Investing in the Next Generation: The Long-Run Impacts of a Liquidity Shock*

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Abstract

Poor entrepreneurs must frequently choose between business investment and children's education. To examine this trade-off, we exploit experimental variation in short-run microenterprise growth among a sample of Indian households and track children's education and business outcomes over eleven years. Treated households, who experience higher initial microenterprise growth, invest more in education and are one-third more likely to send children to college. However, only literate households experience child schooling gains and their enterprises stagnate in the long-run. In contrast, illiterate treatment households experience long-run business gains but declines in children's education. This pattern implies that initial microenterprise growth reduced relative intergenerational educational mobility.

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1 Introduction

Many poverty reduction programs emphasize small enterprise development as a means of generating self-sustaining income growth for the poor. We know less about how microenterprise growth impacts child outcomes, especially human capital investment. Do business growth opportunities for poor households improve their children's educational attainment, and hence disrupt the intergenerational transmission of poverty? While greater liquidity from any source should encourage human capital investment, entrepreneurial households must also evaluate competing business investment opportunities as well as increased demand for child labor, both of which may discourage investment in education.

Using experimental variation in the business income trajectories of poor urban microentrepreneurs, this paper evaluates investment trade-offs between business opportunities and children's human capital — or, put differently, current versus future generation's earning potential. Our study setting is India, which has one of the world's lowest rates of intergenerational educational mobility (Asher et al., 2022). We revisit microfinance borrowers in the city of Kolkata over a decade after they participated in a field experiment in which they were randomly assigned to either a traditional microfinance contract or one with a flexible repayment schedule that encouraged business investment. Treatment generated rapid business growth. Three years after the intervention, the treatment group had 41% higher business profits and 19% higher household income than the control group (Field et al., 2013).[1]

To evaluate the impact of this experimentally-generated business growth on child outcomes, we conduct an 11-year follow-up survey that collects educational and socio-economic outcomes for all children of study participants, including those who have left the household. We find significant educational gains for children in treatment households who were of school-going age at the time of the experiment. Children in treatment households outperformed their control group peers by 0.18 standard deviations on an education investment index, were more than twice as likely to attend private secondary school, and benefited from

[1] Multiple papers show that credit contracts that help borrowers better match business cash-flows to repayment enable profitable investment decisions with positive impacts on business and household outcomes. Examples include: a grace period before repayment begins (Field et al., 2013); seasonal repayment moratoriums or option to reschedule repayments (Barboni and Agarwal, 2018; Czura, 2015); or, choice of repayment schedule akin to a line of credit (Aragón et al., 2020).

21% higher spending on after-school tutoring. Overall, the increase in education spending accounts for roughly 10% of the treatment-induced increase in household income. Gains in tertiary education are substantial: children in treatment households are 10 percentage points more likely to attend college, a 37% increase in attendance rate compared to control group children of the same age. Treatment gains on educational attainment decrease with age at baseline, as younger children experience a longer horizon of investment benefits.

We also find striking differences in investment behavior across treatment households with different levels of parental education. Illiterate parents invest in household enterprises and divest in child schooling when business profits grow.[2] Meanwhile, literate parents invest a high proportion of their marginal income in child education at the expense of business expansion. Among households in which both parents are literate, treatment increases secondary school completion by 12 percentage points and college attendance by 15 percentage points. Children with at least one illiterate parent, on the other hand, are 14 percentage points less likely to complete secondary schooling than their control counterparts and experience no change in college attendance.

Consistent with an investment trade-off, long-run household business outcomes exhibit the opposite pattern with respect to parents' literacy. In 2010, literate and illiterate treated households report substantial economic gains to treatment, though illiterate households report more. These gains only persist for illiterate treated households, who report a 45% increase in profits and a tripling of enterprise capital in 2018 compared to control peers. Household labor patterns also diverge: fewer household members report working in the household enterprise in literate treatment households, whereas more do in illiterate treatment households. Only the latter report increased child self-employment and school drop-out due to economic factors. As a significant fraction of children remain in school in 2018, we cannot directly measure impacts on child income, but can observe impacts on marriage. Over 65% of daughters but only 22% of sons were married by 2018. Marriage incidence is lower for children in treated households, and daughters from treated literate households are

[2] Parental literacy is defined as either (or both) parents being unable to read or write. 22% of sample households are classified as illiterate (85 illiterate and 296 literate households). To account for small illiterate household sample size we also report p-values from randomization inference throughout. We also show similar treatment patterns using years of education based measures.

16 percentage points less likely to report their primary occupation as housewife.

There are two central explanations why investment patterns differ so substantially by parental education, despite comparable short-run income gains: differences in expected returns to child schooling and differences in credit constraints. We find little evidence of credit constraint differences among clients in our sample, the majority of whom are second-time borrowers with similar repayment behavior and equivalent short-run returns to capital in 2010. We, therefore, posit that differences in expected returns to education between more- and less-educated households are the primary driver of divergent household investment responses to microenterprise growth.

By linking investment choices to intergenerational outcomes, this paper extends an experimental literature that has focused on documenting how asset transfer programs yield persistent household income gains (Balboni et al., 2021; Banerjee et al., 2021).[3] Experimental evidence on human capital investments associated with short-run income gains comes primarily from rural study samples, where returns to schooling are lower and the supply of higher education institutions is more limited (Attanasio et al., 2015; Augsburg et al., 2015). Consistent with our findings, this literature highlights that impacts depend on how parents — especially those running enterprises — resolve trade-offs: while paying for school becomes more feasible, households with larger businesses might face higher returns to labor in the enterprise, raising the opportunity cost of children's time and encouraging school drop-out.[4] We study this question in an urban setting where the opportunity cost of pulling children out of school is arguably even larger.

Our findings also speak to a growing body of evidence showing that parental education is a strong predictor of child schooling outcomes in large part because expected returns to chil-

[3] Blattman et al. (2020) is the one exception studying the long-run effect of a cash transfer on child outcomes. In contrast to our results, they report no impacts possibly reflecting the rural study context with fewer opportunities for educational investments or because their sample was less likely to have completed fertility at the point of intervention. Walker et al. (2023) examine the long-run intergenerational effects of a deworming intervention and find a reduction in mortality for recipients' children.

[4] Attanasio et al. (2015) found microcredit improved Mongolian children's education, but only for children of more-educated borrowers. Augsburg et al. (2015) study a Bosnia and Herzegovina credit program and find suggestive evidence that the credit shock increased child labor among low-educated borrowers. The Attanasio et al. (2015) sample and 71% of the Augsburg et al. (2015) sample are rural residents. Non-experimental evidence on how rainfall-induced income shocks impact educational attainment in agricultural communities is mixed (Jensen, 2000; Björkman-Nyqvist, 2013; Shah and Steinberg, 2017; Zimmermann, 2020).

dren's education vary with parents' human capital (Brown, 2006; Boneva and Rauh, 2019; Boneva et al., 2021; Chakravarty and Agarwal, 2021). We provide supportive evidence by showing that marginal propensities to invest in child schooling when their business income grows vary with parental education. In this manner, we shed light on the causal mechanisms that underlie intergenerational transmission of economic status. We also highlight how differences in expected returns are potentially magnified among microentrepreneurs, for whom the opportunity cost of child schooling is particularly high, both in terms of foregone child labor in home production and foregone capital investments in the home business. In doing so, our results provide one explanation for India's poor intergenerational educational mobility in the face of rapid economic growth (Emran and Shilpi, 2015; Asher et al., 2022).

The rest of the paper is organized as follows. Section 2 details the context. Section 3 describes our data. Section 4 presents evidence on household investment choices. Section 5 examines impacts on long-run household and children's earnings and forecasts the evolution of intergenerational earnings mobility. Section 6 concludes.

2 Background

We describe how our experimental intervention spurred business income growth, increasing treated households' ability to invest in children's education and household enterprises. We then discuss how they trade off these options, emphasizing the role of parents' education.

2.1 The Grace Period Experiment

In 2007, we recruited 845 female clients of Village Financial Services (VFS), an urban microfinance institution in Kolkata. Study participants received individual-liability loans and placed in five-member groups, which were randomly assigned into one of two repayment contracts: a standard debt contract with repayment in 22 fortnightly installments beginning two weeks after loan disbursement (control group), or an identical contract but with repayment beginning eight weeks after loan disbursement (treatment or 'grace period' group).

Field et al. (2013) show the grace period contract encouraged high-risk/high-return investments and increased business profitability in a relatively short time-span: three years after loans were disbursed, those assigned to the grace period contract reported a 41% increase in business profits and a 19% increase in household income. Estimated income gains correspond to a monthly return on capital of 13%, in line with other studies of urban microentrepreneurs in poor settings. In this paper, we examine how treatment-induced gains in business income affect household investment behavior in the subsequent decade.

2.2 Household Investment Opportunities

Aside from their microenterprise, study participants typically had at least one other investment opportunity: children's human capital. In 2007, the modal study household had two children, at least one of whom was of school-going age (7–17).[5] We discuss expected costs and returns for these investment alternatives, and how parental education may affect these.

2.2.1 Investing in children's education

Appendix Figure A1, based on the 2019–2021 National Family Health Survey (NFHS), documents a remarkable increase in grade progression and promotion to secondary and university education in urban India during our study period (2007–2018).[6] And, alongside, nation-wide private school enrollment rose by 38.5% between 2010–2016 (Kingdon, 2020).

Educational achievement among control-group study participants and their children reflect national trends. Twenty-three percent of school-age children received some private schooling and 95% report private after-school tutoring in some (or all) academic subjects. For secondary school, average household spending (including school expenditures and after-school tutoring) was ₹33,700 with spending especially high for grades with important exams (10th and 12th). For instance, control households spend ₹8,300 per 10th grade child on school expenditures and after-school tutoring, amounting to 5% of average household income.[7] These costly investments appear to pay off when it comes to college admissions and the labor market. Among secondary school graduates, an additional ₹100,000 of after-

[5] While fertility decisions represent another possible margin of choice, by 2007, 89% of households had completed fertility, and subsequent fertility choices among remaining households are unaffected by treatment. Fertility trends within our sample match those for the nationally representative National Family Health Survey (NFHS): the median urban Indian woman completes fertility by age 26 and 80% complete fertility by age 34, which is our sample's mean client age at baseline.

[6] As NFHS only provides respondent's location at time of survey, significant rural to urban adult migration could lead Appendix Figure A1 to overestimate urban educational investments. However, the 2012 IHDS dataset which allows us to code urban respondents by birth residency demonstrates comparable patterns.

[7] Both public and private schooling incur school uniform and textbook costs. Private schooling additionally incurs annual enrollment fees and monthly school fees.

(secondary)-school tutoring is associated with a 36 percentage point increase in college attendance.[8] College-educated children aged 25 or older earn 25% more per month than those who attended secondary school alone. Consistent with college enabling upward mobility via higher-skilled employment, 84% of college graduate sons engage in salaried work, versus 33% of sons without a college degree.

