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

How do poor entrepreneurs trade off investments in business enterprises versus children's human capital, and how do these choices influence intergenerational socio-economic mobility? To examine this, we exploit experimental variation in household income resulting from a one-time relaxation of household liquidity constraints (Field et al., 2013), and track schooling and business outcomes over the subsequent 11 years. On average, treatment households, who were made wealthier through the experiment, increase human capital investment such that their children are 35% more likely to attend college. However, schooling gains only accrue to children with literate parents, among whom college attendance nearly doubles. In contrast, treatment effects on investment among the illiterate accrue only on the business margin and are accompanied by adverse educational outcomes for children. As a result, treatment lowers relative educational mobility. In a forecasting exercise, we find that earnings gains for literate households are four times larger than the earnings gains for illiterate households, raising earnings inequality. Our findings highlight how parental investment choices can contribute to a growth in intergenerational earnings inequality despite reductions in urban poverty.

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1 Introduction

Do growth opportunities for poor households break the intergenerational transmission of poverty? In theory, interventions that improve households' income trajectory, such as positive liquidity shocks, can increase child schooling investment and disrupt intergenerational poverty traps (Becker and Tomes, 1986; Galor and Zeira, 1993).

In practice, however, poor households may exhibit a low marginal propensity to invest income gains in children's human capital. First, most of the world's poor are self-employed, so when earnings opportunities rise the opportunity costs of child schooling also increases (Shah and Steinberg, 2017). Second, poor households may underestimate returns to education, leading parents — particularly those with high discount rates — to favor investment in household business opportunities over child education (Banerjee, 2004).[1] Third, the risk environment facing the poor may cause them to favor relatively liquid investments. Finally, structural or early life factors may lower the returns to schooling investment for children in poverty. For instance, neighborhood effects may limit children's access to quality education or job networks (Chetty et al., 2016); likewise, epigenetics or health inputs in infancy may reduce lifetime earnings potential from an early age (Attanasio et al., 2021). As a result, income gains among the poor may not increase intergenerational educational or earnings mobility for their children.

In this paper, we exploit experimental variation in the income trajectories of poor urban microentrepreneurs to evaluate investment trade-offs across business opportunities and children's human capital — or, put differently, current versus future family income. Our study setting is India, a country with one of the lowest rates of intergenerational educational mobility in the world (Asher et al., 2021). Using survey data collected 11 years post-intervention, we revisit participants of a 2007 field experiment in which microfinance borrowers in the city of Kolkata were randomly assigned to either the classic microfinance contract or to a contract with a flexible repayment schedule that eased liquidity constraints (now on, grace period contract). Field et al. (2013) showed that, three years after the trial, household income was 20% higher in the treatment group relative to the control group.[2]In addition to business and income measures, our 11 year follow-up survey collected educational and socio-economic outcomes for all children, including those who had left the household. We estimate current economic returns to all household members and forecast lifetime gains for children. In doing so, we account for the fact that parents may divert

[1]Studies document a positive relationship between parental education and children's perceived returns to education (Boneva and Rauh, 2019; Boneva et al., 2021; Chakravarty and Agarwal, 2021), and mixed evidence on the relationship between parental education and parental beliefs (Brown, 2006; Boneva and Rauh, 2018; Attanasio et al., 2020).

[2]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; Battaglia et al., 2021; Shonchoy et al., 2014); or, choice of repayment schedule akin to a line of credit (Aragón et al., 2020).

income gains away from enterprises to invest in children's human capital.

We find that households allocate investment across both portfolio choices, resulting in significant schooling increases. Children in treatment households scored 0.20 standard deviations higher on an education investment index. By 2018, treatment households were more than twice as likely to enroll their children in private secondary school and increased spending on after-school tutoring by 21%. Overall, the increase in education spending represents around 14% of the increase in household income. Reflecting these investments, children in treatment households are 9.6 percentage points more likely to attend college, which amounts to a 35% increase in likelihood to attend college when compared with control group children in the same age group. Younger children — those with more exposure to the treatment — benefit more than older ones: treatment effects on educational investment grow in inverse proportion to child's age at baseline.

Patterns of investment vary systematically with parental education, with implications for intergenerational mobility. Illiterate entrepreneurs systematically favor investment in the household business over children's schooling, and divest in child schooling when liquidity constraints are relaxed, consistent with a complementarity between child labor and business investment. Meanwhile, literate entrepreneurs are more likely to invest in child schooling over the home business when they experience an income shock, reducing short-run household income gains but raising expected intergenerational income. Specifically, in households where both parents are literate, treatment increases college attendance by 15.4 percentage points. In contrast, children with at least one illiterate parent are 13.9 percentage points less likely to complete secondary schooling compared to control counterparts.

Do differences in educational investment reflect limited short-run business (and, therefore, income) treatment gains among illiterate households? Here, we consider a business index with weekly enterprise profits, business capital, and labor as components. In 2010, on average, treatment households with school-going children score 0.26 standard deviations higher on this index. All households report gains though the magnitude is larger for illiterate households. The effects persist for illiterate households; relative to control counterparts, they report a 66 percent increase in profits and a tripling of enterprise capital in 2018. In contrast, treatment effects on the business margin fade for literate households. Consistent with business and education investments being substitutes, children in illiterate treatment households report dropping out due to family factors and the business roster (as of 2012) points to positive (but noisy) impacts on child labor.

Next, we examine the evolution of family income, looking separately at client household and adult children's income. Consistent with treatment differences in schooling and business outcomes, by 2018 treatment effects on household income have diverged: illiterate treatment households report approximately 27% higher monthly income than control counterparts. This estimate is comparable to observed average income gains for the full sample in 2010. In contrast, we no longer observe a treatment difference in income among literate households.

Finally, we study the intergenerational transmission of inequality. We quantify intergenerational mobility in several ways: children's absolute educational mobility, rank-on-rank educational mobility, bottom half mobility, and lastly children's relative earnings inequality. At the time of our 2018 survey, a higher share of treatment children remained in school. Assuming that treatment induced children with the highest earning potential to continue to college, estimated treatment effects on child income among those who have completed school will be biased downward. This lower bound exercise on treatment impacts on child income shows that treatment group sons earn 26% higher income as young adults. Consistent with low work rates among Indian women, we do not observe income gains due to treatment for daughters. Following Hendren and Sprung-Keyser (2020), we estimate the impact of increased educational attainment on lifetime income using age-earning curves from nationally representative data. Our preferred specification accounts for potential gains in child earnings that could accrue both through increased education and through other means. This allows us to account for the fact that children of illiterate parents benefit from treatment by inheriting larger businesses. We find that the net present value of pooled lifetime returns for treatment children is 4,905 USD PPP. While, on average, returns are positive for all children, treatment gains for children of literate parents are four times that of children of illiterate parents. This points to an increase in income inequality for the next generation: the forecasted earnings gap between children of literate and illiterate parents rises from 14% within the control group to 34% among children in the treatment group.

By linking investment choices to intergenerational outcomes, this paper extends an experimental literature that has demonstrated how asset transfer programs can yield persistent household income gains (Balboni et al., 2021; Banerjee et al., 2020). Often, such impact evaluations focus on household-level outcomes. In the longer-run, such evaluations are likely to underestimate true program returns if they neglect to account for future income gains that will accrue from investments in children's human capital.

Experimental evidence on human capital investments associated with short-run income gains comes primarily from rural study samples (Attanasio et al., 2015; Augsburg et al., 2015), where returns to schooling are lower and the supply of higher education institutions is more limited. However, 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.[3]

[3] Attanasio et al. (2015) study the impact of microcredit for borrowers in Mongolia and find that children's education increases as a result of treatment, but only for children of more highly-educated borrowers. Augsburg et al. (2015) report on a credit program for microentrepreneurs in Bosnia and Herzegovina and find suggestive evidence that child labor increases among low-educated borrowers as a result of the credit shock. All of the Attanasio et al. (2015) sample and 71% of the Augsburg et al. (2015) sample live in rural areas. For agricultural settings, evidence on how rainfall-induced income shocks impact educational attainment is also mixed (Jensen, 2000; Björkman-Nyqvist, 2013; Shah and Steinberg, 2017; Zimmermann, 2020).

We study this question in an urban setting where there are high returns to children's secondary and tertiary schooling, and so the opportunity cost of pulling children out of school is larger.

A growing body of literature investigates whether credit constraints for human capital accumulation can engender an intergenerational poverty trap. Much of this research is concerned with developed country contexts (Carneiro and Heckman, 2002; Dahl and Lochner, 2012; Bulman et al., 2021), though a handful of recent studies evaluate whether subsidies for school or college fees increase educational attainment (Angrist et al., 2006; Duflo et al., 2021; Solis, 2017; Londoño-Vélez et al., 2020). To the best of our knowledge, only Blattman et al. (2020) provides experimental evidence on the long-run effect of a cash transfer on child outcomes and, in contrast to our results, reports no impacts. This might be because their study was conducted in a rural setting with fewer opportunities for educational investments or because their sample was much younger and less likely to have completed fertility at the point of the intervention.

A growing body of evidence shows that (perceived) returns to children's education vary with parental education and that parental education is therefore a strong predictor of households' schooling choices (Brown, 2006; Boneva and Rauh, 2019; Boneva et al., 2021; Chakravarty and Agarwal, 2021). We add to this literature by showing how the link between parental and child education has consequences for intergenerational educational and earnings mobility. At a more macro-level, evidence from India suggests that, despite economic growth and gains in absolute mobility, relative educational mobility has been limited (Emran and Shilpi, 2015; Asher et al., 2021). In line with results from the United States (Chetty et al., 2016; Chetty and Hendren, 2018), evidence suggests significant variation in mobility across sub-regions of developing countries, with higher mobility in urban areas (Alesina et al., 2021). Our results help shed light on how rapid economic growth can be paired with increasing inequality and poor intergenerational mobility in India.

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

2 Background and Data

To motivate our analysis, we begin by describing our sample and relevant investment opportunities. After this, we lay out hypotheses regarding anticipated treatment effects. We conclude this section by describing the data utilized in our analysis.

2.1 Context

The grace period microfinance experiment was implemented with 845 low-income micro-entrepreneurs in Kolkata in 2007. Each client received an individual liability loan ranging from Rs. 4,000 -

10,000. Loan disbursement and repayment occurred in five-member groups. Prior to loan disbursement, each group was randomized into either the regular debt contract with repayment in 22 fixed installments starting two weeks after loan disbursement, or a grace period contract in which repayment, instead, started eight weeks after loan disbursement.[4] Relative to the regular contract, the grace period contract, by encouraging higher return (but also higher risk) business investments, significantly increased microenterprise investment, and observed dramatic growth in business profitability. Three years after loan disbursement, household income was 19.5% higher among those assigned to the grace period contract (for further details, see Field et al. (2013)).[5]

How did treated clients allocate this additional income across available investments? At baseline, the median client was 34 years old and had one school-aged child (aged 7-17). Given these demographics, we now provide context for focusing on two portfolio choices: business re-investment and investment in children's human capital.

Business investments Throughout our study period, micro-enterprises remain a primary income-generating activity for client households. At baseline all study households owned at least one enterprise and, in 2018, 85% reported that at least one enterprise was still in operation. These businesses are typically in the retail, piece rate, or service sector and generally employ low-skilled, household labor. Only 16% of enterprises report any non-household employees at baseline.

Despite high returns to capital, credit constraints continued to hinder profitable business investments. When asked in 2012 about all businesses that the household ever had, clients reported that only 34% of businesses were opened with sufficient resources on hand. When asked what they would have done with an additional Rs. 20,000 at the start of their business, clients responded that they would have bought additional equipment or raw material (42%) or started a different business altogether (20%). Thus, investing income gains to expand household businesses remained an attractive option.

Households also have reason to hold liquid assets in order to insure their businesses against idiosyncratic and systemic risks. For instance, between 2012 and 2018, 50% of enterprises owned by control group households shut down, with respondents citing household illness as the cause for 27% of these closures. In terms of systemic risk, the nation-wide microfinance crisis led to a massive negative liquidity shock between 2010 and 2012: during that period, the percentage of control group households that closed at least one business increased from 34% to 57%.

