are binary indicators (0/1) for how a respondent's current occupation compares with their aspirations at age 15, from the aspirations subsection of the adult survey. Appendix Table A.4 shows balance on baseline child characteristics between treated and control groups (number of children, gender composition, age). Appendix Table **??** shows no differential attrition by treatment status or by treatment interacted with baseline characteristics.

Table 9: Effect of Treatment on Children's Education

	Endline 1	Endline 2	Endline 3	Endline 4	Endline 5
	(1)	(2)	(3)	(4)	(5)
Panel A. Treatment Effect on School Attendance					
Sample 1: Children at each wave					
Treatment	0.005	-0.020	0.027	-0.044	0.002
	(0.026)	(0.025)	(0.026)	(0.028)	(0.035)
p-value	0.842	0.423	0.297	0.122	0.953
Control mean	0.564	0.641	0.685	0.649	0.663
Observations	1598	1625	1466	1217	843
Sample 2: Cohort aged 0–18 at Baseline					
Treatment	0.027	-0.014	0.009	-0.019	0.043
	(0.029)	(0.028)	(0.030)	(0.028)	(0.044)
p-value	0.348	0.604	0.756	0.494	0.324
Control mean	0.331	0.291	0.245	0.514	0.194
Observations	851	930	833	980	376
Panel B. Treatment Effect on Grade Level					
Sample 1: Children at each wave					
Treatment	-0.418	0.561	-1.920	-0.574	-0.622
	(0.325)	(0.302)	(2.559)	(0.289)	(0.476)
p-value	0.198	0.064	0.453	0.047	0.191
Control mean	5.358	5.694	4.926	7.072	7.896
Observations	900	1042	1004	790	559
Sample 2: Cohort aged 0–18 at Baseline					
Treatment	-0.605	0.018	0.331	0.053	0.300
	(0.259)	(0.271)	(0.305)	(0.115)	(0.795)
p-value	0.021	0.948	0.280	0.645	0.710
Control mean	5.638	6.528	8.667	7.948	11.452
Observations	282	271	204	504	73

Notes: Robust standard errors are shown in parentheses. The outcome in Panel A is a binary indicator for school attendance (1 = currently attending, 0 = not attending). The outcome in Panel B is the highest grade level completed, conditional on currently attending school. Sample 1 includes all children aged 0–18 with non-missing school attendance. Sample 2 tracks the cohort aged 0–17 at baseline across each subsequent wave, using approximate inter-survey gaps (baseline = 2008; endline 1 = 2009; endline 2 = 2011; endline 3 = 2015; endline 4 = 2018; endline 5 = 2024/25). Each column reports OLS estimates with a treatment indicator and age in years as regressors. Regressions include village-level fixed effects. The control mean is computed over the estimation sample.

Appendix Tables

A. Household Composition

Table A.1: Balance at Baseline

	Treatment mean	Control mean	Difference	Observations
Household Level Variables				
Consumption per capita, month	49.378	49.915	-1.152	978
			(1.893)	
Everyone in HH gets enough food everyday	0.105	0.114	-0.008	978
			(0.019)	
No adults skipped meals	0.086	0.088	0.004	978
			(0.018)	
Agricultural Profits (last month)	0.122	-0.146	0.297	978
			(0.251)	
Nonfarm Microenterprise Income (last month)	31.971	20.769	8.526	978
			(13.530)	
Wage income (last month)	107.595	99.205	8.051	978
			(4.748)	
Self-Reported Economic Status (1-10)	2.012	1.925	0.065	978
			(0.075)	
Total Outstanding Loans	267.444	270.076	-13.013	976
			(34.137)	
Total Savings (last month)	1.879	3.599	-1.953	977
			(3.459)	
Adult Level Variables				
Minutes spent on productive activities in last day	168.190	170.791	-0.702	1731
			(9.910)	
Member has not missed any days due to illness, last month	0.476	0.512	-0.045	1731
			(0.024)	
Activities of Daily Living Score	0.603	0.593	0.010	1731
			(0.017)	
Self-reported happiness	1.682	1.631	0.040	1730
			(0.038)	
Member has not experienced a period of worry in last year	0.203	0.199	-0.004	1725
			(0.019)	

Notes: All values are in 2023 USD adjusted for purchasing power parity. Column 1 reports the treatment group mean, column 2 reports the control group mean, and column 3 reports the coefficient on treatment from a regression of the baseline outcome on treatment status. The regression includes hamlet-level fixed effects. Robust standard errors are shown in parentheses below each coefficient.

Table A.2: Attrition

	Endline 1	Endline 2	Endline 3	Endline 4	Endline 5
	Attrition	Attrition	Attrition	Attrition	Attrition
	(1)	(2)	(3)	(4)	(5)
Panel A. Attrition in Treatment versus Control					
Treatment	-0.01	-0.02	0.02	0.01	0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Treatment mean	0.161	0.096	0.125	0.112	0.171
Control mean	0.174	0.123	0.108	0.101	0.142
Panel B. Attrition and Baseline Characteristics in Treatment versus Control					
	Attrition × treatment	Attrition × treatment	Attrition × treatment	Attrition × treatment	Attrition × treatment
	(1)	(2)	(3)	(4)	(5)
Household Level Variables					
Consumption per capita, month	-0.51	-2.08	1.67	1.18	2.10
	(5.26)	(6.39)	(6.40)	(6.49)	(5.53)
Everyone in HH gets enough food everyday	-0.01	-0.04	-0.03	-0.09	-0.04
	(0.05)	(0.07)	(0.07)	(0.07)	(0.06)
No adults skipped meals	0.02	0.05	0.10	-0.00	0.03
	(0.05)	(0.06)	(0.06)	(0.06)	(0.05)
Agricultural profits (last month)	-0.41	-0.58	-0.50	-0.31	0.11
	(0.70)	(0.84)	(0.84)	(0.86)	(0.73)
Nonfarm microenterprise income (last month)	-2.84	-38.44	6.02	15.41	101.19
	(37.60)	(45.93)	(45.96)	(46.35)	(39.36)
Wage income (last month)	20.92	-4.48	-14.00	5.35	-16.59
	(13.18)	(16.10)	(16.08)	(16.28)	(13.86)
Self-reported economic status (1–10)	0.03	0.31	-0.08	0.06	0.25
	(0.21)	(0.25)	(0.25)	(0.26)	(0.22)
Total outstanding loans	-147.49	-50.43	43.06	-94.99	-107.19
	(94.61)	(115.06)	(115.55)	(116.86)	(99.38)
Total savings (last month)	-19.47	-25.74	-29.11	0.61	-2.06
	(9.56)	(11.65)	(11.66)	(11.86)	(10.10)
Adult Level Variables					
Minutes spent on productive activities, last day	19.40	-19.32	19.00	-21.15	-2.48
	(28.19)	(34.52)	(33.73)	(22.07)	(20.34)
Member has not missed any days due to illness, last month	0.04	0.00	0.07	0.06	0.07
	(0.07)	(0.08)	(0.08)	(0.05)	(0.05)
Activities of Daily Living score	0.01	0.00	-0.03	-0.05	0.04
	(0.05)	(0.06)	(0.06)	(0.04)	(0.03)
Self-reported happiness	0.12	0.15	-0.16	0.08	-0.01
	(0.11)	(0.13)	(0.13)	(0.08)	(0.08)
Member has not experienced a period of worry, last year	0.02	-0.03	-0.00	-0.04	-0.02
	(0.05)	(0.06)	(0.06)	(0.04)	(0.04)

Notes: Panel A reports the coefficient on treatment from a regression of an attrition indicator on treatment status, estimated separately at each of the five endline waves. Panel B reports the coefficient on the interaction between treatment status and an indicator for attrition, from a regression of each row variable on treatment, the attrition indicator, and their interaction, estimated separately at each wave. All specifications include hamlet-level fixed effects. Robust standard errors are shown in parentheses below each coefficient.

Table A.3: Attrition Given Education Level in Previous Wave

	Endline 1 Attrition	Endline 2 Attrition	Endline 3 Attrition	Endline 4 Attrition	Endline 5 Attrition
	(1)	(2)	(3)	(4)	(5)
Differential Attrition (Treatment × Child Characteristics)					
School attendance in previous wave	-0.016	-0.002	0.004	-0.019	0.021
	(0.023)	(0.013)	(0.019)	(0.016)	(0.031)
Female	0.000	0.026	0.020	-0.011	-0.004
	(0.021)	(0.017)	(0.019)	(0.017)	(0.021)
Age (previous wave)	-0.001	0.001	-0.000	-0.000	0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)

Notes: The table reports the coefficient on the interaction between treatment status and the row characteristic, from a regression of an indicator for household attrition on treatment, the characteristic, and their interaction, estimated separately at each of the five endline waves. The sample in each column consists of children observed at that wave. School attendance is measured in the previous wave, age is measured at the given wave, and sex is measured at baseline. All specifications include hamlet-level fixed effects. Robust standard errors are shown in parentheses below each coefficient.