Other Indian studies document high returns to college education: using Mincer equations, Montenegro and Patrinos (2014) find college completion improved earnings by 21% across India, while Rani (2014) find a 24% rate of return to college in urban areas. Khanna (2023) exploits discontinuities in Indian district eligibility of a school expansion program and estimates causal earnings returns to a year of education of 13% for both genders.[9]

2.2.2 Investing in the household enterprise

As microentrepreneurs, study households must balance expenditure on large but high return investments in children's education beyond primary schooling against enterprise investments. As documented in several lower-income settings, credit constraints limit profitable business investment among urban microentrepreneurs (De Mel et al., 2008; Fafchamps et al., 2014; Hussam et al., 2022). This is also true for our sample: in a 2012 survey, control study clients reported that only 36% of household enterprises were started with sufficient resources. If given an extra ₹20,000 at enterprise opening, clients said they would have purchased more equipment or raw materials (42%), or started a new enterprise (20%). They also face idiosyncratic and systemic risk: between 2012 and 2018, 50% of household enterprises in the control group closed, with respondents attributing 27% of closures to household illness. In terms of systemic risk, India's microfinance crisis caused a massive negative liquidity shock

[8] Tutoring is typically associated with higher 12th grade exam scores which, in turn, determine admission to low-cost public colleges (Kingdon, 2020; Berry and Mukherjee, 2019; Sekhri, 2020)

[9] To more broadly summarize existing causal estimates of returns to education in lower-income settings: Duflo (2001) finds returns of 6.8–10.6% from Indonesia's primary school expansion; Spohr (2003) exploits expansion of Taiwan's tuition-free middle school and finds returns of 5.8% for boys and 16.7% for girls; Fang et al. (2016) exploits Chinese compulsory schooling law variation and finds returns of 20%; using the introduction of a Turkish compulsory schooling law, Aydemir and Kirdar (2017) find returns of 2–2.5% for boys and 7–8% for girls; Ozier (2018) evaluates secondary schooling for Kenyan students at test-score cut-off and reports a shift to formal employment for men and lower fertility for women. Conversely, Filmer and Schady (2014) use test score cut-offs for scholarships in Cambodia to find no effect of an additional 0.6 years of secondary schooling on earnings while Duflo et al. (2021) finds secondary school scholarships imply labor market gains for girls but not boys in Ghana. We are unaware of experimental or quasi-experimental studies of the returns to college education in a low-income setting.

between 2010 and 2012: the percentage of control group households that closed at least one enterprise increased from 34% to 57%. Thus, clients have incentives to invest income increases in household enterprises or risk management.

2.3 Parental Education and Investment Choices

Our focus on the role of parental education in shaping household investment choices is motivated by a large empirical literature documenting a positive association between parent and child educational outcomes.[10] Figure 1, based on the nationally representative IHDS survey, shows that, relative to sons of illiterate parents, sons of literate parents are more likely to attend college in 2012 across all 2005 family income quintiles, with the gap rising with wealth.[11] Thus, even as illiterate parents' ability to finance education improves, their children consistently fail to keep up with peers that have literate parents. In our control group sample, sons of literate parents are 114% more likely to have attended college than those of illiterate parents, conditional on household wealth.

Standard household models posit that investment in children's education may vary with parents' own human capital due to disparities in expected returns to schooling, or disparities in credit access. Expected returns to children's human capital vary when either actual returns to schooling or parental beliefs about returns to schooling (perceived returns) differ. On actual returns, research shows that less educated parents are less able to assist their children in acquiring human capital accumulation, including schoolwork assistance (Todd and Wolpin, 2007; Banerji et al., 2017). This could reflect a lack of subject matter knowledge or other skills like cognitive endurance (Brown et al., 2022). They also spend less time on child care (Guryan et al., 2008). Less-educated parents may struggle to guide their children through

[10] Akresh et al. (2023) uses differential exposure to school construction in Indonesia to provide causal evidence that increasing parents' education raises the likelihood that their children attend college. Chevalier (2004) and Maurin and McNally (2008) estimate a positive causal impact of parental education on children's educational attainment in the UK and France, respectively. Black et al. (2005) find that an increase in Norwegian mothers' education increases sons' educational attainment. Other evidence for lower-income countries is largely correlational and includes Brown (2006) for China; Augsburg et al. (2015) for Bosnia and Herzegovina; Attanasio et al. (2015) for Mongolia; Attanasio et al. (2020) for Colombia; Akresh et al. (2023) for Indonesia; and Chakravarty and Agarwal (2021) for India.

[11] The sample includes sons present in both 2005 and 2012 IHDS survey waves and who were aged 11–21 in 2005. We focus on sons since they are less likely to migrate at marriage. 83% percent of literate-parent sons and 88% of illiterate-parent sons can be matched across households surveyed in both rounds. The gap in tracking rates is not significantly different across household income quintiles.

the educational system due to limited exposure to successful pupils in their social circles (Sequeira et al., 2016). On perceived returns, multiple empirical studies document that less educated households underestimate returns to education.[12] Recent papers show that this underestimation extends to children's true ability (Dizon-Ross, 2019; Duhon, 2023). Less educated parents may also have lower educational aspirations for their children (Genicot and Ray, 2020). The net result is that less educated parents have lower expected returns than their more educated counterparts, which should give rise to lower educational investments.

Less educated households are also typically poorer because of lower earnings capacity. This could limit their absolute investment in children's education relative to more educated households (Galor and Zeira, 1993; Banerjee, 2004), and may also impact relative returns to investing marginal income gains in children's education versus household enterprises. For instance, poorer households may be more likely to respond to a liquidity shock by investing in their business — even if education returns are higher — simply because they have a higher discount rate (Jacoby and Skoufias, 1997). They might also do so because of behavioral factors that disproportionately affect the poor, such as higher psychic costs of outstanding cash shortfalls (Kaur et al., 2022). Alternatively, if less-educated households are more credit-constrained, they may prefer business over schooling investment because business investments are more liquid and help households smooth consumption in the event of a negative shock.

We anticipate that, in our setting, differences in credit constraints are less likely to be a primary driver of heterogeneity in human capital investment by parental education than they are in the general population. This is because our partner microfinance institution uses enterprise ownership and home ownership as selection criteria, and screens clients on repayment ability.[13] As a result, literate and illiterate study households are comparable on many observable dimensions of liquidity (Appendix Table A1). For instance, while literate households do better on an asset-based socio-economic index, literate and illiterate households are equally likely to own a business, own a home, and have experienced a recent income shock.[14]

[12]On underestimation of returns by less educated parents, see Jensen (2010) for evidence in the Dominican Republic, Nguyen (2008) in Madagascar, Avitabile and de Hoyos (2018) and Attanasio and Kaufmann (2014) in Mexico. On lower perceived returns for this population see Chakravarty and Agarwal (2021) for evidence from India, Brown (2006) for China, Boneva et al. (2021) for the U.K., Almås et al. (2016) for Norway, Delavande and Zafar (2019) in the U.S.

[13]Seventy-five percent of our study participants are second-time clients who qualify for a larger loan.

[14]See the Data Appendix for a detailed description of the construction of the socio-economic index.

They also have comparable household sizes, suggesting similar shadow costs of labor. In addition, time preference data reveal similar levels of impatience across clients in literate and illiterate households. They also receive comparable loan amounts, and exhibit comparable rates of default.[15] Survey data collected upon completion of the study loan cycle indicate literate and illiterate families made similar business investments, with inventory and raw materials the biggest loan expenditure category. We examine whether business returns in 2010 (three years post-intervention) were the same for literate and illiterate households by replicating Field et al. (2013)'s method of regressing household profits in 2010 on household capital, with the latter instrumented by a treatment dummy. Appendix Table A2 shows that, consistent with similar levels of access to credit, literate and illiterate samples had similar returns to capital.

Finally, research suggests that mothers' and fathers' preferences for spending on children's human capital often differs (Lundberg et al., 1997; Duflo, 2003; Duflo and Udry, 2004). If educated wives have greater bargaining power in the household, and a stronger preference for spending on children's education, then children's education may vary by maternal literacy. However, in our sample, illiterate wives are significantly more likely to report having a major say in education expenses.

Given this evidence, we hypothesize that lower expected returns to children's schooling is the primary reason that less-educated parents invest fewer income gains in children's education within our sample.[16]

3 Data and Measurement

We first describe our analysis sample, primary outcome variables, and preferred measure of parental education, with full details available in the Data Appendix. We then provide descriptive statistics and balance checks. We conclude by relating our empirical analysis to

[15] Field et al. (2013) found that while treatment did not impact repayment behavior, grace period clients were less likely to default. These patterns were similar across literate and illiterate household samples.

[16] Endogenous fertility responses may magnify differences in child educational outcomes between literate and illiterate treatment households in younger populations where treatment may impact fertility. This reflects the standard quantity–quality trade-off: if parents in treatment households were pushed to invest more in child quality, higher income is likely to have had the opposite effect on literate households' fertility incentives, allowing parents to invest more in existing children and thereby magnifying differences in investment between literate and illiterate households.

our pre-analysis plan.

3.1 Data

Household and child sample In 2018 we resurveyed study participants. Our analysis sample, which includes all households with school-age children (7–17 years) in 2007 (henceforth, "school-age sample"), comprises half of the study sample. School-age children in these households form our child sample. They are old enough to have completed K–12 schooling by 2018 but young enough in 2007 that treatment-induced income gains could impact their schooling investments. Appendix Figure A2 plots baseline age distribution of children and shows similar proportion of 7 year-olds by treatment status and, correspondingly, Appendix Table A1 shows balance in child age by treatment status.

Child educational outcomes In 2018, clients reported educational attainment and socio-economic outcomes for all children ever born. Our investment index aggregates college spending and primary and secondary school investment sub-indices. Each school sub-index includes total spending and whether the child attended private school. Since nearly 100% of children are literate and primary school completion is close to universal (95.3%), we focus on secondary school completion, college attendance and years of schooling.[17] For a child still in school, secondary school completion is coded as 0.[18]

Censoring could bias treatment effects if the proportion of children (by age-group) still in secondary school differs by treatment status, which it does not. Later we show that our estimates are robust to alternative age cutoffs. *Attended college* is an indicator that equals 1 if a child has completed or is currently in college. *Years of schooling* is defined as years spent in educational institutions, for children who have completed education. For the 21.3% of our sample still studying in 2018, we define *years of schooling* as years completed at time of survey. To the extent that treatment increases the likelihood of children continuing to college, our conservative approach will underestimate treatment impacts on education. We also report effects for alternative outcome definitions. Finally, recognizing that child

[17] This is consistent with national trends, see Section 2.2. We include primary school expenditures in our investment index as treatment may impact investment in quality of primary schooling.

[18] In 2018, within the control group, 6% of children are still in secondary school. Of these, 60% are in 12th grade and 40% are in 11th grade (Appendix Figure A3).

age impacts measurement of education outcomes, our child-level regressions always include child-age fixed effects.

Household economic outcomes and labor outcomes Our primary economic analysis draws on 2010 and 2018 surveys, which asked comparable questions for profits and capital associated with each household enterprise. We construct household-level measures by summing across household enterprises. Both surveys measured household income, inclusive of income generated by resident children. We combine these three outcomes into a standardized economic index. We separately consider number of household and non-household workers employed in household enterprises in 2010 and 2018. In our robustness analysis (presented graphically) we also report an economic index based on a 2012 enterprise survey. This survey also provides a measure of whether child was ever self-employed before turning 18. Finally, we use parent responses in 2018 survey to categorize reasons for children's school drop-out.