Education investments Our study period (2007-2018) was an era of rising educational attainment and proliferating school expenditures across India. Grade progression and advancement to secondary and tertiary schooling increased dramatically. Figure 1, based on the 2014-15 nationally representative National Family Health Survey (NFHS) data, plots urban school completion rates

[4] Groups faced the same interest charges. However, longer debt maturity (55 versus 44 weeks) combined with the same total interest charges implied that treatment group clients faced a slightly lower effective interest rate.

[5] Consistent with the existing literature, Field et al. (2013) estimate a high monthly return to capital of 13%.

by birth year cohort: The x-axis plots the year at which a respondent turned 18, with shaded grey and brown areas representing cohorts of the same age as parents (old cohorts) and children (young cohorts) in our sample, respectively. Educational patterns for both cohort groups in our sample reflect national trends. Cross-cohort differences are striking: old cohorts have relatively low educational attainment, a significant share is illiterate, and large gender gaps in education remain. In contrast, literacy and primary education are close to universal for the young cohorts and the gender gap has largely closed.

Particularly noteworthy are rising college enrollment rates. Montenegro and Patrinos (2014) estimate that college completion leads to 21% higher earnings across India and Rani (2014) estimates a 24% rate of return to college in urban areas. In our control sample, college education is associated with 25% higher monthly earnings among children aged 25 or older relative to children who only completed secondary school. Reflecting the idea that college is a stepping stone towards upward mobility via higher-skilled employment, 58% of college graduate children in the control sample engage in salaried work (84% among sons).[6] College education is also thought to substantially improve marriage market outcomes (Adams and Andrew, 2019), which could be an important source of returns.

National survey data shows rising educational expenditures: Between 2007 and 2014, annual school investment (tuition and tutoring) per child nearly tripled from roughly US$36 to US$100 (Myroniuk et al., 2017) An important reason is private schooling and after-school tutoring expenditures (largely at secondary school-level), motivated by their value in improving grade 12 scores (Kingdon, 2020; Berry and Mukherjee, 2019) and thereby entrance into low-cost public colleges (Sekhri, 2020). In 2005, 58% of children aged 6-14 years in urban areas were enrolled in private schools (Dubey et al., 2009). In the control group in our sample, 96% of children report private after-school tutoring; this tutoring involved supplementary instructions in some (or all) academic subjects.[7] Among secondary school graduates, an additional Rs 100,000 of after-school tutoring in secondary school is associated with a 36 percentage point increase in college attendance. But private educational spending is costly. In 10th grade, households spend on average Rs 8,300 per child on school expenditures and after-school tutoring, amounting to 4% of average household income in 2010.[8] On average, spending on after-school tutoring is 113% higher than total school expenditures in secondary school.

[6] In contrast, business sectors in which our respondents work — tailoring, food preparation, etc. — do not have an obvious need for high-skilled labor (fewer than 1% of parents in our sample had a college education).

[7] Primary school (grades 1-4) is followed by secondary school (grades 5-10) and then higher secondary school (grades 11-12). Private schools outperform public schools: For example, 55% of private secondary schools are English rather than Bengali medium (existing research documents large returns to English skills (Azam et al., 2013)). While the median grade on Class 12 exams for a control group child in public school is a B, for a private school child the median grade is an A.

[8] Schooling costs include annual enrollment fees; monthly school fees; costs for school uniforms and textbooks; and, if applicable, boarding fees. Conversely, children who attend public primary and secondary school cover costs only for school uniforms and textbooks.

We now use these descriptive facts about our sample and context to lay out predictions regarding household investment choices.

2.2 Predicting investment choices when parents differ in education

Education levels are low on average but vary significantly within our client sample: at least one parent is illiterate in 18% of households.[9] This variation is potentially important since a growing body of evidence suggests that children's educational outcomes vary with parental literacy in lower-income settings. In particular, survey data from Kolkata (Chakravarty and Agarwal, 2021) and other settings (Brown, 2006; Boneva and Rauh, 2019; Boneva et al., 2021) document that illiterate households have lower (perceived) returns to children's schooling. Other research shows that illiterate parents are less equipped to provide complementary inputs to facilitate their children's human capital accumulation, such as help with schoolwork (Banerji et al., 2017). They also find it harder to assess their children's ability (Dizon-Ross, 2019) and navigate the school system because they have limited exposure to successful students via friend and family networks (Sequeira et al., 2016).

Figure 2 — based on nationally representative data from the 2015 NFHS survey — is also consistent with the idea that, conditional on household wealth, parental literacy influences educational investments. The figure shows that children of more wealthy Indian parents are more likely to attend college. Across all wealth quintiles, educational attainment rates are significantly higher for children from literate households. Patterns in data from our study sample mirror national trends: Table A7 shows that, conditional on households wealth, educational investments are correlated with parental literacy among control group households. Controlling for baseline wealth, children of literate parents are 136% more likely to have attended college.

Given the evidence that parental education is predictive of differences in relative returns to education, we examine both average treatment effects on educational and business investments and heterogeneous treatment effects by parental literacy. Under the assumption that discounted returns to educational investment exceed those from continued business investment only for literate households, we test the following predictions:

Treatment impacts for households with literate parents: Relative to literate parents in the control group, literate treatment parents will spend more on education and their children will have higher educational attainment. We anticipate that higher spending on education will be accompanied by a reduction in business spending which will diminish the treatment wedge in business outcomes for literate parent households over time. In the long-run, the household income of literate households in the treatment group (inclusive of adult children's income) will exceed that of their counterparts in the control group; however, this difference will be muted or absent while children are still in school or college.

[9] Appendix Figure A2 shows the distribution of parental education.

Treatment impacts for households with illiterate parents: In contrast, relative to control counterparts, illiterate households in the treatment group will increase investments in business. Further, if capital and labor are complements in the household enterprise production function, then treatment may even lead to a decline in child schooling. We anticipate persistent treatment effects on business outcomes for illiterate parent households. We also anticipate treatment effects on household income to persist, even when children are of school-going age.

Finally, educational investment patterns that differ by parental literacy across treatment and control groups suggest that treatment may also impact intergenerational educational mobility and earnings inequality (we use forecasted child earnings for the latter). If gains in educational attainment are concentrated among children of literate parents, and/or if children of illiterate parents drop out of school to work in the household enterprise, then treatment will (weakly) decrease relative intergenerational educational mobility. We also predict that children from treatment households will, on average, have higher lifetime earnings then their control counterparts but will also exhibit greater earnings inequality.

As a precursor to testing these predictions using data from our eleven year follow up survey, we describe our data collection and key outcomes of interest.

2.3 Data: Surveys and Outcomes

Our primary data source for long-run education, business, and income outcomes is a 2018 household survey conducted. In examining persistence, we compare these outcomes to data from the 2010 survey analyzed in Field et al. (2013).[10]

At the 11-year follow-up, our tracking rate is 88% (747 out of 845 households), which is on par with other long-term studies (Blattman et al., 2020; Banerjee et al., 2020).[11] Appendix Table A1 reports response rate and respondent composition for 2010 and 2018 survey rounds. Panel A shows similar tracking rates across treatment and control groups across rounds. Appendix Table A2 reports the balance check at baseline for the full sample and for our primary analysis sample: households with at least one child aged 7-17 at baseline. We are balanced across most variables and in neither sample can we reject the hypothesis that treatment coefficients are jointly equal to zero. We consider multiple outcomes, some at the child-level (child sample) and others at the household-level (household sample). Here, we describe our main outcome variables, including both outcome indices and their respective components.

Child sample and educational outcomes Our primary analysis sample includes school-age children (7-17 years) at baseline. These children are old enough to have completed K-12 schooling by 2018 and young enough such that short-run treatment income gains could have impacted

[10] In the appendix, we also show intermediary outcomes based on data from a 2012 business survey.

[11] Between 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 between baseline and the final follow-up survey; for 18 of these clients, we interviewed another household member. Twenty-four clients refused consent for the final follow-up survey.

schooling investments. Appendix Figure A1 plots the age distribution and enrollment status in 2018 by child age for the control group. Children younger than 7 and older than 17 in 2007 form the "young child" and the "old child" sample respectively. We also examine our main child outcomes for different school-aged samples and show that our results are robust to changing the sample cut-offs by up to two years in either direction (Appendix Table A5).

Our primary schooling outcomes are college attendance, secondary school completion, and schooling expenditures. The 2018 survey asked clients to report educational attainment and socio-economic outcomes — including residence, income, occupation and marital status — for all of their children, including those living outside of the household. At the time of the 2018 survey, 314 of the 747 tracked clients reported at least one child living outside of the household. In our study context, daughters generally leave the home upon marriage while sons stay living in the same home as their parents, together with their spouse. Consistent with this, 81% of sons still lived in the household in 2018, compared to only 44% of daughters.

Among children who dropped out before secondary school completion, we categorize reasons for leaving school into family, child, and marriage factors. We also create an investment index using school spending data collected in the 2018 survey that aggregates college spending and primary and secondary school investment sub-indices. Each sub-index includes total spending on school fees, total spending on private after-school tutoring, and private school attendance.

Following our pre-analysis plan, we examine heterogeneity in intergenerational mobility according to parental education.[12] As in Alesina et al. (2021), our preferred indicator for parental education is parental literacy at baseline. Our literacy measure captures whether both parents can read and write.[13] We also show our main results under alternate specifications for parental education (Appendix Table A11).

Household sample and enterprise outcomes We consider outcomes for both the full sample and for households with school-going children. Roughly half of households in our study sample have at least one child in this group (Appendix Table A2). Our enterprise outcome variables are from household surveys administered in 2010 and 2018. Our primary outcomes of interest are profits, capital, and labor. For each enterprise, we asked, "Can you tell us the average weekly profit you have now? 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

[12] While our pre-analysis plan specified parent and child health as additional outcomes of interest, we were unable to collect child health outcomes other than survival, which is extremely high, for those not in the household in 2018. Given our focus on treatment impacts for *all* children we exclude health outcomes. We specified, but did not conduct, heterogeneity analyses by whether the client had completed fertility at baseline, since 89% of clients did not have additional children after baseline. Finally, we specified analysis of treatment impacts by clients' decision-making power. We find that treatment impacts on the education investment index are concentrated among households in which the client had a higher self-reported level of financial control, but find no differences for other education outcomes.

[13] Appendix Table A3 compares literate and illiterate households along several baseline variables.

services." We calculate a household-level enterprise profit measure that aggregates across all household enterprises and similarly a household-level business capital measure that sums across raw materials, inventory, and assets for all household enterprises. Our labor measure is the number of workers employed across all household enterprises. Households without a microenterprise in operation are assigned zero values for all business outcomes. Finally, we combine these three outcomes into a business index.

Household and child earnings The 2010 and 2018 surveys measured client income with the same question: "During the past 30 days, how much total income did your household earn?"[14] For child earnings in 2018, we sum up the income from salaried work, daily wage jobs, and self-employment in the past 30 days for each child. We also ask whether children inside and outside the home worked in the past 30 days and, for daughters, whether the child reports being a housewife as a proxy for female work aspirations. Even by 2018, assessing treatment effects on adult children's outcomes is challenging because a substantial share of children (17% percent of sons and 18% of daughters in the control group) remain enrolled in school so education and adult earnings outcomes are censored. That said, we collect work outcomes and earnings over the previous 30 days for all children, including for those in school and working part-time.

Finally, we use data from various waves to construct three child labor measures, including an indicator of whether a child dropped out of school and started work before age 18, an indicator for whether a child who was below 18 in 2012 engaged in any work activity in the previous 30 days, and an indicator for whether a child is listed in the 2012 business employee roster in which we ask about all paid and unpaid employees that ever worked for the enterprise.

Multiple-hypothesis testing We employ two approaches, both described in our pre-analysis plan, to reduce the chance of falsely rejecting a null hypothesis. First, we consider indices of outcomes of interest. Second, we correct for multiple hypothesis testing. Following the approach developed by Benjamini et al. (2006) and described in Anderson (2008), we calculate sharpened q-values that control for the expected share of rejections that are Type I errors — the false discovery rate (FDR) — for two outcome families. The first family is comprised of 7 tests and includes the household-level economic outcomes and child-level education and socio-economic outcomes for the pooled school-age sample (Panel A of Tables 1, 3 and 4). The second family is comprised of 21 tests and includes the same set of outcomes but from our heterogeneity analysis by parental education for the school-aged sample (Panel A of Table 2 and Panel B of Tables 3 and 4).