Table A.4: Balance Gender and Age in Baseline and Endline 5

	Baseline					Endline 5				
	Treatment	Control	Diff	p-value	N	Treatment	Control	Diff	p-value	N
Panel A. Intergenerational Sample										
Proportion of women	0.336	0.407	-0.071	0.036	829	0.262	0.285	-0.023	0.545	540
Age	6.849	7.091	-0.242	0.470	829	23.224	23.686	-0.461	0.292	540
Panel B. All Households										
Average household size	3.928	3.821	0.107	0.320	978	3.802	3.830	-0.028	0.854	835
Proportion of HH members aged 0–16	0.404	0.397	0.007	0.660	978	0.165	0.165	0.000	0.980	835
Boys (0–16)	0.207	0.197	0.010	0.417	978	0.083	0.082	0.001	0.912	835
Girls (0–16)	0.197	0.200	-0.004	0.783	978	0.081	0.082	-0.001	0.939	835
Proportion of HH members aged 17–35	0.307	0.310	-0.003	0.830	978	0.291	0.279	0.012	0.460	835
Men (17–35)	0.122	0.116	0.005	0.574	978	0.169	0.150	0.019	0.106	835
Women (17–35)	0.185	0.194	-0.008	0.425	978	0.121	0.129	-0.007	0.469	835
Proportion of HH members aged 36–55	0.180	0.197	-0.017	0.305	978	0.309	0.308	0.001	0.960	835
Men (36–55)	0.061	0.064	-0.003	0.656	978	0.100	0.106	-0.006	0.574	835
Women (36–55)	0.119	0.133	-0.014	0.358	978	0.209	0.203	0.007	0.689	835
Proportion of HH members aged 56+	0.109	0.094	0.015	0.318	978	0.237	0.251	-0.014	0.556	835
Men (56+)	0.036	0.026	0.011	0.109	978	0.066	0.056	0.011	0.268	835
Women (56+)	0.073	0.069	0.004	0.746	978	0.170	0.195	-0.025	0.280	835

Notes: Panel A shows balance on gender and age for the subsample of children identified at baseline who were found in the household roster during subsequent survey rounds. At endline 5, the sample reached is $N = 540$. Panel B shows balance on household demographics for all households reached at baseline and endline 5.

Table A.5: Migration

	Migration	No. of migrants	Duration	Migrates to Kolkata	Migrates to urban area	Earnings of migrant worker, typical month	Working in business
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel A. Endline 1 (18 months)			
Treatment	-0.015	0.002	11.767	-0.004	0.002	32.075	0.042
	(0.034)	(0.041)	(6.798)	(0.066)	(0.055)	(23.162)	(0.046)
Control mean	0.35	0.39	37.08	0.36	0.83	170.44	0.10
Observations	814	814	285	285	285	285	285
				Panel B. Endline 2 (3 years)			
Treatment	0.029	0.032	14.776	-0.095	-0.039	37.251	0.032
	(0.032)	(0.041)	(15.332)	(0.069)	(0.059)	(36.454)	(0.042)
Control mean	0.29	0.33	125.09	0.38	0.83	281.67	0.15
Observations	840	840	256	256	256	256	256
				Panel C. Endline 3 (7 years)			
Treatment	0.045	0.042	-11.601	0.067	0.012	106.003	0.019
	(0.034)	(0.047)	(12.449)	(0.058)	(0.047)	(41.748)	(0.037)
Control mean	0.37	0.46	123.54	0.30	0.78	441.87	0.11
Observations	844	844	332	332	332	332	332
				Panel D. Endline 4 (10 years)			
Treatment	0.015	0.020	25.167	-0.138	0.033	62.427	-0.029
	(0.032)	(0.046)	(12.743)	(0.059)	(0.053)	(38.032)	(0.042)
Control mean	0.34	0.44	123.96	0.35	0.79	481.19	0.13
Observations	861	861	308	309	309	309	309
				Panel E. Endline 5 (17 years)			
Treatment	-0.008	-0.016	-22.951	-0.037	-0.029	233.406	0.038
	(0.032)	(0.050)	(12.925)	(0.067)	(0.069)	(75.553)	(0.044)
Control mean	0.32	0.45	155.07	0.25	0.41	402.19	0.13
Observations	835	835	264	264	264	264	264

Notes: Robust standard errors are shown in parentheses. Columns 3 through 7 restrict the sample to households with a migrant worker and average outcomes over migrant workers in the household. Column 6 is reported in 2023 USD adjusted for purchasing power parity. All specifications include baseline controls and hamlet-level fixed effects.

B. Business Income

Table A.6: Treatment Impacts on the Probability of Having Business Income > 0

	Having Business Income	
	Logit Model	Linear Probability Model
	(1)	(2)
Treatment	0.495	0.099
	(0.173)	(0.032)
p-value	0.003	0.002
Control mean	0.333	0.333
Observations	835	835

Notes: Standard errors are shown in parentheses. The linear probability model includes robust standard errors and village-level fixed effects. The logit estimates are clustered at the village level and can be interpreted as log odds ratios.

Table A.7: Standard Quantile Regressions on Microenterprise Income (Conditional on Income > 0)

	Nonfarm Microenterprise Income at Endline 5				
	10th percentile	25th percentile	50th percentile	75th percentile	90th percentile
	(1)	(2)	(3)	(4)	(5)
Treatment	0.000	24.763	183.244	396.204	1040.035
	(15.462)	(26.583)	(65.371)	(196.375)	(581.924)
p-value	1.000	0.352	0.005	0.044	0.075
Control mean	39.620	99.051	247.627	594.306	1188.611
Observations, control	14	42	68	109	149
Observations, treatment	19	49	94	141	171

Notes: Robust standard errors are shown in parentheses. Estimates are from a standard quantile regression, conditional on positive business income. The control mean is the estimated intercept for the given quantile. The method follows Koenker (2005). Number of observations per treatment and control is manually imputed.

Table A.8: Multiple Fixed Effect Quantile Regressions on Microenterprise Income (Conditional on Income > 0)

	Nonfarm Microenterprise Income at Endline 5				
	10th percentile	25th percentile	50th percentile	75th percentile	90th percentile
	(1)	(2)	(3)	(4)	(5)
Treatment	53.798	79.206	124.276	322.781	684.443
	(19.921)	(21.401)	(31.186)	(83.287)	(163.887)
p-value	0.007	0.000	0.000	0.000	0.000
Control mean, quantile estimate	39.620	99.051	247.627	594.306	1188.611
Observations, control	14	42	68	109	149
Observations, treatment	19	49	94	141	171

Notes: Robust standard errors are shown in parentheses. Estimates are from a quantile regression with multiple fixed effects, conditional on positive business income. The method follows Machado and Santos Silva (2019). Number of observations per treatment and control is manually imputed.

Table A.9: Treatment Effect on Business Revenue by Business Type

	Microenterprise Revenue at Endline 4	Microenterprise Revenue at Endline 5
	(1)	(2)
Treated HH	-39.528	2.294
	(29.187)	(49.107)
Bidi Business	25.315	158.882
	(73.298)	(75.954)
Treated HH × Bidi Business	85.137	222.349
	(70.899)	(140.603)
Shop Business	383.979	730.072
	(161.900)	(170.426)
Treated HH × Shop Business	81.098	-50.295
	(280.776)	(231.174)
Service Business	-0.506	83.425
	(107.756)	(94.499)
Treated HH × Service Business	150.266	435.021
	(142.320)	(193.412)
p-value	0.176	0.963
p-value, Bidi business	0.730	0.037
p-value, treatment × Bidi business interaction	0.230	0.114
p-value, Shop business	0.018	0.000
p-value, treatment × Shop business interaction	0.773	0.828
p-value, Service business	0.996	0.378
p-value, treatment × Service business interaction	0.291	0.025
Control mean	138.813	135.696
Observations	885	835

Notes: Robust standard errors are shown in parentheses. All values are in 2023 USD adjusted for purchasing power parity. Nonfarm microenterprise revenue is the sales reported in the last 30 days, as reported in the business module of the income section. At endline 5, among the 435 treatment households, 18.4 percent own a Bidi business, 23.4 percent own a Shop business, 24.1 percent own a Service business, and 2.5 percent own another business that is not a Bidi, Shop, or Service business; 51 percent of treatment households do not own a nonfarm microenterprise. Among the 400 control households, 17.75 percent own a Bidi business, 14.5 percent own a Shop business, 15.75 percent own a Service business, and 1.7 percent own another business; 60 percent of control households do not own a nonfarm microenterprise. Other businesses include arts and crafts manufacturing and shoemaking. All specifications include hamlet-level fixed effects.