Parental Education Study participants are significantly less educated than their children. We classify 19% of households as illiterate, meaning that at least one parent is unable to read and write.[19] This household illiteracy measure is our primary measure of less-educated households. This is consistent with the educational mobility literature focus on study populations with low levels of educational attainment. This literature typically employs educational attainment categories rather than years of schooling (Narayan et al., 2018). For these populations, coarse measures, such as literacy, are less prone to measurement error due to recall bias, and responses are typically more accurate and consistently more meaningful. Moreover, when average years of education are relatively low, grade attainment is a poor proxy for human capital and skill.[20] Parental literacy, in particular, as a skill-based measure of human capital, may impact children's educational outcomes beyond the channels associated with years of education. For instance, navigating the school system is harder for an illiterate person (e.g. submitting documents to register a child in school), which can reduce their ability to invest in children's education. That said, in Section 4.2.3 we examine the robustness

[19] In 4% of sample both parents are illiterate, in 10% (5%) only the father (mother) is literate.

[20] Angrist et al. (2021) note that "in rural India, half of grade 3 students cannot solve a two-digit subtraction problem such as 46 minus 17." Similarly, a 2005 survey conducted by the NGO Pratham found that close to half of fifth-graders could not read a simple paragraph at the second-grade level or solve a two digit subtraction problem with borrowing.

of our education results using an alternative primary-school-completion-based definition of parental education that follows Alesina et al. (2021), and using average years of parental schooling.[21]

3.2 Descriptive Statistics and Experimental Validity

Appendix Table A1 presents descriptive statistics and balance tests for the school-age household sample and literate and illiterate subsamples. Panel A presents household characteristics. Study participants are long-term married residents in reasonably well-established neighborhoods of Kolkata: four-fifths own their residence and the majority reside in neighborhoods with a sewage system. At baseline, when unprompted, 78% reported owning at least one business with over half owning multiple. The literate and illiterate sub-samples are well-balanced on covariates. A joint test shows that we cannot reject equality of means across treatment and control in any sample. We include these covariates as possible controls in each regression (selected using double LASSO).

The average child in our sample was 12 years old at baseline and 93% of children were in school at the time (the median grade was class 6). Panel B shows that our child sample is balanced on gender and over 90% lived with their parents at baseline. By 2018, 41% of sample households had at least one child residing elsewhere. In our study context, daughters generally leave the home upon marriage while sons continue to reside with their parents, together with their spouse. Consistent with this, 91% of sons still live in the household in 2018, compared to only 37% of daughters.[22]

Our survey tracking rate — 92% in 2018 — is on par with that of other long-term studies (Blattman et al., 2020; Banerjee et al., 2021).[23] Appendix Table A3 Panel A shows that attrition rates are balanced across treatment and control for all samples. Panel B shows limited treatment-related attrition differences across a set of household characteristics. Attrited treated households are younger and literate households drive these differences. They are also slightly larger (with more children), but these effects are similar across literate and

[21] In 39% of sample households at least one parent has less than a primary school education, while 80% of women have only completed primary school. Only 1% went to college.

[22] Ninety-seven percent of all children living outside the household at the time of the 2018 survey are married.

[23] In 2010, our tracking rate was 94%. In 2018, 2.5% of surveys were conducted with a different household member due to client death. (All 2010 surveys were with the client.)

illiterate samples. We do not see significant treatment differences for attrited households on educational expenditures. Finally, attrited treatment households in the illiterate household sub-group score lower on the socio-economic index. Since literate households score higher on this index, such attrition would, if anything, lead us to underestimate treatment differences in investment behavior. Two aspects of our analysis further limit concerns of attrition-related imbalance driving results: our child-level analysis includes child-age fixed effects and we include baseline covariates as controls (chosen using LASSO).

3.3 Pre-Analysis Plan

Our analysis of long-term household economic outcomes follows the specification used in Field et al. (2013). We registered a pre-analysis plan (PAP) for the (new) child education analysis.[24] Appendix Table A4 summarizes our analysis table-wise and deviations from what was pre-specified. The PAP specified outcomes for child analysis, but not the age cut-offs for defining the child sample (and the corresponding household sample). Our child-level regressions include child-age fixed effects and Appendix Tables A7 and A8 show robustness to varying child age cut-offs. Further, the PAP specified heterogeneity analysis by parental education but did not specify the choice of parental education categories. Section 3.1 discusses our rationale for using parental literacy, and Section 4.4 provides robustness checks.[25]

Following the PAP, we implement two approaches to reduce the chance of falsely rejecting a null hypothesis. First, we consider indices of outcomes of interest. Second, to correct for multiple hypothesis testing we calculate sharpened q-values that control for expected share of rejections that are Type I errors — the false discovery rate (FDR) — for two outcome families (Benjamini et al., 2006; Anderson, 2008). The first comprises 12 tests including child-level and household-level education and economic outcomes for the pooled school-age sample (Panel A of Tables 1, 3, and 4). The second family comprises 36 tests and includes the same

[24] AEA registry ID AEARCTR-0003572; PAP at `https://www.socialscienceregistry.org/trials/3572`.

[25] We did not pre-specify analyzing child labor outcomes or the specification which interacts child gender with parental education. The PAP specified parent and child health as outcomes of interest, but we could only collect child survival for all children and this is extremely high. We specified, but did not conduct, heterogeneity analyses by whether the client completed fertility at baseline, since this was true for 89% of clients. Finally, we specified analysis of treatment impacts by clients' decision-making power. We find no difference in treatment effects based on whether the client has the majority of say in educational expenses at baseline (results available from the authors upon request).

set of outcomes but from our heterogeneity analysis by parental education for the school-age sample (Panel B of Tables 1, 3, and 4).[26] Appendix Figure A4 plots sharpened q-values against p-values for the first outcome family (outcomes for the pooled school-age sample) and second outcome family (outcomes for the school-age sample by parental education), respectively. Finally, given the limited number of illiterate households in our schooling, and recognizing that outliers or imbalances at baseline may be influencing findings, we report p-values based on randomization inference.

4 How did households invest their economic gains?

We empirically investigate how treatment-induced income gains were allocated across children's education and household enterprises, and whether this varied with parental literacy.

4.1 Children's educational outcomes: visual evidence

In Figure 2 we plot local polynomial regressions of our main educational outcomes of interest — education investment index, secondary school completion, and college attendance — on child age at baseline, by treatment and control.

Panel A (the pooled sample) shows three distinct patterns: First, among all cohorts of primary school age at baseline (ages 5–13), treatment children's investment index outpaces that of their control counterparts. Second, treatment effects on this index grow in magnitude with cohort age from baseline ages 0–11, corresponding with a decline in the rate of censoring of schooling outcomes with child age. For instance, 3-year-olds at baseline were only 14 at endline, so they lacked the opportunity to experience gains in tertiary education or high school degree completion. Indeed, we see similar but noisier treatment effects on secondary school completion and college attendance, suggesting that treatment effects accumulate until well past (endline) age 14. Consistent with this, scores on the investment index are similar for treatment and control group children under age 3 at baseline, indicating that secondary and tertiary school investment are key margins. Third, treatment effects are significantly

[26] Both families include the following outcomes: educational investment index, completed secondary school, attended college, years of education, economic index in 2010 and 2018, number of (i) household workers and (ii) non-household workers, ever self-employed under 18, dropout due to (i) economic considerations, (ii) child ability and (iii) marriage for the pooled school-age sample. We do not include outcomes in Table 2 as they represent a different specification.

less pronounced for children who were old enough to be in secondary school at baseline (ages 14–18), which is consistent with the fact that children of primary-school age in 2007 were exposed to more years of treatment-induced schooling investment.

Panels B and C figures reveal stark differences in the pattern and direction of treatment effects across literate and illiterate subgroups. For children of literate parents (Panel B), treatment leads to substantial gains in the investment index and secondary and tertiary educational attainment. In stark contrast, treatment lowers educational attainment among children with illiterate parents experience a decrease (Panel C). This reversal of treatment effects is particularly strong for secondary school completion: while control group children in illiterate-parent households achieve a schooling attainment rate of 45% at the peak age of observable attainment, illiterate-parent children in the treatment group never achieve a completion rate higher than 20%. These patterns suggest literate and illiterate parents make very different educational choices in response to treatment-induced income gains. We examine the robustness of these patterns in a regression framework.

4.2 Children's educational outcomes: regression estimates

For child i from household h in microfinance group g with treatment status T_g, we estimate:

$$Y_{ihg} = \alpha + \beta T_g + \theta_g + \phi_{ihg} + \gamma X_{ihg} + \epsilon_{ihg}. \tag{1}$$

Y_{ihg} references educational outcome, θ_g are stratification dummies, ϕ_{ihg} is a child age fixed effect and X_{ihg} are baseline controls selected via a double LASSO approach from Appendix Table A1 Panel A covariates. We control for whether a non-client household member was survey respondent. Standard errors clustered by loan group and randomization inference p-values are reported. For heterogeneity analysis by characteristic C_{hj} (here, parental literacy), we estimate:

$$Y_{ihg} = \alpha + \beta_1 T_g C_{hj} + \beta_2 T_g(1 - C_{hj}) + \pi C_{hj} + \theta_g + \phi_{ih} + \gamma X_{ihg} + \epsilon_{ihg}. \tag{2}$$

β_1 and β_2 capture treatment effects for children of literate- and illiterate-parent households, respectively, and π captures differences in educational outcomes between children of literate and illiterate control group households. We report the p-value testing $\beta_1 = \beta_2$.

4.2.1 Average effects

Table 1 regression results mirror Figure 2 patterns. In the pooled sample (Panel A), treatment children score 0.18 standard deviations higher on the education investment index (p-value = 0.015; column 1). Turning to constituent sub-indices, while the treatment effect on primary-school investment is positive but statistically insignificant (column 2), the secondary schooling investment index is 0.25 standard deviations higher for treatment children and significant at 1% level (column 3). Index component results in Appendix Table A5 show that, compared to control group peers, treatment children are three times as likely to attend private secondary school (p-value = 0.004; column 4), and their parents spend an additional ₹5,006 per child on after-secondary-school tutoring (p-value = 0.007; column 6). Treatment parents report 43% higher college expenditures (p-value = 0.076; column 7).

Importantly, increased education expenditure, especially at the post-secondary level, is associated with higher schooling attainment for treatment children. Among control group children, 42% complete secondary school; treatment has a positive but statistically insignificant impact on this completion rate (column 5). Conversely, only 27% of control group children attend college; treatment causes a 10 percentage point increase in college attendance (p-value = 0.009; column 6). The gain amounts to a 38% increase in the likelihood of attending college when compared to control group peers. This supports prior research findings that tertiary schooling is particularly sensitive to household liquidity constraints. For instance, Duflo et al. (2021) find that secondary school scholarships in urban Ghana increase the likelihood of enrolling in college by 29%. In Chile, Solis (2017) finds that providing access to a loan for college education increases college enrollment by 50%. Finally, treatment increases total years of education by one-third of a year, but this result is not statistically significant (column 7).

