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. Our findings suggest that microenterprise growth has the potential to reduce 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 support a growing body of evidence showing that parental education is a strong predictor of child schooling outcomes, as expected returns to children's education vary

[3] Blattman et al. (2020) is the one exception studying the long-run effect of a cash transfer on child outcomes. Unlike 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. For a Bosnia and Herzegovina credit program, Augsburg et al. (2015) find suggestive evidence that the credit shock increased child labor among low-educated borrowers. The Attanasio et al. (2015) sample and 71% of 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).

with parents' human capital (Brown, 2006; Black and Devereux, 2011; Boneva et al., 2021; Chakravarty and Agarwal, 2021). We provide supporting evidence that marginal propensities to invest in child schooling as business income grows vary with parental education. Our findings 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. Our findings provide one explanation for India's low 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 were placed in five-member groups, which were then 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 that, on average, a client spent 83 percent of her loan on business-related activities. Moreover, 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 spending over the subsequent decade. That is, as households continue to realize the returns to more profitable microenterprises, how do they allocate these gains across reinvestment in their business versus other categories of expenditure?

To gain preliminary evidence on the patterns of long-run differences in household spending, Appendix Table A2 examines treatment effects on broad categories of non-business expenditure. Ten years after the initial intervention, we observe no treatment impacts on household consumption (food, alcohol/cigarettes, festivals, and housing; columns 1-4), health care spending (column 5), savings or migration (columns 9-10).[5] Nor do we see evidence that households reallocated household resources towards *additional* members: Treatment has no effect on household size or the fertility behavior of women who were still of childbearing age (under 40) at the time of the intervention (columns 7 and 8). In contrast, treatment households report a roughly 37% increase in educational expenditures (column 6).

Given these patterns, we focus our analysis on how treatment-induced income gains shape household choices across investments in children's education and household enterprises. The fact that micro-enterprise capital and educational human capital are the only relevant investment margins also makes sense for this population: 89% of our sample households had completed fertility by the start of the experiment in 2007, which also means that their children are well past the critical window for child health investments.[6] Access to secure savings products is low among our MFI client population: only 18% of sample households had a savings account at baseline.[7] Finally, migration rates among this subsample are also

[5]Data on alcohol, cigarette, and total food expenditures is only available in the 2018 survey. The recall period for festival, renovation, health, and education expenditures is 30 days in both survey rounds. The recall period for food, alcohol, and cigarette expenditures is 7 days.

[6]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.

[7]Our partner MFI provides clients access to loans but not savings accounts, as Indian regulation prevents

low given that they are living in a thriving urban center and have access to finance and a viable business.

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). 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).[8] 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.[9] 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-(secondary)-school tutoring is associated with a 36 percentage point increase in college attendance.[10] 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

MFIs from holding savings.

[8]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.

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

[10]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)

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.[11]

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 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.

[11] 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.

2.3 Parental Education and Investment Choices

Our focus on the role of parental education in shaping household investment choices is motivated by a significant body of empirical evidence documenting a positive association between parent and child educational outcomes (with several studies using empirical designs that allow them to identify a causal link).[12] These patterns are evident in urban India: Figure 1, based on the nationally representative IHDS survey, shows that sons of literate parents are more likely to attend college in 2012 than sons of illiterate parents across all 2005 family income quintiles, with the gap rising with wealth.[13] 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.

What may explain this positive correlation? 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. Parental education may directly increase returns to children's schooling by equipping parents with the skills needed to assist children with schoolwork or otherwise help them in accumulating human capital (Todd and Wolpin, 2007; Banerji et al., 2017). These skills may include subject matter knowledge or other tools acquired via schooling, such as cognitive endurance (Brown et al., 2022). Evidence also shows that more educated parents spend more time on child care (Guryan et al., 2008).

[12] 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.

[13] 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.

Additionally, parental education may also have an indirect effect on children's education through the heritability of traits such as learning ability (Black and Devereux, 2011).

Less-educated parents may invest less in their children's education not because actual returns are lower, but because they mis-perceive them to be so. Multiple empirical studies document that less-educated households are more likely to underestimate returns to schooling.[14] 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 would give rise to lower educational investments.

Parents with lower levels of education tend to be poorer and may face more severe credit constraints, which could limit their absolute investment in children's education relative to more educated households (Galor and Zeira, 1993; Banerjee, 2004). These constraints 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, credit constraints may lead less-educated households to prefer business over schooling investments 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

[14]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. Sequeira et al. (2016) show, in India, that parents update about the value of schooling upon observing schooling success among their children's peers; less-educated parents' underestimation of returns may be due in part to limited exposure of successful pupils within their social circle.

uses enterprise ownership and home ownership as selection criteria, and screens clients on repayment ability.[15] 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.[16] They also have comparable household sizes, suggesting similar labor shadow costs. In addition, time preference data show that clients in literate and illiterate households are equally impatient. They also receive comparable loan amounts, and exhibit comparable rates of default.[17] Survey data collected upon study loan cycle completion 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 A3 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 less-educated parents invest fewer income gains in children's education in our sample mainly due to lower expected returns to children's schooling.[18]

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

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

[17] 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.

[18] 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.

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 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.[19] For a child still in school, secondary school completion is coded as 0.[20]

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%

[19]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.

[20]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).

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 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.[21] 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.[22] Parental literacy, in particular, as a skill-based measure of

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

[22] 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.

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 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.[23]

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.[24]

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).[25] Appendix Table A4 Panel A shows

[23]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.

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

[25]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.)

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 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.[26] Appendix Table A5 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 A8 and A9 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.[27]

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

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

[27] 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).

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 set of outcomes but from our heterogeneity analysis by parental education for the school-age sample (Panel B of Tables 1, 3, and 4).[28] 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

[28] 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.

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 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 A6 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).[29] For all three educational attainment measures, we can reject 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).[30] 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

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

[30] Heterogeneous impacts by gender and by gender interacted with parental literacy for individual components of the sub-indexes are shown in Appendix Table A7.

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 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 A8 and A9). 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 A10).

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 A10).

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 A11 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.[31]

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.[32]

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 A11, 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

[31] 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.

[32] 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.

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 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.[33] We start with the short-run standardized economic index (column 1), followed by index components: profits, 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).[34] 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,

[33] See Appendix Table A12 for treatment effects for the full sample that includes households without school-age children at baseline.

[34] 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 A13 considers household income measured in logs and finds treatment increases income by 19 percent in 2010 (significant at the 10 percent level).

average treatment impacts are no longer statistically significant (Appendix Table A14).

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 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.[35]

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

[35] 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 A14).

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 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.[36]

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 A15 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

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

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 substantially higher educational attainment.

Taken together, the findings presented in Table 5 and Appendix Table A15 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.[37]

5.1 Educational mobility

The results in Section 4.2.2 on parental literacy imply that treatment decreased intergenerational mobility, or 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. We quantify the degree to which treatment strengthened the association between parent and son outcomes (Chetty et al., 2014).[38]

Figure 3 provides a visual representation: if a child's education rank is entirely decided by her parents' education, the dotted 45-degree line results; whereas absent such a relation, the dotted horizontal line at 0.5 results. The treatment group has 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

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

[38]Parental education is measured as the average of maternal and paternal educational attainment.

within-sample intergenerational mobility. Appendix Table A16 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. However, treatment households show an additional 0.25 percentage point increase in a child rank (p-value = 0.016; column 1). In other words, treatment widens the expected rank difference between children from the most- and least-educated families by more than two-thirds.

While treatment reduced education mobility in the sample, its impact on population-level education mobility depends on household distribution across parental ranks. For example, the treatment could increase *population-level* intergenerational mobility if children from higher ranks of our within-sample parent schooling distribution 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. Appendix Figure A8 shows that relative to the nationally-representative 2012 IHDS sample, our sample's distribution of parental education over-represents the middle of the distribution. This aligns with microfinance screening criteria, which exclude the least-educated and poorest households.[39]

To assess the potential impact of treatment on population-level mobility, we utilize the IHDS urban sample and identify a subsample of households who meet typical microfinance inclusion criteria (henceforth, 'IHDS microfinance sample').[40] Using the full urban IHDS sample, we estimate rank–rank slopes between 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).[41] At the population level, microenterprise growth and the corresponding changes in human capital investment at different parent ranks continue to imply a decrease in intergenerational rank–rank mobility (see Appendix Figure A9). In Appendix Table A16 we see that the slope of the rank–rank relationship

[39]VFS verifies 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, compared to 1% of VFS parents. We restrict the IHDS sample to urban households with a co-resident adult son aged 18–28.

[40]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 comprise 12% of the urban sample.

[41]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 (see Appendix C).

between average parental education and children's education increases from 0.54 to 0.56 (columns 2 and 3). This predicted IHDS sample effect size is less than the VFS treatment effect. In addition to the forces identified above, it is also the case that only a small part of the overall population meets the typical microfinance eligibility criteria. To summarize, this exercise shows that the net effect on educational mobility of any policy targeting microenterprise growth will be dependent on where target households fall in terms of parent education rank and what fraction of the population is covered by the policy.

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 A17 (Panel B, column 7).[42,43] 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. Khanna (2023) does not separately estimate returns to

[42]The estimates for years of education in Table A17 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.

[43] 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.

education by parental literacy; to the best of our knowledge, these causal estimates do not exist for India. Therefore, we 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 (actual or perceived) to children's education. If literate-parent sons have higher real returns to education, then our exercise underestimates the transfer size needed to prevent wealth inequality from increasing while overestimating the transfer size needed to ensure that illiterate-parent sons are not made worse off by treatment. 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.

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 A13). 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.

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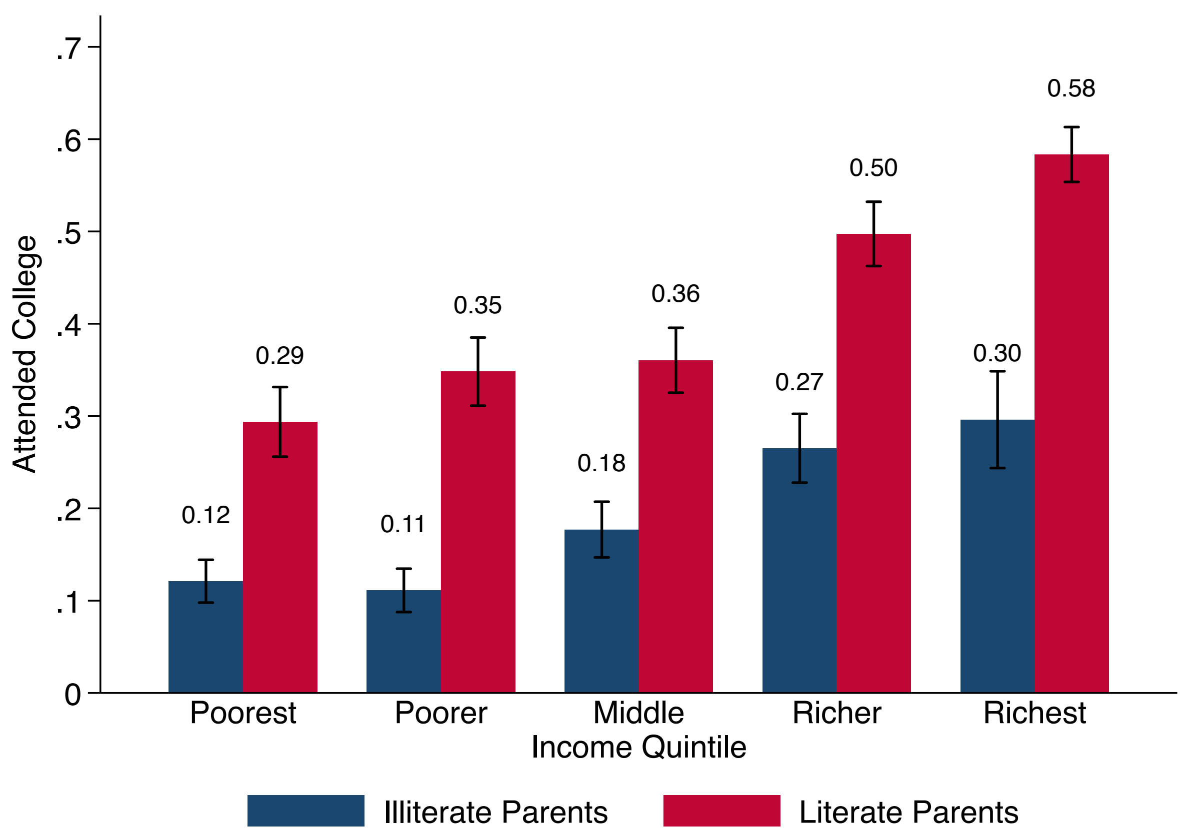
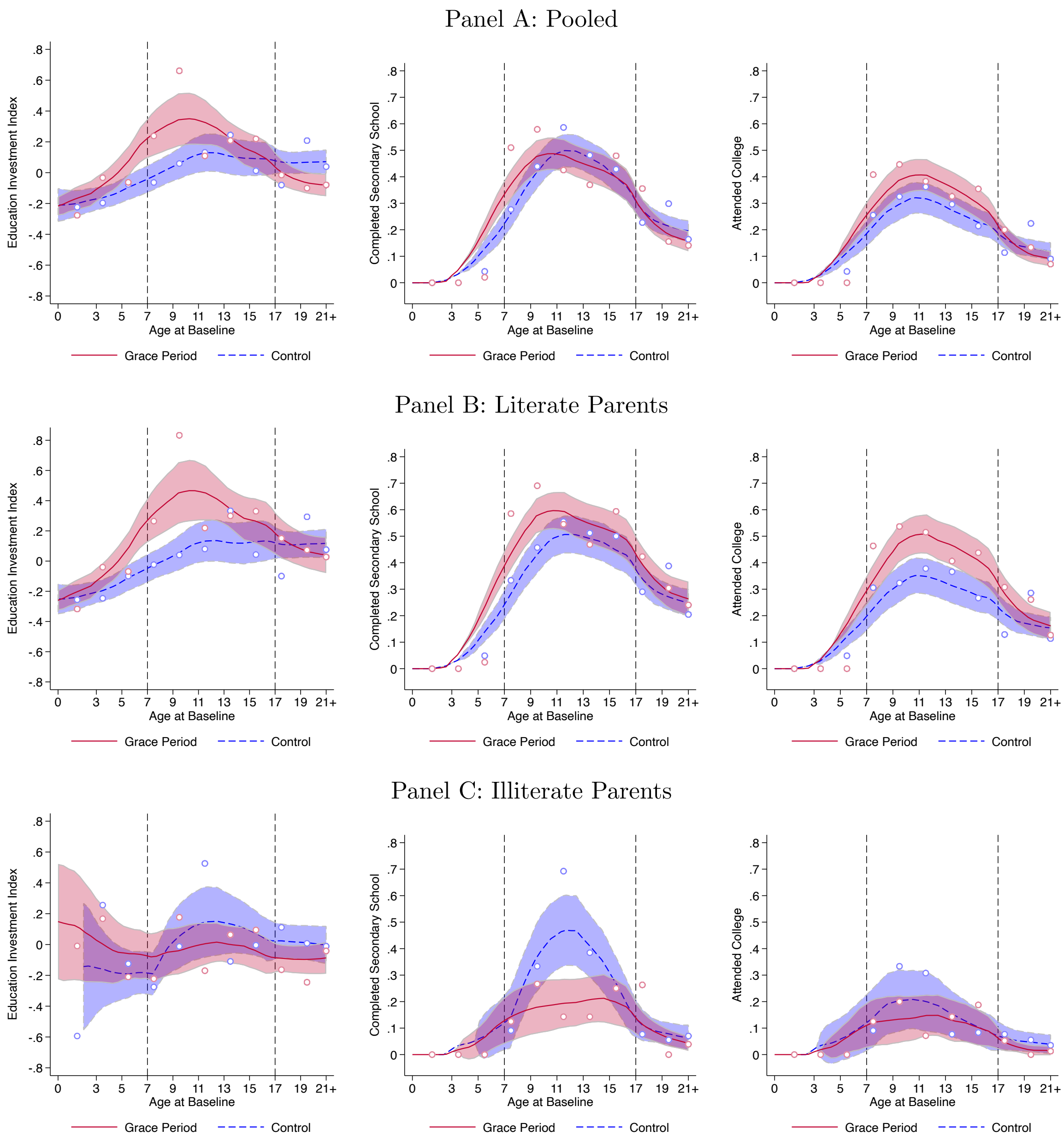
Figure 1: Sons' College Attendance and Household Wealth by Parental Literacy in Urban India