C. Business inputs and TFP Estimation

Table A.10: Labor and Capital Inputs in Highest Income Business

		Labor inputs				Capital inputs		Material inputs
	Revenue	Nº Workers in business	Nº HH members working in business	Active days in business per month	Total daily nº hours worked	Number productive assets	Value productive assets	Material cost
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Endline 5								
Treatment	203.430	0.302	0.182	2.777	1.300	-0.033	94.662	25.730
	(108.553)	(0.151)	(0.085)	(1.257)	(0.549)	(0.149)	(85.574)	(88.116)
P-value	0.062	0.046	0.034	0.028	0.018	0.825	0.269	0.770
Control mean	368.394	1.126	0.962	17.038	5.030	1.830	221.086	262.056
N	372	372	372	372	372	372	372	372
Panel B. Endline 4								
Treatment	12.547	-5.533	0.005	-0.167	0.480	0.033	161.342	57.916
	(34.550)	(5.586)	(0.063)	(1.010)	(0.404)	(0.102)	(75.468)	(53.939)
P-value	0.717	0.323	0.941	0.869	0.236	0.745	0.033	0.284
Control mean	201.958	1.060	1.045	18.055	5.214	1.353	132.893	221.413
N	434	434	434	434	434	434	434	434

Notes: Robust standard errors are shown in parentheses. All values are in 2023 USD adjusted for purchasing power parity. At endline 5, the highest-earning business accounts for 92.4 percent of household business revenue among treatment households and 97.0 percent among control households, on average. At endline 4, the corresponding figures are 84.9 percent and 86.6 percent. Outcomes are aggregated for the highest-earning business in the household, and monetary values are winsorized at the 1 percent level. The total number of hours worked (column 5) is summed across all workers in the business on a typical working day. Productive assets are assets used in the business in the following categories: equipment and machinery, furniture, small or large tools, large and small vehicles, and other durable goods. The number of productive assets is the sum of binary indicators for ownership of these assets. All specifications include hamlet-level fixed effects.

Table A.11: Total Factor Productivity and Inputs for the Highest Earning Business

	Endline 1		Endline 4		Endline 5	
	Treatment	Control	Treatment	Control	Treatment	Control
TFP (A)	11.25	10.60	10.18	10.61	12.55	9.06
	(6.41)	(6.98)	(10.55)	(14.35)	(10.59)	(6.47)
Revenue Y (2023 PPP USD/month)	251.26	281.80	334.37	365.35	857.99	490.53
	(339.93)	(1020.42)	(471.48)	(881.89)	(1476.18)	(819.74)
Materials M (2023 PPP USD/month)	115.50	146.59	389.67	309.23	617.67	405.87
	(216.81)	(684.16)	(735.68)	(598.62)	(1535.21)	(803.04)
Labor L (hrs/month)	33.36	32.37	201.38	181.27	221.29	187.35
	(16.58)	(14.75)	(128.64)	(115.05)	(129.63)	(104.27)
Capital K (2023 PPP USD)	64.20	47.94	308.19	157.69	499.80	362.55
	(115.90)	(80.03)	(1128.35)	(578.13)	(1356.44)	(1433.72)
Average age, workers	34.20	35.59	41.67	40.12	41.45	41.53
	(13.74)	(12.92)	(15.77)	(14.74)	(12.50)	(12.59)
Minimum age, workers	32.06	33.40	39.86	38.62	39.06	40.15
	(14.73)	(13.76)	(17.30)	(15.87)	(14.26)	(13.86)
Maximum age, workers	36.47	37.82	43.34	41.68	43.96	43.07
	(14.38)	(13.77)	(15.36)	(14.75)	(12.95)	(12.64)
Proportion female, workers	0.61	0.70	0.50	0.63	0.44	0.53
	(0.43)	(0.42)	(0.47)	(0.47)	(0.46)	(0.49)
Maximum TFP (A)	40.65	40.65	86.84	86.84	58.50	35.36
Observations, non-missing TFP	138	120	167	142	145	89

Notes: Total factor productivity (TFP, denoted A) is calculated using a Cobb-Douglas production function with capital (K), materials (M), and labor (L), and the Cobb-Douglas elasticities estimated by Janes et al. (2021): $A = Y / \left(K^{0.175} \cdot L^{0.165} \cdot M^{0.405}\right)$. These metrics correspond to the highest-revenue-earning enterprise per household. Age and gender are averaged across all recorded workers of the selected enterprise. The sample is restricted to households with non-missing TFP in each wave.

Table A.12: Average Total Factor Productivity and Inputs

	Endline 1		Endline 4		Endline 5	
	Treatment	Control	Treatment	Control	Treatment	Control
TFP (A)	11.25	10.60	9.19	9.94	11.09	8.56
	(6.41)	(6.98)	(8.66)	(13.85)	(9.01)	(6.38)
Revenue Y (2023 PPP USD/month)	251.26	281.80	274.81	319.03	658.37	432.84
	(339.93)	(1020.42)	(364.95)	(840.39)	(1274.25)	(781.53)
Materials M (2023 PPP USD/month)	115.50	146.59	334.14	276.25	441.53	361.38
	(216.81)	(684.16)	(668.33)	(562.77)	(1160.54)	(778.66)
Labor L (hrs/month)	33.36	32.37	39.01	31.92	206.44	187.37
	(16.58)	(14.75)	(29.98)	(20.52)	(118.82)	(99.61)
Capital K (2023 PPP USD)	64.20	47.94	303.65	156.24	510.00	370.92
	(115.90)	(80.03)	(1118.82)	(568.51)	(1347.97)	(1427.85)
Average age, workers	34.20	35.59	40.76	39.41	40.22	40.33
	(13.74)	(12.92)	(15.26)	(14.28)	(12.17)	(11.77)
Minimum age, workers	32.06	33.40	36.41	36.24	35.07	37.04
	(14.73)	(13.76)	(17.75)	(16.46)	(15.36)	(14.58)
Maximum age, workers	36.47	37.82	44.62	42.43	45.56	44.40
	(14.38)	(13.77)	(15.47)	(14.27)	(13.22)	(12.01)
Proportion female, workers	0.61	0.70	0.56	0.67	0.51	0.60
	(0.43)	(0.42)	(0.41)	(0.42)	(0.40)	(0.45)
Maximum TFP (A)	40.65	40.65	86.84	86.84	50.12	35.36
Observations, non-missing TFP	138	120	170	147	149	90

Notes: Total factor productivity (TFP, denoted A) is calculated using a Cobb-Douglas production function with capital (K), materials (M), and labor (L), and the Cobb-Douglas elasticities estimated by Janes et al. (2021): $A = Y / \left(K^{0.175} \cdot L^{0.165} \cdot M^{0.405}\right)$. These metrics correspond to the average across enterprises within each household. Age and gender are averaged across all recorded workers of the selected enterprise. The sample is restricted to households with non-missing TFP in each wave.

Table A.13: Treatment Effects on Log of Total Factor Productivity at Endline 5

	(1) Log TFP: highest-income business	(2) Log TFP from average inputs	(3) Log average TFP (share-weighted gradient)
Panel A. Log of Total Factor Productivity			
Treatment	0.285	0.263	0.243
	(0.115)	(0.114)	(0.044)
p-value	0.015	0.023	0.000
Control mean	2.050	2.130	1.981
Observations	222	200	227

Notes: Total factor productivity (TFP) is defined as $A = Y/(K^{0.175} \cdot L^{0.165} \cdot M^{0.405})$ following Janes et al. (2021), where Y is monthly revenue, K is household productive assets shared across enterprises, L is monthly labor hours, and M is business costs (materials). All regressions include village fixed effects. Standard errors in parentheses are obtained via the delta method, which propagates the joint uncertainty of treatment effects on the log inputs using a cluster-robust variance-covariance matrix, clustered at the village level and estimated via `suest`.

Column (1): TFP for the highest-income enterprise. The delta-method gradient is $\mathbf{g} = (1, -0.175, -0.165, -0.405)$ applied to treatment effects on $(\ln Y, \ln K, \ln L, \ln M)$.

Column (2): TFP from household-averaged inputs. TFP is then $\bar{Y}/(\bar{K}^{0.175}\bar{L}^{0.165}\bar{M}^{0.405})$. The delta method applies as in (1) the four-element gradient to treatment effects on the log of each averaged input.

Column (3): Outcome is $\ln \bar{A}$ where $\bar{A} = n^{-1}\sum_j A_j$. The gradient is taken with respect to $\theta = (\ln Y_j, \ln K, \ln L_j, \ln M_j)$, the log inputs for each enterprise j, with each enterprise's inputs weighted by its TFP share $s_j = A_j/\sum_k A_k$: $\mathbf{g}_{Y_j} = s_j$, $\mathbf{g}_{L_j} = -0.165\, s_j$, $\mathbf{g}_{M_j} = -0.405\, s_j$, $\mathbf{g}_K = -0.175$. The full variance-covariance matrix is obtained from a joint `suest` estimation across all enterprise-input equations.