3 How did households invest their economic gains?

We consider, in turn, treatment impacts on education and business outcomes. In both cases, we examine whether impacts differ by parental literacy.

[14] We follow Field et al. (2013) and top-code income at the 99.5 percentile.

3.1 Educational outcomes

A. Average outcomes

We start with non-parametric estimation results for the full child sample for our two primary educational outcomes of interest: college attainment and educational investment. Figure 3 plots a local polynomial regression of either college attainment or investment on child age at baseline, by treatment and control. The vertical dotted line marks the cut-off point for our main child sample of children aged 7-17 years at baseline. Treatment effects are concentrated in the main child sample. Consistent with existing evidence that raising college attendance requires investment early in a child's educational career (Carneiro and Heckman, 2002; Chetty et al., 2016), treatment effects on educational investment index grow in inverse proportion to child age at baseline.

Next, we investigate treatment effects on educational outcomes in a regression framework. Here, we split the sample by child age at baseline as discussed in Section 2: children aged 7-17 at baseline comprise the "school-age" sample; children above 17 at baseline (93% of whom had completed schooling at baseline) form the "old child" sample; and children under 7 at baseline (who have yet to complete schooling) form the "young child" sample.[15] Among the young child sample, we consider primary school outcomes.

We estimate the following specification for child i from household h in microfinance group g:

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

T_g indicates whether the individual was in a treatment loan group, θ_g are stratification dummies for treatment group batch and X_{ihg} are baseline control variables selected via a double lasso approach (Belloni et al., 2014).[16] We include a control for whether the client died before the 2018 survey and the interview was with a non-client household member. Standard errors are clustered by loan group.

Panel A of Table 1 considers results for the school-aged child sample. Overall, the grace period treatment led parents to substantially increase educational investment: treatment children score 0.20 standard deviations higher (significant at the 1% level) on an aggregate investment index (column 1), which comprises school expenses (admissions fees, school fees, uniforms, textbooks, and after-school tutoring expenses), college spending, and indicator variables for whether children attended private primary or secondary schools. While we find a positive but statistically insignificant treatment effects on primary-school investment (column 2), the secondary schooling investment index is 0.25 standard deviations higher for treatment children and statistically significant at the 1% level (column 3). Appendix Table A4 shows results on individual index

[15] In 2018, 78% of control group children in the young child sample are still in secondary school.

[16] Appendix Table A2 shows the potential set of lasso controls, which also includes dummies that indicate missing information for control variables and additional squared terms for continuous control variables.

components. Treatment children are more than twice as likely to attend private school, and their parents spend an extra Rs. 4,868 per child on after-school tutoring when compared to control group counterparts. Finally, treatment parents report 0.17 standard deviations higher college expenditures, significant at the 10% level (column 4).[17]

Increased human capital investment translates into enhanced schooling attainment for treatment children. Column (5) of Panel A Table 1 shows that, relative to control children in same age group, children in treatment households are 9.6 percentage points more likely to have completed, or be currently enrolled in, college (now on, attend college). This amounts to a 35% increase in likelihood of college attendance compared with similarly aged control group children. As a benchmark, Duflo et al. (2021) find that secondary school scholarships in urban Ghana increase the likelihood of enrolling in college by 26% (4 percentage points on a base of 15.2 percent college attendance). Meanwhile, Parker and Vogl (2021) report no impacts of the Progresa conditional cash transfer program in Mexico on college attendance. Treatment has a positive, but statistically insignificant, impact on secondary school completion (column 6 Panel A).

Panel B of Table 1 examines whether treatment effects differ by child gender. Consistent with broader trends in urban India (see Section 2.1), we observe convergence in schooling attainment for boys and girls and no gender gap in school spending or college attendance in control households (columns 1-4). Among control group children, daughters are as likely as sons to attend college (column 5). Across all educational attainment and investment measures, we cannot reject equivalent treatment effects for sons and daughters. One important implication is that, relative to control group counterparts, daughters in the treatment group are 9.5 percentage points more likely to attend college (column 5) and treatment parents invest 0.16 standard deviations more in their daughters' schooling (column 1).

In Panel C and D of Table 1, we consider educational outcomes for children too old (Panel C) or too young (Panel D) to have had their college education impacted by treatment. We do not observe treatment effects on any educational outcome for the old child sample (Panel C), nor do we detect differences in primary school investment for the young child sample (Panel D). In the latter case, it could be that, as with the school-going age group, treatment parents make differential investments only at higher schooling levels.

We now examine how results on educational outcomes are impacted by FDR corrections to account for multiple hypothesis testing. Appendix Figure A4 plots the sharpened q-values against p-values for the first outcome family (outcomes for the pooled school-age sample) and second outcomes family (outcomes for the school-age sample by parental education), respectively. We plot all of the tests with their corresponding q-values. In Panel A of Table 1, prior to the FDR correction, two of the three coefficients are statistically significant at least at the 5% level.[18] The

[17] We also find similar results when we look at household education expenditures in the past 30 days.
[18] We do not implement p-value corrections on components of overall indices.

q-values on both coefficients is 0.03.

Our pre-analysis plan did not specify age cut-offs for our school-aged child sample (7-17 years at baseline). In Appendix Table A5, we show that our educational attainment and investment outcomes are robust to changing the sample cut-offs by up to two years in either direction.

B. Heterogeneity by parental literacy

We have argued that, relative to illiterate parents, literate parents will perceive higher returns to schooling. If the (discounted) returns to education dominate business returns only for literate parents then we anticipate heterogeneity in treatment effects on educational outcomes by parental literacy. Table 2 examines this possibility.

Among children with literate parents, the treatment leads to a 0.34 standard deviation increase in the schooling investment index, significant at the 1 percent level (Panel A column 1; the treatment coefficient is equal to the sum of the coefficients on the grace period indicator term and the interaction term and the corresponding p-value is listed under Panel A statistics). Further, treatment increases college attendance by 15.4 percentage points and secondary school completion by 12.1 percentage points among children of literate parents. This translates into 0.5 years of additional K-12 schooling. Both results are significant at the 5 percent level (columns 5-6).

Meanwhile, for children of illiterate parents, the treatment led to very different outcomes: All treatment coefficients on schooling investment and attainment are negative in magnitude. They are noisily estimated for the investment index and college education. Treatment children with illiterate parents are 13.9 percentage points less likely to complete secondary schooling, as compared to children of illiterate parents in the control group (result is statistically significant at the 5 percent level; column 6).[19]

Recall that roughly 1% of parents attended college. Thus, if we consider the likelihood that a child attended college while the parents did not as a measure of absolute upward educational mobility among households, we see that treatment increased this only for literate households. This suggests that relative intergenerational educational mobility likely fell among treatment households - and we examine this more fully in Section 4.2.

In Panel B, we conduct the same heterogeneity exercise with the old child sample where we anticipate that educational investment decisions were largely completed prior to the intervention. Consistent with this, we do not observe differences in treatment effects by parental literacy. Moreover, consistent with our hypothesis that parental literacy is positively correlated with perceived returns to schooling, children of literate parents (irrespective of treatment status) benefit from higher levels of educational investments and improved schooling outcomes.

We conclude by reporting FDR corrections for outcomes for our school-age child sample from our heterogeneity analysis by parental literacy. All q-values of coefficients that were significant

[19] We also see significant declines in years of K-12 schooling.

at traditional levels remain below 0.055.

Since our pre-analysis plan did not specify parental education cutoffs, we examine whether our main findings from our heterogeneity analysis hold for alternate measures of parental education. In Appendix Table A6, we assess impacts on our main child schooling and attainment outcomes using heterogeneity by parents' years of education; whether at least one parent attended at least some secondary school; and separately by mother's and father's literacy. Most alternate measures lead to results in line with our preferred specification. Our results are weaker when we focus on father's literacy, which might be driven by higher literacy rates among fathers relative to mothers (91% vs 85%). We also explore the relationship between parent and child education non-parametrically. The first graph in Figure 4 plots local polynomial regressions of college attendance on parents' highest year of education by treatment and control groups. Consistent with the assumption that (perceived or actual) returns to children's education are increasing in parental education, college attendance increases steadily with parental education.[20] The figure also provides suggestive evidence that treatment effects are increasing with parental education. Appendix Figure A3 shows similar patterns for the education investment index.

We now evaluate treatment effects on business outcomes in order to explore whether illiterate parent households instead use income gains to re-invest in their household business.

3.2 Impacts on business outcomes

A. Average impacts

Our unit of observation is now household h which belongs to microfinance group g. We estimate treatment effects separately for the 2010 and 2018 survey rounds using the specification:

$$Y_{hgt} = \alpha + \beta T_g + \theta_g + \gamma X_{hg} + \epsilon_{hgt}. \tag{2}$$

Y_{hgt} denotes business outcome in survey year t, T_g indicates whether household was in a treatment loan group, θ_g are stratification dummies for treatment group batch, and X_{hg} are baseline control variables selected via a double lasso approach (Belloni et al., 2014). We include a control for whether the interview was with a non-client household member. Standard errors are clustered by loan group.

Our primary interest is the trade-off between investments in children's schooling and the household enterprise, which are likely to be sharpest in households with at least one school-aged child (7-17 years of age) at baseline. However, for comparability of persistence results with Field et al. (2013), we also report results for the full study sample.

To evaluate business performance, we create a household business index which has three sub-

[20] In Appendix Table A7, we regress children's education on parental education and find that it is positively and significantly correlated with educational investment and attainment even after controlling for wealth.

components: weekly enterprise profits, business capital and labor. Panel A of Table 3 considers household enterprise outcomes in 2010 for the full sample. Columns 1-4 show that treatment households have 0.2 standard deviations higher score on business index and this captures gains in business profits and capital. We find similar effects when we restrict the sample to the school-age household sample (Panel C). We see no impact on the number of workers employed by the enterprise.

The trajectory of treatment effects over the subsequent eight years is consistent with overall convergence. In columns 5-8 of Panel A, we see that average impacts on profits, capital, and the business index are positive but no longer statistically significant in 2018. In Appendix Table A8, we show results from our 2012 survey round, where findings are similar: impact on profits is no longer statistically significant, though treatment enterprises have 85% more capital than control households in 2012, significant at the 5% level. Panel C shows similar patterns for the subsample of school-aged households.

B. Heterogeneity by parental literacy

How do treatment households use income gains when they do not invest them in their children's education? In Panels B and D of Table 3, we examine business outcomes separately for households with literate and illiterate parents for the full and school-aged household samples respectively. Both types of households report enterprise gains in the short run (columns 1-4). In Panel D, there is early evidence of larger gains for illiterate parent households in the school-aged household sample. By 2018, however, we observe a stark divergence in treatment impacts: for the full sample illiterate households report a 0.29 increase in business index score and literate households see no effect on business outcomes (Panel B column 5). In Panel D we observe similar and more precisely estimated differences. In the school-aged sample, this translates into illiterate households reporting a 66 percent increase in profits in 2018 (significant at the 1 percent level); a tripling of enterprise capital (significant at the 10 percent level); and, 0.65 additional workers (significant at the 5 percent level). Literate households do not see treatment impacts on any of these outcomes.[21,22]

Our findings align with the idea that the trade-off between investing additional capital in the household enterprise or in children's education is resolved based on the (perceived) returns to schooling. For children of literate parents — or, in other words, for children who are more likely to have higher (perceived) returns to schooling — the increased ability to afford educational investments outweighs the increased returns in the household business. Meanwhile, for children

[21] For 3 of the 4 alternative measures of parental education measures, the results are qualitatively similar (Appendix Table A9). However, the long-run effects for illiterate households and the divergence by parental literacy are only statistically significant when considering mother's literacy and the school-age household sample.