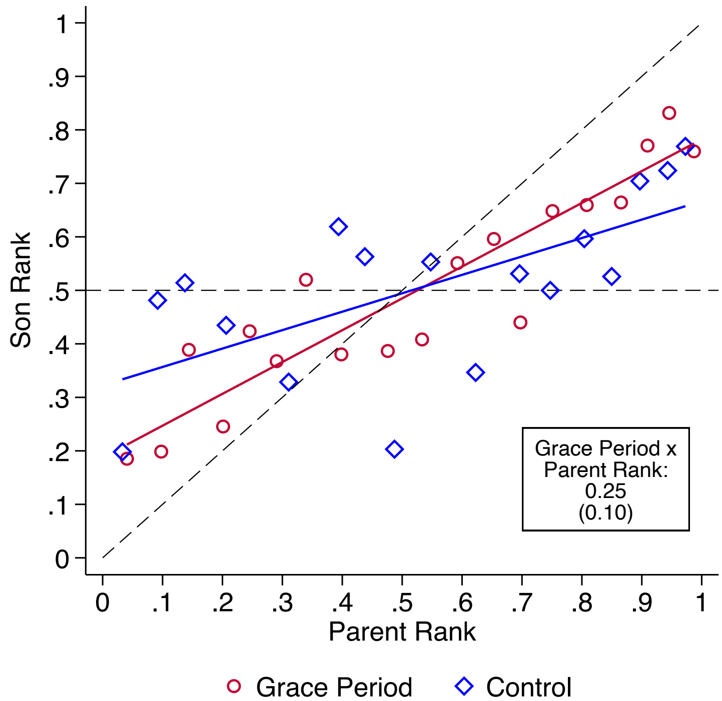
Notes: This figure uses panel data from the Indian Human Development Survey waves 1 and 2 (2005 and 2012). The sample is restricted to men who feature in the household roster in both survey waves; were between 11–21 years of age in 2005; and reside in an urban area (5,431 observations). The figure plots 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



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: Son-Parent Rank-Rank Relationship by Treatment Group



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. 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). The 45-degree line corresponds to complete immobility and the horizontal line corresponds to perfect mobility. See Online Appendix Table A16 for the regression results.

Table 1: Treatment Effects on Educational Outcomes

	Investment Index	Investment Index Components: Primary School Investment Subindex	Investment Index Components: Secondary School Investment Subindex	Investment Index Components: College Spending (Standardized)	Completed Secondary School	Attended College	Years of Education
	(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 A6 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 A7 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 (Standardized)	Capital (Standardized)	Household Income (Standardized)	Economic Index	Profits (Standardized)	Capital (Standardized)	Household Income (Standardized)
	(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 =	0.60	0.61	0.92	0.61	0.11	0.10	0.23	0.11
Grace Period × Illiterate Parents	[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 A13 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

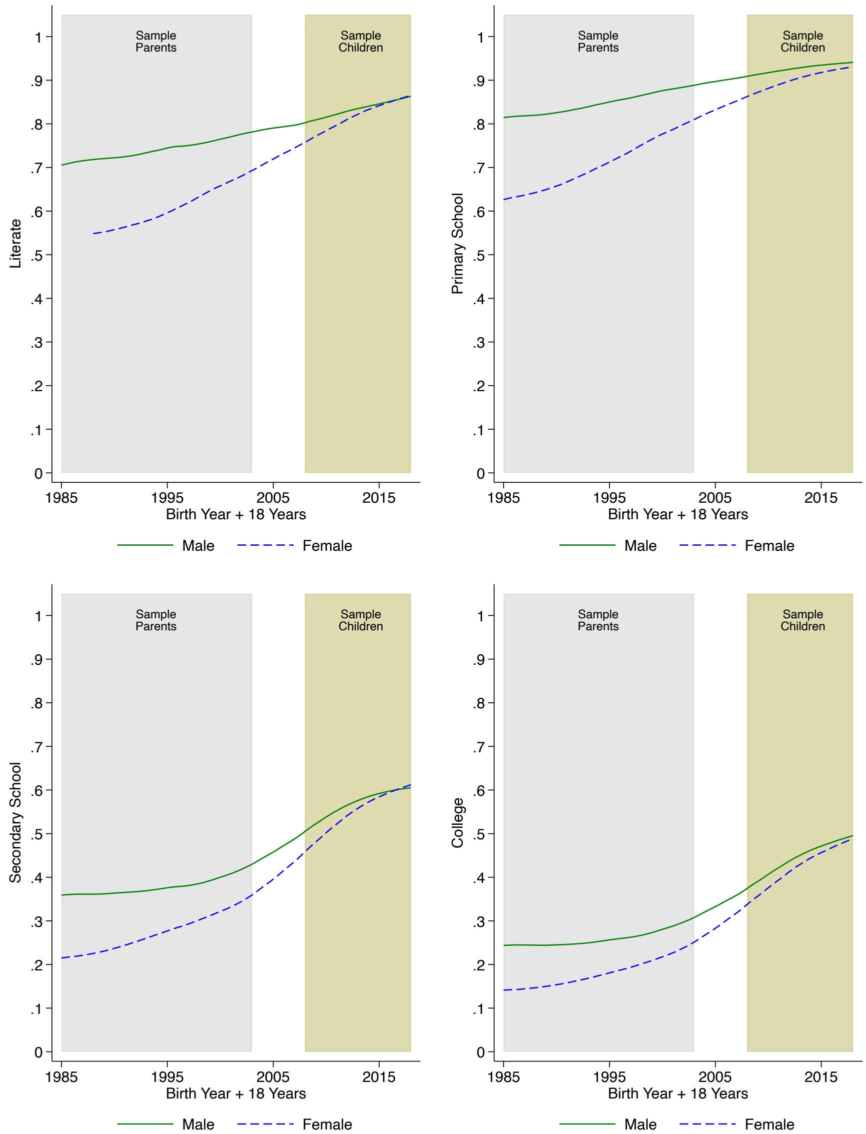
	Education Outcomes				Economic Outcomes	
	Investment Index	Completed Secondary School	Attended College	Years of Education	2010 Economic Index	2018 Economic Index
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Parental Literacy Only						
Grace Period × Literate Parents	0.25	0.25	0.16	1.93	-0.15	-0.22
	(0.13)	(0.08)	(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.

For Online Publication

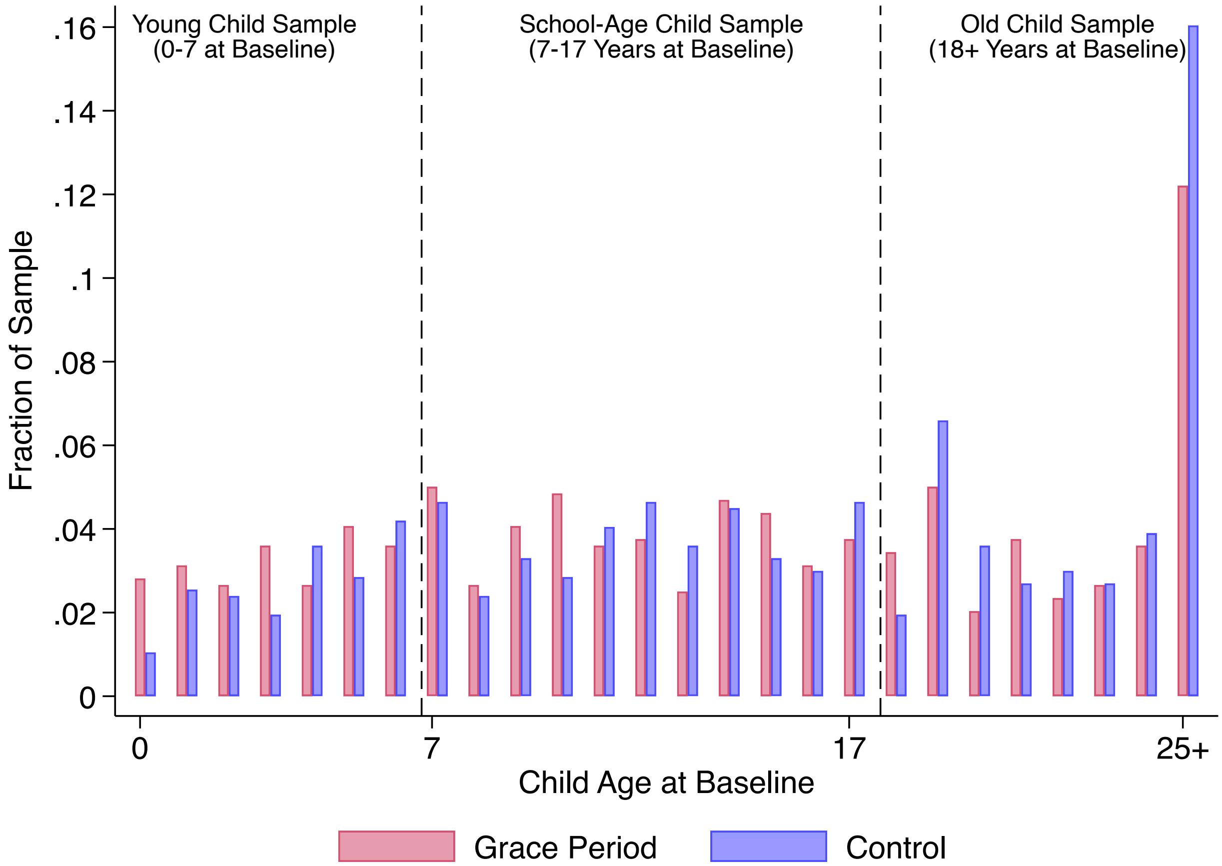
A. Appendix Tables and Figures

Figure A1: Educational Trends in India



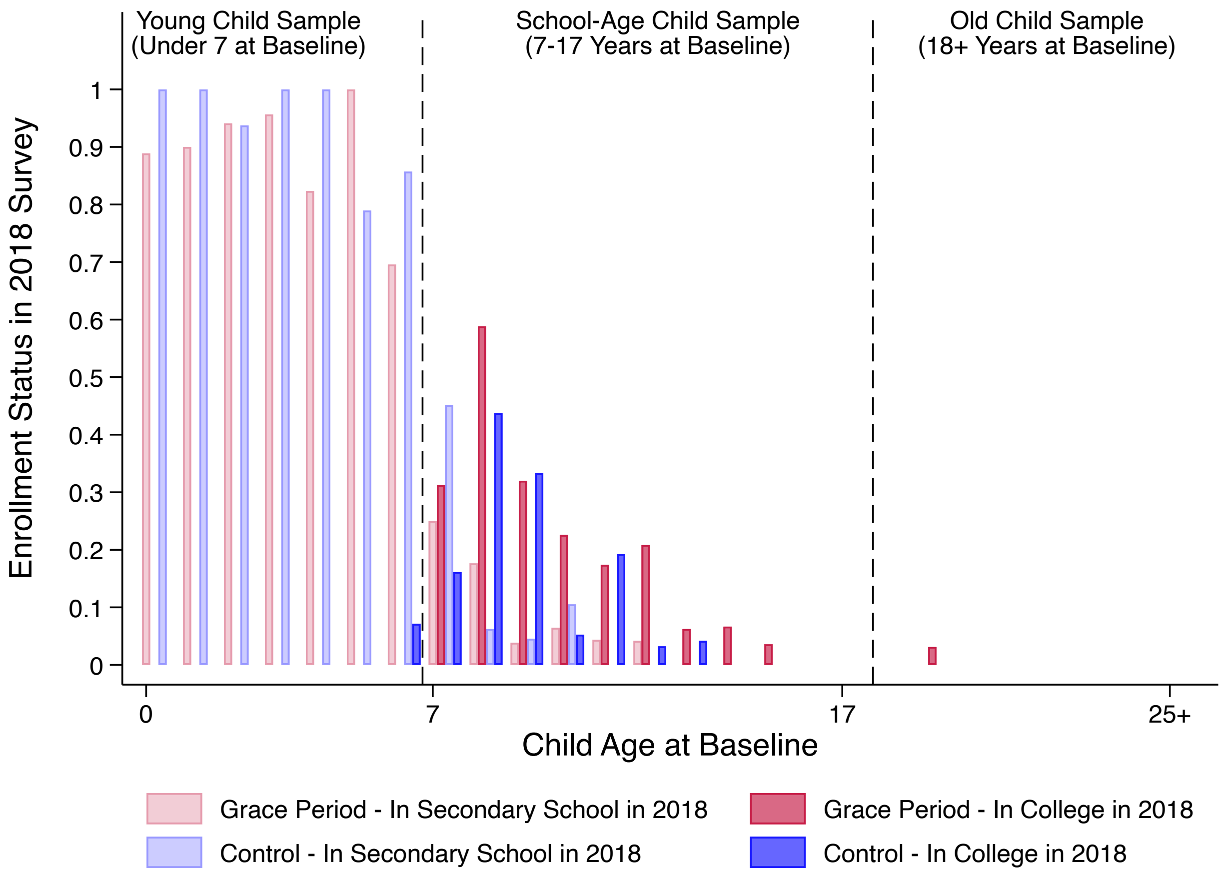
Notes: These figures plot trends in educational attainment by gender across birth year cohorts in the National Family Health Survey-5. The lines correspond to local regressions (bandwidth = 2, kernel = epanechnikov). The x-axis shows the year in which the person turned 18 years of age and the y-axis varies by panel. Clockwise from the top left panel, the y-axis shows the following outcomes: literacy; primary school completion; secondary school completion; and any college attendance. In all panels, the solid line corresponds to men and the dotted line corresponds to women. The right shaded area in each panel denotes the age range of the VFS school-age child sample (aged 7-17 years at baseline) and the left shaded area in each panel denotes the age range of their parents. The sample includes all individuals aged 18-80 in urban areas in the National Family Health Survey-5 (175,372 observations for the top left panel and 349,115 observations for all other panels).