Table A.14: Treatment Effects on Log of Total Factor Productivity, Under Different Cobb-Douglas Elasticities

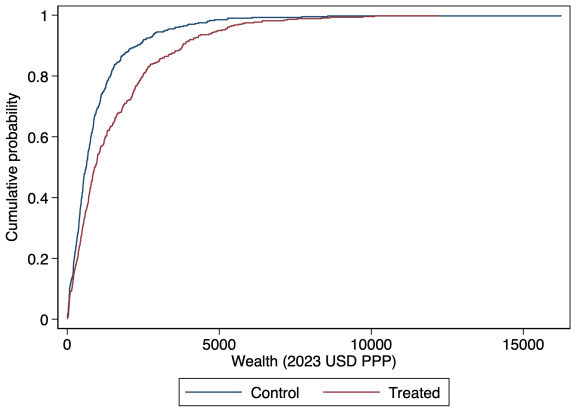
	Highest-income business			TFP from average inputs			Average TFP (share-weighted gradient)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Midpoint	Lower bound	Upper bound	Midpoint	Lower bound	Upper bound	Midpoint	Lower bound	Upper bound
Panel A. Log TFP, Robustness to Cobb-Douglas Elasticities									
Treatment	0.285	0.320	0.249	0.263	0.307	0.219	0.243	0.279	0.209
	(0.115)	(0.117)	(0.116)	(0.114)	(0.114)	(0.117)	(0.044)	(0.033)	(0.056)
p-value	0.015	0.007	0.034	0.023	0.008	0.064	0.000	0.000	0.000
Control mean	2.050	2.504	1.597	2.130	2.604	1.656	1.981	2.434	1.527
Observations	222	222	222	200	200	200	227	227	227

Notes: TFP is defined as $A = Y/(K^{\beta_k} \cdot L^{\beta_l} \cdot M^{\beta_m})$ following Janes et al. (2021), where Y is monthly revenue, K is household productive assets shared across enterprises, L is monthly labor hours, and M is business costs. All regressions include village fixed effects. Standard errors in parentheses are delta-method standard errors clustered by village, following the same methodology described in the notes to Table A.13. Each group of three columns uses the same TFP measure but varies the elasticities: columns (1), (4), and (7) use the Janes et al. (2021) midpoint elasticities $(\beta_k, \beta_l, \beta_m) = (0.175, 0.165, 0.405)$; columns (2), (5), and (8) use the lower bounds $(0.12, 0.13, 0.39)$; columns (3), (6), and (9) use the upper bounds $(0.23, 0.20, 0.42)$. See Technical Appendix B for more details on the construction of TFP and propagation of standard error uncertainty.

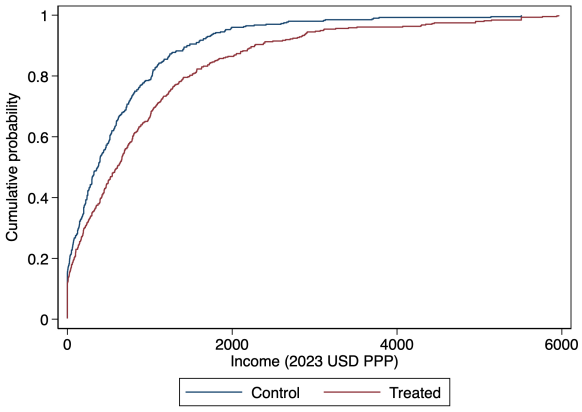
D. Poverty Trap

Figure A.1: Cumulative Distribution Functions

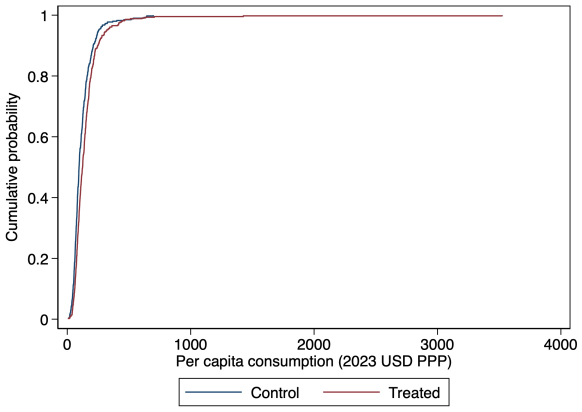
Panel A: Wealth (USD 2023 PPP)



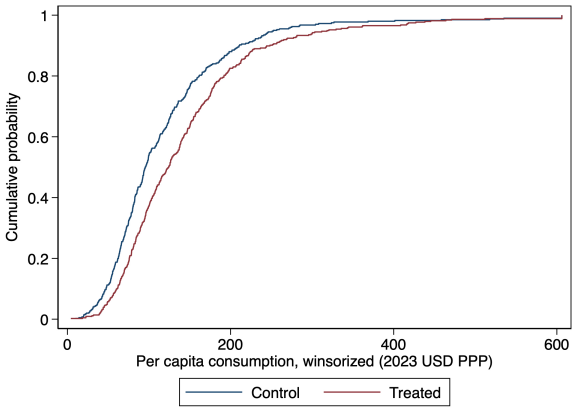
Panel B: Income (USD 2023 PPP)



Panel C: Per Capita Consumption (USD 2023 PPP)



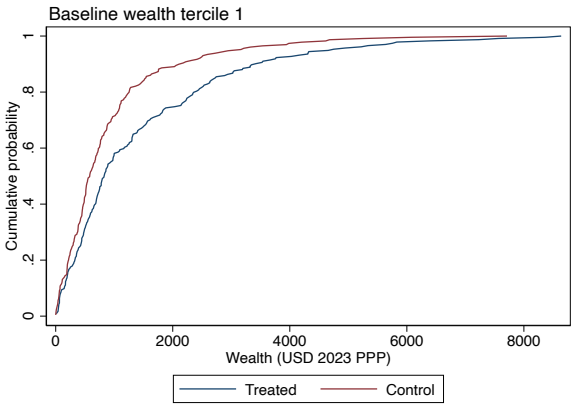
Panel D: Per Capita Consumption, winsorized (USD 2023 PPP)



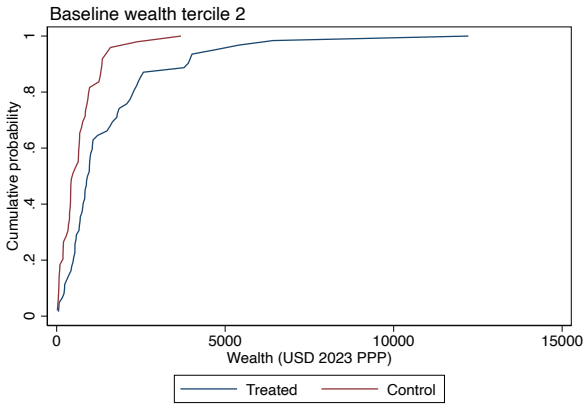
Note: All values are reported in 2023 USD corrected for purchasing power parity. We plot the Cumulative Distribution Functions (CDFs) of wealth, income, and per capita consumption. Wealth is the sum of all livestock, productive and household assets owned. Household assets include: TV sets, radios, electric fans, refrigerators, phones, bicycles, paddle rickshaws / vans/totos/e-rickshaws, sewing machines, chairs, cots, tables, watches / clocks, pairs of clothes, pairs of shoes, and other assets (like tractor, power tiller etc.). Productive assets are defined as equipment used in business, fishing, agriculture, and livestock (excluding animals). Respondents reported owning household and livestock assets but not the self-reported value. We infer best estimates market/resale values of any given asset in our context. For per capita consumption, we constructed it using a detailed consumption module asking about household food, non-food, and durable purchases. For per capita consumption, we plot unwinsorized values, as well as right-winsorized at the 99th percentile values. Income sums over income earned by each individual member from various activities in a typical month, as reported in the household roster, and remittances received by the household.