[22] We do not find the same divergence by parental literacy in the 2012 survey (Appendix Table A8). Instead, the treatment effects on business outcomes tend to be larger for literate households in 2012. A potential explanation is that literate households were better able to cope with the microfinance crisis that occurred at the end of 2010.

of illiterate parents, the (perceived) returns to education are lower and so treatment leads parents to instead increase capital and labor investments in the enterprise.

If capital and labor are complements in the household enterprise, then higher enterprise investments and lower schooling investments among illiterate households may also lead to a corresponding increase in the likelihood that a child works in the household business. Appendix Table A10 considers the school-aged child sample and examines whether increased dropout among treatment children of illiterate parents is associated with increases in child labor. We classify parent-reported reasons for why their child left school into three categories: family factors (includes money reasons, a good job opportunity, or feeling that school was not worthwhile); child factors (includes reporting that the child disliked school or had low test scores); and, dropout for marriage or pregnancy. We detect no differences in reason for drop-out among treatment and control group children in households with literate parents (columns 1-4). In contrast, treatment children in households with illiterate parents are more likely to report dropping out from school because of family factors (relative to their counterparts in the control group).

We construct a dummy for whether children below the age of 18 worked in the past 30 days based on the 2018 survey (column 4). We also construct a dummy for whether the child engaged in any work activities in the past 30 in the 2012 survey (column 5). Finally, we collected a detailed module of household and non-household employees in household businesses in the 2012 survey —it included a listing of all family and non-family workers in each business from inception until the day of the survey (column 6). Across all measures of child labor, we find positive (but noisy and insignificant) treatment effects for children of illiterate parents. We see no differences for treatment children of literate parents.[23]

All coefficients in Table 3 Panels C and D (along with their corresponding joint tests) that were statistically significant at traditional levels prior to the FDR corrections continue to be so after the p-value adjustments. Coefficients are statistically significant at the same significance cut-offs since q-values are only marginally higher than p-values.

3.3 Alternative channels of influence

We hypothesize that differences in investment decisions by parental literacy are driven by differences in perceived returns to schooling. However, it is also possible that actual returns to education differ by parental education, e.g. because of better social networks that can help with job search. To provide suggestive evidence on this channel, we regress monthly income on parental literacy and college completion for children who completed schooling in our control group. Conditional on college completion, parental literacy does not have a separate effect on child earnings, which suggests that differences in actual returns to education are less likely (Appendix Table A7,

[23] Appendix Table A11 shows results on dropout and child labor under alternate specifications for parental education. Results are consistent with those from our primary specification. For three of our four alternate specifications for parental education, we find a statistically significant increase in child labor among illiterate households.

column 7). An alternative explanation could be that more educated parents have lower discount rates. This could influence schooling investment decisions since returns to schooling are typically realized after a longer delay than are returns to business investments (Castillo et al., 2011; Mayer et al., 2019). We examine this channel using baseline information on client discount rates. While we find some suggestive evidence that illiterate households are more likely to be impatient (defined as having an above-median discount rate) we do not observe heterogeneous treatment effects by baseline discount rates (Appendix Table A12, Panel A).

Another possibility is that illiterate parents have higher returns to business investments. As shown in Table 3, illiterate parents have lower levels of capital in 2010 while having similar levels of profits, suggesting higher returns to business investments. But our results by parental literacy also hold when we include an interaction term with baseline wealth[24] and the treatment dummy in the regression (Appendix Table A12, Panel B).

It is worth noting that these mechanisms assume that the intervention made households wealthier and therefore enabled parents to send their children to college. A different explanation is that the treatment increased returns not only to capital and unskilled labor but also to high skilled labor in the enterprise. However, the business sectors in which our sample respondents work do not require high-skilled labor and most college graduates go on to do salaried work after graduation.

Finally, it is also possible that the intervention impacted educational investments via increasing women's bargaining power. If female clients and their spouses have non-aligned preferences, the intervention — which targeted loans to women — may have increased education expenditures by increasing the client's bargaining power within the household. However, we do not observe treatment impacts on female empowerment in the 2018 survey (not shown).[25]

4 Long-run earnings and intergenerational outcomes

We first consider treatment impacts on household and children earnings, eleven years after the intervention. Following this, we evaluate the impacts of treatment on intergenerational outcomes. Here, we focus on intergenerational educational mobility and (forecasted) earnings inequality.

4.1 Earnings

A. Household

In 2010 and 2018, we asked respondents to report total household income, inclusive of income earned by resident children. Table 4 reports the findings. Column (1) of Panel A replicates the finding in Field et al. (2013): in 2010, treated households saw a 16.6 percent increase in household

[24] Enterprise capital was not measured in the baseline survey.

[25] That we do not find impacts of the grace period on women's empowerment may reflect the fact that loans were often invested in male-operated businesses among households in our sample (Bernhardt et al., 2019).

income.[26] Column (2) shows a smaller positive, but statistically insignificant, treatment effect on 2018 earnings. In columns (3) and (4) we observe similar patterns when we restrict to households with school-aged children. FDR corrections only make a marginal difference: the q-value on 2010 income in column 3 is 0.059 and on the 2018 income in column (4) is 0.093.

Panel B column (1) shows that, for the full sample, the earnings increase in 2010 is indistinguishable across literate and illiterate households. But, by 2018, treatment effects fully diverge across the two groups: illiterate treatment households continue to report approximately 27% higher income than counterparts in the control (column 2), while the difference in incomes between treatment and control literate households has fully disappeared. Columns (3) and (4) show similar patterns for the school-going sample with the heterogeneity in treatment effects already apparent in 2010 (suggesting that differences in investment choices had begun by 2010).

Our interpretation that heterogeneity in treatment effects by parental literacy reflects differences in investment choices is consistent with children's schooling, living arrangement and labor supply outcomes in 2018. We examine these outcomes in Appendix Table A13, with the caveat that many children are still transitioning into the labor market. In 2018, 18% of the school-aged child control sample was still in school with the number significantly higher for the treatment group and no significant differences across literate and illiterate households (column 1, Panel A). In contrast, treatment effects for living arrangements differ by household literacy: in the literate household sample, treatment children are more likely to be living at home. As daughters, but not sons, typically leave home upon marriage this difference largely reflects treatment-induced delayed marriage outcomes for daughters. Finally, in columns (8)-(10) of Appendix Table A13, we examine work outcomes conditional on school completion. In Panel A we observe that treatment is, on average, associated with higher work incidence for the school-aged sample. Panel B shows that these treatment effects are concentrated among children of illiterate households. Consistent with our previous results, the effect for children from illiterate households is driven by increases in self employment (column 10). Salaried work, which is more common among more educated children, decreases for this subsample (column 9).

In sum, our income results are in line with the idea that literate parent households focused their investments on their children's education, rather than the household enterprise. Many of these children were yet to enter the labor market leading to dissipation of the treatment effects on income among literate households. Meanwhile, illiterate parent households' continued enterprise investments lead to income gains even 11 years post-intervention.

[26] Differences to estimates shown in Field et al. (2013) are caused by a different selection of controls. In the current paper, we consider a wider set of potential control variables and select them through the double-lasso approach.

B. Children

In columns (5) and (6) of Table 4, we examine treatment impacts on children's income in 2018, conditional on school completion. Since children rarely earn income while studying, and since treatment increases the likelihood that children are in school at the time of the 2018 survey, estimated treatment effects on child income are likely biased downwards. Assuming that treatment induced children with the highest earning potential to continue on to college, the estimates from the conditional regression constitute a lower bound on treatment effects on child income (Duflo et al., 2021). Panel A shows that the treatment leads to a 26% increase in son's income, but has no effects on daughters. We do not find variation by parental education; sons of both high- and illiterate parents experienced income gains, which suggests that the treatment had economic returns independent from those that accrue due to educational attainment. That said, the estimates are noisy, which reflects the fact that many children are still in school at the time of the survey. In Panel C, we observe a marginally significant increase in the income of sons in the older child sample (aged 18 years or older at baseline), which further supports this argument. Panel D shows evidence of heterogeneity by parental literacy for daughters but not sons. We interpret this as consistent with marriage market returns for daughters from (now richer) illiterate households in the treatment group. It is likely that these daughters marry better-off men and are subsequently less likely to work (potentially because their parents can afford higher dowries).

4.2 Intergenerational outcomes

Finally, we assess treatment impacts on relative educational mobility and forecasted child earnings. We calculate relative mobility based on the distributions of parents' and children's educational attainment for the school-age sample. For earnings, we forecast expected future earnings for a representative child aged 12 years at baseline based on outcome means in our sample and age-earnings curves in the Indian Human Development Survey. We report average earnings gain and then earnings inequality separately for the pooled sample and for literate and illiterate households.

A. Relative educational mobility

In Figure 5, we plot the school-age parent's level of education against the average level of education of the children in two ways: in Panel A, we plot the number of years of education of the most educated parent and the average number of years of child education for that level of parental education; in Panel B, we convert the education levels of the parents and then of the children to percentile ranks (computed separately for households in the treatment and in the control groups).[27]

Looking at Panel A, we see that absolute educational mobility on average increased at almost every level of the parent's own education - the average education level of the children is above

[27]Following Alesina et al. (2021), we calculate rank using parents' mean level of education.

the 45 degree line in nearly all cases and they tend to fall on the 45 degree line for high levels of parental education. The treatment increased absolute educational mobility for the school-age sample. The share of children who have more years of education than their most educated parent increased from 72% in the control group to 83% in the treatment group.

The treatment, however, also lowered relative educational mobility. Visually inspecting Panel A of 5 we note that to the right of 7 years of parental education (so for more educated parents), the average level of children's education in the treatment group is nearly always above the average level of children's education in the control group. To the left of 7 years of education (so less educated parents), the treatment group average is almost always below that of the control group.

To quantify the decline in relative mobility, we estimate two measures from the literature: first, following Dahl and DeLeire (2008) and Chetty et al. (2014), we estimate the correlation between child and parent education ranks.[28] Among households in the control group, we find that a 1 percentage point (pp) increase in parent education rank is associated with a 0.332 pp increase in the child's mean rank. The grace period strengthens this rank-rank relationship: among households in the treatment group, a 1 pp increase in parent education rank is associated with a 0.388 pp increase in the child's mean rank, statistically significant at the 10 percent level.

We provide a visual representation of this result in Panel B of 5. The 45 degree line in this plot represents what would happen if a child's education rank was fully determined by their parent's education. The horizontal line at 0.5 represents what we would expect if the parent's education rank had no relationship with a child' educational attainment. The difference in the slopes of the treatment and control lines, which as we report above is significant at the 10 percent level, tell us that in the parent's own education is more important in predicting child's education in the treatment group.

Next, we follow Asher et al. (2021) and calculate bottom-half mobility: the expected rank of a child born to a parent in the bottom half of the education distribution. If children's educational attainment were not tied to their parents', then the expected rank of a child — irrespective of their parents' own educational attainment — would be the 50th percentile. Among children in our control group, we find instead that children born to parents in the bottom half of the parental education distribution can expect to obtain the 36.7th percentile. This is in line with Asher et al. (2021)'s finding that bottom half mobility across India is in the range of $[37.5, 37.9]$. The grace period has a negative effect on mobility: the average child from a bottom half family in the treatment group can expect to obtain only the 32.6th percentile.

[28] Since we observe educational outcomes for children who have left the household, our estimates do not suffer from truncation bias (Emran and Shilpi, 2015), a common critique of this approach.

B. Forecasting lifetime earnings for children

As the final step in our analysis, we forecast treatment effects on child lifetime earnings. First, we estimate the impact of treatment-induced human capital gains on earnings by using differences in educational outcomes between treatment and control group children. Next, we allow treatment to affect economic returns independent of educational attainment. In both cases, we report estimated welfare effects for the full sample and separately for children of literate and illiterate parents.