Figure A2: Histogram of Child Age



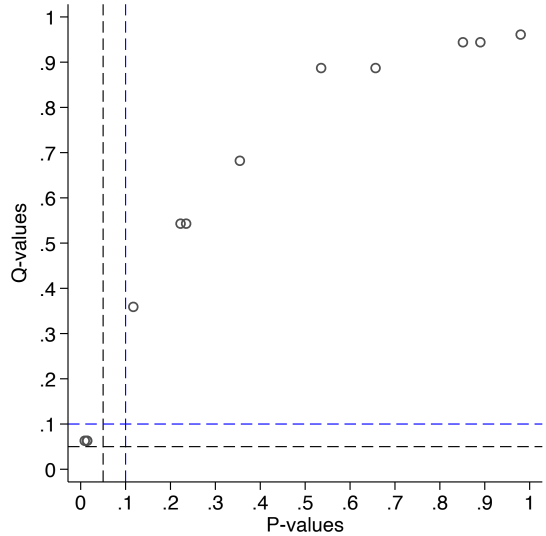
Notes: This figure shows the distribution of sample children by age at baseline separately by treatment and control (N=1,303). There are 268 observations in the young child sample, 543 in the school-age child sample, and 492 in the old child sample. The two dotted lines denote the child age cut-offs (7 and 17 years old) for inclusion in our school-age child sample.

Figure A3: Enrollment Status by Child Age at Baseline

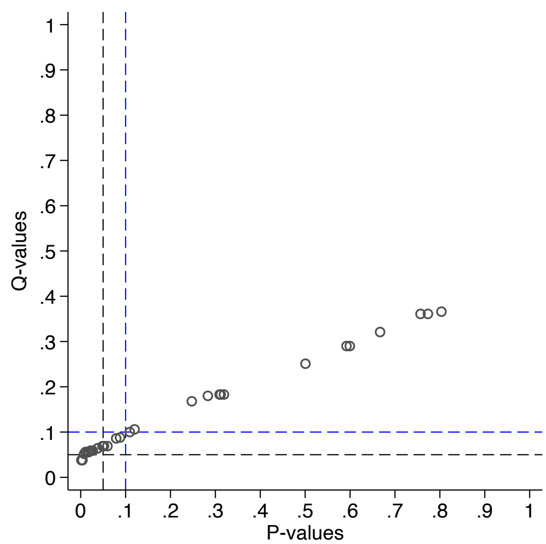


Notes: This figure shows enrollment in either secondary school or college in 2018 by child age at baseline by treatment group (N=1,303). There are 268 observations in the young child sample, 543 in the school-age child sample, and 492 in the old child sample. The two dotted lines denote the child age cut-offs (7 and 17 years old) for inclusion in our school-age child sample.

Figure A4: Corrections for Multiple Hypothesis Testing



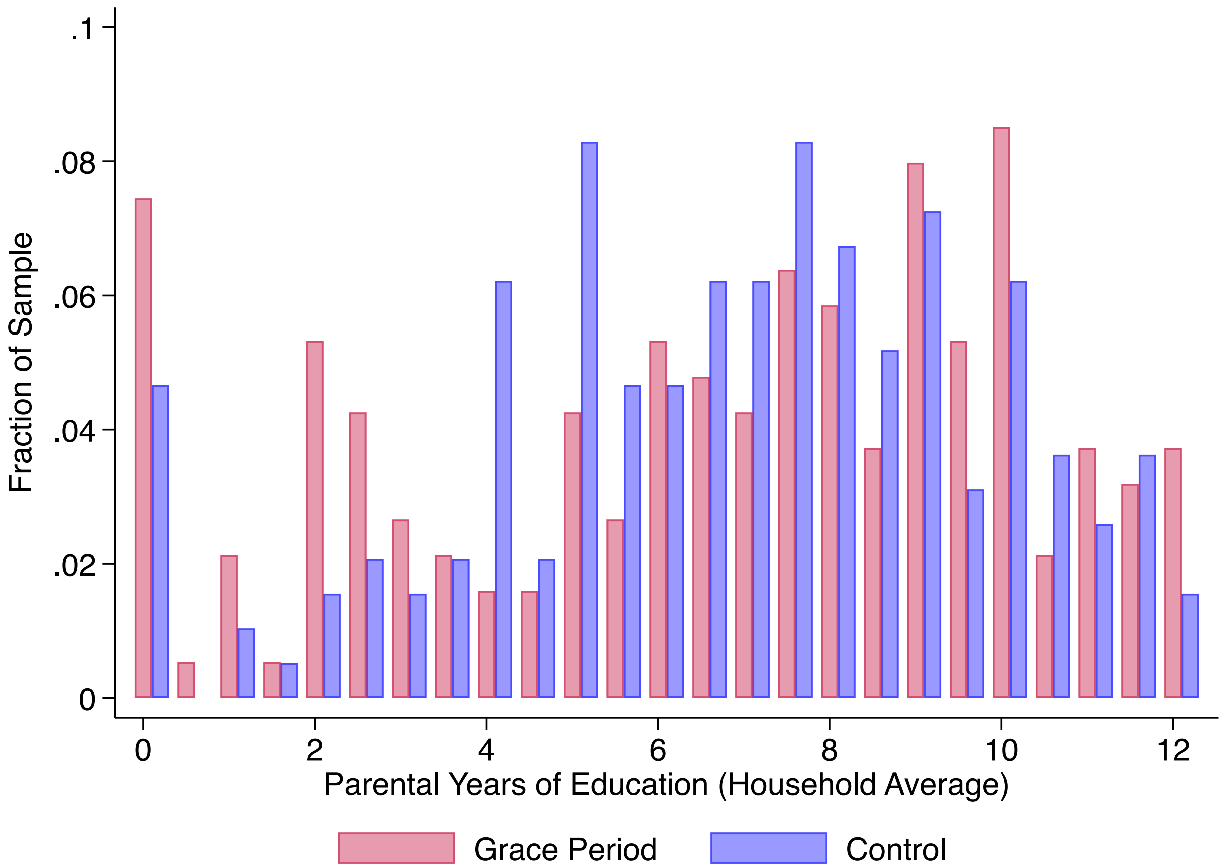
Panel A: Outcome Family 1



Panel B: Outcome Family 2

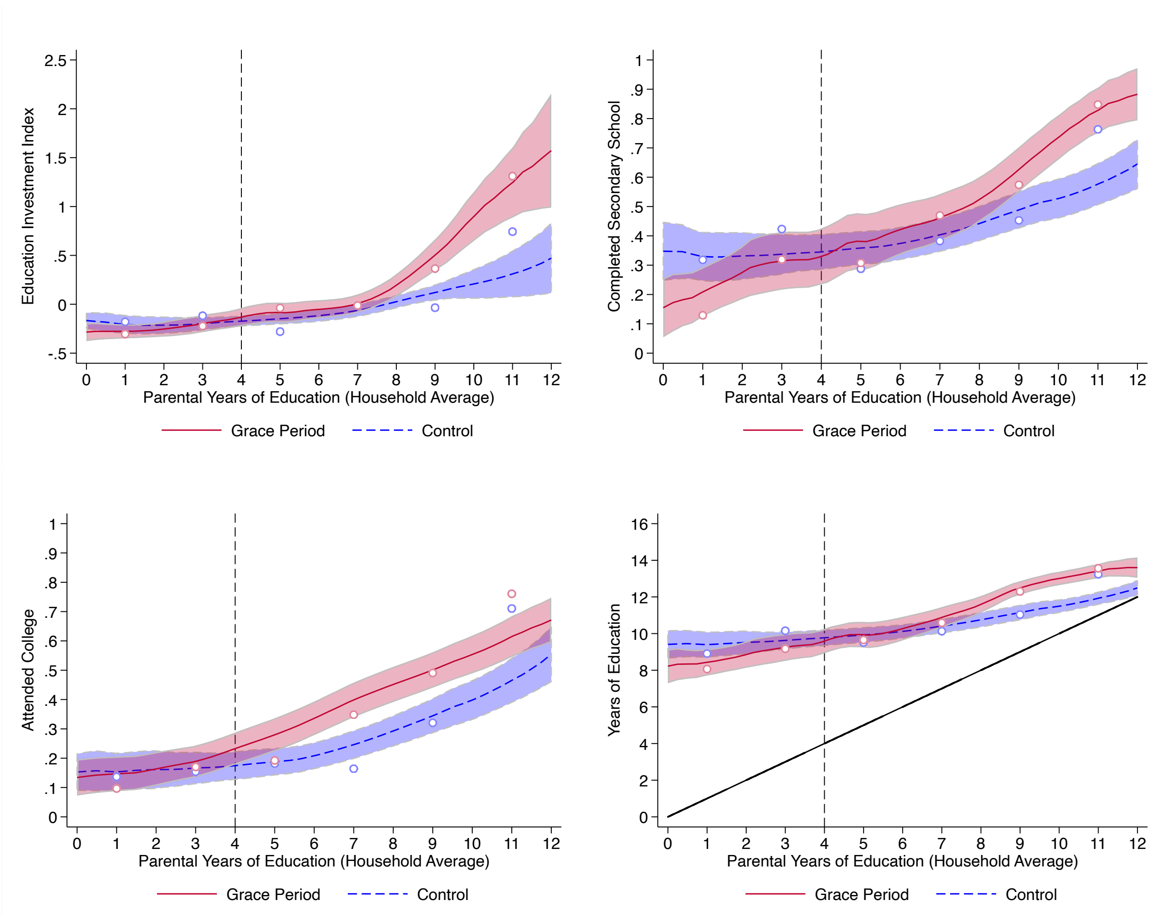
Notes: The figures plot sharpened q-values against unadjusted p-values. Both figures include the following household-level economic outcomes and child-level education and socio-economic outcomes: educational investment index, completed secondary school, attended college, years of education, economic index, number of household workers, number of non-household workers, ever self-employed under 18, dropout due to economic considerations, dropout due to child ability, dropout due to marriage for the pooled school-aged sample. The left panel shows the corrections for the first outcome family which is comprised of 12 tests (Panel A of Tables 1, 3 and 4). The right panel shows the corrections for the second outcome family which considers the heterogeneity analysis by parental education and comprises 36 tests (Panel A of Table 2 and Panel B of Tables 3 and 4). Sharpened q-values are calculated using the approach developed by Benjamini et al. (2006) and described in Anderson (2008).

Figure A5: Histogram of Parental Education



Notes: This figure shows the distribution of household-level average of mother's and father's education by treatment status (N=381).

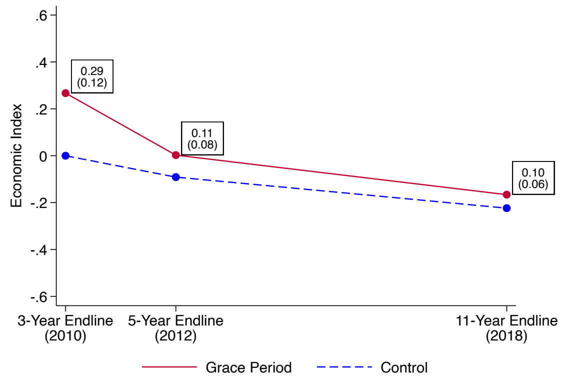
Figure A6: Child Education Outcomes by Parental Education and Treatment Group



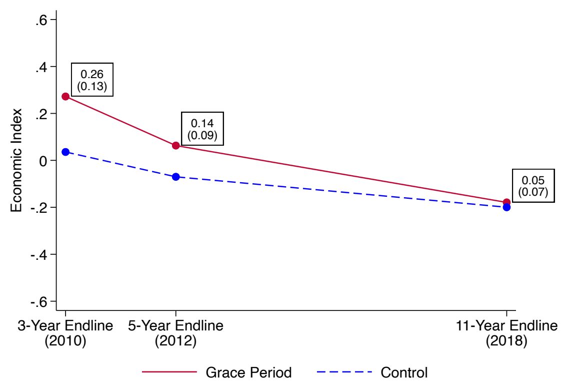
Notes: These figures plot the distribution of educational outcomes by average years of parental education (average of mother's and father's education). We separately estimate local regressions (bandwidth = 2, kernel = epanechnikov) for children in treatment (solid red line) and control (dotted blue line) households. The x-axis shows average parental years of education and the y-axis varies by panel. Clockwise from the top left panel, the y-axis shows the following outcomes: educational investment index; secondary school completion; any college attendance; years of education. The shaded areas correspond to 90 percent confidence intervals. The hollow circles correspond to the raw means of each outcome variable. For all panels, the sample consists of school-age children (7-17 at baseline; N=543). See Data Appendix for details on variable definitions and construction.

Figure A7: Treatment Impacts on Standardized Economic Index Over Time: Average Treatment Effects

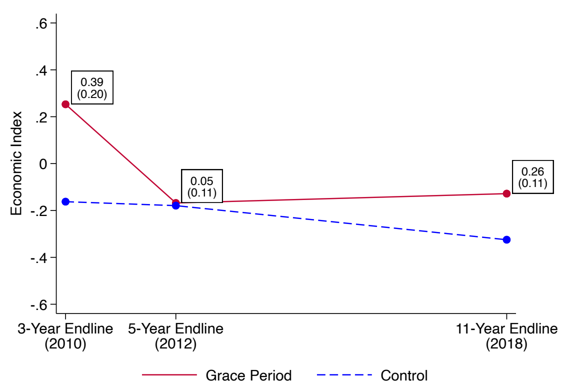
Panel A: Pooled



Panel B: Literate Parents

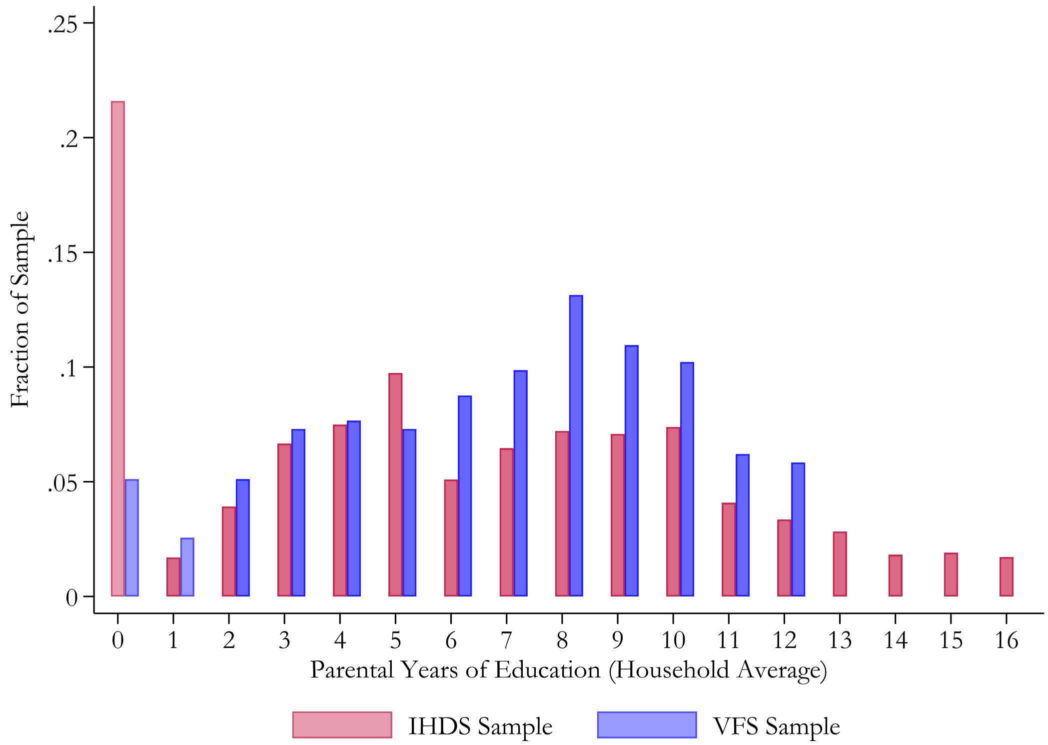


Panel C: Illiterate Parents



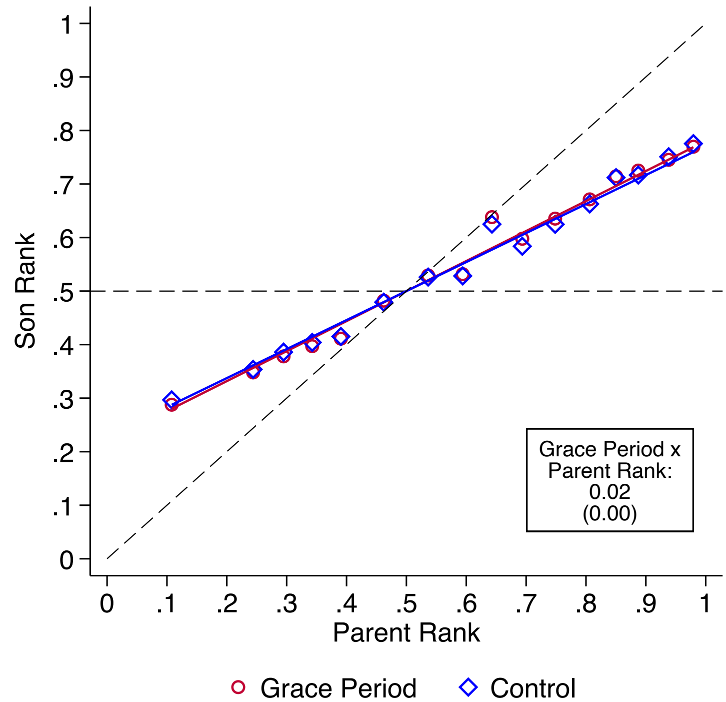
Notes: These figures plot the mean of the economic index variable by treatment (solid red line) and control group (dotted blue line) for each survey year. The figure in Panel A uses the pooled sample of all households (N=363 in 2010, N=369 in 2012, N=381 in 2018) while the figures in Panels B and C show plots for the literate- and illiterate-parent subsamples (N=281 in 2010, N=285 in 2012, N=296 in 2018 for literate parents and N=80 in 2010, N=84 in 2012, N=85 in 2018 for illiterate parents). The boxes report the treatment effects from a regression in which we regress the 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). Regressions are shown in Table 1 and Appendix Table A14.