Figure A.2: CDFs of Wealth at Endline 5 by Baseline Wealth Terciles

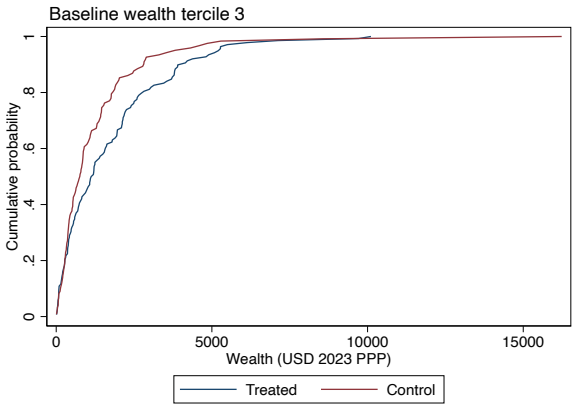
Panel A: Baseline Wealth Tercile 1



Panel B: Baseline Wealth Tercile 2



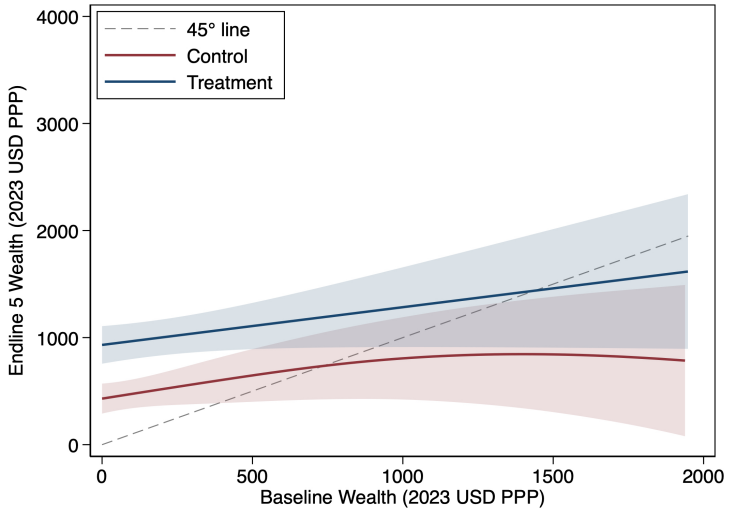
Panel C: Baseline Wealth Tercile 3



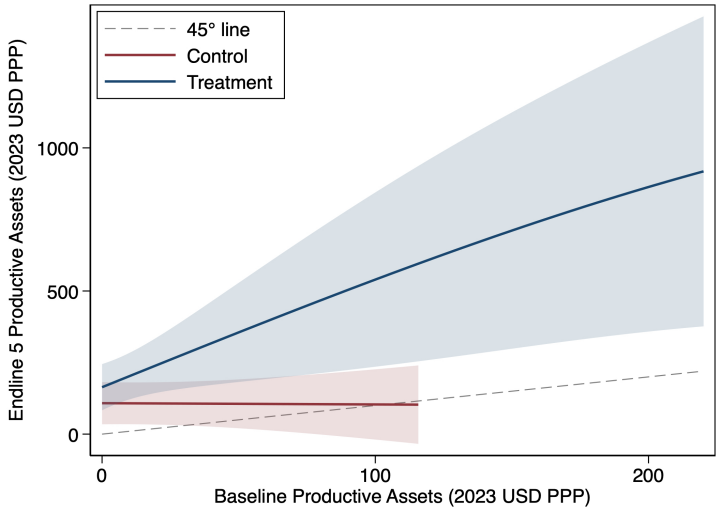
Note: All values are reported in 2023 USD corrected for purchasing power parity. We plot for each tercile of baseline total asset holdings, the Cumulative Distribution Functions (CDFs) of the total wealth at Endline 5 (TFP). Wealth is the sum of all livestock, productive and household assets owned. Household assets include: TV sets, radios, electric fans, refrigerators, phones, bicycles, paddle rickshaws / vans/totos/e-rickshaws, sewing machines, chairs, cots, tables, watches / clocks, pairs of clothes, pairs of shoes, and other assets (like tractor, power tiller etc.). Productive assets are defined as equipment used in business, fishing, agriculture, and livestock (excluding animals). Respondents reported owning household and livestock assets but not the self-reported value. We infer best estimates market/resale values of any given asset in our context.

Figure A.3: Partial Linear Model Estimates of the Transition Function

Panel A: Wealth (USD 2023 PPP)



Panel B: Productive Assets (USD 2023 PPP)



Notes: The figure plots estimated the transition function from baseline assets to future assets, measured in 2023 PPP USD. Wealth is the sum of all livestock, productive, and household assets owned. Household assets include: TV sets, radios, electric fans, refrigerators, phones, bicycles, paddle rickshaws / vans/totos/e-rickshaws, sewing machines, chairs, cots, tables, watches / clocks, pairs of clothes, pairs of shoes, and other assets (like tractor, power tiller etc.). Productive assets are defined as equipment used in business, fishing, agriculture, and livestock (excluding animals). Respondents reported owning household and livestock assets but not the self-reported value. We infer best estimates market/resale values of any given asset in our context. Productive assets correponds to the sum of the reported value of assets used in fishing, microenterprises, agriculture, and husbandry (excluding livestock value). Estimates come from partial linear models that fit the asset transition non-parametrically using penalized splines, while controlling for community random effects, estimated via REML. The dashed line is the 45-degree line; shaded regions are point-wise 95 percent confidence bands (±1.96 asymptotic standard errors) from the generalized additive model.

E. COVID-19 Pandemic and Recovery Outcomes

Table A.15: Impact of COVID

	Treatment	Control	Observations	Difference	p-value
Panel A. Non-Migrant Employed Household Members					
Lost job	0.478	0.501	738	-0.023	0.530
Reduced work hours or got demoted	0.141	0.084	738	0.057	0.015
Lost job temporarily	0.269	0.219	738	0.050	0.119
Lost job permanently	0.194	0.216	738	-0.022	0.465
Switched job from formal to informal	0.000	0.000	738	0.000	.
Switched occupation	0.100	0.084	738	0.016	0.449
Took up NREGA work	0.015	0.012	738	0.004	0.655
Panel B. Business Outcomes					
Farm closed/lost	0.277	0.231	351	0.046	0.327
Non-farm closed/lost	0.621	0.615	351	0.005	0.922
Farm earnings lost	0.200	0.192	351	0.008	0.858
Non-farm earnings lost	0.590	0.571	351	0.019	0.718
Panel C. Migrant Employed Household Members					
Lost job	0.776	0.813	151	-0.037	0.576
Reduced work hours or got demoted	0.197	0.160	151	0.037	0.552
Lost job temporarily	0.250	0.253	151	-0.003	0.963
Lost job permanently	0.500	0.547	151	-0.047	0.569
Switched job from formal to informal	0.000	0.000	151	0.000	.
Switched occupation	0.224	0.107	151	0.117	0.054
Took up NREGA work	0.000	0.000	151	0.000	.
Panel D. Asset Sales					
Sold livestock	0.044	0.050	835	-0.006	0.666
Sold land/fixed assets	0.002	0.000	835	0.002	0.338
Sold durables	0.014	0.015	835	-0.001	0.884
Panel E. Indexes					
Index consumption smoothing	0.699	0.663	835	0.037	0.001
Index delayed life events	0.004	0.005	835	-0.001	0.569
Index social protection	0.286	0.270	835	0.016	0.209

Notes: All outcomes are household-level dummies equal to one if at least one member experienced the event. Sample sizes (Observations) vary across panels: Panel A includes households with non-migrant workers; Panel B is restricted to households operating a business or farm; Panel C focuses on households with migrant workers; Panels D and E use the full sample of 835 households surveyed at endline 5. A pre-covid job loss is *temporary* if the member reported reduced hours or demotion rather than full separation. If that question was unanswered, a loss is classified as temporary if the member was out of work for four months or fewer and returned to the same pre-covid job. A job loss is *permanent* if the member reported a full separation. If that question was unanswered, a loss is classified as permanent if the member subsequently changed jobs, never returned to work, or passed away. The household dummy equals one if at least one member meets the criterion. The *Index consumption smoothing* is the mean of eight binary indicators for whether the household: drew down savings; received a loan; consumed goods before they were needed; took a loan for regular consumption; increased time spent foraging; engaged in begging; postponed essential purchases; or sought out a loan. The *Index delayed life events* is the mean of seven binary indicators for whether the household postponed a wedding or engagement, a funeral, opening a new business, taking a large loan, a child starting college, a child progressing in school, or migration for work. The *Index social protection* is the mean of six binary indicators for receipt of National Food Security Act rations, Jan Dhan Yojana transfers, or cash and in-kind support from government schemes, an NGO, or an individual.

Table A.16: Recovery from COVID

	Treatment	Control	Observations	Difference	p-value
Panel A. Non-Migrant Workers Recovery					
Non-migrant found a job	0.914	0.954	361	-0.040	0.132
Got a new job	0.107	0.057	361	0.049	0.089
Returned same job	0.834	0.931	361	-0.097	0.004
Non-migrant regained earnings	0.604	0.546	361	0.058	0.264
Non-migrant time out of job	3.666	4.298	361	-0.632	0.164
Panel B. Business Recovery					
Farm reopened	0.944	1.000	90	-0.056	0.154
Non-farm reopened	0.959	0.906	217	0.052	0.120
Months farm lost income	5.067	4.375	88	0.692	0.570
Months non-farm lost income	5.683	6.095	215	-0.411	0.600
Panel C. Migrant Workers Recovery					
Migrant found a job	0.825	0.952	125	-0.126	0.025
Got a new job	0.159	0.161	125	-0.003	0.969
Returned same job	0.698	0.823	125	-0.124	0.106
Migrant regained earnings	0.571	0.629	125	-0.058	0.515
Migrant time out of job	5.081	5.750	124	-0.669	0.504
Went back to place of migration	0.377	0.365	116	0.012	0.893

Notes: All outcomes are measured at endline 5 (September to November 2024), approximately four years after the onset of the COVID-19 lockdown in March 2020. Sample sizes (Observations) are restricted to households that experienced an initial negative shock as defined in Table A14: Panel A includes households where at least one non-migrant worker lost their job (361 households); Panel B includes households with a closed farm (90 households) or non-farm (217 households) business; Panel C includes households where at least one migrant worker lost their job (125 households). *Found a job* equals one if the worker had returned to any employment by endline 5. *Returned same job* equals one if the worker returned to their pre-pandemic employer or occupation. *Got a new job* equals one if the worker found work with a different employer or in a different occupation. *Regained earnings* equals one if earnings had recovered to pre-pandemic levels by endline 5. *Time out of job* is the number of months without employment. For businesses, *reopened* equals one if operations had resumed by endline 5, and *months lost income* is the number of months the business was closed or not generating revenue. *Found a job* in this table includes both same-job returns and new jobs, and therefore exceeds the share classified as having experienced a temporary loss in Table A14, where a permanent loss is defined to include job changers.