For each outcome, p-values from randomization inference (in square brackets) are very similar to those from standard asymptotic inference. We also adjust for multiple-hypothesis testing: Appendix Figure A4 shows that after FDR corrections, q-values on the coefficients for overall investment, secondary school investment, and college attendance within the pooled sample remain statistically significant at the 0.10 level (Panel A).

4.2.2 Heterogeneity by Parental Education

Panel B of Table 1 examines whether treatment impacts vary with parental literacy. Consistent with Figure 2 patterns, treatment causes a 0.27 standard deviation increase in the educational investment index among children with literate parents, significant at 1 percent (column 1). This reflects increased spending on secondary and college education (columns 3 and 4). In contrast, treatment has no impact on the education investment index, or any of its component sub-indices, among children of illiterate parents. We reject equality of treatment impacts for literate- and illiterate-parent children for all educational investment measures aside from the primary school investment sub-index (for which we observe no effects among either sub-sample).

For children of treated literate parents, we find that investments are accompanied by educational gains: treatment leads to a 12 percentage point increase in the likelihood of secondary school completion (p-value = 0.025; column 5) and an almost 50% increase in college attendance (p-value = 0.004; column 6), making treatment children almost three times as likely to attend college as control group children of illiterate parents. These gains imply an increase in treated children's total years of schooling of 0.85 years (p-value = 0.016; column 7). In sharp contrast, all treatment coefficients on educational attainment for children of illiterate parents are negative, and sometimes significantly so. Relative to control group peers, treatment children with illiterate parents are 14 percentage points less likely to complete secondary schooling (p-value = 0.018; column 5), which amounts to a 44% drop in completion. Treatment children with illiterate parents are no more likely to attend college (column 6) and have 1.04 fewer total years of education than children with illiterate parents in the control group, a difference that amounts to just over 10 percent of the control mean (p-value = 0.026; column 7).[27] For all three educational attainment measures, we can reject

[27] We find similar but noisier results for two alternative outcome definitions. First, if we redefine the outcome in column (5) as either having completed secondary school *or* currently being in secondary school, we observe a decline by 10 percentage points ($p = 0.169$). Second, we redefine the outcome in column 4 – we impute the total years of education that currently-enrolled children will complete by estimating the years of education that control group children who have finished their education attain, conditional on completing a specific grade. For children that are currently enrolled in college, we assume that they complete their program. For this outcome, we find a decline in years of education by 0.89 years ($p = 0.083$). For both

equality of treatment impacts between children of literate and illiterate parents.

In recent decades, urban India has seen a remarkable convergence in educational attainment across genders (Appendix Figure A1). In our control group, fathers are more than twice as likely as mothers to complete secondary school, whereas sons and daughters are equally likely to complete secondary school and to attend college. However, labor market outcomes continue to diverge among sons and daughters, with marriage markets serving as an essential moderator. Against this backdrop, we examine gender differences in education and marriage-related treatment effects.

In Table 2, Panel A reports results from estimating a gender-specific version of equation (2), while Panel B investigates if differential impacts by gender also vary with parental literacy. On average, boys and girls experience similar treatment-induced educational gains (Panel A, columns 1–4). Consistent with Table 1 results, these gains are concentrated among sons and daughters of literate parents (Panel B).[28] For children of illiterate parents, the aggregate investment index is unaffected and all three schooling attainment metrics are negatively impacted. The negative impacts are concerningly large for daughters in illiterate-parent households: for instance, treatment leads to a 26 percentage point decrease in the secondary school completion rate (p-value = 0.008; column 2). We can reject equality of effects between sons and daughters of illiterate parents (p-value = 0.098). As a result, the secondary school completion disparity between daughters of literate and illiterate parents increases from 7 percentage points in the control group to 47 percentage points in the treatment group (column 2). These findings support a broad literature on son preference in India, which shows that daughters' education is at greater risk than sons' when the household has competing economic needs.

In parallel, marriage and fertility trajectories diverge. Treatment delayed marriage for both sons and daughters of literate parents (column 5). Though we are under-powered to detect statistically significant effects when estimating separately by gender, the combined treatment effect on the marriage dummy for sons and daughters in literate households has

of these alternative outcomes, we continue to find significant increases for treatment children with literate parents.

[28] Heterogeneous impacts by gender and by gender interacted with parental literacy for individual components of the sub-indexes are shown in Appendix Table A6.

p-value of 0.085. For daughters, treatment lowers the likelihood that they report their labor force status as "housewife" by 29% (p-value = 0.017; column 7). Meanwhile, treatment sons in illiterate households are 78% *more* likely to be married at endline than their control counterparts (p-value = 0.087; column 5). They are also more than twice as likely to have had any children (p-value= 0.050; column 6). The estimated effects on marriage and fertility outcomes of daughters of illiterate parents are smaller and more noisily estimated. This likely reflects the fact that marriage and fertility rates are already quite high for this sub-group: at endline, 86% of daughters with illiterate parents in the control group are married and 69% have had a child (column 5).

4.2.3 Robustness Checks

In 2018, most children aged 6 or below at baseline were still studying while all children aged 18 or above had graduated (Appendix Figure A3). The patterns of results and statistical significance for Table 1 regressions are robust to varying the 7 and 17 age cut-offs for sample inclusion by ± 1 year (Appendix Tables A7 and A8). The results also hold when we expand to the full sample of children ever born to the client at baseline, including those older than 18 and younger than 6 (Appendix Table A9).

Appendix Figure A4 Panel B addresses concerns over multiple hypothesis testing: after FDR corrections, q-values of Tables 1 and 2 coefficients that were significant at traditional levels remain below 0.10. The smaller illiterate household sample size highlights the concern that treatment differences may reflect unobserved differences between literate and illiterate households. The fact that randomization inference based p-values and those from standard asymptotic inference show similar levels of statistical significance provides reassurance. We also provide a placebo check using the sample of children who were at least 18 years old in 2007. They are too old to have had treatment impact most educational decisions: at baseline, the majority of "old child" sample children (93%) had completed schooling. Consistent with this, we find no impacts on expenditures or attainment and no difference by parental literacy on any educational outcome for children in this age group (Appendix Table A9).

Examining how differing levels of parental literacy are associated with child schooling investment can further help assess the role of unobserved household characteristics. Pointing against spurious impacts, Appendix Table A10 Panel A shows that negative treatment effects are concentrated among households with the least educated parents – that is, households where both parents or the mother is illiterate. The latter finding mirrors previous findings from the intergenerational mobility literature.[29]

Finally, we turn to alternative measures of household educational status. Appendix Figure A6 graphs child educational outcomes for control and treatment groups against parental education measured by average years of schooling completion. While somewhat noisier, we see a very similar pattern: for households in which average parental education is less than four years of schooling (i.e. less than primary school completion), educational outcomes are similar or higher for control group households relative to treatment households. This pattern is reversed above this threshold with treatment positively impacting children's attainment.[30]

We also use two alternative measures of parental education based on years of education. Following Alesina et al. (2021), we construct an indicator variable for whether both parents completed primary school. Seventy-two percent of sample households fall into this category. In Appendix Table A10, Panel B shows that, with this measure, treatment-induced increases in educational attainment remain concentrated among children of parents who completed primary school. For instance, they are 10 percentage points more likely to complete secondary education (p-value = 0.067; column 5) and 13 percentage points more likely to attend college (p-value = 0.012; column 6) relative to the children of parents who completed primary school in the control group. Overall, children with treated parents who completed primary school gain an extra 0.72 years of education (p-value = 0.05; column 7). In contrast, treatment children of parents without primary school education do not see educational gains. However, while the coefficients for secondary school completion and years of education are negative for this group, the decline in educational attainment is no longer significant. Among the six outcomes for which we could reject equality of impacts between literate and illiterate households in Table 1, we can continue to reject equality of impacts with the alternative

[29] Akresh et al. (2023) shows increase in mother's, but not father's, educational attainment improves Indonesian children's educational outcomes. Similarly, using variation in parental compulsory schooling in Norway, Black et al. (2005) finds only mother's schooling matters for children's outcomes. Conversely, Chevalier (2004) exploit variation in parental schooling attainment in the UK and finds father's education matters for sons while mother's education matters for daughters.

[30] The bottom right panel also shows that both treatment and control groups saw rising absolute mobility over this period: Years of education among children, on average, exceeds that of their parents.

measure of parental education for all but one (attended college; column 6). In Panel C, we consider parental years of education. The patterns are similar but more noisily estimated.

Overall, estimates using alternative parental education measures remain consistent with the parental literacy estimates, although the declines in educational attainment among the less educated are somewhat sensitive to choice of educational measure. Our preferred interpretation is that illiteracy directly lowers expected returns to education for illiterate households (see Section 3.1). It is also the case that, in lower income settings like ours, years of education are a very noisy proxy for gains in learning. Reflecting this, our estimates suggest that parental literacy is most comparable to the primary schooling summary measure.

4.3 Impacts on household economic outcomes

Treatment impacts on children's human capital differ by parents' literacy, suggesting either that the intervention disproportionately impacted enterprise income for literate households, or that literate and illiterate households had different investment responses to similar income gains. To distinguish between these explanations we investigate treatment impacts on business growth.

4.3.1 Enterprise Outcomes and Household Income

To estimate the trajectory of economic outcomes Y_{hgt} for household h from microfinance group g, we separately estimate treatment effects for $t = \{2010, 2018\}$ as:

$$Y_{hgt} = \alpha + \beta T_g + \theta_g + \gamma X_{hg} + \epsilon_{hgt}. \tag{3}$$

T_g is the treatment dummy, θ_g is a vector of stratification dummies, and X_{hg} is a vector of control variables selected via double LASSO. In all regressions we also include a dummy indicator for proxy respondents. We report standard errors (clustered by loan group) and randomization inference p-values.

Table 3 presents both short-run (3 years post-intervention) and long-run (11 years post-intervention) treatment impacts on household enterprise outcomes.[31] We start with the short-run standardized economic index (column 1), followed by index components: profits,

[31] See Appendix Table A11 for treatment effects for the full sample that includes households without school-age children at baseline.

capital, and household income (columns 2–4). In Panel A, we see that treatment households score 0.29 standard deviations higher on the economic index than control group households (p-value = 0.014). They report, on average, 0.51 standard deviations higher weekly profits (p-value = 0.004) and 0.25 standard deviations higher enterprise capital (p-value = 0.086). Consistent with enterprise ownership being a primary source of earnings for households, treatment households report 0.11 standard deviations higher household income three years post-intervention (p-value = 0.330).[32] In Panel B, we examine economic outcomes separately for literate- and illiterate-parent households. Both groups report substantial economic gains. If anything, column (2) and (4) coefficients suggest a larger, albeit noisily estimated, treatment effect on profits and income for illiterate parent households (we cannot reject equality of treatment impact across groups). This suggests that the absence of educational investments by treated illiterate parents did not reflect an absence of short-run income gains.