Figure A8: Distribution of Average Parent Education in VFS and IHDS Samples



Notes: This histogram plots the distribution of average years of parental education (average of mother's and father's education) in the IHDS (N=6,892) and VFS samples (N=274). The VFS sample is limited to the parents of school-age sons. The IHDS sample is limited to parents of sons who are 18-28 in the IHDS (2012) and who live in urban areas. For ease of visualization, average parent education is always rounded up to the nearest integer value.

Figure A9: Predicted Son-Parent Rank-Rank Relationship by Treatment Group for Full Population



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. 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 A16 for the regression results.

Table A1: Balance Check

	Pooled			Literate			Illiterate		
	Control Mean	Grace Period Coeff.	N	Control Mean	Grace Period Coeff.	N	Control Mean	Grace Period Coeff.	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Household-Level Variables									
Client's Age	34.26	0.36	381	33.96	0.27	296	35.51	-0.12	85
	[5.89]	(0.62)		[5.90]	(0.66)		[5.76]	(1.52)	
Client Is Married	0.96	-0.01	381	0.96	0.00	296	0.97	-0.07	85
	[0.19]	(0.02)		[0.19]	(0.02)		[0.16]	(0.07)	
Client Has Financial Control	0.87	-0.04	379	0.88	-0.02	295	0.84	-0.09	84
	[0.34]	(0.04)		[0.33]	(0.05)		[0.37]	(0.09)	
Empowered Client	0.57	0.01	346	0.55	0.01	277	0.66	0.02	69
	[0.50]	(0.06)		[0.50]	(0.07)		[0.48]	(0.14)	
Client Is Impatient	0.53	-0.04	363	0.52	-0.03	287	0.56	-0.09	76
	[0.50]	(0.06)		[0.50]	(0.07)		[0.50]	(0.14)	
Spouse's Age	41.00	0.68	364	40.64	0.53	284	42.50	0.35	80
	[6.84]	(0.72)		[6.81]	(0.77)		[6.85]	(1.71)	
Household Size	4.34	-0.02	380	4.29	-0.07	295	4.54	0.20	85
	[1.31]	(0.14)		[1.32]	(0.14)		[1.30]	(0.40)	
Education Expenditure 2007	635.66	11.86	380	681.89	62.45	295	440.77	-115.75	85
	[588.19]	(72.99)		[613.04]	(86.78)		[422.83]	(91.65)	
Muslim	0.01	0.02	381	0.00	0.01	296	0.05	0.04	85
	[0.10]	(0.01)		[0.00]	(0.01)		[0.23]	(0.05)	
Household Shock	0.63	0.02	375	0.64	0.00	292	0.58	0.02	83
	[0.48]	(0.07)		[0.48]	(0.07)		[0.50]	(0.12)	
Number of Children in Household	1.85	0.04	380	1.76	-0.01	295	2.22	0.13	85
	[0.91]	(0.10)		[0.80]	(0.09)		[1.25]	(0.32)	
Household Has a Business	0.78	0.05	380	0.78	0.04	295	0.76	0.08	85
	[0.42]	(0.05)		[0.41]	(0.05)		[0.43]	(0.10)	
Loan Amount ₹4,000	0.02	-0.01	381	0.02	-0.02	296	0.00	0.01	85
	[0.12]	(0.01)		[0.14]	(0.01)		[0.00]	(0.01)	
Loan Amount ₹5,000	0.05	0.01	381	0.04	-0.01	296	0.08	0.05	85
	[0.21]	(0.03)		[0.19]	(0.03)		[0.28]	(0.07)	
Loan Amount ₹6,000	0.30	-0.09	381	0.32	-0.10	296	0.22	-0.03	85
	[0.46]	(0.05)		[0.47]	(0.06)		[0.42]	(0.10)	
Loan Amount ₹7,000	0.01	-0.01	381	0.00	0.00	296	0.03	-0.04	85
	[0.07]	(0.01)		[0.00]	(0.00)		[0.16]	(0.04)	
Loan Amount ₹8,000	0.55	0.01	381	0.54	0.03	296	0.62	-0.08	85
	[0.50]	(0.06)		[0.50]	(0.07)		[0.49]	(0.09)	
Loan Amount ₹10,000	0.08	0.09	381	0.08	0.10	296	0.05	0.08	85
	[0.27]	(0.04)		[0.28]	(0.04)		[0.23]	(0.07)	
Owns Home	0.85	-0.03	377	0.85	0.01	294	0.86	-0.15	83
	[0.35]	(0.04)		[0.36]	(0.04)		[0.35]	(0.11)	
Socio-Economic Index	-0.14	0.18	333	-0.05	0.30	258	-0.48	-0.19	75
	[1.17]	(0.15)		[1.20]	(0.17)		[0.98]	(0.28)	
No Drain in Neighborhood	0.13	0.01	375	0.10	0.02	292	0.25	-0.01	83
	[0.33]	(0.05)		[0.30]	(0.04)		[0.44]	(0.10)	
Literate Parents	0.81	-0.07	381						
	[0.39]	(0.05)							
Joint Test p-value		0.41			0.27			0.54	
		[0.73]			[0.62]			[0.99]	
Panel B: Child-Level Variables									
Female	0.51	-0.02	543	0.50	-0.03	399	0.54	0.01	144
	[0.50]	(0.05)		[0.50]	(0.05)		[0.50]	(0.11)	
Birth Order	1.79	-0.06	543	1.71	-0.07	399	2.06	-0.16	144
	[0.99]	(0.10)		[0.97]	(0.12)		[1.00]	(0.21)	
Resides with Parents	0.91	0.01	543	0.92	0.00	399	0.89	-0.01	144
	[0.28]	(0.03)		[0.27]	(0.04)		[0.31]	(0.07)	

Notes: This table shows balance for baseline covariates measured in 2007. Panel A reports on household-level outcomes and Panel B on child-level outcomes. Panel A include households with at least one 7-17 aged child in 2007 and who were surveyed in 2018. Columns (1)-(3) present the pooled school-aged sample (N=381 in Panel A and N=543 in Panel B). Columns (4)-(6) are limited to the literate sample (N=296 in Panel A and N=399 in Panel B) and columns (7)-(9) are limited to the illiterate sample (N=85 in Panel A and N=144 in Panel B). Differences in sample sizes across variables reflect missing data. Columns (1), (4), and (7) report the control mean of the dependent variable for each relevant subgroup (standard deviations in brackets). Columns (2), (5), and (8) report the difference in the dependent variable from OLS regressions of each outcome on an indicator variable for assignment to the grace period treatment and stratification dummies. Panel B regressions include child age fixed effects. Standard errors clustered by loan group are reported in parentheses. Randomization inference p-values for the joint tests from 1,000 permutations of the treatment assignment are reported in brackets. Data Appendix provides details on variable definitions and construction.

Table A2: Consumption and Additional Investment Opportunities

	Expenditures									
	Past 7 Days		Past 30 Days							
	2018 Survey		Pooling 2012 & 2018 Surveys				2018 Survey			
	Food	Alcohol/ Cigarettes	Festival	Renovations	Health	Education	Household Size	Number of New Children Since Baseline	Total Savings	Permanently Migrated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Grace Period	25.15	-16.20	-12.54	-249.53	84.61	185.76	0.12	-0.02	444.80	0.02
	(48.01)	(13.13)	(116.47)	(317.36)	(95.09)	(88.33)	(0.13)	(0.03)	(2863.98)	(0.02)
	[0.61]	[0.21]	[0.91]	[0.45]	[0.40]	[0.05]	[0.35]	[0.44]	[0.90]	[0.32]
Control Group Mean	822.19	59.51	438.25	898.53	635.34	503.57	3.67	0.08	12495.45	0.037
Observations	381	370	749	748	749	738	381	303	376	462

Notes: This table shows the effect of the grace period treatment on consumption and variety of alternative investment opportunities. Columns (1)-(2) and (7)-(10) use data from the 2018 (N=381) survey and columns (3)-(6) pool data from the 2012 (N=369) and 2018 (N=381) surveys. The sample in column (8) is restricted to households in which the client was younger than 40 years at baseline. We regress each outcome on an indicator variable for assignment to the grace period treatment, stratification dummies, an indicator variable for non-client respondents, and baseline controls selected by LASSO (equation 3). In columns (3)-(6), we also include survey year dummies. 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 Data Appendix for details on variable definitions and construction.

Table A3: Returns to Enterprise Capital

	2010	
	Capital OLS (1)	Profits 2SLS (2)
Panel A: Pooled		
Grace Period	16872.04	
	(9356.01)	
Capital		0.04
		(0.02)
Observations	361	355
Panel B: Heterogeneity by Parental Literacy		
Grace Period × Literate Parents	18827.47	
	(10785.40)	
Grace Period × Illiterate Parents	18613.83	
	(13709.73)	
Capitol × Literate Parents		0.03
		(0.02)
Capitol × Illiterate Parents		0.05
		(0.04)
p-value: Grace Period × Literate Parents = Grace Period × Illiterate Parents	0.989	
p-value: Capital × Literate Parents = Capital × Illiterate Parents		0.675
Observations (Literate Parents)	281	277
Observations (Illiterate Parents)	80	78

Notes: This table estimates household-level returns to capital in 2010. In column (1), we regress capital on an indicator variable for assignment to the grace period treatment, stratification dummies and hours worked on the business by all household members in the previous week. In column (2), the outcome variable is profits and we instrument for capital using treatment status. Panel A reports the results for the pooled sample. Panel B reports a variant which includes the fully interacted effects of treatment and parental literacy (the parental literacy indicator is included in regression but not reported). Differences in the number of observations between columns (1)-(2) are due to missing profits data. Standard errors clustered by loan group are reported in parentheses. See Data Appendix for details on variable definitions.

Table A4: Attrition Check for 2018 Survey

	Pooled			Literate			Illiterate		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Attrition									
	Treat	SE	N	Treat	SE	N	Treat	SE	N
Attrited	-0.04	(0.03)	462	-0.04	(0.03)	365	-0.01	(0.05)	97
	[0.12]			[0.20]			[0.85]		
Control Mean	0.10			0.11			0.05		
Panel B: Attrition and Baseline Characteristics									
	Attrited x Treat	SE	N	Attrited x Treat	SE	N	Attrited x Treat	SE	N
Client's Age	-3.26	(1.78)	462	-3.94	(1.86)	365	4.28	(3.73)	97
Client Is Married	-0.12	(0.14)	462	-0.17	(0.15)	365	0.10	(0.11)	97
Spouse's Age	-3.02	(2.77)	437	-2.90	(3.18)	345	-2.47	(3.56)	92
Household Size	1.22	(0.67)	461	1.24	(0.78)	364	0.89	(0.81)	97
Education Expenditure 2007	134.35	(149.97)	461	79.42	(170.34)	364	25.37	(389.80)	97
Number of Children in Household	0.60	(0.26)	461	0.59	(0.28)	364	0.26	(0.69)	97
Socio-Economic Index	-0.67	(0.42)	399	-0.34	(0.34)	315	-1.85	(0.62)	84
Literate Parents	0.02	(0.13)	462						

Notes: This table shows the relationship between treatment status and attrition in the 2018 survey. In Panel A, we regress an attrition indicator for the 2018 survey round on an indicator variable for assignment to the grace period treatment and stratification dummies. In Panel B, outcomes shown in columns (1), (4), and (7) are from regressions of a baseline characteristic on a grace period indicator, an attrition indicator for the 2018 survey round, and an interaction between the two. The table reports the coefficient on the interaction term. The sample consists of households who had either a school-age child in 2007 according to the full child roster in the 2018 survey or a school-age child in 2007 according to the household roster in the 2007 survey. Columns (1)-(3) present the pooled school-aged sample at baseline (N=462); columns (4)-(6) are limited to the literate sample (N=365) and columns (7)-(9) are limited to the illiterate sample (N=97). All regressions control for stratification dummies and cluster standard errors by loan group. Randomization inference p-values from 1,000 permutations of the treatment assignment are reported in brackets. See Data Appendix for variable definitions.

Table A5: Pre-Analysis Plan and Implemented Analysis

Table	Specified in PAP	Deviations
Table 2 - Educational Outcomes		
Outcomes:		
(1) Investment Index	*"We will test impacts on standardized indexes of sub-outcomes for measures of educational attainment and investments."*	None
(2) Primary School Investment Subindex (3) Secondary School Investment Subindex (4) College Spending Standardized	*"Analyze the cost and quality of education and extracurricular activities [after-school tutoring]."* The primary and secondary school indexes are composed of the cost of school fees, cost of after-school tutoring, and whether the child went to private school. We only collected cost measures for college expenditures.	Due to data collection constraints, we focus on cost measures.
(5) Years of Education (6) Completed Secondary School (7) Attended College	*"We will analyze years of schooling and the quantity of education"*	None.
Specification:		
Panel A	Child-level regression for educational outcomes specified.	(i) Age fixed effects included; (ii) Age-cutoffs for child-sample were not specified. Our choice is discussed in Section 2, and robustness check are provided in Appendix Tables A8, A9, and A10. Non-linear treatment effects for all children are shown in Figure 2.
Panel B	*"The child-level measures of intergenerational educational mobility will be based separately on mothers' and fathers' education levels."*	Measure of parental education was not specified. Our preferred measure is parental literacy, with justification in Section 2. Robustness checks using alternative specifications of parental education include: (a) Figure A6 - non-linear treatment effects by mean years of parental education (b) Table A11 - heterogeneity by literacy of both the mother and the father (Panel A), Alesina et al. (2021) measure of parental primary school completion (Panel B), and parental years of education (Panel C)."
Table 3 - Effects by Gender	*"We will analyze heterogeneous treatment impacts by child gender"*	None.
Outcomes:		
(1) Investment Index - (4) Attended College	See explanation of outcomes under Table 2 above.	None.
(5) Married (6) Any Children	*"We will analyze impacts on children's demographic outcomes including "marital status" and "fertility"."*	None.
(7) Housewife	*"We will analyze "impacts on children's economic activity" including "labor force participation, occupation, and income"."*	Given that 21% of our study sample children are still in school (and disproportionately more in the treatment group), we restrict analysis to the single outcome of housewife which is closely linked to marriage.
Table 4 - Household Enterprise Outcomes	The pre-analysis plan only focused on the child-level analysis. The main household-level outcomes are the same as in Table 2 in Field et al. (2013).	Field et al. (2013) did not include the creation of a household economic index.
Table 5 - Dropout and Child Labor	*"We will analyze performance and reasons for dropping out of school"*	Due to data collection constraints, we focus on reasons for dropout.