F. Gender-disaggregated results

Table A.17: Effect on Household and Adult Outcomes

	Physical Health Index	Mental Health Index	Productive time use	Political Involvement Index
		Baseline		
Baseline mean (men)	0.18	0.27	0.30	0.29
Baseline mean (women)	-0.10	-0.16	-0.17	-0.17
Observations, men	638	638	638	638
Observations, women	1903	1903	1903	1903
Observations	1,731	1,731	1,731	1,731
		Panel A. Endline 1 (18 months)		
Treatment	0.120	0.087	0.286	-0.066
	(0.042)	(0.038)	(0.088)	(0.051)
Women	-0.188	-0.099	-0.365	-0.320
	(0.039)	(0.034)	(0.081)	(0.048)
Treatment×	-0.111	0.043	-0.021	0.107
Women	(0.050)	(0.047)	(0.107)	(0.064)
Control mean (men)	0.28	0.41	0.51	0.16
Control mean (women)	0.02	0.26	0.05	-0.18
p-value, women ITT	0.792	0.000	0.000	0.336
Standard error, women ITT	0.032	0.037	0.059	0.043
Observations, men	596	596	596	596
Observations, women	909	909	909	909
Observations	1,504	1,502	1,504	1,504
		Panel B. Endline 2 (3 years)		
Treatment	0.096	-0.027	0.130	0.062
	(0.042)	(0.051)	(0.081)	(0.048)
Women	-0.248	-0.182	-0.518	-0.236
	(0.038)	(0.043)	(0.078)	(0.050)

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Table A.17 continued from previous page

	Physical Health Index	Mental Health Index	Productive time use	Political Involvement Index
	(1)	(2)	(3)	(4)
Treatment×	-0.124	0.061	-0.060	-0.080
Women	(0.049)	(0.058)	(0.101)	(0.064)
Control mean (men)	0.40	0.90	0.63	0.26
Control mean (women)	0.09	0.66	0.04	0.04
p-value, women ITT	0.368	0.427	0.196	0.682
Standard error, women ITT	0.031	0.043	0.054	0.042
Observations, men	749	749	749	749
Observations, women	1,082	1,082	1,082	1,082
Observations	1,757	1,757	1,756	1,757
		Panel C. Endline 3 (7 years)		
Treatment	0.122	0.180	0.310	0.038
	(0.044)	(0.053)	(0.080)	(0.045)
Women	-0.226	-0.180	-0.416	-0.267
	(0.041)	(0.044)	(0.074)	(0.045)
Treatment×	0.006	0.110	-0.252	-0.019
Women	(0.055)	(0.061)	(0.099)	(0.060)
Control mean (men)	0.76	1.24	0.20	0.38
Control mean (women)	0.45	1.00	-0.19	0.19
p-value, women ITT	0.001	0.000	0.266	0.611
Standard error, women ITT	0.038	0.050	0.052	0.036
Observations, men	786	786	786	786
Observations, women	1,148	1,148	1,148	1,148
Observations	1,900	1,899	1,901	1,903
		Panel D. Endline 4 (10 years)		
Treatment	0.186	0.135	0.323	-0.067
	(0.070)	(0.069)	(0.109)	(0.045)
Women	-0.183	-0.137	-0.231	-0.199

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Table A.17 continued from previous page

	Physical Health Index	Mental Health Index	Productive time use	Political Involvement Index
	(1)	(2)	(3)	(4)
	(0.063)	(0.064)	(0.101)	(0.042)
Treatment×	-0.007	0.099	-0.276	0.059
Women	(0.082)	(0.083)	(0.131)	(0.059)
Control mean (men)	0.61	1.11	0.22	0.21
Control mean (women)	0.15	0.85	-0.23	0.12
p-value, women ITT	0.000	0.000	0.448	0.810
Standard error, women ITT	0.046	0.053	0.062	0.036
Observations, men	983	983	983	983
Observations, women	1,217	1,217	1,217	1,217
Observations	1,229	1,229	1,229	1,229
		Panel E. Endline 5 (17 years)		
Treatment	0.153	0.224	0.114	0.001
	(0.063)	(0.068)	(0.109)	(0.036)
Women	-0.168	0.011	-0.616	-0.048
	(0.057)	(0.060)	(0.091)	(0.032)
Treatment×	0.118	0.017	-0.064	-0.014
Women	(0.074)	(0.080)	(0.124)	(0.041)
Control mean (men)	0.44	1.13	0.37	0.67
Control mean (women)	0.16	1.03	-0.23	0.66
p-value, women ITT	0.000	0.000	0.374	0.517
Standard error, women ITT	0.047	0.051	0.056	0.020
Observations, men	726	726	726	726
Observations, women	1,107	1,107	1,107	1,107
Observations	1,486	1,486	1,486	1,486

Notes: This table reports treatment effects on each outcome separately for men and women, for endlines 1 through 5. Women is an indicator equal to one for female respondents. Treatment × Women is the interaction between the treatment indicator and Women. The rows labeled "p-value, women ITT" and "Standard error, women ITT" report the p-value and standard error for the total treatment effect on women, given by the sum of the Treatment and Treatment × Women coefficients. Baseline mean and Control mean are reported separately for men and women. Robust standard errors are shown in parentheses below each coefficient.

A Appendix: Cost-benefit analysis

To get an estimate of costs, Banerjee et al (2021) follow Banerjee et al (2015) to convert all monetary figures from INR to USD PPP at the start of the program. They then convert from USD PPP for that year to 2014 USD values by multiplying by the ratio of the 2014 US Consumer Price Index to the US CPI for that year. This paper further converts the cost figures from Table A8 in Banerjee et al (2021) to 2023 USD PPP by multiplying by the ratio of the 2023 US CPI to 2018 US CPI. They are then inflated to year 17 net present values using a social discount rate of 5%, as in Banerjee et al (2021) using the following formula.

$$cost_{year17} = cost_{year0} * (1.05)^{17}$$

This rate is chosen in harmony with the World Bank and IMF policy[18]. Costs were provided by Bandhan, following a template provided by Banerjee et al (2015). The organizations provided details of the total program costs and each line item, and the total number of participants in the program. Costs were then converted to a per-person basis. Only 52% of treated group households took up the program: the cost-benefit exercise conservatively uses the per-person cost only of those who received the program (rather than the total costs divided by the number of all people selected in the randomization), although benefits are as per the ITT estimates.

To get an estimate of benefits in nondurable consumption this paper converts monetary figures from INR to USD PPP in the year of the survey. To convert into 2023 USD corrected for PPP it then multiplies the figure by the ratio of the 2023 US CPI to that year's US CPI. These benefits are then inflated to year 17 net present values using the discount rate 5%. For post-intervention years in which there is no survey, we assume benefits persist at the same level since the most recent survey wave. Non-durable per capita monthly consumption is converted to annual household-level values by multiplying by the avg. household size (3.8) and 12 (months/year).

To get the value of future consumption in perpetuity, we assume consumption gains at 17-year endline continue in perpetuity and calculate its value as:

$$= \frac{\frac{\text{Year 17 ITT effect on nondurable consumption}}{1.05}}{0.05}$$

The value of assets was not asked in the surveys. To get an estimate of how much the gain in household asset is worth, Banerjee et al (2015) then use the following formula:

$$HH\ Asset\ ValueITT = HHAssetIndexITT * \frac{\gamma * asset_i}{1effectsize} * \frac{\alpha * USD}{1asset_i}$$

[18]The World Bank, "Staff guidance note on the application of the joint bank-fund debt sustainability framework for low-income countries" 2013.

, where γ is the asset weight in the index and α is the asset price. They find the resulting value as being \$6 (2014 USD PPP, which becomes 7 in 2023 USD PPP). This is small because households owned virtually no assets at baseline. We do not have asset prices in 2023 and so simply use the \$7 value. Since the value is small, it does not materially affect the estimate of benefits, which are largely driven by gains in consumption. By way of magnitude, the ITT effect on annual household consumption at year 17 is \$1,741.

The PPP corrected exchanges rates are obtained from the World Bank. The US CPIs are obtained from the US Bureau of Labor Studies. Yearly rates for both are in table A.18.

Table A.18: US CPI and PPP-Adjusted Exchange Rate by Year

Year	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020	2021	2022	2023	2024	2025
US CPI	207.342	215.303	214.537	218.056	224.939	229.594	232.957	236.736	237.017	240.007	245.12	251.107	255.657	258.811	270.97	292.655	304.702	313.689	321.943
PPP-adjusted exchange rate	12.131	12.929	13.609	14.653	15.109			16.997	16.996	17.522	17.729	17.729						19.517	19.517

The benefit/cost ratio is calculated as the net present value of the benefits over the net present value of the costs. Table A.19 provides the resulting cost and benefit figures.