Following Hendren and Sprung-Keyser (2020), we estimate the impact of increased educational attainment on lifetime income using age-earnings curves for multiple educational categories from a sample of nationally-representative adults using the 2011-2012 Indian Human Development Survey (IHDS).[29] We restrict the sample to urban residents aged 18-59 years who had completed their education at the time of the survey. Motivated by the linear relationship between individual earnings and age in the IHDS data (Appendix Figure A5), we regress annual earnings against a linear age term separately for school dropouts, secondary school graduates, and college graduates.[30]

Our welfare calculation considers a child who was 12 years old at baseline, the midpoint of our child sample age range. We assume early dropouts leave school after completing grade 9 (at age 14), the median years of schooling among school dropouts in our sample. We assume school dropouts start working at age 15 and those who complete secondary school, but not college, start working at age 18. College graduates start working at age 21. We use a social discount rate of 5% (consistent with, for instance, Bandiera et al. (2017)) and make the conservative assumption of no growth in real wages. We obtain average annual costs in 2018 for the last three years of secondary school (Rs. 9275) and college (Rs. 4241) from the control group.

Given these assumptions, we predict child earnings by weighing the IHDS age-earnings curves by the school completion rates from our experimental data (Appendix Table A14). The first three columns of Table 5 reports estimates for our first forecasting exercise, which takes into account earnings gains due treatment-induced increases in educational attainment. We first show estimates for the pooled sample (column 1) and then separately by parental literacy (columns 2 and 3). The first and second row of each column shows net present value of lifetime earnings for children in the control and treatment group, respectively. The third row reports the treatment gain in earnings. For the pooled sample, we estimate a treatment-induced increase in the net present value of lifetime earnings of $2,344$ USD PPP. Because treatment children of illiterate parents are more likely to drop out of school early, we find that they experience a net loss in lifetime earnings of $2,131$ USD PPP. Conversely, earning gains associated with college education mean that treatment children of literate parents experience a large increase in total lifetime earnings of

[29] We rely on the IHDS rather than using data from our control group because our control group sample is small and does not span all age groups. Appendix Figure A6 shows the age-earnings IHDS curves are comparable to age-earnings curves in our data.

[30] Panel A of Appendix Table A14 shows the corresponding estimates.

4,392 USD PPP.[31]

In columns (4)-(6), we report estimates of lifetime earnings from our second forecasting exercise, where we allow for gains to children's earnings aside from those that accrue due to improved educational attainment. We estimate economic returns by dividing the raw mean of treatment children's earnings in 2018 by the raw mean of control children's earnings (Appendix Table A14 Panel D) and do so separately for children at three levels of educational attainment ((i) dropout, (ii) secondary school graduate, and (iii) college graduate). We assume constant relative economic returns of the treatment over a child's lifetime. Our results are consistent with the idea that less educated children, defined as children without a college degree, benefit economically from the treatment by inheriting larger household businesses. Following this approach, average treatment gains to the net present value of lifetime earnings increase to 4,905 USD PPP for the pooled sample. As shown in column 5, taking into account treatment income gains that accrue via inherited assets leads to an estimated net gain in lifetime earnings for children of illiterate parents. Treatment gains for children of literate parents also remain positive (column 6). Appendix Figure A7 shows the resulting income trajectories for each group.

To summarize, the treatment increased average lifetime earnings for children in our sample. Yet, because earnings gains are more than four times larger for children of literate parents treatment also increased earnings *inequality*. Among children in the control group, children of literate parents earn 14% more than children of illiterate parents (row 1, columns 5 and 6 of Table 5). For children in treatment households, this earnings differential increases to 34% (row 2, columns 5 and 6). Thus, treatment raises earnings for the next generation but lowers relative intergenerational educational mobility and earnings equality.[32]

5 Conclusion

Our results demonstrate how a positive shock to household liquidity can raise incomes and have enduring effects on the next generation through increased human capital investment in children. To estimate intergenerational treatment effects we required data collection on all children ever born—not just those living at home at the time of our final follow-up survey. Our findings reinforce the importance of long-term follow-up surveys and of estimating intervention impacts using a broad definition of the household. Based on our findings, Table 5 shows that the grace period contract, when only considering child-level gains, has an internal rate of return of 28.3% and benefit-cost ratio of 387. By comparison, Hamory et al. (2021) estimate that deworming has an internal rate of return of 36.7% and Parker and Vogl (2021) find that the lower bound of the

[31] Selection could lead us to overestimate the returns to educational attainment as well as economic returns to the treatment. However, if wealth constraints limit educational investment, then marginal students may not be of lower ability than students who attend college independent of treatment.

[32] Appendix Table A15 replicates the welfare analysis for a social discount rate of 10%. In that case, we still observe positive earnings gains for literate and illiterate parents but the earnings gains for literate parents are only 70% higher than the earnings gains for illiterate households.

benefit-cost ratio of Progresa is 1.5.

The economic forces we highlight in this paper — namely that credit constraints and lumpy education investments can produce an intergenerational poverty trap — may be an important constraint to intergenerational mobility in India overall. We exploit the IHDS panel structure to examine the relationship between household income in 2005 and children's college attendance in 2012. In Figure 6, we plot a kernel-weighted local polynomial regression of whether a child had attended college in 2012 on household income in 2005.[33] We also plot median treatment and control incomes in 2010 for our sample households. The relationship between initial income and eventual college attendance exhibits an s-shape, a canonical representation of poverty traps: the level of college attendance rises slowly, then rises rapidly at a certain level of income, and then levels off again. Not only do our sample households fall on the steep part of the curve, we additionally also observe an s-shaped relationship between baseline wealth and college attendance in our own sample (Figure 4).

Our results demonstrate how average educational gains in a period of economic growth can be accompanied by declines in relative intergenerational educational and earning mobility. Recall, that the average child from a bottom half family in the treatment group can expect to obtain only the 32.6th percentile while her counterpart in the control group places at the 36.7th percentile. These impacts on relative outcomes in the long-run are important for assessing programs, in so far as we care about inequality effects of interventions, and not exclusively on average treatment effects (Deaton and Cartwright, 2018). They also point to the limits of just relying on income growth to sustainably improve economic well-being among poor households. Rather, preventing the emergence of greater inequality in periods of economic growth likely requires additional interventions, including conditional transfers targeted towards children of low-education parents.

[33] To parallel our own analysis sample, we limit attention to households that in 2005 had 7-17 year olds. We focus on boys because, in India, girls move to their husband's home after marriage and would therefore be disproportionately absent from the IHDS household roster in 2012.

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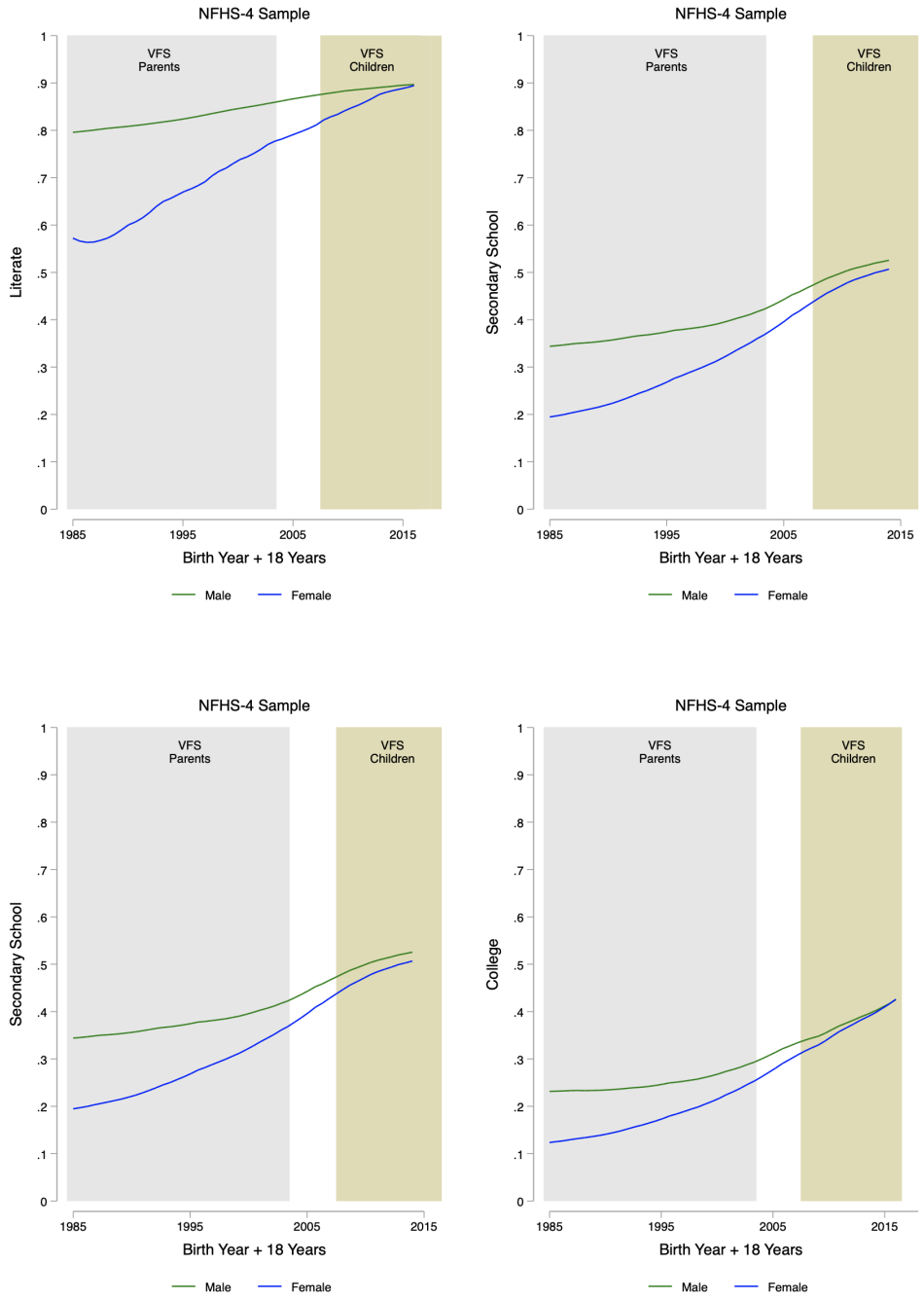
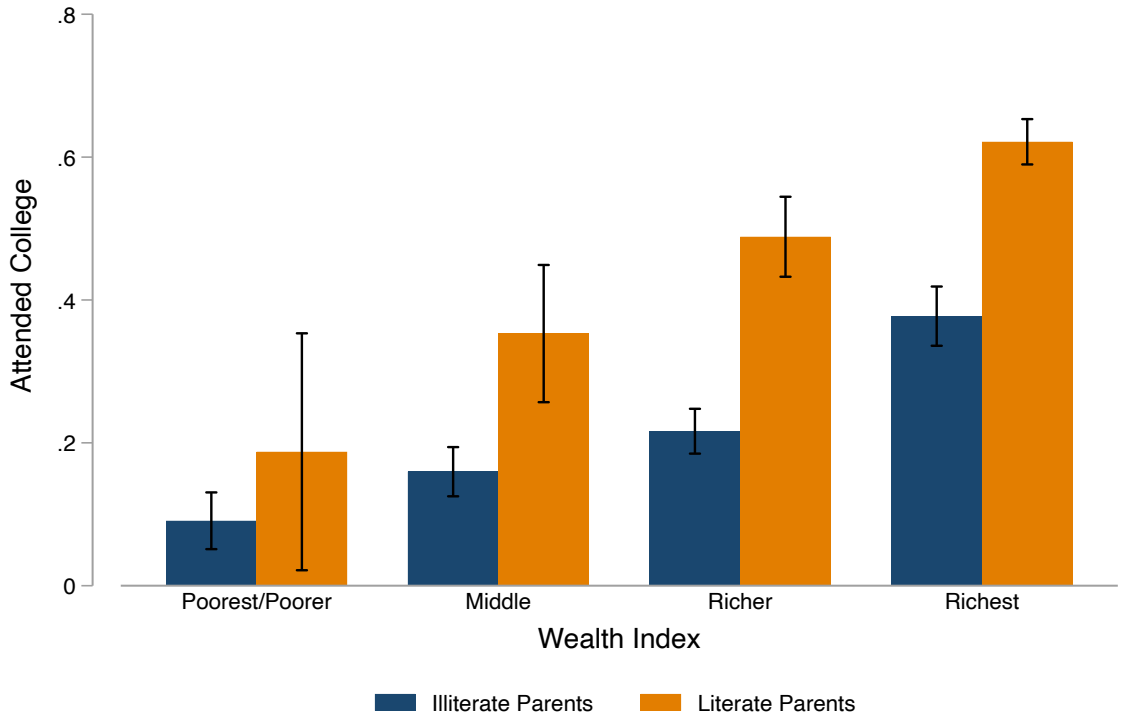
Figure 1: Educational Trends in India Based on the National Family Health Survey-4