Notes: The project was pre-registered under AEA registry ID AEARCTR-0003572; the PAP can be found at `https://www.socialscienceregistry.org/trials/3572`.

Table A6: Treatment Effects on Educational Investment Subindex Components

	Primary School Investment Subindex Components			Secondary School Investment Subindex Components			
	Private School	Total School Fees	Total After-School Tutoring	Private School	Total School Fees	Total After-School Tutoring	College Spending
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: School-Age Child Sample (7-17 Years at Baseline), Pooled							
Grace Period	0.07	1359.53	143.46	0.06	2120.23	5006.49	1650.37
	(0.04)	(1149.11)	(812.86)	(0.02)	(1535.42)	(1849.84)	(929.51)
	[0.12]	[0.25]	[0.87]	[0.00]	[0.15]	[0.02]	[0.10]
Control Group Mean	0.23	6573.27	8155.80	0.02	10993.63	23411.48	3827.34
Observations	543	518	542	543	513	535	531
Panel B: School-Age Child Sample (7-17 Years at Baseline), Heterogeneity by Parental Literacy							
Grace Period × Literate Parents	0.09	1749.36	-15.86	0.08	3665.05	5837.69	2876.40
	(0.05)	(1440.75)	(944.33)	(0.03)	(1854.43)	(2342.42)	(1332.77)
	[0.14]	[0.26]	[0.99]	[0.00]	[0.06]	[0.02]	[0.04]
Grace Period × Illiterate Parents	0.04	206.48	417.13	-0.01	-2291.61	1835.23	-1502.33
	(0.05)	(944.26)	(1639.28)	(0.01)	(1528.65)	(3126.69)	(1448.33)
	[0.55]	[0.82]	[0.79]	[0.41]	[0.14]	[0.60]	[0.28]
p-value: Grace Period × Literate Parents =	0.51	0.33	0.82	0.00	0.01	0.31	0.03
Grace Period × Illiterate Parents	[0.56]	[0.34]	[0.81]	[0.00]	[0.01]	[0.33]	[0.03]
Control Group Mean (Literate Parents)	0.29	7456.41	7951.28	0.02	12033.33	24982.54	4223.05
Control Group Mean (Illiterate Parents)	0.03	3735.66	8807.13	0.00	7652.95	18403.70	2603.68
Observations (Literate Parents)	399	379	398	399	378	393	388
Observations (Illiterate Parents)	144	139	144	144	135	142	143

Notes: This table shows the effect of the grace period treatment on child educational investment subindex components 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. See Data Appendix for details on variable definitions and construction.

Table A7: Heterogeneous Treatment Effects on Educational Investment Subindex Components by Gender

		Primary School Investment Subindex Components				Secondary School Investment Subindex Components			
	Primary School Investment Subindex	Private School	Total School Fees	Total After-School Tutoring	Secondary School Investment Subindex	Private School	Total School Fees	Total After-School Tutoring	College Spending
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: School-Age Child Sample (7-17 Years at Baseline), Heterogeneity by Gender									
Grace Period × Male	0.14	0.06	2001.16	1192.40	0.26	0.06	771.43	6164.26	1485.10
	(0.11)	(0.05)	(1762.02)	(1005.58)	(0.12)	(0.03)	(2365.62)	(2566.93)	(1438.94)
	[0.21]	[0.26]	[0.27]	[0.26]	[0.03]	[0.03]	[0.73]	[0.04]	[0.34]
Grace Period × Female	0.05	0.08	684.25	-878.13	0.25	0.05	3627.39	3578.12	1819.04
	(0.09)	(0.05)	(1,427.33)	(1,049.80)	(0.10)	(0.02)	(2,038.12)	(2,423.04)	(1,214.45)
	[0.61]	[0.15]	[0.61]	[0.46]	[0.01]	[0.05]	[0.04]	[0.19]	[0.18]
p-value: Grace Period × Male = Grace Period × Female	0.49	0.76	0.57	0.12	0.97	0.64	0.37	0.44	0.86
	[0.50]	[0.77]	[0.56]	[0.14]	[0.97]	[0.60]	[0.30]	[0.50]	[0.87]
Panel B: School-Age Child Sample (7-17 Years at Baseline), Heterogeneity by Gender & Parental Literacy									
Grace Period × Literate Parents × Male	0.19	0.08	2646.19	1431.23	0.31	0.09	1775.78	5944.55	3107.77
	(0.13)	(0.07)	(2272.89)	(1170.37)	(0.15)	(0.04)	(2986.37)	(3097.77)	(2094.10)
	[0.17]	[0.27]	[0.26]	[0.25]	[0.04]	[0.01]	[0.55]	[0.08]	[0.14]
Grace Period × Illiterate Parents × Male	-0.03	-0.02	291.98	99.62	0.11	-0.00	-1804.60	4456.13	-3418.15
	(0.15)	(0.09)	(1,441.78)	(2,090.63)	(0.14)	(0.01)	(1,966.49)	(4,786.78)	(2,146.33)
	[0.87]	[0.86]	[0.81]	[0.97]	[0.42]	[0.81]	[0.30]	[0.42]	[0.06]
Grace Period × Literate Parents × Female	0.03	0.08	778.18	-1343.42	0.36	0.06	5540.77	5290.53	2629.48
	(0.11)	(0.07)	(1,767.38)	(1,250.49)	(0.13)	(0.03)	(2,387.86)	(3,100.22)	(1,557.89)
	[0.80]	[0.25]	[0.65]	[0.34]	[0.00]	[0.06]	[0.01]	[0.13]	[0.13]
Grace Period × Illiterate Parents × Female	0.12	0.09	57.96	279.03	-0.06	-0.01	-2700.67	-820.59	363.88
	(0.13)	(0.05)	(1,048.70)	(2,101.87)	(0.12)	(0.02)	(2,749.70)	(3,894.77)	(1,796.38)
	[0.41]	[0.09]	[0.96]	[0.89]	[0.64]	[0.22]	[0.28]	[0.84]	[0.83]
p-value: Grace Period × Literate Parents × Male = Grace Period × Literate Parents × Female	0.31	0.97	0.53	0.09	0.82	0.54	0.34	0.87	0.85
p-value: Grace Period × Illiterate Parents × Male = Grace Period × Illiterate Parents × Female	0.44	0.28	0.89	0.95	0.34	0.69	0.80	0.40	0.17
Control Group Mean (Male, Literate Parents)	0.03	0.30	8005.62	6637.68	0.12	0.03	13926.77	25009.38	4569.19
Control Group Mean (Male, Illiterate Parents)	-0.18	0.07	3718.53	9021.97	-0.24	0.00	6784.32	17663.47	3571.04
Control Group Mean (Female, Literate Parents)	0.10	0.28	6907.20	9277.63	0.01	0.02	10139.89	24955.18	3859.26
Control Group Mean (Female, Illiterate Parents)	-0.25	0.00	3751.19	8622.98	-0.18	0.00	8440.15	19056.84	1774.51
Observations (Male, Literate Parents)	205	205	192	204	205	205	193	202	200
Observations (Male, Illiterate Parents)	69	69	68	69	69	69	67	69	69
Observations (Female, Literate Parents)	194	194	187	194	194	194	185	191	188
Observations (Female, Illiterate Parents)	75	75	71	75	75	75	68	73	74

Notes: This table shows the effect of the grace period treatment by gender on child educational investment subindex components 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 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. See Data Appendix for details on variable definitions and construction.

Table A8: Robustness Checks for Child Age Cut-Offs

	Investment Index	Investment Index Components: Primary School Investment Subindex	Secondary School Investment Subindex	College Spending (Standard-ized)	Completed Secondary School	Attended College	Years of Education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: 6-16 Years at Baseline							
Grace Period	0.16	0.09	0.19	0.17	0.03	0.09	0.40
	(0.07)	(0.07)	(0.08)	(0.09)	(0.04)	(0.04)	(0.29)
	[0.04]	[0.23]	[0.03]	[0.09]	[0.57]	[0.05]	[0.25]
Panel B: 6-17 Years at Baseline							
Grace Period	0.16	0.10	0.20	0.15	0.04	0.09	0.35
	(0.07)	(0.07)	(0.08)	(0.08)	(0.04)	(0.04)	(0.26)
	[0.04]	[0.19]	[0.01]	[0.08]	[0.29]	[0.02]	[0.22]
Panel C: 6-18 Years at Baseline							
Grace Period	0.14	0.06	0.19	0.14	0.05	0.09	0.37
	(0.07)	(0.07)	(0.07)	(0.08)	(0.04)	(0.04)	(0.28)
	[0.06]	[0.47]	[0.02]	[0.08]	[0.27]	[0.02]	[0.21]
Panel D: 7-16 Years at Baseline							
Grace Period	0.20	0.09	0.24	0.14	0.04	0.10	0.46
	(0.08)	(0.08)	(0.08)	(0.10)	(0.04)	(0.04)	(0.32)
	[0.02]	[0.25]	[0.00]	[0.16]	[0.47]	[0.03]	[0.20]
Panel E: 7-18 Years at Baseline							
Grace Period	0.17	0.08	0.23	0.15	0.05	0.10	0.39
	(0.07)	(0.08)	(0.08)	(0.08)	(0.04)	(0.04)	(0.30)
	[0.03]	[0.30]	[0.01]	[0.09]	[0.21]	[0.01]	[0.22]
Panel F: 8-16 Years at Baseline							
Grace Period	0.17	0.05	0.21	0.17	0.01	0.09	0.38
	(0.08)	(0.08)	(0.08)	(0.10)	(0.05)	(0.05)	(0.34)
	[0.06]	[0.57]	[0.02]	[0.13]	[0.79]	[0.06]	[0.34]
Panel G: 8-17 Years at Baseline							
Grace Period	0.18	0.04	0.22	0.16	0.04	0.10	0.26
	(0.08)	(0.08)	(0.08)	(0.09)	(0.04)	(0.04)	(0.31)
	[0.04]	[0.67]	[0.02]	[0.12]	[0.44]	[0.03]	[0.43]
Panel H: 8-18 Years at Baseline							
Grace Period	0.14	0.03	0.20	0.15	0.03	0.09	0.27
	(0.07)	(0.08)	(0.08)	(0.08)	(0.04)	(0.04)	(0.30)
	[0.09]	[0.69]	[0.02]	[0.11]	[0.49]	[0.04]	[0.41]

Notes: This table shows the effect of the grace period treatment on child educational outcomes as measured by the 2018 survey for different child age cut-offs. The age cut-off is specified in each panel label. In all panels, 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). 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. See Data Appendix for details on variable definitions and construction.

Table A9: Robustness Checks for Child Age Cut-Offs: Heterogeneity by Parental Literacy

		Investment Index Components					
	Investment Index	Primary School Investment Subindex	Secondary School Investment Subindex	College Spending (Standardized)	Completed Secondary School	Attended College	Years of Education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: 6-16 Years at Baseline							
Grace Period × Literate Parents	0.22	0.09	0.26	0.24	0.10	0.13	0.76
	(0.09)	(0.08)	(0.09)	(0.12)	(0.05)	(0.05)	(0.37)
	[0.01]	[0.27]	[0.02]	[0.06]	[0.08]	[0.03]	[0.06]
Grace Period × Illiterate Parents	0.04	0.06	0.03	-0.09	-0.15	-0.02	-0.78
	(0.12)	(0.12)	(0.10)	(0.14)	(0.07)	(0.07)	(0.57)
	[0.74]	[0.64]	[0.75]	[0.51]	[0.02]	[0.78]	[0.16]
Panel B: 6-17 Years at Baseline							
Grace Period × Literate Parents	0.22	0.12	0.26	0.26	0.11	0.13	0.78
	(0.09)	(0.09)	(0.09)	(0.11)	(0.05)	(0.05)	(0.33)
	[0.02]	[0.21]	[0.01]	[0.03]	[0.03]	[0.01]	[0.03]
Grace Period × Illiterate Parents	-0.01	0.03	0.02	-0.12	-0.13	-0.01	-0.99
	(0.11)	(0.11)	(0.09)	(0.13)	(0.06)	(0.06)	(0.44)
	[0.93]	[0.76]	[0.86]	[0.30]	[0.04]	[0.88]	[0.03]
Panel C: 6-18 Years at Baseline							
Grace Period × Literate Parents	0.20	0.10	0.25	0.24	0.09	0.12	0.72
	(0.08)	(0.09)	(0.09)	(0.11)	(0.05)	(0.05)	(0.35)
	[0.02]	[0.28]	[0.01]	[0.04]	[0.08]	[0.01]	[0.05]
Grace Period × Illiterate Parents	-0.03	0.01	0.00	-0.11	-0.10	-0.00	-0.78
	(0.11)	(0.11)	(0.08)	(0.12)	(0.05)	(0.06)	(0.46)
	[0.73]	[0.89]	[0.98]	[0.31]	[0.09]	[1.00]	[0.11]
Panel D: 7-16 Years at Baseline							
Grace Period × Literate Parents	0.25	0.09	0.32	0.25	0.11	0.15	0.84
	(0.09)	(0.09)	(0.10)	(0.12)	(0.06)	(0.06)	(0.40)
	[0.01]	[0.35]	[0.00]	[0.05]	[0.05]	[0.01]	[0.05]
Grace Period × Illiterate Parents	0.05	0.10	0.04	-0.10	-0.17	-0.03	-0.87
	(0.12)	(0.12)	(0.11)	(0.14)	(0.07)	(0.08)	(0.59)
	[0.65]	[0.40]	[0.71]	[0.46]	[0.02]	[0.71]	[0.15]
Panel E: 7-18 Years at Baseline							
Grace Period × Literate Parents	0.22	0.09	0.31	0.24	0.11	0.14	0.82
	(0.08)	(0.09)	(0.09)	(0.11)	(0.05)	(0.05)	(0.35)
	[0.01]	[0.34]	[0.00]	[0.03]	[0.05]	[0.01]	[0.04]
Grace Period × Illiterate Parents	-0.03	0.03	0.01	-0.12	-0.10	-0.00	-1.08
	(0.11)	(0.11)	(0.09)	(0.13)	(0.06)	(0.06)	(0.44)
	[0.77]	[0.81]	[0.94]	[0.28]	[0.13]	[0.95]	[0.02]
Panel F: 8-16 Years at Baseline							
Grace Period × Literate Parents	0.22	0.06	0.29	0.24	0.08	0.14	0.79
	(0.10)	(0.10)	(0.10)	(0.13)	(0.06)	(0.06)	(0.43)
	[0.04]	[0.57]	[0.02]	[0.09]	[0.21]	[0.04]	[0.09]
Grace Period × Illiterate Parents	0.06	0.11	0.04	-0.08	-0.23	-0.04	-1.07
	(0.13)	(0.13)	(0.11)	(0.15)	(0.07)	(0.08)	(0.60)
	[0.65]	[0.43]	[0.74]	[0.59]	[0.00]	[0.57]	[0.06]
Panel G: 8-17 Years at Baseline							
Grace Period × Literate Parents	0.23	0.09	0.30	0.26	0.09	0.14	0.82
	(0.10)	(0.11)	(0.10)	(0.13)	(0.06)	(0.06)	(0.38)
	[0.03]	[0.42]	[0.02]	[0.05]	[0.12]	[0.02]	[0.05]
Grace Period × Illiterate Parents	0.04	0.07	0.02	-0.12	-0.16	-0.03	-1.22
	(0.11)	(0.12)	(0.10)	(0.14)	(0.06)	(0.06)	(0.45)
	[0.70]	[0.59]	[0.85]	[0.33]	[0.02]	[0.63]	[0.01]
Panel H: 8-18 Years at Baseline							
Grace Period × Literate Parents	0.20	0.05	0.27	0.27	0.08	0.14	0.80
	(0.09)	(0.10)	(0.09)	(0.12)	(0.06)	(0.05)	(0.38)
	[0.05]	[0.63]	[0.01]	[0.04]	[0.16]	[0.01]	[0.05]
Grace Period × Illiterate Parents	-0.01	0.03	0.01	-0.11	-0.11	-0.00	-1.24
	(0.11)	(0.12)	(0.09)	(0.13)	(0.06)	(0.06)	(0.43)
	[0.89]	[0.78]	[0.92]	[0.36]	[0.10]	[0.97]	[0.01]

Notes: This table shows the effect of the grace period treatment on child educational outcomes as measured by the 2018 survey by parental literacy for different child age cut-offs. The age cut-off is specified in each panel label. In all panels, 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, baseline controls selected by LASSO and fully interacted effects of treatment and a dummy for 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. See Data Appendix for details on variable definitions and construction.