In columns (5)–(8), we turn to long-run economic outcomes, as measured in 2018. For both treatment and control groups, profits, capital, and income decline over time (Panel A). Among control group households, enterprise profits are 73% of their 2010 level. This decline is consistent with households operating in a high risk environment where a large fraction of businesses fail to grow (Hsieh and Olken, 2014), though we cannot rule out other time-related factors (like clients retiring). Second, average treatment impacts remain positive but decline over time. In 2018, treatment households score 0.10 standard deviations higher on the economic index (p-value = 0.117; column 5); individually, the impacts on profits, capital, and income remain positive but statistically insignificant. This decline in treatment impacts on enterprise outcomes begins at least six years earlier: in an interim survey round in 2012 we find that, while treatment enterprises continue to report higher profits, capital, and income, average treatment impacts are no longer statistically significant (Appendix Table A13).

Strikingly, by 2018, treatment effects have fully diverged across literate and illiterate households (Panel B): illiterate treated households report a 0.24 standard deviation increase in profits (p-value = 0.026; column 6); a 0.45 standard deviation increase in enterprise capital (p-value = 0.060; column 7); and 0.09 standard deviations higher income than counterparts

[32] We present household income in levels to be consistent with other economic outcomes shown in Table 3. However, the outcome is noisily estimated; Appendix Table A12 considers household income measured in logs and finds treatment increases income by 19 percent in 2010 (significant at the 10 percent level).

in the control group (p-value = 0.025; column 8). Together, these gains translate to a 0.26 standard deviation increase in households' score on the economic index (p-value = 0.022; column 5). Literate households, on the other hand, do not see treatment impacts on any of these outcomes. Appendix Figure A7 shows these trends visually by plotting the trajectory of economic index by household treatment status and parental literacy over time.[33]

The temporal divergence in profits and household income between illiterate and literate households is consistent with differences in investment patterns: illiterate parents are more likely to invest in their business, whereas literate parents are more likely to invest in children's education. If capital and labor are complimentary in household enterprises, treatment could lead illiterate-parent households to increase workers. The need for labor may be met by resident children, further reducing illiterate-household children's schooling attainment. Table 4 reports treatment impacts on labor outcomes. In columns (1) and (2), we pool data on household and non-household workers in 2010 and 2018 and estimate a specification similar to equation (3), but where we include survey year fixed effects. Panel A shows no impact of treatment on average enterprise labor outcomes. In Panel B, once we allow for heterogeneity by parental literacy, impacts diverge: among literate-parent households, treatment reduces number of household workers by 31% (p-value = 0.052; column 1) with no significant change in number of non-household workers (column 2). Conversely, among illiterate-parent households, the number of non-household workers almost quadruples (p-value = 0.027) and the number of household workers almost doubles (p-value = 0.088), going from 0.19 to 0.36 workers. This pattern mirrors treatment impacts on enterprise capital (Table 3) and is consistent with complementarities between capital and labor within household enterprises.

In column (3), we turn to our school-age child sample and consider an indicator variable for whether a child was under 18 and self-employed in either the household enterprise or their own business at the time of the 2012 survey. Only 2% of the control group report being self-employed but, among children of illiterate parents, treatment leads to a six percentage point increase in this activity (p-value = 0.037). Conversely, we find no impact on self-employment among literate-parent children. Next, we examine school dropout. For each child who did

[33] Appendix Figure A7 also incorporates data from a 5-year enterprise survey in 2012. Possibly reflecting the fact that literate households were better able to cope with the microfinance crisis shock between 2010–12, we do not see a similar divergence in 2012 (Appendix Table A13).

not complete secondary school, we ask that child's parent why they dropped out of school early. Parents' stated primary reason is categorized as: economic considerations (money reasons, a good work opportunity, or the perception that school was not worthwhile); child ability (child disliked school or had low test scores); or marriage (dropout for marriage or pregnancy). Each indicator variable equals 0 if the child completed secondary school. In columns (4)–(6) of Panels A and B, we see no treatment impact on reason for school dropout for the pooled sample or for literate-parent children. For children of illiterate parents, on the other hand, treatment children are more than twice as likely to report dropping out of school due to economic considerations than their counterparts in the control group (p-value = 0.010; column 4). We anticipate that, among other reasons, drop out in this category includes work in the household business. Overall, we infer that the sharp declines in schooling among illiterate-parent children in the treatment group are due at least in part to a concurrent increase in the use of these children's labor in household enterprises.

As a robustness check for Table 3 and Table 4 outcomes, Appendix Figure A4 demonstrates that all economic outcomes that are significant at the 10% level have q-values below 0.10 after FDR corrections (for the pooled and for the heterogeneity by parental-literacy specifications). Additionally, Tables 3 and 4 show that p-values from randomization inference are very similar to those derived from standard asymptotic inference.

4.4 Alternative channels

Our preferred interpretation for the observed divergence in educational and business investments by parental literacy is differences in expected returns to children's education. One concern is that parental literacy may proxy for dimensions of sample heterogeneity that predict treatment effects on schooling. In Section 2.3, we discuss the possibility that household wealth or earnings differences may both be correlated with parental literacy and influence household credit constraints and present descriptive evidence that literate and illiterate households in our sample face similar credit constraints. We provide additional evidence in Table 5 that estimated treatment impacts by parental literacy on education outcomes and economic index are robust to including additional household and individual characteristics interacted with treatment. For child i we estimate regressions of the form:

$$Y_{ihg} = \alpha + \beta_0 T_g + \sum_j \beta_j T_g C_{hj} + \sum_j \pi_j C_{hj} + \theta_g + \phi_{ih} + \gamma X_{ihg} + \epsilon_{ihg}. \tag{4}$$

C_{hj} stands for characteristic j of household h measured at either household or client level.[34]

For comparison, Panel A reports baseline regressions where we omit interactions with client characteristics. Panel B regressions include three baseline variables that, in different ways, may proxy for household credit constraints. These include a socio-economic index, household size, and wage earner present in the household. Treatment impacts for literate parent sample remain robust and the point estimates actually rise. None of the client characteristics have explanatory power for treatment differences in schooling and economic outcomes. Panel C regressions show treatment impacts are robust to additionally including two client-level baseline characteristics – discount rate and female empowerment (an indicator variable for whether client has a major say in educational expenses).

A second concern is supply side differences: literate- and illiterate-parent households may differ in their access to high quality schooling. Our partner microfinance institution selects clients from similar neighborhoods, reducing this concern. That said, in Appendix Table A14 we examine whether our core heterogeneity results hold after conditioning on loan recipient neighborhoods. Panels A and B include thana and ward fixed effects respectively. Our sample includes 10 thanas with, on average, 11 wards per thana. Panel A results closely align with Tables 1 and 3. Panel B shows positive educational impacts for literate parent children remain large in magnitude and statistically significant (columns 1–4). Illiterate-parent children's treatment impacts are negative and similarly sized to our original specification, but significantly noisier. Long-term economic impacts for illiterate-parent households remain large in magnitude and statistically significant (column 6). Since loan officers must visit these households to collect repayments, VFS builds loan groups based on geographic proximity. Panel C estimates are for regressions with loan fixed effects; since treatment was assigned at loan-level we only estimate the differential impact for literate households: even among sample households in the same loan group, children of literate parents have

[34]Household- and client-level characteristics that are interacted with treatment are excluded from LASSO.

substantially higher educational attainment.

Taken together, the findings presented in Table 5 and Appendix Table A14 support the interpretation that differences in expected returns to education contribute to the observed differences in investment patterns across literate and illiterate parents.

5 Intergenerational Outcomes: Educational and Economic Mobility

The treatment differentially affected business growth and human capital attainment across more- and less-educated households. We now examine the implications of this pattern for intergenerational educational and earnings mobility. To be consistent with subsequent comparisons with the IHDS sample, we restrict the VFS sample throughout to sons.[35]

5.1 Educational mobility

The Section 4.2.2 results on parental literacy imply that treatment decreased intergenerational mobility — the association between parents' and children's ranks in the within-generation educational distribution — in our sample. To formally assess this, we estimate and compare rank–rank slopes for parent and son educational attainment for treatment and control sub-samples and quantify the degree to which treatment strengthened the association between parent and son outcomes (Chetty et al., 2014).[36]

Figure 3, Panel A provides a visual representation: if a child's education rank is entirely decided by her parents' education, the dotted 45-degree line results, whereas if there is no such relation, the dotted horizontal line at 0.5 results. The treatment group exhibits a steeper slope than the control group, indicating that parental education in treated households is more predictive of children's education; in other words, treatment-induced microenterprise expansion reduced within-sample intergenerational mobility. Appendix Table A15 regression estimates show that a one percentage point increase in parent education rank is associated with a 0.36 percentage point increase in child's rank in control households, whereas in treatment households we observe an additional 0.25 percentage point increase in a child rank

[35] The IHDS only collects educational outcomes for co-resident children implying high attrition for adult daughters who typically migrate at marriage.

[36] Parental education is measured as the average of mother's and father's years of educational attainment.

(p-value = 0.016; column 1). In other words, treatment expands the gap in expected rank between children from the most- and least-educated families by more than two-thirds.

Despite this, treatment may have increased intergenerational mobility at the *population* level. This is because, while children who benefited from treatment in our sample came from the upper ranks of the within-sample parent schooling distribution, they are more likely to belong to middle ranks of the population-level parent education distribution. As a result, population-level mobility may rise even while within-sample mobility falls. Indeed, Appendix Figure A8 shows that when compared to the nationally-representative 2012 IHDS sample, the distribution of parental education in our sample over-represents the middle of the distribution. This is consistent with the presence of microfinance screening criteria which exclude the least-educated and poorest households.[37]

To approximate the impact of treatment on population-level mobility, we identify a sub-sample of households within the IHDS sample that are comparable to our VFS study clients, which we define to be households in urban areas that meet the inclusion criteria for most microfinance lending (henceforth, 'IHDS microfinance sample').[38] Using the urban IHDS sample, we then estimate rank–rank slopes among sons and their parents, before and after adjusting sons' educational attainment within the IHDS microfinance sample by the predicted local treatment effect (at each parent education level) from our experimental estimates.[39] The results suggest that, at the population level, microenterprise growth — and the corresponding changes in human capital investment at different parent ranks — continues to imply a decrease in intergenerational rank–rank mobility. Figure 3, Panel B, shows that the slope of the rank–rank relationship between average parental education and children's education increases from 0.54 to 0.56 (Appendix Table A15; columns 2 and 3). More broadly, this exercise makes clear that the net effect on mobility of any microenterprise growth policy

[37] VFS checks home and enterprise ownership before loan approval. In IHDS, urban households with zero average parental years of education are 24% less likely to own a business than those with at least one year but no tertiary degree. Conversely, 11% of IHDS urban households have at least one parent with a tertiary degree, while 1% of VFS parents do. We restrict the IHDS sample to urban households with a co-resident adult son (age 18–28).

[38] We apply the following criteria (detailed in Appendix C): household operates a non-farm enterprise, owns the home they live in, and annual household income was below ₹120,000 (a 2011 central bank guideline for microcredit eligibility). These households make up 12% of the urban sample.

[39] We estimate local treatment effects in VFS sample with a local polynomial regression where we regress son's years of education on parents' level of education by treatment assignment (details in Appendix C).

is sensitive to whom exactly is targeted and — in particular — where target households fall in terms of parent education rank.

5.2 Economic Mobility

The effects of treatment on intergenerational economic mobility depend not only on educational mobility, but also on whether treated literate children's income gains from more years of schooling outweigh additional intergenerational transfers received by treated children of illiterate parents from higher household enterprise wealth. In other words, does treatment, which entails a decline in schooling, make children from illiterate households less wealthy in the long-run, notwithstanding possible bequest gains?