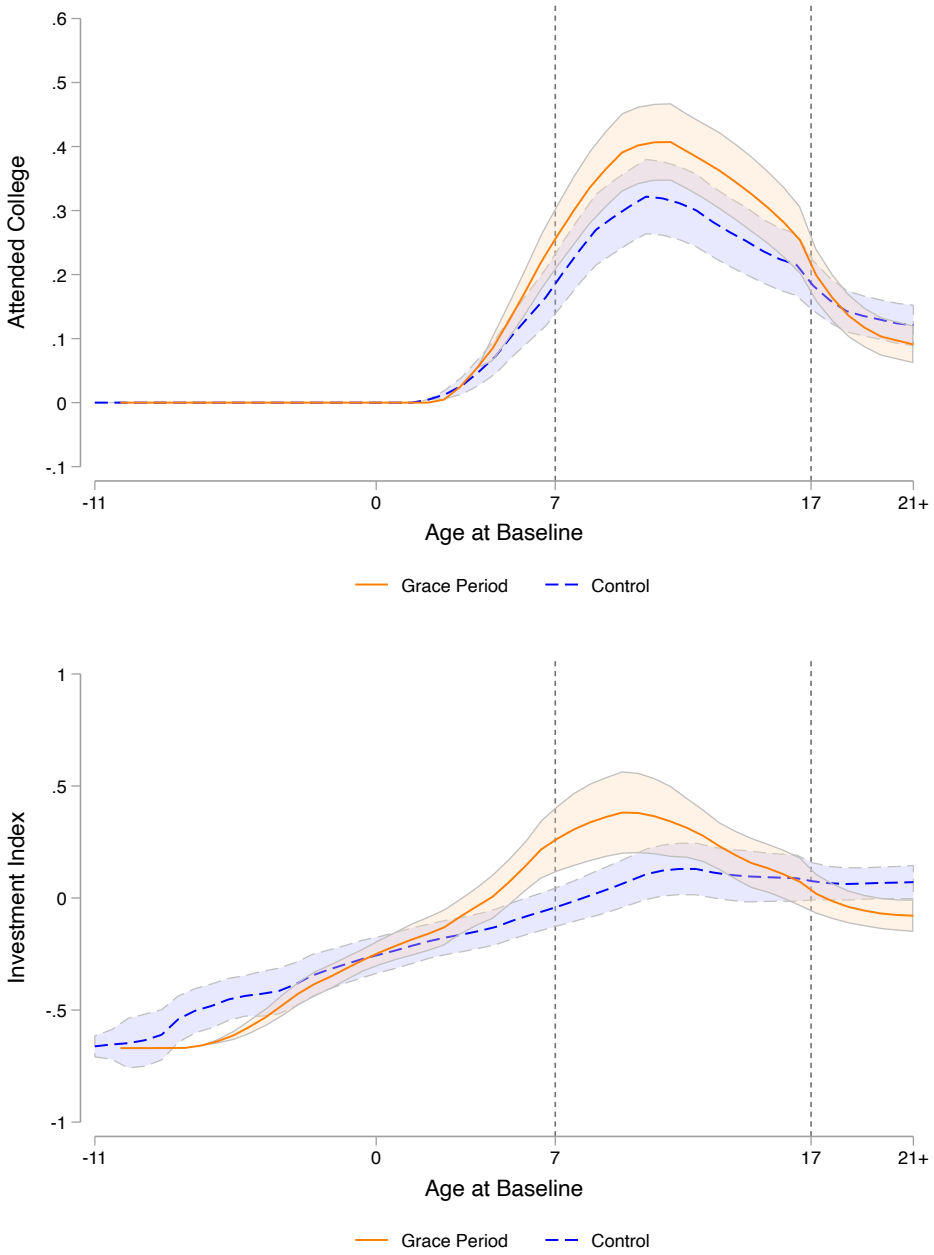
Notes: The figures plot local regressions. Data consists of all household members aged 18-80 urban areas in National Family Health Survey-4. The x-axis shows the year in which the person turned 18 years. The green lines correspond to men and the blue lines correspond to women The brown-shared area shows the age range of the VFS school-age child sample (aged 7-17 years at baseline) and the grey-shaded area shows the age range of their parents.

Figure 2: Relationship between College Attendance and Household Wealth by Parental Literacy in the National Family Health Survey-4



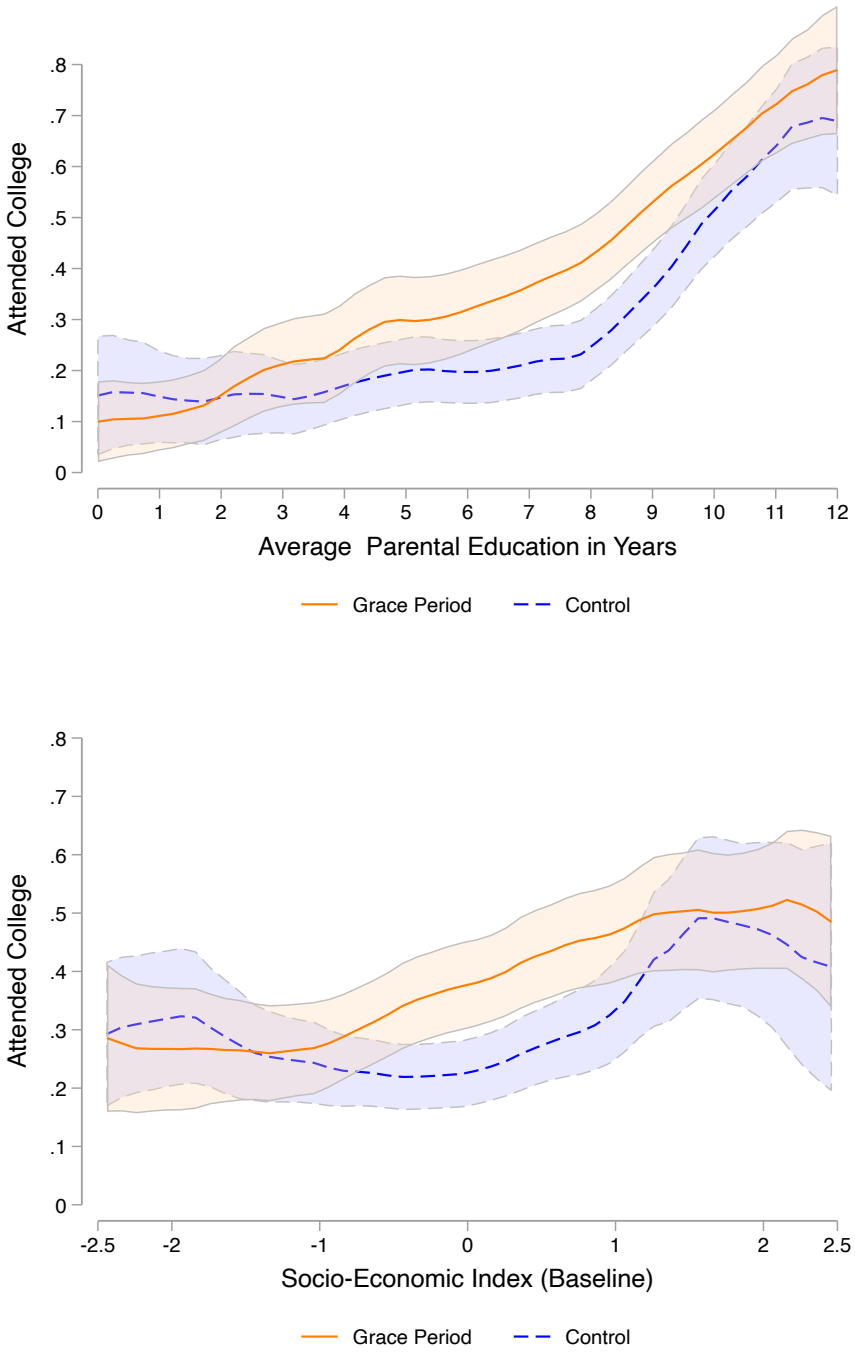
Notes: The figure plots average college attendance rates by wealth quintiles and parental literacy. We combine the lowest two wealth quintiles due to a low number of observations for children of literate parents in these groups. The range plots correspond to 90 percent confidence intervals. The data comes from the National Family Health Survey-4. We restrict the sample to men aged 19-24 years who live in urban areas.

Figure 3: College Attendance and Investment Index by Age at Baseline and Treatment Group



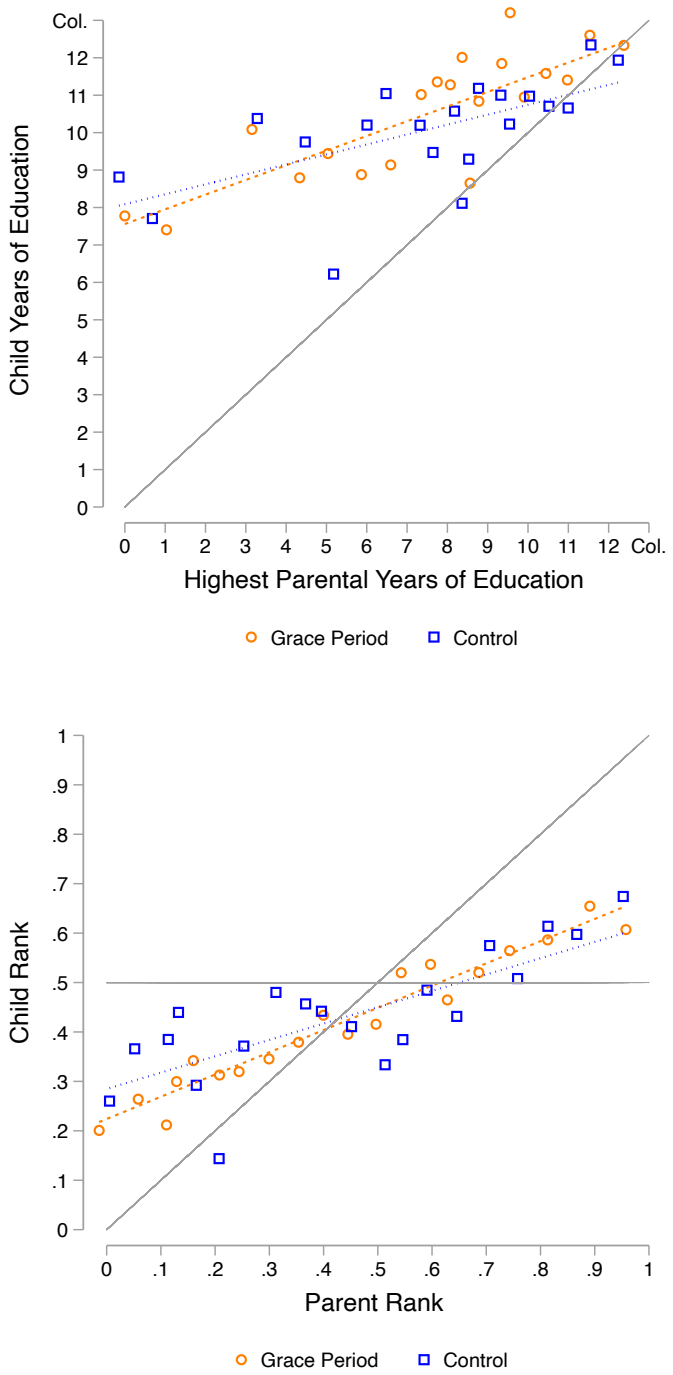
Notes: The figures plots local regressions. The sample consists of all children of the client that were still alive at the time of the 2018 survey. The x-axis shows age at baseline. Negative numbers indicate the number of years that the child was born after baseline. The dotted vertical lines indicate the school-age child sample. The orange lines correspond to the treatment group and the blue lines correspond to the control group. The shaded areas in the figure correspond to 90 percent confidence intervals that are not adjusted for clustering. See Data Appendix for detailed variable definitions.