Table A10: Alternative Child Samples

	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: All Child Sample, Pooled							
Grace Period	0.07	0.03	0.04	0.08	0.02	0.05	-0.05
	(0.05)	(0.05)	(0.05)	(0.06)	(0.02)	(0.02)	(0.19)
	[0.20]	[0.56]	[0.40]	[0.17]	[0.33]	[0.03]	[0.82]
Control Group Mean	0.06	0.04	0.07	0.02	0.26	0.17	9.48
Observations	1303	1303	1303	1303	1303	1301	1303
Panel B: All Child Sample, Heterogeneity by Parental Literacy							
Grace Period × Literate Parents	0.11	0.04	0.08	0.17	0.05	0.08	0.23
	(0.06)	(0.06)	(0.06)	(0.08)	(0.03)	(0.03)	(0.22)
	[0.06]	[0.51]	[0.18]	[0.05]	[0.08]	[0.00]	[0.33]
Grace Period × Illiterate Parents	-0.06	0.02	-0.06	-0.09	-0.06	-0.01	-0.80
	(0.07)	(0.08)	(0.06)	(0.08)	(0.04)	(0.03)	(0.37)
	[0.39]	[0.81]	[0.36]	[0.22]	[0.13]	[0.70]	[0.08]
p-value: Grace Period × Literate Parents =	0.05	0.84	0.09	0.03	0.02	0.02	0.02
Grace Period × Illiterate Parents	[0.05]	[0.85]	[0.11]	[0.02]	[0.02]	[0.03]	[0.04]
Control Group Mean (Literate Parents)	0.13	0.09	0.14	0.05	0.28	0.19	9.88
Control Group Mean (Illiterate Parents)	-0.17	-0.13	-0.18	-0.09	0.17	0.08	8.12
Observations (Literate Parents)	940	940	940	940	940	938	940
Observations (Illiterate Parents)	361	361	361	361	361	361	361
Panel C: Old Child Sample (18+ Years at Baseline), Pooled							
Grace Period	-0.07	-0.11	-0.06	-0.03	0.01	0.01	-0.15
	(0.06)	(0.07)	(0.06)	(0.08)	(0.04)	(0.03)	(0.36)
	[0.35]	[0.16]	[0.39]	[0.67]	[0.74]	[0.61]	[0.68]
Control Group Mean	0.00	-0.00	-0.00	0.00	0.20	0.13	8.86
Observations	492	492	492	492	492	492	492
Panel D: Old Child Sample (18+ Years at Baseline), Heterogeneity by Parental Literacy							
Grace Period × Literate Parents	-0.04	-0.14	-0.04	-0.02	0.04	0.04	0.23
	(0.10)	(0.09)	(0.10)	(0.12)	(0.06)	(0.04)	(0.43)
	[0.71]	[0.17]	[0.67]	[0.89]	[0.50]	[0.33]	[0.58]
Grace Period × Illiterate Parents	-0.06	0.04	-0.07	-0.13	-0.02	-0.02	-0.52
	(0.08)	(0.09)	(0.07)	(0.08)	(0.04)	(0.03)	(0.61)
	[0.51]	[0.70]	[0.35]	[0.12]	[0.59]	[0.40]	[0.47]
p-value: Grace Period × Literate Parents =	0.87	0.14	0.79	0.46	0.36	0.21	0.29
Grace Period × Illiterate Parents	[0.88]	[0.17]	[0.81]	[0.48]	[0.39]	[0.21]	[0.33]
Control Group Mean (Literate Parents)	0.11	0.07	0.12	0.06	0.26	0.16	9.69
Control Group Mean (Illiterate Parents)	-0.28	-0.17	-0.29	-0.16	0.06	0.04	6.83
Observations (Literate Parents)	308	308	308	308	308	308	308
Observations (Illiterate Parents)	184	184	184	184	184	184	184

Notes: This table shows the effect of the grace period treatment on child educational outcomes as measured by the 2018 survey for alternative child samples. In Panels A and B, the sample is all children ever born to the household before the baseline survey (N=1,303). In Panels C and D, the sample is all children aged 18 years or older in 2007. In Panels A and C, 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). Panels B and D report 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. See Data Appendix for details on variable definitions and construction.

Table A11: Treatment Effects on Educational Outcomes for Alternative Measures of Parental Education

	Investment Index	Investment Index Components: Primary School Investment Subindex	Investment Index Components: Secondary School Investment Subindex	Investment Index Components: College Spending (Standard-ized)	Completed Secondary School	Attended College	Years of Education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Parental Literacy Breakdown							
Grace Period × Literate Parents	0.25	0.11	0.33	0.26	0.12	0.15	0.84
	(0.09)	(0.09)	(0.10)	(0.12)	(0.05)	(0.05)	(0.35)
	[0.01]	[0.25]	[0.00]	[0.04]	[0.05]	[0.01]	[0.05]
Grace Period × Literate Mother, Illiterate Father	0.11	0.16	-0.01	0.19	-0.10	0.15	-0.57
	(0.14)	(0.13)	(0.17)	(0.15)	(0.12)	(0.11)	(1.09)
	[0.50]	[0.31]	[0.97]	[0.28]	[0.45]	[0.26]	[0.65]
Grace Period × Illiterate Mother, Literate Father	-0.03	0.09	0.12	-0.34	-0.07	-0.11	-1.16
	(0.14)	(0.16)	(0.13)	(0.13)	(0.09)	(0.09)	(0.57)
	[0.80]	[0.61]	[0.34]	[0.00]	[0.46]	[0.23]	[0.06]
Grace Period × Illiterate Parents	-0.16	-0.25	-0.13	-0.24	-0.32	-0.07	-1.46
	(0.30)	(0.24)	(0.22)	(0.45)	(0.12)	(0.11)	(0.71)
	[0.62]	[0.37]	[0.58]	[0.65]	[0.02]	[0.58]	[0.08]
Control Group Mean (Literate Parents)	0.07	0.07	0.07	0.04	0.46	0.31	10.76
Control Group Mean (Literate Mother, Illiterate Father)	-0.32	-0.37	-0.16	-0.24	0.33	0.10	9.29
Control Group Mean (Illiterate Mother, Literate Father)	-0.26	-0.22	-0.29	-0.10	0.29	0.19	9.84
Control Group Mean (Illiterate Parents)	0.01	0.04	-0.07	0.07	0.38	0.15	9.69
Observations (Literate Parents)	399	399	399	399	399	397	399
Observations (Literate Mother, Illiterate Father)	47	47	47	47	47	47	47
Observations (Illiterate Mother, Literate Father)	63	63	63	63	63	63	63
Observations (Illiterate Parents)	34	34	34	34	34	34	34
Panel B: Heterogeneity by Parental Primary School Completion							
Grace Period × Primary School Parents	0.29	0.13	0.36	0.25	0.10	0.13	0.72
	(0.10)	(0.10)	(0.10)	(0.12)	(0.05)	(0.05)	(0.37)
	[0.00]	[0.21]	[0.00]	[0.05]	[0.09]	[0.03]	[0.08]
Grace Period × Non-Primary School Parents	0.01	0.01	0.01	-0.04	-0.06	0.04	-0.39
	(0.10)	(0.10)	(0.11)	(0.11)	(0.06)	(0.05)	(0.44)
	[0.90]	[0.91]	[0.92]	[0.71]	[0.36]	[0.52]	[0.42]
p-value: Grace Period × Primary School Parents = Grace Period × Non-Primary School Parents	0.04	0.40	0.02	0.08	0.04	0.20	0.06
	[0.03]	[0.38]	[0.02]	[0.06]	[0.05]	[0.20]	[0.06]
Control Group Mean (Primary School Parents)	0.08	0.07	0.06	0.07	0.46	0.33	10.90
Control Group Mean (Non-Primary School Parents)	-0.20	-0.18	-0.14	-0.16	0.33	0.14	9.49
Observations (Primary School Parents)	373	373	373	373	373	371	373
Observations (Non-Primary School Parents)	170	170	170	170	170	170	170
Panel C: Parental Years of Education							
Grace Period × Parental Years of Education	0.05	0.03	0.07	0.03	0.02	0.01	0.08
	(0.03)	(0.03)	(0.03)	(0.04)	(0.01)	(0.01)	(0.08)
	[0.16]	[0.28]	[0.03]	[0.48]	[0.04]	[0.27]	[0.28]
Grace Period	-0.07	-0.07	-0.18	-0.01	-0.09	0.02	-0.04
	(0.18)	(0.16)	(0.18)	(0.21)	(0.08)	(0.08)	(0.60)
	[0.72]	[0.63]	[0.34]	[0.98]	[0.25]	[0.75]	[0.93]
Parental Years of Education	0.07	0.05	0.08	0.08	0.03	0.04	0.34
	(0.02)	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.06)
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

Notes: This table shows the effect of the grace period treatment on child educational outcomes as measured by the 2018 survey for alternative measures of education. In Panels A-C, the sample is children aged 7-17 (school-age) in 2007 (N=543). In Panel C, we report a variant of equation (1) which includes the fully interacted effects of treatment and four mutually exclusive dummies for the literacy status of the parents (equation 2; we do not report the different parental literacy dummies). In Panel B, 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, baseline controls selected by LASSO and fully interacted effects of treatment and a dummy for whether both parents completed primary school (equation 2; we do not report parental primary schooling dummy in Table). In Panel C, we instead regress each outcome on an indicator variable for assignment to grace period treatment, a continuous variable of average parentel years of education, an interaction between treatment and average parental years of education, stratification dummies, child age fixed effects, an indicator for non-client respondent in 2018 survey, and baseline controls selected by LASSO. 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. See Data Appendix for details on variable definitions and construction.

Table A12: Treatment Effects on Household Enterprise Outcomes for Full Household Sample

	2010 Survey				2018 Survey			
		Index Components				Index Components		
	Economic Index	Profits (Standardized)	Capital (Standardized)	Household Income (Standardized)	Economic Index	Profits (Standardized)	Capital (Standardized)	Household Income (Standardized)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Pooled								
Grace Period	0.23	0.29	0.25	0.15	0.03	0.03	0.05	0.01
	(0.07)	(0.09)	(0.09)	(0.08)	(0.03)	(0.04)	(0.07)	(0.02)
	[0.00]	[0.01]	[0.01]	[0.07]	[0.35]	[0.58]	[0.46]	[0.66]
Control Group Mean	-0.00	-0.00	-0.00	-0.00	-0.20	-0.20	-0.12	-0.30
Observations	769	769	769	769	744	744	744	744
Panel B: Heterogeneity by Parental Literacy								
Grace Period × Literate Parents	0.25	0.31	0.31	0.16	0.00	-0.01	0.02	-0.00
	(0.07)	(0.10)	(0.11)	(0.09)	(0.04)	(0.05)	(0.08)	(0.03)
	[0.04]	[0.02]	[0.12]	[0.52]	[0.52]	[0.74]	[0.54]	[0.78]
Grace Period × Illiterate Parents	0.172	0.202	0.166	0.156	0.150	0.143	0.223	0.087
	(0.15)	(0.22)	(0.15)	(0.17)	(0.07)	(0.09)	(0.14)	(0.03)
	[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	0.624	0.660	0.440	0.999	0.073	0.112	0.206	0.022
	[0.68]	[0.71]	[0.94]	[0.60]	[0.15]	[0.14]	[0.26]	[0.17]
Control Group Mean (Literate Parents)	0.01	-0.01	0.02	0.02	-0.19	-0.19	-0.10	-0.28
Control Group Mean (Illiterate Parents)	-0.06	0.03	-0.10	-0.11	-0.27	-0.24	-0.21	-0.37
Observations (Literate Parents)	618	618	618	618	593	593	593	593
Observations (Illiterate Parents)	149	149	149	149	149	149	149	149

Notes: This table shows the effect of the grace period treatment on household income and enterprise outcomes from the 2010 (N=766) and the 2018 (N=744) surveys for the full household sample. 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 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. See Data Appendix for details on variable definitions and construction.

Table A13: Treatment Effects on Household Economic Index Components

	2010 Survey				2018 Survey			
	Economic Index Components				Economic Index Components			
	Profits	Capital	Household Income	Log Household Income	Profits	Capital	Household Income	Log Household Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Pooled								
Grace Period	711.32	16053.79	2461.38	0.19	99.15	12529.33	517.02	0.10
	(255.76)	(9440.17)	(2524.60)	(0.10)	(99.94)	(10043.02)	(627.52)	(0.07)
	[0.01]	[0.08]	[0.34]	[0.07]	[0.33]	[0.21]	[0.42]	[0.14]
Control Group Mean	1204.30	28747.84	14441.38	9.05	874.44	21253.05	7746.82	8.73
Observations	355	361	363	351	346	351	378	378
Panel B: Heterogeneity by Parental Literacy								
Grace Period × Literate Parents	618.56	16563.34	1838.85	0.13	21.46	7660.12	220.77	0.06
	(275.18)	(10853.67)	(2804.23)	(0.11)	(115.78)	(11805.71)	(728.89)	(0.07)
	[0.02]	[0.12]	[0.52]	[0.25]	[0.88]	[0.52]	[0.78]	[0.44]
Grace Period × Illiterate Parents	901.64	18309.41	4573.12	0.40	323.50	27620.24	1865.53	0.28
	(525.13)	(14873.21)	(4890.50)	(0.22)	(163.65)	(16485.05)	(849.35)	(0.13)
	[0.22]	[0.41]	[0.35]	[0.07]	[0.06]	[0.12]	[0.04]	[0.05]
p-value: Grace Period × Literate Parents = Grace Period × Illiterate Parents	0.63	0.92	0.61	0.25	0.12	0.31	0.11	0.13
	[0.73]	[0.94]	[0.60]	[0.27]	[0.18]	[0.35]	[0.19]	[0.18]
Control Group Mean (Literate Parents)	1238.49	32282.73	15013.05	9.10	909.36	23012.86	8110.76	8.77
Control Group Mean (Illiterate Parents)	1046.18	12787.27	11842.90	8.82	717.26	13696.20	6212.34	8.55
Observations (Literate Parents)	277	281	283	273	270	273	294	294
Observations (Illiterate Parents)	78	80	80	78	76	78	84	84

Notes: This table shows the effect of the grace period treatment on non-standardized household economic index components and log income 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 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. See Data Appendix for details on variable definitions and construction.