To gain insight into this question, we provide a back-of-the envelope calculation of the transfer size from illiterate treatment parents to their sons necessary to compensate for reduced earnings from lower educational attainment, in both absolute (compared to illiterate sons in the control group) and relative terms (compared to treated sons of literate parents). We obtain monthly earning estimates e_i from 2012 IHDS, causal estimates for returns to education r for men from Khanna (2023) and treatment estimates for sons' years of education t from Table A16 (Panel B, column 7).[40,41] Treatment-induced earnings difference among sons of literate and illiterate parents is given by $\Delta E_L = e_L \times [(1 + r \times t) - 1]$ and $\Delta E_I = e_I \times [(1 - r \times t) - 1]$ respectively. The Data Appendix details the estimation. We find that, at age 30, illiterate treatment sons require monthly transfers of ₹307 to be as wealthy as illiterate control sons, and monthly transfers of ₹1,336 to be fully compensated for treatment-induced differences in earned income between themselves and children of literate parents.[42]

[40] The estimates for years of education in Table A16 are based on assigning total years of completed education as of the time of the 2018 survey to currently enrolled children. Alternatively, we can impute the total years of education that currently-enrolled children will complete by estimating the years of education that control group children who have finished their education attain, conditional on completing a specific grade. For children that are currently enrolled in college, we assume that they complete their program. Using this as the outcome variable, treatment children of literate parents gain 1.12 years and treatment children of illiterate parents lose 0.47 years of education.

[41] Our estimation uses wages from 2012 IHDS and profits from the 2018 VFS survey (both converted to 2007 ₹). Importantly, CPI data from the World Bank and ILO wage data show no growth in real wages in India between 2012 and 2018.

[42] Khanna (2023) does not separately estimate returns to education by parental literacy; to the best of our knowledge, these causal estimates do not exist for India. We therefore assume the same r for both groups. Note that our main hypothesis for observed heterogeneity in investment decisions by parental literacy is that literate-parent households have higher expected returns (real or perceived) to children's education. If

Are these transfer sizes reasonable given estimated differences in treatment effects on enterprise wealth between literate and illiterate households? Assuming constant profit increases from treatment (at their 2018 level), treatment illiterate households would earn an extra ₹1,294 in monthly profits over and above their control group counterparts (column 5, Appendix Table A12). This means that illiterate parents would have to transfer 24% of their extra monthly profits to compensate their sons for their reduced earning ability. Moreover, even if illiterate treatment parents transferred all of their extra profits from the business to the sons, it would not be sufficient to prevent an increase in earnings inequality. Overall, these estimates suggest that treatment is likely to have increased earnings inequality between children of literate and illiterate parents, whereas children of illiterate parents are less likely to be worse off than they would have been in the absence of treatment.

6 Conclusion

Our findings demonstrate how a positive shock to household liquidity has long-term consequences for the next generation by raising human capital investment in children. To estimate intergenerational treatment effects, we needed data on all children who had ever been born, not just those who were living at home at the time of our follow-up survey. Our findings emphasize the importance of long-term follow-up surveys as well as evaluating intervention impacts using a broad definition of the household. They emphasize the need, from a policy standpoint, to look at relative treatment effects among more and less vulnerable populations, and not exclusively at average treatment effects (Deaton and Cartwright, 2018).

Average educational gains in our sample were accompanied by losses in relative intergenerational educational mobility during a period of economic growth. We show that the extent of such declines when such a program is scaled up is sensitive to assumptions about which population segment is targeted. Having said that, we believe the study's findings highlight the limitations of relying on income growth to reduce economic inequality. Our research also highlights the trade-offs inherent in encouraging microenterprise growth as an anti-poverty strategy.

real returns to education are higher for literate-parent sons, then our exercise underestimates the transfer size needed to prevent growth in wealth inequality while overestimating the transfer size needed to ensure that illiterate-parent sons are not worse off as a result of treatment.

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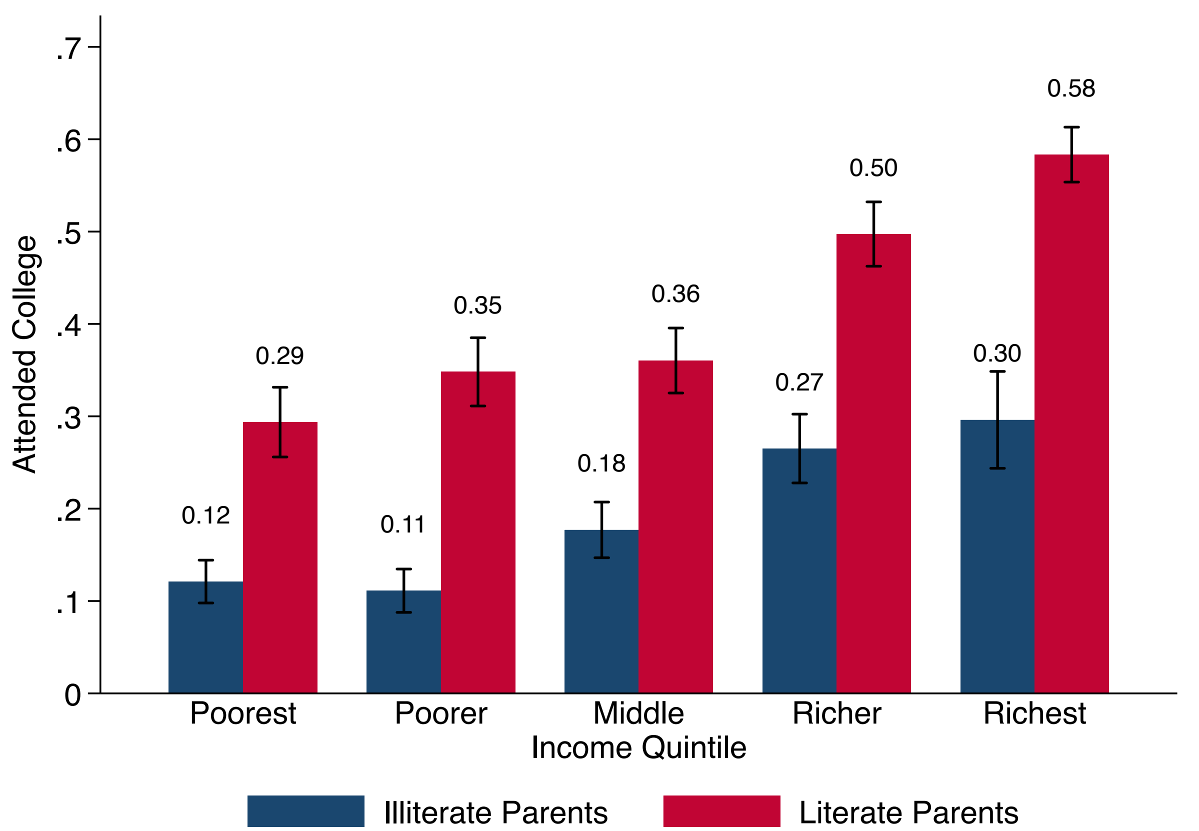
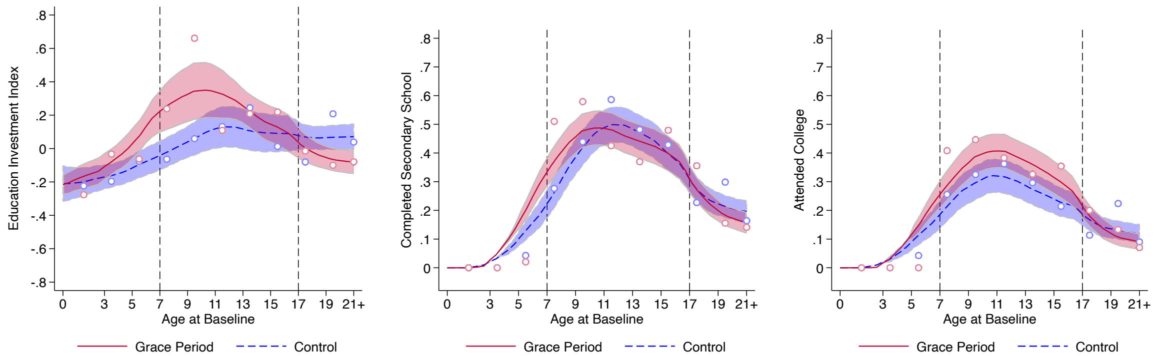
Figure 1: Sons' College Attendance and Household Wealth by Parental Literacy in Urban India



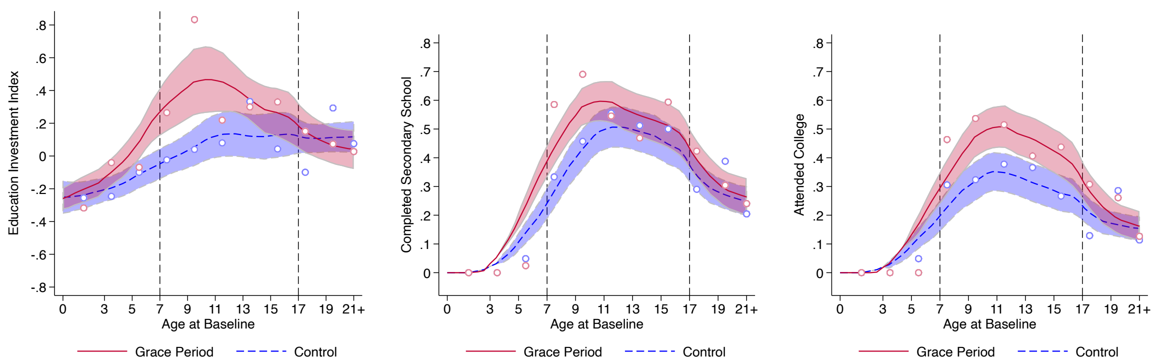
Notes: This figure uses panel data from the Indian Human Development Survey waves 1 and 2 (2005 and 2012). The sample is restricted to men who feature in the household roster in both survey waves; were between 11–21 years of age in 2005; and reside in an urban area (5,431 observations). The figure plots average college attendance rate in 2012 by household income quintile and parental literacy in 2005. The whiskers correspond to the 95% confidence intervals. See Data Appendix for additional details on sample construction.