Figure 4: College Attendance by Parental Education and Baseline Wealth



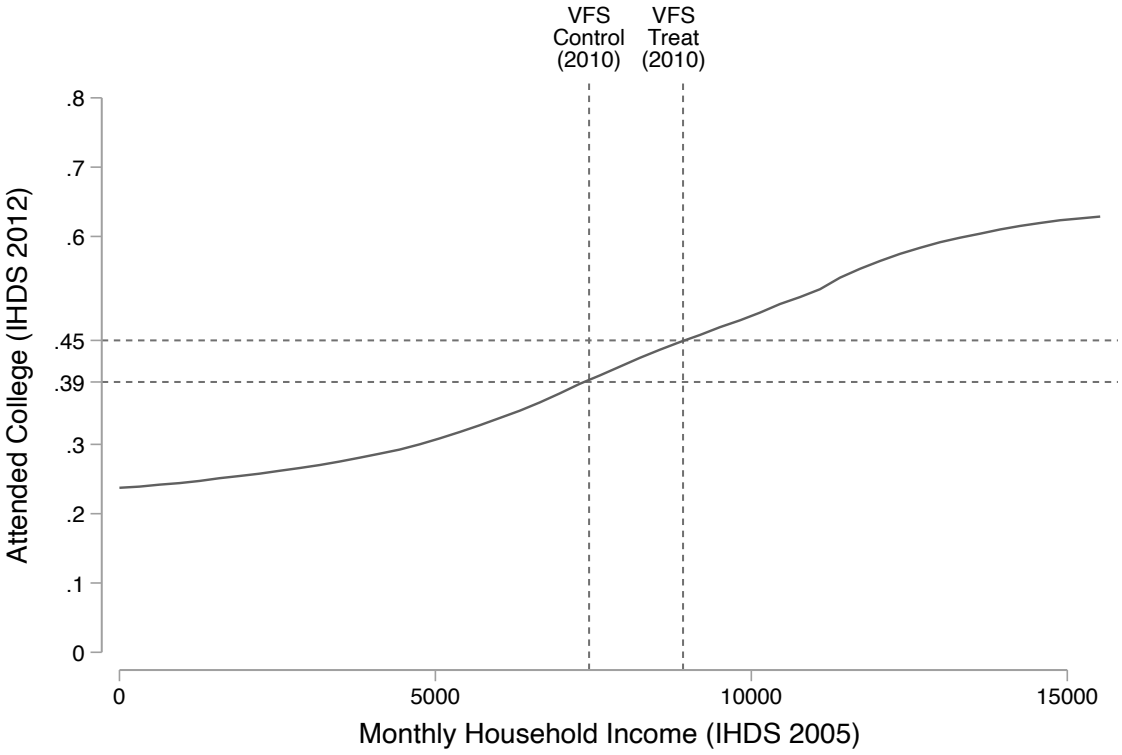
Notes: The figures plots local regressions. The sample consists of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey. The orange lines correspond to the treatment group and the blue lines correspond to the control group. The shaded areas in the figure correspond to 90 percent confidence intervals that are not adjusted for clustering. Average parental education in years is top-coded at 12 years and the socio-economic index is top-coded at 95%. See Data Appendix for detailed variable definitions.

Figure 5: Absolute and Relative Educational Mobility



Notes: Notes: The figures plots binned scatter plots for treatment and control groups after controlling for stratification dummies. In the upper figure, the 45-degree line corresponds to the situation in which children get the same level of educational attainment as their most educated parent. In the lower figure, the 45-degree line corresponds to complete immobility and the horizontal line corresponds to perfect mobility.

Figure 6: Relationship Between College Attendance and Household Income in IHDS



Notes: The figure plots local regressions. Data comes from the Indian Human Development Survey. We restrict the sample to men who are part of the household rosters in both survey waves, live in urban areas, and are 7-17 years old in the first survey wave. The x-axis shows monthly household income in the first survey wave. Income is deflated to 2007 prices using CPI data published by the World Bank. The y-axis shows college attendance in the second survey wave. The vertical lines show median household income in the 2010 survey in the VFS sample for the treatment and control group.

Table 1: Treatment Effects on Educational Outcomes (as of 2018)

		Investment Index Components				
	Investment Index	Primary School Investment Subindex	Secondary School Investment Subindex	College Spending (Standard-ized)	Attended College	Completed Secondary School
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: School-Age Child Sample (7-17 Years at Baseline), Pooled						
Grace Period	0.200***	0.076	0.252***	0.166*	0.096**	0.044
	(0.072)	(0.073)	(0.079)	(0.089)	(0.038)	(0.041)
Panel B: School-Age Child Sample (7-17 Years at Baseline), Heterogeneity by Child Gender						
Grace Period	0.237**	0.135	0.283**	0.125	0.096*	0.044
	(0.109)	(0.108)	(0.118)	(0.142)	(0.050)	(0.056)
Grace Period × Female	-0.082	-0.126	-0.070	0.084	-0.001	-0.004
	(0.143)	(0.130)	(0.155)	(0.193)	(0.072)	(0.077)
Female	-0.009	0.035	-0.020	-0.049	0.044	0.030
	(0.081)	(0.083)	(0.083)	(0.117)	(0.053)	(0.056)
Panel C: Old Child Sample (18+ Years at Baseline)						
Grace Period	-0.084	-0.110	-0.048	0.010	0.014	0.020
	(0.065)	(0.071)	(0.067)	(0.074)	(0.024)	(0.033)
Panel D: Young Child Sample (Under 7 Years at Baseline)						
Grace Period		0.063				
		(0.084)				
Panel A Statistics						
Mean of Omitted Group	-0.000	-0.000	0.000	0.000	0.272	0.425
Observations	543	543	543	531	541	543
Panel B Statistics						
p-value: Grace Period + Grace Period x Female	0.093	0.923	0.039	0.081	0.092	0.490
Mean of Omitted Group	0.026	-0.015	0.039	0.051	0.267	0.430
Observations	543	543	543	531	541	543
Panel C Statistics						
Mean of Omitted Group	0.000	-0.000	-0.000	-0.000	0.126	0.201
Observations	492	492	492	482	492	492
Panel D Statistics						
Mean of Omitted Group		-0.000				
Observations		341				

Notes: Standard errors are clustered by loan group and appear in parentheses. All regressions are run on the child level and include stratification dummies, a dummy for whether the client was dead at the point the 2018 survey, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The sample in Panels A and B consist of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey. Children that are under age 7 at baseline are excluded from these panels because they have not reached age 18 at the point of the 2018 survey. The sample in Panel C consists of children of the client aged 18+ at baseline that are still alive at the time of the 2018 survey. The sample in Panel D consists of children of the client aged 6 years or younger at baseline and that are still alive at the time of the 2018 survey, including children born after baseline if they are at least 5 years old at the point of the 2018 survey. All outcomes are obtained from the 2018 survey. See Data Appendix for detailed variable definitions and Appendix Table A4 for treatment effects on index components. The primary and secondary school investment subindices consist of a dummy for whether the child went to private school, total school fees, and total spending on after-school tutoring. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table 2: Heterogeneous Treatment Effects on Educational Outcomes by Parental Literacy

		Investment Index Components				
	Investment Index	Primary School Investment Subindex	Secondary School Investment Subindex	College Spending (Standard-ized)	Attended College	Completed Secondary School
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: School-Age Child Sample (7-17 Years at Baseline)						
Grace Period	-0.079	0.038	0.062	-0.231*	-0.050	-0.139**
	(0.082)	(0.104)	(0.082)	(0.126)	(0.062)	(0.064)
Grace Period × Literate Parents	0.418***	0.083	0.318***	0.549***	0.204***	0.260***
	(0.121)	(0.126)	(0.123)	(0.187)	(0.073)	(0.083)
Literate Parents	0.207**	0.262***	0.131*	0.051	0.071	0.027
	(0.085)	(0.089)	(0.077)	(0.134)	(0.055)	(0.063)
Panel B: Old Child Sample (18+ Years at Baseline)						
Grace Period	-0.044	0.041	-0.056	-0.107	-0.028	-0.010
	(0.084)	(0.096)	(0.077)	(0.082)	(0.027)	(0.042)
Grace Period × Literate Parents	0.011	-0.168	0.026	0.156	0.067	0.042
	(0.126)	(0.131)	(0.121)	(0.150)	(0.052)	(0.072)
Literate Parents	0.254***	0.232***	0.304***	0.074	0.063*	0.083*
	(0.078)	(0.089)	(0.069)	(0.103)	(0.032)	(0.044)
Panel A Statistics						
p-value: Grace Period + Grace Period x Literate Parents	0.001	0.171	0.000	0.014	0.002	0.020
Mean of Omitted Group	-0.236	-0.248	-0.218	-0.089	0.169	0.339
Observations	543	543	543	531	541	543
Panel B Statistics						
p-value: Grace Period + Grace Period x Literate Parents	0.741	0.196	0.772	0.696	0.370	0.574
Mean of Omitted Group	-0.265	-0.167	-0.274	-0.153	0.039	0.066
Observations	492	492	492	482	492	492

Notes: Standard errors are clustered by loan group and appear in parentheses. All regressions are run on the child level and include stratification, a dummy for whether the client was dead at the point the 2018 survey, a dummy for missing information on parental literacy, an interaction between the dummy for missing information on parental literacy and the grace period variable, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The sample in Panel A consists of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey. Children that are under age 7 at baseline are excluded from this panel because they have not reached age 18 at the point of the 2018 survey. The sample in Panel B consists of children of the client aged 18+ at baseline that are still alive at the time of the 2018 survey. All outcomes are obtained from the 2018 survey. See Data Appendix for detailed variable definitions and Appendix Table A4 for treatment effects on index components. The primary and secondary school investment subindices consist of a dummy for whether the child went to private school, total school fees, and total spending on after-school tutoring. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table 3: Treatment Effects on Household Enterprise Outcomes

	2010 Survey				2018 Survey			
		Index Components				Index Components		
	Business Index (1)	Profits (2)	Capital (3)	Number of Workers (4)	Business Index (5)	Profits (6)	Capital (7)	Number of Workers (8)
Panel A: Full Household Sample, Pooled								
Grace Period	0.199***	479.428***	17478.234***	0.135	0.037	41.882	3533.238	0.020
	(0.067)	(160.114)	(6543.638)	(0.213)	(0.055)	(77.713)	(5149.701)	(0.118)
Panel B: Full Household Sample, Heterogeneity by Parental Literacy								
Grace Period	0.160	372.921	12052.205	0.214	0.287*	298.048*	18955.515*	0.428
	(0.152)	(388.675)	(10722.025)	(0.382)	(0.147)	(163.927)	(10655.527)	(0.326)
Grace Period × Literate Parents	0.051	156.372	7371.679	-0.138	-0.294*	-323.175*	-16120.446	-0.497
	(0.169)	(429.293)	(12693.768)	(0.459)	(0.160)	(178.590)	(12504.758)	(0.339)
Literate Parents	0.031	-127.906	8557.341	0.117	0.102	77.931	7654.549	0.178
	(0.084)	(225.081)	(6330.302)	(0.313)	(0.095)	(119.983)	(6830.010)	(0.216)
Panel C: School-Age Household Sample, Pooled								
Grace Period	0.255**	711.322***	16053.795*	-0.047	0.078	90.519	11781.061	-0.033
	(0.116)	(262.150)	(9429.591)	(0.270)	(0.082)	(100.201)	(10163.902)	(0.176)
Panel D: School-Age Household Sample, Heterogeneity by Parental Literacy								
Grace Period	0.453*	973.377*	19660.806	0.785*	0.427***	469.105***	33788.233*	0.648**
	(0.246)	(581.846)	(15684.588)	(0.439)	(0.159)	(169.205)	(17638.797)	(0.321)
Grace Period × Literate Parents	-0.261	-334.746	-1835.394	-1.166**	-0.421**	-465.905**	-23183.411	-0.847**
	(0.270)	(626.828)	(18080.467)	(0.531)	(0.178)	(203.510)	(20597.663)	(0.331)
Literate Parents	0.220**	92.202	17466.405*	0.712**	0.244***	235.033*	10509.398	0.579***
	(0.101)	(210.056)	(8955.837)	(0.308)	(0.089)	(130.422)	(11688.445)	(0.144)
Panel A Statistics								
Mean of Omitted Group	0.000	1173.808	26412.013	1.214	-0.000	846.281	18698.563	0.516
Observations	769	752	766	762	708	681	682	708
Panel B Statistics								
p-value: Grace Period + Grace Period x Literate Parents	0.004	0.004	0.008	0.765	0.900	0.772	0.633	0.564
Mean of Omitted Group	-0.031	1242.187	20098.226	1.102	-0.082	791.462	10734.213	0.389
Observations	769	752	766	762	708	681	682	708
Panel C Statistics								
Mean of Omitted Group	0.000	1204.298	28747.838	1.153	-0.000	873.151	21607.202	0.544
Observations	363	355	361	361	358	341	345	358
Panel D Statistics								
p-value: Grace Period + Grace Period x Literate Parents	0.123	0.023	0.100	0.213	0.953	0.979	0.365	0.267
Mean of Omitted Group	-0.198	1067.920	13468.403	0.581	-0.192	709.110	10647.021	0.091
Observations	363	355	361	361	358	341	345	358