Table A14: Treatment Effects on Household Enterprise Outcomes in 2012

		Index Components		
	Economic Index	Profits (Standard-ized)	Capital (Standard-ized)	Household Income (Standard-ized)
	(1)	(2)	(3)	(4)
Panel A: Pooled				
Grace Period	0.11	0.13	0.14	0.07
	(0.08)	(0.15)	(0.14)	(0.05)
	[0.14]	[0.38]	[0.29]	[0.10]
Control Group Mean	-0.09	0.05	-0.13	-0.20
Observations	369	369	369	369
Panel B: Heterogeneity by Parental Literacy				
Grace Period × Literate Parents	0.14	0.20	0.17	0.06
	(0.09)	(0.18)	(0.17)	(0.05)
	[0.12]	[0.27]	[0.32]	[0.22]
Grace Period × Illiterate Parents	0.05	-0.07	0.10	0.13
	(0.11)	(0.22)	(0.17)	(0.08)
	[0.60]	[0.71]	[0.63]	[0.18]
p-value: Grace Period × Literate Parents =	0.54	0.32	0.78	0.40
Grace Period × Illiterate Parents	[0.52]	[0.30]	[0.79]	[0.44]
Control Group Mean (Literate Parents)	-0.07	0.07	-0.09	-0.19
Control Group Mean (Illiterate Parents)	-0.18	-0.01	-0.29	-0.24
Observations (Literate Parents)	285	285	285	285
Observations (Illiterate Parents)	84	84	84	84

Notes: This table shows the effect of the grace period treatment on household income and enterprise outcomes from the 2012 (N=369) survey. 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 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. See Data Appendix for details on variable definitions and construction.

Table A15: Treatment Effects on Educational Outcomes with Neighborhood FEs

	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: Thana Fixed Effects						
Grace Period × Literate Parents	0.24	0.12	0.15	0.85	0.27	0.08
	(0.09)	(0.05)	(0.05)	(0.36)	(0.13)	(0.08)
	[0.01]	[0.05]	[0.01]	[0.04]	[0.05]	[0.30]
Grace Period × Illiterate Parents	-0.02	-0.13	-0.01	-1.02	0.40	0.30
	(0.12)	(0.06)	(0.07)	(0.47)	(0.19)	(0.12)
	[0.85]	[0.04]	[0.89]	[0.04]	[0.11]	[0.02]
p-value: Grace Period × Literate Parents =	0.05	0.00	0.06	0.00	0.58	0.11
Grace Period × Illiterate Parents	[0.05]	[0.00]	[0.06]	[0.00]	[0.69]	[0.15]
Fixed Effects	Thana	Thana	Thana	Thana	Thana	Thana
Control Group Mean (Literate Parents)	0.07	0.46	0.31	10.76	0.04	-0.20
Control Group Mean (Illiterate Parents)	-0.22	0.32	0.15	9.63	-0.16	-0.32
Observations (Literate Parents)	395	395	393	395	281	294
Observations (Illiterate Parents)	144	144	144	144	80	85
Panel B: Ward Fixed Effects						
Grace Period × Literate Parents	0.17	0.10	0.15	0.93	0.33	0.12
	(0.09)	(0.06)	(0.05)	(0.40)	(0.15)	(0.08)
	[0.11]	[0.14]	[0.04]	[0.07]	[0.05]	[0.16]
Grace Period × Illiterate Parents	-0.15	-0.10	-0.14	-0.78	0.58	0.45
	(0.14)	(0.08)	(0.09)	(0.62)	(0.27)	(0.15)
	[0.30]	[0.25]	[0.10]	[0.22]	[0.10]	[0.00]
p-value: Grace Period × Literate Parents =	0.04	0.04	0.00	0.02	0.37	0.04
Grace Period × Illiterate Parents	[0.06]	[0.04]	[0.00]	[0.04]	[0.53]	[0.08]
Fixed Effects	Ward	Ward	Ward	Ward	Ward	Ward
Control Group Mean (Literate Parents)	0.08	0.45	0.30	10.74	0.03	-0.22
Control Group Mean (Illiterate Parents)	-0.24	0.34	0.17	9.81	-0.15	-0.34
Observations (Literate Parents)	372	372	370	372	266	278
Observations (Illiterate Parents)	124	124	124	124	72	77
Panel C: Loan Group Fixed Effects						
Grace Period × Literate Parents	0.58	0.30	0.31	1.28	0.07	-0.50
	(0.22)	(0.15)	(0.13)	(1.20)	(0.22)	(0.27)
	[0.01]	[0.05]	[0.01]	[0.31]	[0.74]	[0.44]
Fixed Effects	Loan Group	Loan Group	Loan Group	Loan Group	Loan Group	Loan Group
Control Group Mean (Literate Parents)	0.07	0.46	0.31	10.76	0.04	-0.20
Control Group Mean (Illiterate Parents)	-0.22	0.32	0.15	9.63	-0.16	-0.32
Observations (Literate Parents)	399	399	397	399	283	296
Observations (Illiterate Parents)	144	144	144	144	80	85

Notes: This table shows how the effect of the grace period treatment on child-level education outcomes and household-economic outcomes with different neighborhood fixed effects. Columns (1)-(4) estimate child-level regressions on outcomes from the 2018 survey (N=543). Columns (5)-(6) estimate household-level regressions on outcomes from the 2010 (N=363) and 2018 (N=381) surveys. Differences in sample sizes across variables are due to missing data. 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, baseline controls selected by LASSO, Thana fixed effects and fully interacted effects of treatment and a dummy for parental literacy (equation 2; we do not report parental literacy dummy in Table). Panel B uses the same specification but includes ward fixed effects instead of Thana fixed effects. In Panel C, we regress each outcome on an indicator variable for assignment to grace period treatment, a parental literacy dummy, an interaction the grace period dummy and the parental literacy dummy, stratification dummies, child age fixed effects, an indicator for non-client respondent in 2018 survey, baseline controls selected by LASSO, and loan group fixed effects (we do not report parental literacy dummy in 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 A6 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 A16: Treatment Effects on Intergenerational Mobility Measures

	Dependent Variable is Son Rank. Sample is:		
	VFS	IHDS	
	(1)	(2)	(3)
Panel A: VFS			
Grace Period × Parent Rank	0.25		
	(0.10)		
	[0.03]		
Grace Period	-0.14		
	(0.07)		
	[0.08]		
Parent Rank	0.36		
	(0.07)		
Observations	274		
Panel B: IHDS			
Parent Rank		0.54	0.56
		(0.01)	(0.01)
p-value col 2 vs. col 3			0.000
Observations		6892	6892
Microfinance sub-sample		0	814

Notes: This table shows rank–rank measures of intergenerational mobility in the VFS and IHDS (2012) samples. In Panel A, we regress son's education rank 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, baseline controls selected by LASSO, and fully interacted effects of treatment and a dummy for mean parent education rank. Son and parent ranks are computed within the VFS sample education distribution separately by treatment group and are assigned using the mid-rank method. Standard errors clustered by loan group are reported in parentheses. In Panel B, we regress son's education rank on mean parent education rank for sons who are 18-28 in the IHDS sample and who live in urban areas. Son and parent ranks are computed within the IHDS sample education distribution and are assigned using the mid-rank method. In column 3, we categorize sons by whether their households meet the microfinance eligibility criteria (have at least one non-farm enterprise, own their home, and the household's yearly earnings are less than Rs.120,000). 817 out of 6,892 meet these criteria. If they do, we add the VFS treatment effects on level of education by mean parent education level to their level of education. If they do not, their level of education is not adjusted. We then generate a son rank using this new education distribution. Bootstrapped standard errors in parentheses.

Table A17: Treatment Effects on Educational Outcomes for Sons

	Investment Index	Investment Index Components: Primary School Investment Subindex	Investment Index Components: Secondary School Investment Subindex	Investment Index Components: College Spending (Standard-ized)	Completed Secondary School	Attended College	Years of Education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: School-Age Son Sample (7-17 Years at Baseline), Pooled							
Grace Period	0.25	0.13	0.25	0.12	0.07	0.11	0.54
	(0.12)	(0.12)	(0.13)	(0.12)	(0.06)	(0.05)	(0.40)
	[0.05]	[0.23]	[0.08]	[0.40]	[0.30]	[0.05]	[0.23]
Control Group Mean	0.03	-0.01	0.04	0.05	0.43	0.27	10.35
Observations	274	274	274	274	274	274	274
Panel B: School-Age Son Sample (7-17 Years at Baseline), Heterogeneity by Parental Literacy							
Grace Period × Literate Parents	0.31	0.20	0.34	0.26	0.11	0.16	1.12
	(0.14)	(0.14)	(0.16)	(0.19)	(0.07)	(0.07)	(0.44)
	[0.04]	[0.14]	[0.05]	[0.19]	[0.16]	[0.03]	[0.03]
Grace Period × Illiterate Parents	0.03	-0.03	0.09	-0.36	-0.02	-0.08	-0.65
	(0.18)	(0.17)	(0.17)	(0.22)	(0.10)	(0.08)	(0.81)
	[0.85]	[0.87]	[0.60]	[0.07]	[0.84]	[0.37]	[0.53]
p-value: Grace Period × Literate Parents =	0.21	0.26	0.23	0.03	0.26	0.02	0.05
Grace Period × Illiterate Parents	[0.21]	[0.28]	[0.28]	[0.02]	[0.29]	[0.04]	[0.09]
Control Group Mean (Literate Parents)	0.09	0.03	0.12	0.07	0.48	0.30	10.66
Control Group Mean (Illiterate Parents)	-0.19	-0.18	-0.24	-0.02	0.27	0.17	9.27
Observations (Literate Parents)	205	205	205	205	205	205	205
Observations (Illiterate Parents)	69	69	69	69	69	69	69

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 sons aged 7–17 (school-age) in 2007 (N=274). 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. See Data Appendix for details on variable definitions and construction.

B. Data Appendix

Our outcome variables draw on surveys done in 2010 and 2018. In 2018, our tracking rate is 88% (747 out of 845 households). Between the baseline and the final survey, 51 clients moved cities, 6 could not be located, and 16 were not surveyed due to illness. Nineteen clients died before 2018; for 18 of these clients, we interviewed another household member. Twenty-four clients refused consent for the 2018 survey.

Our main sample consists of households with at least one child aged 7-17 years at baseline, as measured in the 2018 survey. For the attrition check, we additionally include 81 households that had at least one child aged 7-17 present in the household in the baseline survey.

All continuous outcomes are top-coded at the 99.5th percentile. All monetary values are deflated to 2007 prices using CPI data published by the World Bank.

Household-Level Outcome Variables

- *Economic Index:* standardized index consisting of: profits, capital, and income. Standardization is based on the 2010 survey control means.

- *Profits:* obtained from survey question: "Can you please tell us the average weekly profit you have now or when your business was last operational?. By 'profits', I mean the income you receive from sales (revenues) after subtracting the costs (raw materials, wages to employees, etc.) of producing the items or services." Households without an enterprise in operation are assigned zero values.

- *Capital:* value (₹) of raw materials and inventory plus equipment across all businesses in operation at time of survey. Households without an enterprise in operation are assigned zero values for these outcomes.

- *Household Income:* In 2010 and 2018 survey, outcome is obtained from the survey question: "During the past 30 days, how much total income did your household earn?". In 2012 survey, the outcome is obtained from the survey question: "What is the average income for the whole household per month now?"

- *Household Workers:* sum of all household workers across all household businesses in operation at the time of the survey.

- *Non-Household Workers:* sum of all non-household workers across all household businesses in operation at the time of the survey.

- *Food Expenditures:* obtained from the following survey question in the 2018 survey: "How much did your household spend on food expenses in total during the past 7 days?". We did not collect information on total food expenditures in the 2012 survey.

- *Alcohol/Cigarettes Expenditures:* obtained from the following survey question in the 2018 survey: sum of household spending on alcohol and cigarettes in the past 7 days. We did not collect information on alcohol and cigarettes in the 2012 survey.

- *Festival Expenditures:* obtained from the following survey question: "How much did your household spend on festivities (marriages, births, funerals, festivals etc) expenses during the past 30 days?"
- *Renovation Expenditures:* obtained from the following survey question: "How much did your household spend on household renovations and damage expenses during the past 30 days?"
- *Health Expenditures:* obtained from the following survey question: "How much did your household spend on medical treatment expenses during the past 30 days?"
- *Education Expenditures:* obtained from the following survey question: "How much did your household spend on educational expenses during the past 30 days?"
- *Household Size:* obtained from the following survey question: " How many people live in the household? By that I mean all people, including children, who live under this roof or within the same house at least 30 days in the past year, and when they are together, they share food from a common source, and contribute to and/or share in a common resource pool."
- *Number of New Children Since Baseline:* the total number of children born to the client after the baseline survey.
- *Total Savings:* the sum of total savings held inside or outside of a bank account.
- *Permanently Migrated:* indicator variable that is equal to one if the household permanently outmigrated from Kolkata.

Child-Level Outcome Variables

For our sample, primary school (grades 1-4) is followed by secondary school (grades 5-10) and then higher secondary school (grades 11-12).

- *Investment Index:* standardized index that consists of: primary school investment subindex, secondary school investment subindex, and college spending,
- *Primary School Investment Subindex:* standardized index that consists of: private primary school, total primary school fees, and total primary school after-school tutoring.
- *Secondary School Investment Subindex:* standardized index that consists of the following variables: private secondary school, total secondary school fees, and total secondary school after-school tutoring.
- *Private School:* indicator variable that is equal to one if the child attended at least one year of private primary school (grades 1-4) or private secondary school (grades 5-12) respectively.

- *Total School Fees:* obtained from the question: “How much were/are the total school fees for (CHILD) in class X (including textbooks, uniforms, school fees, admission fees etc.)?”. The question was explicitly asked for grades 1, 10 and 12 and whenever the child changed a school.[44] For the remaining classes, we impute the value by copying the value from the class below. The value is 0 if the child did not complete the corresponding class. We compute total primary school fees by summing fees for grades 1 to 4 and total secondary school fees by summing fees for grades 5 to 12.

- *Total After-School Tutoring:* obtained from the following survey question: “How much did you spend in total on private tuition for (CHILD) in class X?”. The question was explicitly asked for grades 1, 10 and 12 and whenever the child changed a school. For the remaining classes, we impute the value by copying the value from the class below. The value is zero if the child did not complete the corresponding class. We then compute total primary school after-school tutoring by summing all tutoring costs for grades 1 to 4 and total secondary school after-school tutoring by summing all tutoring costs for grades 5 to 12.