Table A.19: Cost-benefit benefit analysis at 17-year endline

Panel A: Program costs per household, USD PPP 2023	
Direct transfer costs	897
Asset cost	561
Food stipend	337
Total supervision costs	522
Salaries of implementing organization staff	381
Materials	1
Training	24
Travel costs	22
Other supervision expenses	93
Total direct costs	1419
Start-up expenses	49
Indirect costs	143
Total costs, calculated as if all incurred immediately at beginning of year	1610
Total costs, inflated to year 17 at 5% annual discount rate	3691
Panel B: Benefits per households, USD PPP 2023, all values inflated or deflated to year 17 at 5% annual social discount rate	
Year 1 annual consumption ITT, assuming treatment effect equal to 18 month endline	994
Year 2 annual nondurable consumption ITT treatment effect equal to 18 month endline	947
Year 3 annual nondurable consumption ITT treatment effect equal to 3 year endline	817
Year 4 annual nondurable consumption ITT treatment effect equal to 3 year endline	778
Year 5 annual nondurable consumption ITT treatment effect equal to 3 year endline	741
Year 6 annual nondurable consumption ITT treatment effect equal to 3 year endline	706
Year 7 annual nondurable consumption ITT treatment effect equal to 7 year endline	1923
Year 8 annual nondurable consumption ITT treatment effect equal to 7 year endline	1832
Year 9 annual nondurable consumption ITT treatment effect equal to 7 year endline	1744
Year 10 annual nondurable consumption ITT treatment effect equal to 10 year endline	1606
Year 11 annual nondurable consumption ITT treatment effect equal to 10 year endline	1530
Year 12 annual nondurable consumption ITT treatment effect equal to 10 year endline	1457
Year 13 annual nondurable consumption ITT treatment effect equal to 10 year endline	1387
Year 14 annual nondurable consumption ITT treatment effect equal to 10 year endline	1321
Year 15 annual nondurable consumption ITT treatment effect equal to 10 year endline	1258
Year 16 annual nondurable consumption ITT treatment effect equal to 10 year endline	1199
Year 17 annual nondurable consumption ITT treatment effect equal to 17 year endline	1741
Year 17 household asset ITT treatment effect	7
Total benefits after 17 years	21715
Total benefits if sustained for 15 more years:	34277
Total benefits if sustained in perpetuity:	54877
Panel C: Benefit/cost ratios	
Total benefits/total costs ratio at 17 years: total benefits/total costs	588%
Total benefits/total costs ratio if sustained in perpetuity: total benefits/total costs	1487%

B Appendix: Technical note on estimating TFP of household enterprises

Technical Note: Construction of Total Factor Productivity

Production Function and Factor Elasticities

We construct our estimate of TFP with factor elasticity estimates that we obtain from Janes et al. (2025) paper "Do Capital Grants Improve Microenterprise Productivity?". They fit a standard Cobb-Douglas production function to estimate elasticities among household microenterprises in low-income settings (Sri Lanka and Ghana), using similar household surveys. The authors point out that in low-skill, low-capital microenterprises like ours, material input costs make up a large share of production inputs. They propose the following Cobb-Douglas production function:

$$Y_{ijt} = A_{ijt} \cdot K_{it}^{\beta_k} \cdot L_{ijt}^{\beta_l} \cdot M_{ijt}^{\beta_m} \tag{3}$$

where Y_{ijt} is output (revenue), K_{it} is capital, L_{ijt} is labour, M_{ijt} is material costs, and A_{ijt} is total factor productivity for household i's business j at survey wave t. To calculate the implied change in TFP, we take log differences of equation (1):

Janes et al. (2025) obtain factor elasticity estimates for the production function above using two standard methods in industrial organization: Blundell and Bond (1998) ("BB") and Gandhi-Navarro-Rivers (2020) ("GNR"). Both estimators attempt to overcome the challenge that firms' choice of inputs may be endogenous to unobserved variation in their β's. Janes and co-authors prefer the Blundell and Bond estimator for their setting of microenteprises in low-income settings, as "GNR method requires that firms choose optimally their level of intermediate inputs", which is a strong assumption for theirs and our context. Importantly, however, BB and GNR yield similar estimates for β's, they estimate the following ranges for β across the two countries (Table 1 of their paper).:

- $\beta_k \in [0.12, 0.23]$
- $\beta_l \in [0.13, 0.2]$
- $\beta_m \in [0.39, 0.42]$

We cannot directly estimate the factor shares $\beta_k, \beta_l,$ and β_m directly, and instead take Janes and co-authors' estimates "off the shelf," plugging them into our data on $L, K, M,$ and Y to estimate A in the production function above. To be clear, we assume that all businesses at endline 5–across treatment and control, and across industries–the same production function (including the values of β) as in Janes et al. (2025).

Although this is a strong assumption, we believe it is the best for the following reasons. First, the microenterprises in Janes et al. (2025) are similar to our setting in size, industry, and employment patterns. The Sri Lankan and Ghanaian enterprises have a capital stock of less than $1,000 and are split into roughly 30% small-scale manufacturing, 30% retail shops, 15% in services, and remainder in specialty trade and manufacturing. By comparison, the Bengali enterprises have a capital stock of less than $1,000 (90% of them) and are split into rougly 30% in retail shops, 30% in services, and 40% in bidi making.

As in our setting, business owners are self-employed and hire virtually no pair labor. Second, our household enterprise survey uses similar metrics for self-reported profits and sales (Y), labor hours (L), and capital items (K) directly reported at each endline. The exception is measurement of inputs. Janes and co-authors use surveys in which inputs are directly listed. Instead, we only observe the businesses' aggregate monthly cost. Because there are no payments to labor and enterprises have simple production technologies, we assume that the vast majority of costs are input payments and operationalize this such that 100% of costs are modeled as "input costs" M.

Third, Janes et al justify using the same production function (the same β's) for microenterprises across different industries and treatment statuses empirically. In an appendix, they allow the factor shares to vary across treatment and control firms, and across industries, but find that they do not lead to large or significant differences.

In the calculations below we use the midpoints $\beta_k = 0.175$, $\beta_m = 0.405$, and the midpoint $\beta_l = 0.165$. In Appendix Tables A.14, we show that our results are robust to using the endpoints of the ranges instead.

Variable Construction

Output (Y). Revenue is the total income received by the enterprise in the previous 30 days. This is elicited via survey question C5.09: *"During the past 30 days, how much money has the business received from the sales of goods or services?"* We record this separately for each enterprise operated by the household (up to seven enterprises across survey waves). The enterprise roster itself is constructed from C5.0a (*"Over the past 12 months, has anyone in your household operated any non-agricultural enterprise... ?"*) and C5.0b (*"How many such businesses or shops in total have been operated by members of your household over the past 12 months?"*).

Labour (L). Labour is the total number of hours worked on the enterprise in the past 30 days, combining household and hired workers. Household worker hours are constructed from C5.05: *"Which members of this household worked on this enterprise in the last 30 days? How many hours do they work on this enterprise on a usual working day?"* We sum across all household members in the business to get the daily hours. We then multiply reported daily hours by the

number of working days in the month (assumed 30). We do not include hired worker hours, since the question C5.06: *"During the past 30 days, how many people did this enterprise employ who are not members of this household?"* does not include the number of hours, and we are interested in the productivity impact on those who are in our experimental sample. We assume all households with enterprises have 30 days of business activity in a month, for comparison reasons.

Capital (K). Capital is the total self-reported current value of productive assets owned by the household and used in its enterprise(s). Assets are elicited item by item across six categories: equipment and machinery (C5.29), furniture (C5.30), small or large tools (C5.31), large vehicles (C5.32), small vehicles (C5.33), and other durable goods such as fans and lights (C5.34). The total is computed by summing the reported current values across all categories. Asset value is recorded at the household level, not per enterprise. We assign the full household asset value to each enterprise. This is standard practice for household micro-firms where assets are not tracked per activity, but implies TFP levels are not fully comparable across households that operate multiple enterprises simultaneously.

Materials (M). Material costs are the total business expenses incurred in the past 30 days. This is elicited via C5.12: *"In the past 30 days, what costs have you incurred for this business?"* We assume that all reported costs are material input costs. This is consistent with the context — these are very small, home-based enterprises with essentially no hired workers — and follows the same conservative assumption made by **?**.

Correction for Bidi Enterprises with Zero Reported Material Costs

A substantial share of enterprises in the sample produce bidis (hand-rolled cigarettes), a common cottage industry in the study area. For these enterprises, we frequently observe $M = 0$ despite positive reported revenues ($Y > 0$). We confirmed with the field coordinator (see email from Ramaprasad, Bandhan) that this reflects the standard operating model for home-based bidi production: a middleman supplies tobacco and rolling materials on informal credit, collects the finished bidis, and pays the household the net amount — revenue minus material costs — directly at pick-up. As a result, what we observe as "revenue" for these households is in fact net revenue $V = Y - M$, and their reported material cost is zero because the deduction never appears as a separate cash transaction.

To address this, we impute gross revenue and material costs for bidi enterprises with $M = 0$ and $Y > 0$. Let V denote the observed (net) revenue. Under the Cobb-Douglas share assumption, the implied gross revenue and material cost are:

$$Y^* = \frac{V}{1-\beta_m}, \qquad M^* = \frac{\beta_m \cdot V}{1-\beta_m} \tag{4}$$

With $\beta_m = 0.405$, this yields $Y^* = V/0.595$ and $M^* = 0.680 \cdot V$. TFP is then computed using Y^* and M^* in place of the observed values.

To validate this approach, we use the subsample of bidi enterprises that *do* report $M > 0$ and $Y > 0$. For these observations, we treat $V = Y - M$ as the hypothetical observed net revenue, apply equation (4) to produce predicted $\hat{Y}$ and $\hat{M}$, and compare these predictions to the actual reported values. The means of predicted and actual revenue are virtually identical (predicted mean: 204 USD PPP, actual mean: 200 USD PPP), as are the means for material costs (predicted mean: 84 USD PPP, actual mean: 79 USD PPP).

The imputation assumes that the Cobb-Douglas material share $\beta_m = 0.405$ accurately characterises the true input cost ratio for bidi production. This is consistent with bidi being a highly material-intensive activity. To the extent that β_m is overstated, we overestimate material costs and therefore understate TFP levels; the imputation is thus conservative with respect to finding treatment effects.