Figure 2: Education Outcomes by Age and Treatment Group

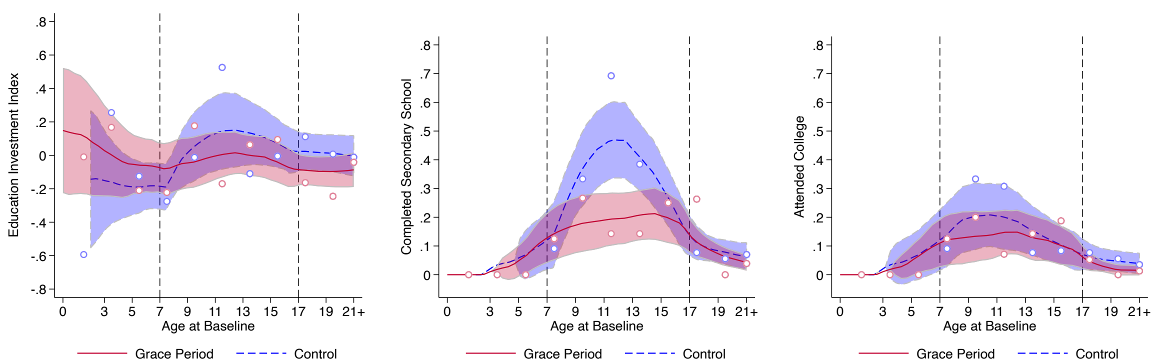
Panel A: Pooled



Panel B: Literate Parents

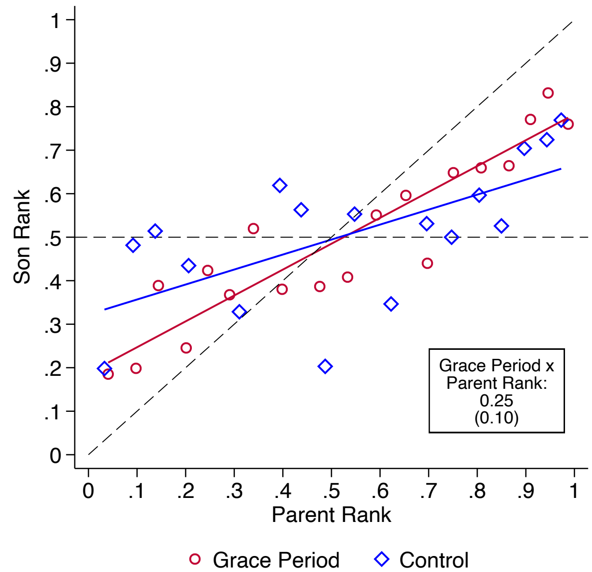


Panel C: Illiterate Parents

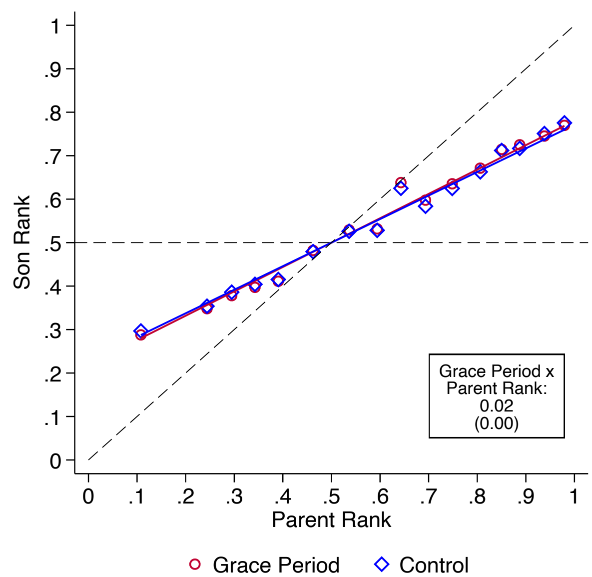


Notes: These figures plot the distribution of educational outcomes by child age at baseline. Figures in Panel A use the pooled sample of all children born prior to baseline (N=1,306) while Panels B and C show plots for the literate- and illiterate-parent subsamples (N=942 and N=362, respectively). The dotted vertical lines indicate the school-age child sample. We separately estimate local regressions (bandwidth = 2, kernel = epanechnikov) for children in treatment (solid line) and control (dotted line) households. The shaded areas correspond to 90 percent confidence intervals. The hollow circles correspond to the raw means of each outcome variable. See Data Appendix for more details on variable definitions.

Figure 3: Relative Educational Mobility



Panel A: Son-Parent Rank-Rank Relationship in VFS Sample



Panel B: Son-Parent Rank-Rank Relationship in IHDS Sample

Notes: These figures plot binned scatter plots of the rank–rank relationship between sons and parents education rankings. Parent's education is defined as the average of mother's and father's education. In Panel A, we show the rank–rank relationship for the treatment (red line and circles) and control groups (blue line and squares), separately, within the VFS sample. The VFS sample is limited to school-age sons and their parents (N=274). In Panel B, we show the status-quo relationship (blue line and squares) and the relationship adding the VFS treatment effects for the microfinance sons' subsample (red line and circles) in IHDS data. The 45-degree line corresponds to complete immobility and the horizontal line corresponds to perfect mobility. The IHDS sample is limited to sons (and their parents) who are 18-28 in IHDS (2012) data and who live in urban areas (N=6892). See Online Appendix Table A15 for the regression results.

Table 1: Treatment Effects on Educational Outcomes

	Investment Index	Primary School Investment Subindex	Secondary School Investment Subindex	College Spending (Standardized)	Completed Secondary School	Attended College	Years of Education
		Investment Index Components					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: School-Age Child Sample (7-17 Years at Baseline), Pooled							
Grace Period	0.18	0.10	0.25	0.15	0.05	0.10	0.34
	(0.08)	(0.08)	(0.08)	(0.08)	(0.04)	(0.04)	(0.29)
	[0.03]	[0.22]	[0.00]	[0.09]	[0.27]	[0.02]	[0.29]
Control Group Mean	-0.00	0.00	0.00	-0.00	0.42	0.27	10.49
Observations	543	543	543	543	543	541	543
Panel B: School-Age Child Sample (7-17 Years at Baseline), Heterogeneity by Parental Literacy							
Grace Period × Literate Parents	0.27	0.11	0.34	0.26	0.12	0.15	0.85
	(0.09)	(0.09)	(0.10)	(0.12)	(0.05)	(0.05)	(0.35)
	[0.00]	[0.23]	[0.00]	[0.04]	[0.05]	[0.01]	[0.05]
Grace Period × Illiterate Parents	0.03	0.05	0.02	-0.13	-0.14	-0.02	-1.04
	(0.11)	(0.11)	(0.09)	(0.13)	(0.06)	(0.06)	(0.47)
	[0.74]	[0.68]	[0.78]	[0.29]	[0.03]	[0.80]	[0.04]
p-value: Grace Period × Literate Parents =	0.08	0.63	0.01	0.03	0.00	0.04	0.00
Grace Period × Illiterate Parents	[0.08]	[0.64]	[0.02]	[0.03]	[0.00]	[0.04]	[0.00]
Control Group Mean (Literate Parents)	0.07	0.07	0.07	0.04	0.46	0.31	10.76
Control Group Mean (Illiterate Parents)	-0.22	-0.22	-0.21	-0.11	0.32	0.15	9.63
Observations (Literate Parents)	399	399	399	399	399	397	399
Observations (Illiterate Parents)	144	144	144	144	144	144	144

Notes: This table shows the effect of the grace period treatment on child educational outcomes as measured by the 2018 survey. In Panels A and B, the sample is children aged 7–17 (school-age) in 2007 (N=543). In Panel A, we regress each outcome on an indicator variable for assignment to grace period treatment, stratification dummies, child age fixed effects, an indicator for non-client respondent in 2018 survey, and baseline controls selected by LASSO (equation 1). Panel B reports a variant of equation (1) which includes the fully interacted effects of treatment and parental literacy (equation 2; we do not report the parental literacy dummy in the table). All regressions are estimated by OLS and standard errors clustered by loan group are reported in parentheses. Randomization inference p-values from 1,000 permutations of the treatment assignment are reported in brackets. Appendix Table A5 provides regression estimates for each index component contained in the sub-indices in columns (2)-(4). See Data Appendix for details on variable definitions and construction.

Table 2: Heterogeneous Treatment Effects by Gender

	Investment Index	Completed Secondary School	Attended College	Years of Education	Married	Any Children	Housewife
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: School-Age Child Sample (7-17 Years at Baseline), Heterogeneity by Gender							
Grace Period × Male	0.20	0.05	0.10	0.44	0.01	0.05	
	(0.11)	(0.06)	(0.05)	(0.37)	(0.05)	(0.04)	
	[0.08]	[0.41]	[0.07]	[0.29]	[0.77]	[0.20]	
Grace Period × Female	0.17	0.04	0.10	0.31	-0.05	-0.05	-0.12
	(0.09)	(0.06)	(0.06)	(0.40)	(0.06)	(0.06)	(0.06)
	[0.08]	[0.45]	[0.09]	[0.49]	[0.43]	[0.38]	[0.08]
p-value: Grace Period × Male =	0.78	0.93	0.99	0.81	0.33	0.14	
Grace Period × Female	[0.79]	[0.94]	[0.99]	[0.83]	[0.33]	[0.14]	
Panel B: School-Age Child Sample (7-17 Years at Baseline), Heterogeneity by Gender & Parental Literacy							
Grace Period × Literate Parents × Male	0.30	0.08	0.14	0.78	-0.06	0.01	
	(0.14)	(0.07)	(0.06)	(0.42)	(0.05)	(0.04)	
	[0.04]	[0.26]	[0.04]	[0.10]	[0.27]	[0.76]	
Grace Period × Illiterate Parents × Male	0.02	-0.03	-0.01	-0.67	0.18	0.18	
	(0.15)	(0.09)	(0.07)	(0.77)	(0.11)	(0.09)	
	[0.90]	[0.78]	[0.88]	[0.41]	[0.14]	[0.06]	
Grace Period × Literate Parents × Female	0.24	0.14	0.15	0.87	-0.10	-0.10	-0.16
	(0.11)	(0.07)	(0.07)	(0.47)	(0.07)	(0.07)	(0.07)
	[0.04]	[0.06]	[0.05]	[0.11]	[0.17]	[0.20]	[0.04]
Grace Period × Illiterate Parents × Female	0.05	-0.26	-0.02	-1.46	0.03	0.08	-0.05
	(0.14)	(0.10)	(0.11)	(0.70)	(0.10)	(0.11)	(0.13)
	[0.75]	[0.01]	[0.86]	[0.06]	[0.76]	[0.49]	[0.70]
p-value: Grace Period × Literate Parents × Male	0.74	0.52	0.92	0.88	0.60	0.16	
= Grace Period × Literate Parents × Female	[0.75]	[0.56]	[0.92]	[0.90]	[0.63]	[0.20]	
p-value: Grace Period × Illiterate Parents × Male	0.86	0.10	0.96	0.48	0.25	0.47	
= Grace Period × Illiterate Parents × Female	[0.85]	[0.09]	[0.95]	[0.51]	[0.27]	[0.50]	
Control Group Mean (Male, Literate Parents)	0.09	0.48	0.30	10.66	0.20	0.09	
Control Group Mean (Male, Illiterate Parents)	-0.19	0.27	0.17	9.27	0.23	0.10	
Control Group Mean (Female, Literate Parents)	0.05	0.44	0.32	10.87	0.62	0.47	0.55
Control Group Mean (Female, Illiterate Parents)	-0.25	0.37	0.14	9.94	0.86	0.69	0.69
Observations (Male, Literate Parents)	205	205	205	205	204	204	
Observations (Male, Illiterate Parents)	69	69	69	69	69	69	
Observations (Female, Literate Parents)	194	194	192	194	195	195	195
Observations (Female, Illiterate Parents)	75	75	75	75	75	75	75

Notes: This table shows the effect of the grace period treatment by gender on child educational outcomes as measured in the 2018 survey. The sample is children aged 7–17 (school-age) in 2007 (N=543). In Panel A, we regress each outcome on the fully interacted effects of treatment and child gender (dummy for child gender omitted from the table), stratification dummies, child age fixed effects, an indicator variable for non-client respondent to the 2018 survey, and baseline controls selected by LASSO (equation 2; we do not report gender dummy in the table). Panel B reports a variant of equation (2) which includes the fully interacted effects of treatment, child gender, and parental literacy (all related two-way interactions are included in regression but not reported in the table). All regressions are estimated by OLS and standard errors clustered by loan group are reported in parentheses. Randomization inference p-values are from 1,000 permutations of the treatment assignment and are reported in brackets. Appendix Table A6 provides the regression estimates for each index component entering the index in column (1). See Data Appendix for details on variable definitions and construction.