- *College Spending:* obtained from the survey question: “How much did (CHILD) spend in total until now on all post-secondary schooling (excluding living costs such as board or food)?”

- *Completed Secondary School:* indicator variable that is equal to one if the child completed grade 12. Children still attending secondary school at the point of the survey are coded as 0.

- *Attended College:* indicator variable that is equal to one if the child attended or had completed post-secondary school (excluding vocational schooling) in the 2018 survey. Post-secondary school degrees include graduate degrees (science, art, commerce), medical/engineering degrees, post-graduate degrees, and engineering diplomas. Children that are still attending secondary school at the point of the survey are coded as 0.

- *Married:* child is married at the point of the 2018 survey.

- *Any Children:* child has at least one child at the point of the 2018 survey.

- *Housewife:* indicator variable that is equal to one if the respondent answered ”housewife only” to at least one of the following questions: “What is currently the primary occupation of (NAME)?”.

- *Dropout Reasons:* obtained from the following survey question: “Why did (NAME) stop attending school?” This question was asked for all children that did not complete grade 12. Multiple choices were allowed. The value is equal to zero if the child completed grade 12. Economic considerations consist of the following reasons: money

[44] 80% of children switched schools when transferring to secondary school in class 5. Nominal fees mostly remain the same across classes in the same school. In 98% of cases, the imputed schools fees in class 9 are the same as the reported school fees in class 10. We explicitly ask for school fees and after-school tutoring in class 10 and 12 since students need to take important exams at these points.

reasons, a good job opportunity, or feeling that school was not worthwhile. Child ability consists of the following reasons: child disliked school or had low test scores. Marriage factors include marriage- and pregnancy-related reasons.

- *Ever self-employed under 18:* indicator variable that is equal to one if the child ever engaged in self-employment under the age of 18 according to the 2012 survey. We use two sources of information to construct this variable: (1) the child engaged in self-employment in the past 30 days according to the 2012 household roster and (2) the child was ever listed as a household worker in the 2012 business roster.

- *Child Years of Education:* total years of education the child completed at the time of the 2018 survey. For college graduates, we use the average length of the completed degree program. For children who are still attending college, we are adding two years of education if the child is aged 20+ years and one year of education otherwise. We are also adding one year of education if the child is currently enrolled in a vocational school or if the child is currently pursuing a second bachelor's degree.

- *Child Rank:* percentile rank of the child based on the child's years of education variable. This variable is calculated separately for the treatment and control group.

Control Variables

- *Client's Age:* age of the client in years at baseline.

- *Client is Married:* indicator variable that is equal to one if the client was married at baseline.

- *Client Has Financial Control:* obtained from the following survey question: "If a close relative like your parents or siblings fell sick and needed money, would you be able to lend money to that relative, if you had the extra money?".

- *Empowered Client:* indicator variable that is equal to one if the client was listed in response to the following survey question: "Who has the major say in how much to spend on education?".

- *No Drain in Neighborhood:* indicator variable that is equal to one if the neighborhood has no drainage based on the enumerator's observation. A neighborhood is a collection of 10-15 houses surrounding client's house.

- *Client Is Impatient:* indicator variable that is equal to one if the client has a discount rate above the median.

- *Spouse's Age:* age of the client's spouse at baseline.

- *Literate Parents:* indicator variable that is equal to one if both parents can read and write. If the client is divorced or widowed at baseline, we use the literacy status of the client.

- *Household Size:* number of household members at baseline.

- *Education Expenditure 2007:* this variable sums all household education expenses in the past 30 days at baseline, including school fees, personal teaching expenses, and spending on textbooks.

- *Muslim:* indicator variable that is equal to one if the head of the household is Muslim.

- *Household Shock:* indicator variable that is equal to one if the household experienced a birth, death, or heavy rain in the last 30 days at baseline.

- *Number of Children in Household:* the number of children of the client at baseline that were in the household roster in the baseline survey.

- *Household Has a Business:* indicator variable that is equal to one if the household reported to have at least one business in operation at baseline, excluding businesses formed either during 30 days prior to or after loan group formation.

- *Loan Amount:* VFS loan amount given to client.

- *Owns Home:* indicator variable that is equal to one if the household owned the home at baseline.

- *Socio-Economic Index:* consists of the first component of a principal component analysis of whether the household had owned a radio, cassette player, camera, refrigerator, washing machine, heater, television, VCR, pressure lamp, tube well, wristwatch, or clock for longer than one year.

- *Female:* indicator variable that is equal to one if the child is female.

- *Child Age:* age of the child at baseline.

- *Birth Order:* birth order of the child.

- *Resides with Parents:* indicator variable that is equal to one if the child was part of the household roster at baseline.

Additional Variables

- *Parental Years of Education:* this variable is the average of highest grade of education completed across client and her spouse. We top-code individual schooling variable by 12 years since only one client and two spouses completed more than 12 years of education.

- *Primary School Parents:* indicator variable that equals one if both parents completed primary school. For client divorced or widowed at baseline, we use her educational attainment.

- *Parental Literacy Breakdown:* classifies the sample into four groups based on the literacy status of the client and her spouse at baseline. If the client is divorced or widowed at baseline, we assign the household either to “Literate Parents’ or “Illiterate Parents’ based on client’s literacy status at baseline.

- *Parent Rank:* percentile rank of parents based on the mean parental years of education variable, calculated separately for the treatment and control group.

Construction of Standardized Indices

1. If a component value in an index is missing and therefore cannot be standardized, we replace it with the relevant treatment group's average separately by parental literacy status, as long as there is at least one non-missing observation for the individual's remaining components of the index.

2. For each component, standardize with respect to the control group mean (subtract off the mean and divide by the standard deviation of the control group). For the household economic index, we standardize with respect to the control group mean in 2010.

3. Divide the standardized value by the number of components in the sub-index.

4. After completing steps 1-3 for each component, sum the values achieved in step 3 to obtain the index value.

Indian Human Development Survey

We use data from the Indian Human Development Survey to create Figure 1 and implement the education mobility and income inequality exercises in Section 5. The India Human Development Survey is a nationally representative panel survey of 42,152 households in 1,420 villages and 1,042 urban neighborhoods across India. The first round was conducted in 2005-2006 and the second round was conducted in 2011-2012.

For construction of Figure 1 and the education mobility exercise in Section 5, we restrict the sample to men aged 18-28 in 2012 (or 11-21 in 2005) who live in urban areas and who have at least one parent living in their household. For the earnings inequality exercise in Section 5, we restrict the sample to men aged 30 in 2012 who live in urban areas and who have at least one parent living in their household.

Variable construction

To construct parental education and literacy outcome, we first identify who the son's parents are using a variable that asks all household members to identify which roster member (if any) is their father and their mother. To construct average parental education, we take the mean education of the mother and the father. If only one parent's education level is non-missing, we use that to proxy for the average level of education of the parents. In the sample selection subsection below, we note how frequently this is the case for each of the samples. Parental literacy is defined as a dummy for having two literate parents. As with education, if only one parent's literacy is known, that is used to proxy for parental literacy. Our results are quantitatively unaffected if, instead, we defined parental education variables as missing if only one parent's level of education is known.

Son's years of education and college attendance are always measured in 2012. If the son is still in school, we utilize number of years of completed education.

We use the mid-rank method to construct parent and son education ranks. For example, if 20% of parents have 0 average years of parent education, their rank is 0.1. If another 5% have an average of 0.5 years of education, their rank is assigned as 0.225. And so on.

Sample and Selection

Figure 1 relies on the IHDS panel structure. 8,665 men aged 11-21 in 2005 live in urban areas. In 2005, 95% of these men live with at least one parent (85% live with both parents) and have non-missing parental education/literacy information. Of these 8,273 men present in the household roster in 2005, 5,431 are present in 2012; 1,891 are missing because the entire household was not surveyed in 2012 and 951 because the man is not present in the household although the household was surveyed. Conditional on the household being present in 2012, there is no statistically significantly different selection by parental literacy or by the interaction of parental literacy and household income quintile (see Section 2).

For our mobility exercises, we do not leverage the panel structure. For the educational mobility exercise, we restrict the sample to men aged 18-28 in 2012 years who lived in urban areas and co-reside with at least one parent (N=6892). Selection out of the sample will be very similar to that described above for Figure 1 since sons who are 18-28 in 2012 were 11-21 in 2005.[45] For the earnings mobility exercise, we restrict the sample to men aged 30 in 2012 years who lived in urban areas and co-reside with at least one parent (N=371).

C. Mobility Analysis

To understand population-level intergenerational mobility, we turn to the IHDS, which allows us to understand the status quo mobility for children in our school-age group age range and to then simulate how mobility would change as a result of our treatment. We use the IHDS for two reasons: first, it allows us to construct a dataset of child and parent education, and second, because it is a household panel, we can quantify what parts of the intended child-parent population distribution are absent in the sample due to children moving out of the household.

Educational Mobility

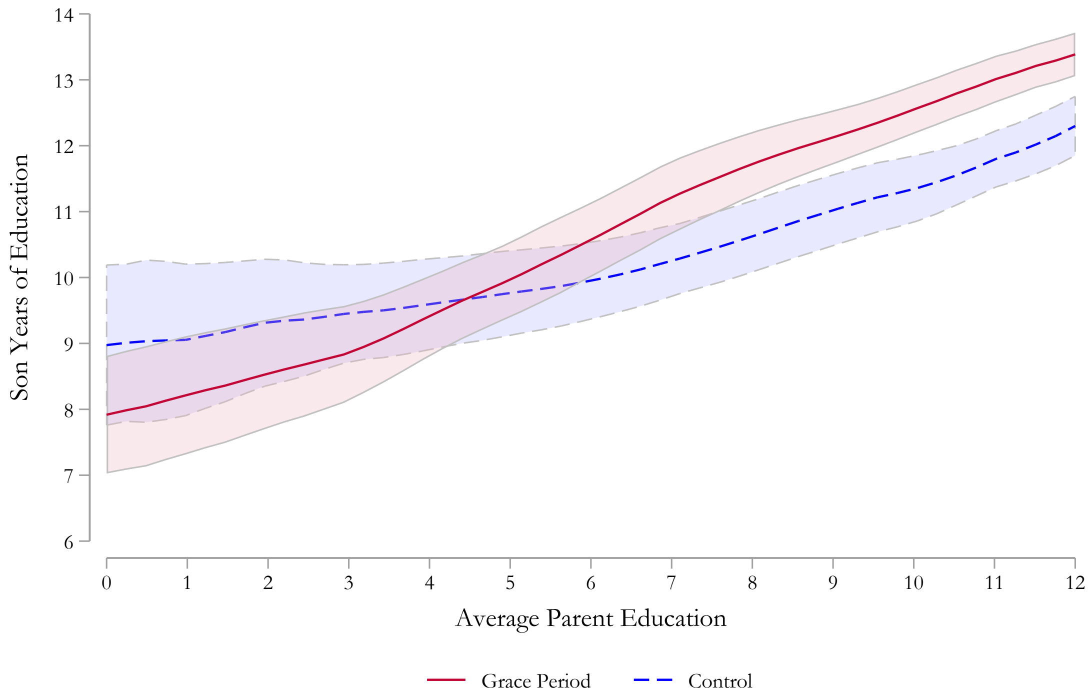
We begin with IHDS-2, which was conducted in 2012, and limit the sample to the 7,543 men who were 18–28 years of age and resided in an urban area.[46] We can ascertain the son's average parent level of education for 6,892 men who coreside with a parent. To generate ranks, we take the total years of education that the parents and sons completed. If sons were currently attending school, we assume that they completed the grade they were attending. We convert the son's level of education and the average level of parent into ranks and implement a rank–rank regression in Panel B, column (2) of Appendix Table A16.

To understand how treatment would affect this rank–rank correlation at the population level, we return to the VFS sample. First, we estimate the treatment effect on son's years of education by parent level of education using a local polynomial regression: we regress son's years of education on parent level of education by treatment assignment (Appendix Figures A10).

We estimate the treatment effect for each level of parent education by taking the difference of the fitted treatment value and the fitted control value of son's years of education. For

[45] The only difference is driven by a small number of inconsistencies in the ages reported in 2005 and 2012.
[46] We focus on men because 55% of women in that age range leave the household between 2005 and 2012, likely in large part due to marriage.

Figure A10: Local Polynomial Regression of Son's Years of Education and Average Parent Education



Notes: These figures plot the distribution of son's years of education by average years of parental education (average of mother's and father's education). We separately estimate local regressions (bandwidth = 2, kernel = epanechnikov) for sons in treatment (solid line) and control (dotted line) households. The x-axis shows average parental years of education. The shaded areas correspond to 90 percent confidence intervals. The hollow circles correspond to the raw means of each outcome variable. The sample consists of school-age sons (7-17 at baseline; N=274).

each level of parent education, we estimate how many more (or fewer) years of education a son attained due to the treatment.

To approximate treatment impact on population-level mobility, we identify a subsample of IHDS households that are comparable to our VFS study clients. These are urban households that meet the inclusion criteria for most microfinance lending (henceforth, "IHDS microfinance sample"). In 2011, the Reserve Bank of India mandated that to qualify for microfinance, an urban household could not earn more than ₹ 120,000 per year.[47] VFS utilized three additional metrics to ascertain loan risk: (i) whether the household had an enterprise, (ii) whether the household owned the structure they lived in, and (iii) whether the borrowing female client was married. We therefore identify the IHDS microfinance sample using the criteria: household operates a non-farm enterprise, owns the home they live in, and annual household income was below ₹120,000. If a son in the sample has a parent with a matching level of education in VFS, and he meets the household criteria, then we add to his level of

[47] https://www.rbi.org.in/commonperson/English/Scripts/Notification.aspx?Id=945

education the corresponding treatment effect. 817 of the 6,892 sons in the sample meet these criteria. We then re-rank all the young men in the sample based on the adjusted levels of education and regress this new rank on the parent rank. Panel B, column 3 of Appendix Table A16 presents this result. We also report the p-value of an F-test of equality of the rank–rank coefficient in column (2) relative to column (3).

Economic Mobility

In the IHDS-2 dataset, we limit the sample to men who are 30 years old, reside in an urban area with their parents such that we can observe the parents' literacy status. 596 men meet these criteria. Parallel to VFS analysis, parents as illiterate if either or both the mother and father are illiterate. Roughly 52% of men have literate parents. The monthly earnings of men with literate parents is Rs. 6294 and for men with illiterate parents it is Rs. 3231, in 2007 INR.

The treatment induced sons of literate parents to attain 1.12 more years of education (Panel B, column 4 of Appendix Table A17). Khanna (2023) finds average return to education for males is 14.6% per year. So we estimate that sons of literate parents in the treatment group earn $e_L \times (1 + r \times t) = 6294 \times (1 + 0.146 \times 1.12) = 7323$. Therefore $\Delta E_L = e_L \times [(1 + r \times t) - 1]$ captures the treatment induced difference in earnings between treatment and control sons of literate parents. The treatment induced the sons of illiterate parents to attain 0.65 fewer years of education (Panel B, column 4 of Appendix Table A17). So we estimate that sons of illiterate parents in the treatment group earn $e_I \times (1 - r \times t) = 3231 \times (1 - 0.146 \times 0.65) = 2924$. Therefore $\Delta E_I = e_I \times [(1 - r \times t) - 1]$ captures the treatment induced difference in earnings between treatment and control sons of illiterate parents. As Khanna (2023) does not provide estimates of the return to education by parental literacy, we use the same r for both groups.