TFP Computation

Given Y, K, L, and M — with the corrections described above applied where relevant — TFP for household i's business j at survey wave t is computed as:

$$A_{ijt} = \frac{Y_{ijt}}{K_{it}^{\beta_k} \cdot L_{ijt}^{\beta_l} \cdot M_{ijt}^{\beta_m}} \tag{5}$$

Observations are set to missing if any input is missing, or if K, L, or M equals zero (which would make the denominator undefined or zero). Enterprises with $Y = 0$ but non-zero inputs are retained with $A = 0$, flagged separately as inactive. TFP is computed separately for each enterprise slot j and wave t.

TFP Outcome Definitions and Delta-Method Standard Errors

We estimate treatment effects on log TFP at Endline 5 under three outcome definitions, motivated by the challenge that households operate multiple enterprises simultaneously.

Column (1) — Highest-income enterprise. For household i, let $j^*(i)$ denote the enterprise slot with the highest reported revenue at wave t. The outcome is

$$\ln A_{it}^{\text{hi}} = \ln Y_{ij^*t} - \beta_k \ln K_{it} - \beta_l \ln L_{ij^*t} - \beta_m \ln M_{ij^*t}. \tag{6}$$

This is a clean single-enterprise measure that avoids any aggregation across businesses.

Column (2) — TFP from household-averaged inputs. We first average each input across the n_i active enterprises of household i, then compute a single TFP from those averages:

$$\bar{A}_{it}^{\text{inp}} = \frac{\bar{Y}_{it}}{K_{it}^{\beta_k} \cdot \bar{L}_{it}^{\beta_l} \cdot \bar{M}_{it}^{\beta_m}}, \qquad \text{so that} \qquad \ln \bar{A}_{it}^{\text{inp}} = \ln \bar{Y}_{it} - \beta_k \ln K_{it} - \beta_l \ln \bar{L}_{it} - \beta_m \ln \bar{M}_{it}, \quad (7)$$

where $\bar{X}_{it} = \frac{1}{n_{it}} \sum_j X_{ijt}$. Intuitively, this treats the household as operating a single representative enterprise with mean-scale inputs — an appealing simplification given that assets K are already recorded at household level and not allocated across businesses. The log TFP in (7) is an exact linear function of log-averaged inputs, which makes the delta-method standard error straightforward to compute. Note that $\ln(\bar{X}) \neq \overline{\ln X}$ by Jensen's inequality, so this measure differs conceptually from Column (3) below.

Column (3) — Average of per-enterprise TFPs. We compute TFP separately for each enterprise j and average across the household i:

$$\bar{A}_{it} = \frac{1}{n_{it}} \sum_{j=1}^{n_i} A_{ijt}, \qquad A_{ijt} = \frac{Y_{ijt}}{K_{it}^{\beta_k} \cdot L_{ijt}^{\beta_l} \cdot M_{ijt}^{\beta_m}}. \quad (8)$$

The outcome in the regression is $\ln \bar{A}_i$. This measure preserves the within-household differentiation across enterprises: households choose how many businesses to operate and how to allocate inputs across them, so the productivity of each business is economically meaningful. The treatment effect on $\ln \bar{A}_i$ is therefore informative about the household's overall productive efficiency while respecting the multi-enterprise structure of the data. The cost is that $\ln \bar{A}_i$ is no longer a linear function of log inputs, requiring a non-standard delta-method derivation described below. Columns (2) and (3) yield similar point estimates and the same qualitative conclusions (Table A.12), which suggests that the choice of aggregation does not drive the results.

Delta-method standard errors. Since TFP is a constructed measure — a nonlinear function of the estimated treatment effects on log inputs — we propagate sampling uncertainty from the input regressions through the production function using the delta method. For each log input $X \in \{Y, K, L, M\}$, let $\hat{\theta}_X$ denote the OLS estimate of the treatment effect on $\ln X$ from a within-village-demeaned regression. Let $\hat{\mathbf{V}}$ denote the joint cluster-robust variance-covariance matrix of all input-treatment estimates, obtained from a simultaneous system estimated via `suest` with clustering at the village level. The delta-method variance of any smooth function $h(\hat{\theta})$ is then $\mathbf{g}'\hat{\mathbf{V}}\mathbf{g}$ where $\mathbf{g} = \partial h / \partial \hat{\theta}$.

Columns (1) and (2). The log TFP outcomes in (6) and (7) are linear in the treatment effects on

log inputs, so the gradient is simply

$$\mathbf{g} = \big(1,\ -\beta_k,\ -\beta_l,\ -\beta_m\big) \tag{9}$$

applied to the treatment effects on $(\ln Y, \ln K, \ln L, \ln M)$. $\hat{\mathbf{V}}$ is the 4×4 joint VCV from a four-equation `suest`.

Column (3) — derivation of the share-weighted gradient. The treatment effect on $\ln \bar{A}_i$ requires differentiating through the average in (8). Let θ_{Y_j} denote the treatment effect on $\ln Y_j$. By the chain rule:

$$\frac{\partial \ln \bar{A}}{\partial \theta_{Y_j}} = \frac{1}{\bar{A}} \cdot \frac{\partial \bar{A}}{\partial \theta_{Y_j}} = \frac{1}{\bar{A}} \cdot \frac{1}{n} \frac{\partial A_j}{\partial \theta_{Y_j}}. \tag{10}$$

Since $A_j = \exp\big(\ln Y_j - \beta_k \ln K - \beta_l \ln L_j - \beta_m \ln M_j\big)$ and θ_{Y_j} shifts $\ln Y_j$ one-for-one,

$$\frac{\partial A_j}{\partial \theta_{Y_j}} = \frac{\partial A_j}{\partial \ln Y_j} \cdot \frac{\partial \ln Y_j}{\partial \theta_{Y_j}} = A_j \cdot 1 = A_j, \tag{11}$$

where $\partial A_j / \partial \ln Y_j = A_j$ follows from $A_j = e^{\ln A_j}$. Substituting (11) into (10) and using $n\bar{A} = \sum_k A_k$:

$$\frac{\partial \ln \bar{A}}{\partial \theta_{Y_j}} = \frac{A_j}{n\bar{A}} = \frac{A_j}{\sum_k A_k} \equiv s_j, \tag{12}$$

where s_j is enterprise j's share of total household TFP. The same logic applies to L_j and M_j:

$$\frac{\partial \ln \bar{A}}{\partial \theta_{L_j}} = -\beta_l \cdot s_j, \qquad \frac{\partial \ln \bar{A}}{\partial \theta_{M_j}} = -\beta_m \cdot s_j. \tag{13}$$

Because capital K_i is recorded at the household level and enters every enterprise's production function identically, its gradient aggregates across all enterprises:

$$\frac{\partial \ln \bar{A}}{\partial \theta_K} = \sum_j s_j \cdot (-\beta_k) = -\beta_k. \tag{14}$$

The full gradient vector, ordered as $(Y_1, \ldots, Y_n, K, L_1, \ldots, L_n, M_1, \ldots, M_n)$, is therefore

$$\mathbf{g} = \big(\underbrace{s_1, \ldots, s_n}_{Y_j},\ \underbrace{-\beta_k}_{K},\ \underbrace{-\beta_l s_1, \ldots, -\beta_l s_n}_{L_j},\ \underbrace{-\beta_m s_1, \ldots, -\beta_m s_n}_{M_j}\big). \tag{15}$$

The shares s_j are estimated at sample means. $\hat{\mathbf{V}}$ is the $(3n+1) \times (3n+1)$ joint cluster-robust VCV from a simultaneous `suest` over all active enterprise-input pairs, and the delta-method variance is

$\mathbf{g}'\hat{\mathbf{V}}\mathbf{g}$.

Note that (15) collapses to (9) when $n = 1$ (single enterprise, $s_1 = 1$), confirming that Columns (1) and (3) use the same methodology and that Column (3) is the exact multi-enterprise generalisation. Intuitively, the gradient down-weights enterprises that contribute little to household TFP — in our sample, one enterprise tends to dominate, which is why the standard errors in Column (3) are smaller than a naive equal-weighting approximation would suggest.

	TFP still missing						TFP non-missing					
	(1) All		(2) Treatment		(3) Control		(4) All		(5) Treatment		(6) Control	
	Avg. rev.	N	Avg. rev.	N	Avg. rev.	N	Avg. rev.	N	Avg. rev.	N	Avg. rev.	N
Panel A: EL5												
Service	431	4	475	3	297	1	747	77	887	56	373	21
Shop	495	1	495	1	-	-	1231	74	1253	47	1192	27
Bidi -	-	-	-	-	-	-	202	71	314	35	93	36
Other	-	-	-	-	-	-	431	12	695	7	62	5
Total	444	5	480	4	297	1	718	234	858	145	491	89
Panel B: EL4												
Service	241	7	266	3	222	4	360	99	372	63	338	36
Shop	103	1	-	-	103	1	682	78	539	41	840	37
Bidi	-	-	-	-	-	-	87	119	86	55	88	64
Other	-	-	-	-	-	-	662	13	699	8	601	5
Total	224	8	266	3	199	5	349	309	334	167	365	142