Table 3: Treatment Effects on Household Enterprise Outcomes

	2010 Survey				2018 Survey			
		Index Components				Index Components		
	Economic Index	Profits (Standard-ized)	Capital (Standard-ized)	Household Income (Standard-ized)	Economic Index	Profits (Standard-ized)	Capital (Standard-ized)	Household Income (Standard-ized)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Pooled								
Grace Period	0.29	0.51	0.25	0.11	0.10	0.08	0.19	0.02
	(0.12)	(0.18)	(0.15)	(0.12)	(0.06)	(0.07)	(0.15)	(0.03)
	[0.01]	[0.01]	[0.08]	[0.34]	[0.12]	[0.22]	[0.20]	[0.41]
Control Group Mean	0.00	-0.00	0.00	-0.00	-0.22	-0.24	-0.12	-0.31
Observations	363	363	363	363	381	381	381	381
Panel B: Heterogeneity by Parental Literacy								
Grace Period × Literate Parents	0.26	0.44	0.26	0.09	0.05	0.03	0.11	0.01
	(0.13)	(0.19)	(0.17)	(0.13)	(0.07)	(0.08)	(0.17)	(0.03)
	[0.04]	[0.02]	[0.12]	[0.52]	[0.52]	[0.74]	[0.54]	[0.78]
Grace Period × Illiterate Parents	0.39	0.66	0.29	0.21	0.26	0.24	0.46	0.09
	(0.20)	(0.38)	(0.23)	(0.23)	(0.11)	(0.11)	(0.24)	(0.04)
	[0.12]	[0.21]	[0.41]	[0.35]	[0.04]	[0.04]	[0.07]	[0.04]
p-value: Grace Period × Literate Parents = Grace Period × Illiterate Parents	0.60	0.61	0.92	0.61	0.11	0.10	0.23	0.11
	[0.68]	[0.71]	[0.94]	[0.60]	[0.15]	[0.14]	[0.26]	[0.17]
Control Group Mean (Literate Parents)	0.04	0.03	0.06	0.03	-0.20	-0.22	-0.09	-0.29
Control Group Mean (Illiterate Parents)	-0.16	-0.12	-0.25	-0.12	-0.32	-0.36	-0.24	-0.38
Observations (Literate Parents)	283	283	283	283	296	296	296	296
Observations (Illiterate Parents)	80	80	80	80	85	85	85	85

Notes: This table shows the effect of the grace period treatment on household income and enterprise outcomes from the 2010 (N=363) and the 2018 (N=381) surveys. In Panel A, we regress each outcome on an indicator variable for assignment to the grace period treatment, stratification dummies, an indicator variable for non-client respondent to the 2018 survey, and baseline controls selected by LASSO (equation 3). Panel B reports a variant of equation (3) which includes the fully interacted effects of treatment and parental literacy (the parental literacy indicator is included in regression but not reported in the table). All regressions are estimated by OLS and standard errors clustered by loan group are reported in parentheses. Randomization inference p-values are from 1,000 permutations of the treatment assignment and are reported in brackets. Appendix Table A12 provides regression estimates for the non-standardized index components in columns (2)-(4) and (6)-(8). See Data Appendix for details on variable definitions and construction.

Table 4: Treatment Effects on Dropout and Child Labor

	Household Sample		Child Sample			
				Whether dropped out due to		
	Number of Household Workers	Number of Non-Household Workers	Ever Self-Employed Under 18	Economic Considera-tions	Child Ability	Marriage
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Pooled						
Grace Period	-0.05	-0.02	-0.00	0.02	-0.02	-0.01
	(0.05)	(0.14)	(0.01)	(0.04)	(0.04)	(0.03)
	[0.38]	[0.91]	[0.98]	[0.52]	[0.73]	[0.87]
Control Group Mean	0.32	0.53	0.02	0.21	0.21	0.11
Observations	725	724	540	533	533	532
Panel B: Heterogeneity by Parental Literacy						
Grace Period × Literate Parents	-0.11	-0.16	-0.02	-0.04	-0.02	-0.03
	(0.06)	(0.16)	(0.02)	(0.04)	(0.04)	(0.03)
	[0.08]	[0.37]	[0.32]	[0.38]	[0.76]	[0.35]
Grace Period × Illiterate Parents	0.17	0.50	0.06	0.20	-0.04	0.06
	(0.10)	(0.23)	(0.03)	(0.08)	(0.08)	(0.05)
	[0.09]	[0.07]	[0.07]	[0.05]	[0.65]	[0.31]
p-value: Grace Period × Literate Parents = Grace Period × Illiterate Parents	0.02	0.02	0.01	0.01	0.77	0.12
	[0.02]	[0.04]	[0.02]	[0.03]	[0.77]	[0.17]
Control Group Mean (Literate Parents)	0.35	0.62	0.03	0.22	0.17	0.11
Control Group Mean (Illiterate Parents)	0.19	0.13	0.00	0.15	0.34	0.11
Observations (Literate Parents)	564	563	398	392	392	391
Observations (Illiterate Parents)	161	161	142	141	141	141

Notes: This table shows the effect of the grace period treatment on household labor and child labor outcomes. Columns (1) and (2) use pooled household sample across 2010 (N=363) and 2018 (N=381). In Panel A, we regress each outcome on an indicator variable for assignment to grace period treatment, stratification dummies, an indicator variable for non-client respondent in 2018 survey, an 2018 survey wave fixed effect and baseline controls selected by LASSO (equation 1). Panel B reports a variant which includes the fully interacted effects of treatment and parental literacy (equation 2; we do not report the parental literacy dummy in the table). Column (3) estimate child-level regressions on an outcome that is constructed from the 2012 and 2018 survey. Columns (4)-(6) estimate child-level regressions on outcomes from the 2018 survey. Reductions in child sample from 543 reflects missing data on outcome variables. All regressions are estimated by OLS and standard errors clustered by loan group are reported in parentheses. Randomization inference p-values are from 1,000 permutations of the treatment assignment and are reported in brackets. See the Data Appendix for details on variable definitions and construction.

Table 5: Alternative Channels of Influence

	Education Outcomes				Economic Outcomes	
	Investment Index	Completed Secondary School	Attended College	Years of Education	2010 Economic Index	2018 Economic Index
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Parental Literacy Only						
Grace Period × Literate Parents	0.25	0.25	0.16	1.93	-0.15	-0.22
	(0.13)	(0.08)	(0.08)	(0.60)	(0.23)	(0.13)
	[0.06]	[0.01]	[0.04]	[0.00]	[0.62]	[0.12]
Panel B: Household-level Covariates						
Grace Period × Literate Parents	0.27	0.23	0.18	1.82	-0.04	-0.23
	(0.13)	(0.09)	(0.08)	(0.67)	(0.22)	(0.13)
	[0.02]	[0.00]	[0.01]	[0.00]	[0.88]	[0.10]
Grace Period × Socio-Economic Index	-0.00	-0.01	0.01	-0.11	-0.08	0.01
	(0.09)	(0.03)	(0.03)	(0.22)	(0.10)	(0.06)
	[0.83]	[0.90]	[0.59]	[0.54]	[0.28]	[0.60]
Grace Period × Household Size	0.05	0.00	0.03	0.12	0.03	-0.02
	(0.06)	(0.03)	(0.03)	(0.24)	(0.06)	(0.05)
	[0.52]	[0.71]	[0.41]	[0.23]	[0.59]	[0.54]
Grace Period × Wage Earner	0.19	0.02	0.02	-0.14	-0.42	-0.18
	(0.16)	(0.08)	(0.08)	(0.57)	(0.23)	(0.12)
	[0.26]	[0.88]	[0.91]	[0.96]	[0.10]	[0.18]
Panel C: Additional Individual Characteristics						
Grace Period × Literate Parents	0.22	0.25	0.15	1.62	-0.06	-0.23
	(0.12)	(0.09)	(0.09)	(0.77)	(0.22)	(0.12)
	[0.04]	[0.00]	[0.03]	[0.00]	[0.92]	[0.13]
Grace Period × Socio-Economic Index	0.02	0.00	0.02	0.11	-0.11	-0.00
	(0.09)	(0.03)	(0.03)	(0.26)	(0.10)	(0.06)
	[0.77]	[0.86]	[0.56]	[0.61]	[0.23]	[0.62]
Grace Period × Household Size	0.03	-0.00	0.02	0.08	0.06	-0.01
	(0.07)	(0.03)	(0.03)	(0.21)	(0.06)	(0.05)
	[0.45]	[0.62]	[0.39]	[0.21]	[0.39]	[0.69]
Grace Period × Wage Earner	0.24	0.01	0.03	0.08	-0.43	-0.21
	(0.16)	(0.08)	(0.08)	(0.60)	(0.23)	(0.12)
	[0.17]	[0.88]	[0.90]	[0.77]	[0.11]	[0.09]
Grace Period × Impatient	-0.01	0.14	0.02	0.69	-0.22	-0.03
	(0.15)	(0.08)	(0.08)	(0.67)	(0.23)	(0.12)
	[0.34]	[0.14]	[0.70]	[0.82]	[0.56]	[0.67]
Grace Period × Empowered Mother	-0.05	-0.18	-0.05	-0.70	-0.08	-0.24
	(0.20)	(0.11)	(0.10)	(0.70)	(0.22)	(0.16)
	[0.95]	[0.23]	[0.86]	[0.72]	[0.73]	[0.15]
Control Group Mean	-0.00	0.42	0.27	10.49	0.00	-0.22
Observations	543	543	541	543	363	381

Notes: This table shows how the effect of the grace period treatment on child-level education outcomes and household-economic outcomes differs along different household and individual characteristics. Columns (1)-(4) estimate child-level regressions on outcomes from 2018 survey (N=543). Columns (5)-(6) estimate household-level regressions on outcomes from 2010 (N=361) and 2018 (N=381) surveys. We regress each outcome on a grace period indicator variable, baseline household characteristics (listed in table and described in Data Appendix), interaction of grace period dummy and these characteristics, stratification dummies, non-client respondent indicator variable, and baseline controls selected by LASSO excluding characteristics interacted with the grace period dummy (equation 4; we do not report the grace period dummy and the household characteristics in the table). Child-level regressions include child age fixed effects. All regressions are estimated by OLS and standard errors clustered by loan group are reported in parentheses. Randomization inference p-values are from 1,000 permutations of the treatment assignment and are reported in brackets. See the Data Appendix for details on variable definitions and construction.