Notes: Standard errors are clustered by loan group and appear in parentheses. All regressions include survey wave dummies, stratification dummies, a dummy for whether the client was dead at the point the 2018 survey, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The regressions in Panels B and D also include a dummy for missing information on parental literacy and an interaction between the dummy for missing information on parental literacy and the grace period variable. The sample in Panels A and B consists of all households. The sample in Panels C and D consists of all households with at least one child aged 7-17 years at baseline according to the 2018 survey. Profits, capital, and the number of workers are top-coded at 99.5% for each survey round. Profits and capital are deflated to 2007 prices using CPI data published by the World Bank. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table 4: Treatment Effects on Household and Child Income

	Full Household Sample		School-Age Household Sample		School-Age Child Sample (Conditional on School Completion)	
	Log Household Income		Log Household Income		Child Income in 2018	
	2010	2018	2010	2018	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Pooled						
Grace Period	0.166**	0.070	0.218**	0.096	751.961**	-112.508
	(0.072)	(0.048)	(0.109)	(0.065)	(374.208)	(229.005)
Panel B: Heterogeneity by Parental Literacy						
Grace Period	0.124	0.273**	0.354*	0.255*	1069.426**	-390.938
	(0.161)	(0.118)	(0.213)	(0.140)	(510.285)	(412.177)
Grace Period × Literate Parents	0.035	-0.240*	-0.240	-0.185	-328.026	436.446
	(0.176)	(0.127)	(0.229)	(0.152)	(650.311)	(424.805)
Literate Parents	0.024	0.132	0.208	0.221**	672.502	14.288
	(0.119)	(0.101)	(0.167)	(0.112)	(464.068)	(351.644)
Panel C: Old Child Sample (18+ Years at Baseline), Pooled						
Grace Period					781.785*	68.283
					(444.499)	(175.108)
Panel D: Old Child Sample (18+ Years at Baseline), Heterogeneity by Parental Literacy						
Grace Period					1,145.820*	-509.395*
					(623.225)	(266.952)
Grace Period × Literate Parents					-84.640	852.724*
					(884.152)	(439.001)
Literate Parents					795.981	-485.288*
					(608.551)	(283.617)
Panel A Statistics						
Mean of Omitted Group	9.016	8.668	9.047	8.724	2864.626	583.429
Observations	749	738	351	378	193	206
Panel B Statistics						
p-value: Grace Period + Grace Period x Literate Parents	0.046	0.531	0.309	0.341	0.098	0.875
Mean of Omitted Group	8.923	8.478	8.851	8.532	2302.017	517.383
Observations	749	738	351	378	193	206
Panel C Statistics						
Mean of Omitted Group					4003.444	344.342
Observations					190	188
Panel D Statistics						
p-value: Grace Period + Grace Period x Literate Parents					0.094	0.247
Mean of Omitted Group					3580.658	656.994
Observations					190	188

Notes: Standard errors are clustered by loan group and appear in parentheses. The regressions in columns 1-4 are run on the household level and the regressions in columns 5-6 are run on the child level. All regressions include stratification dummies, a dummy for whether the client was dead at the point the 2018 survey, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The regressions in Panels B and D also include a dummy for missing information on parental literacy and an interaction between the dummy for missing information on parental literacy and the grace period variable. The household sample in columns 1-2 consists of all households and the household sample in columns 3-4 consists of all households with at least one child aged 7-17 years at baseline according to the 2018 survey. The child sample in Panels A and B (columns 5-6) consists of children of the client aged 7-17 at baseline that are still alive and have completed schooling at the time of the 2018 survey. Children that are under age 7 at baseline are excluded from these panels because they have not reached age 18 at the point of the 2018 survey. The child sample in Panels C and D (columns 3-4) consists of children of the client aged 18+ at baseline that are still alive and have completed schooling at the time of the 2018 survey. The sample in column 1 is restricted male children and the sample in column 2 is restricted to female children. Child income and log household income are top-coded at 99.5% and deflated to 2007 prices using CPI data published by the World Bank. See Data Appendix for detailed variable definitions. Column 5 includes income from children that live outside of the household at the point of the 2018 survey. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

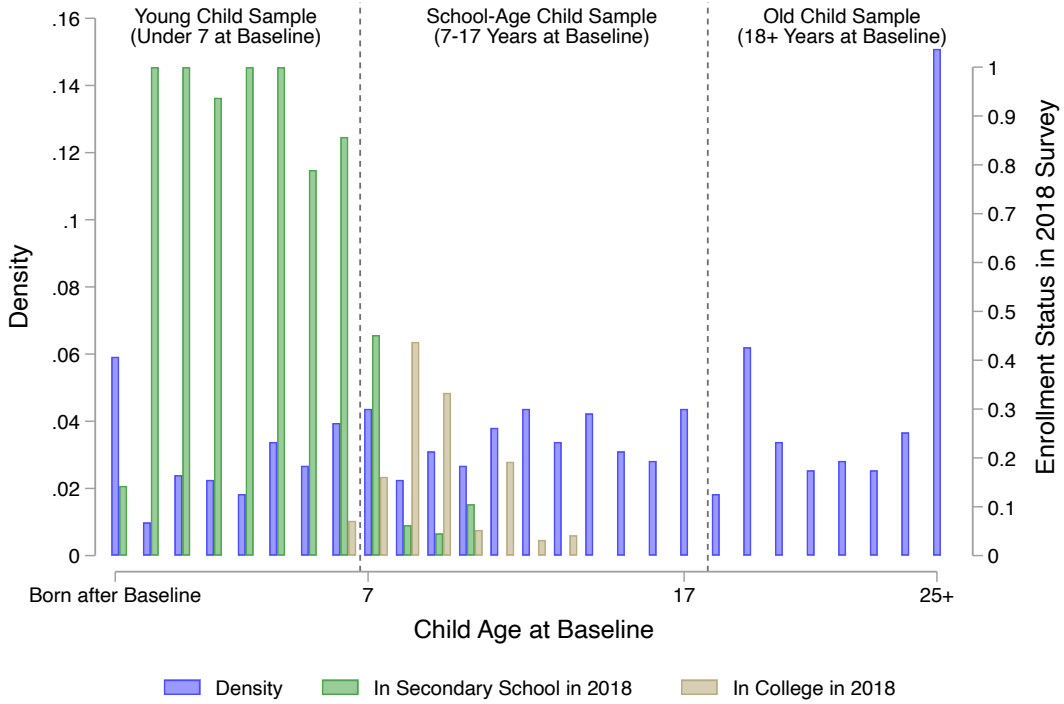
Table 5: Welfare Analysis

	Case 1: Educational Returns Only			Case 2: Educational & Economic Returns		
	Pooled	Illiterate Parents	Literate Parents	Pooled	Illiterate Parents	Literate Parents
	(1)	(2)	(3)	(4)	(5)	(6)
A: Net-Present Value of Private Lifetime Earnings (Control) in INR	329706.3	297649.1	339857.4	329706.3	297649.1	339857.4
(in USD PPP)	(28074.4)	(25344.8)	(28938.8)	(28074.4)	(25344.8)	(28938.8)
B: Net-Present Value of Private Lifetime Earnings (Treatment) in INR	357233.6	272622.4	391433.3	387310.0	311931.7	417876.3
(in USD PPP)	(30418.4)	(23213.8)	(33330.5)	(32979.4)	(26560.9)	(35582.1)
C: Treatment Gains (B-A)	27527.3	-25026.8	51575.8	57603.7	14282.6	78018.9
(in USD PPP)	(2343.9)	(-2131.0)	(4391.7)	(4904.9)	(1216.2)	(6643.3)
D: Cost of Treatment	149	149	149	149	149	149
(in USD PPP)	(12.7)	(12.7)	(12.7)	(12.7)	(12.7)	(12.7)
E: Benefit-Cost Ratio (C/D)	184.7		346.1	386.6	95.9	523.6
F: Internal Rate of Return (in %)	17.8		15.6	28.3	102.7	19.8

Notes: The table shows the results of two welfare calculations on the child level. In the first case (columns 1-3), we only account for income gains through differences in educational attainment. In the second case (columns 1-3), we also allow the treatment to affect child income separately from educational attainment. Columns 2 and 5 show the results for children of literate parents and columns 3 and 6 show the results for children of literate parents. See Section 4 for a detailed discussion and Appendix Table A14 for inputs to welfare analysis. The net present value calculation assumes a social discount rate of 5%. Appendix Table A15 shows the results for a social discount rate of 10%. Incomes are deflated to 2007 prices using CPI data published by the World Bank and converted to 2007 USD PPP based on conversation tables published by the OECD.

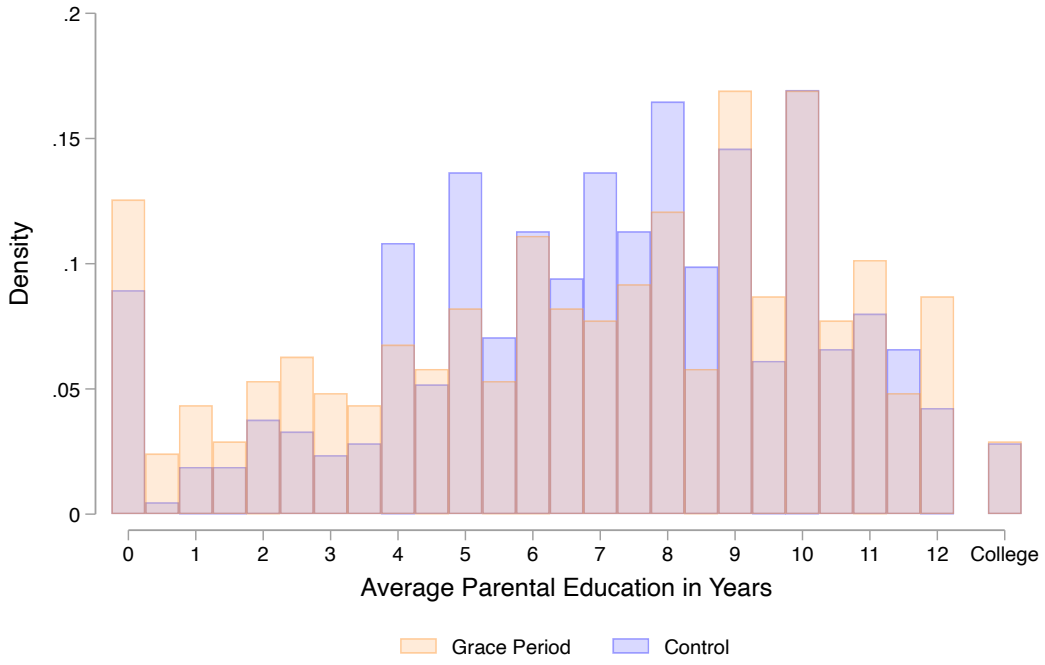
A. Appendix Tables and Figures: Additional Analysis

Figure A1: Child Age Distribution and Enrollment Status by Child Age in the Control Group



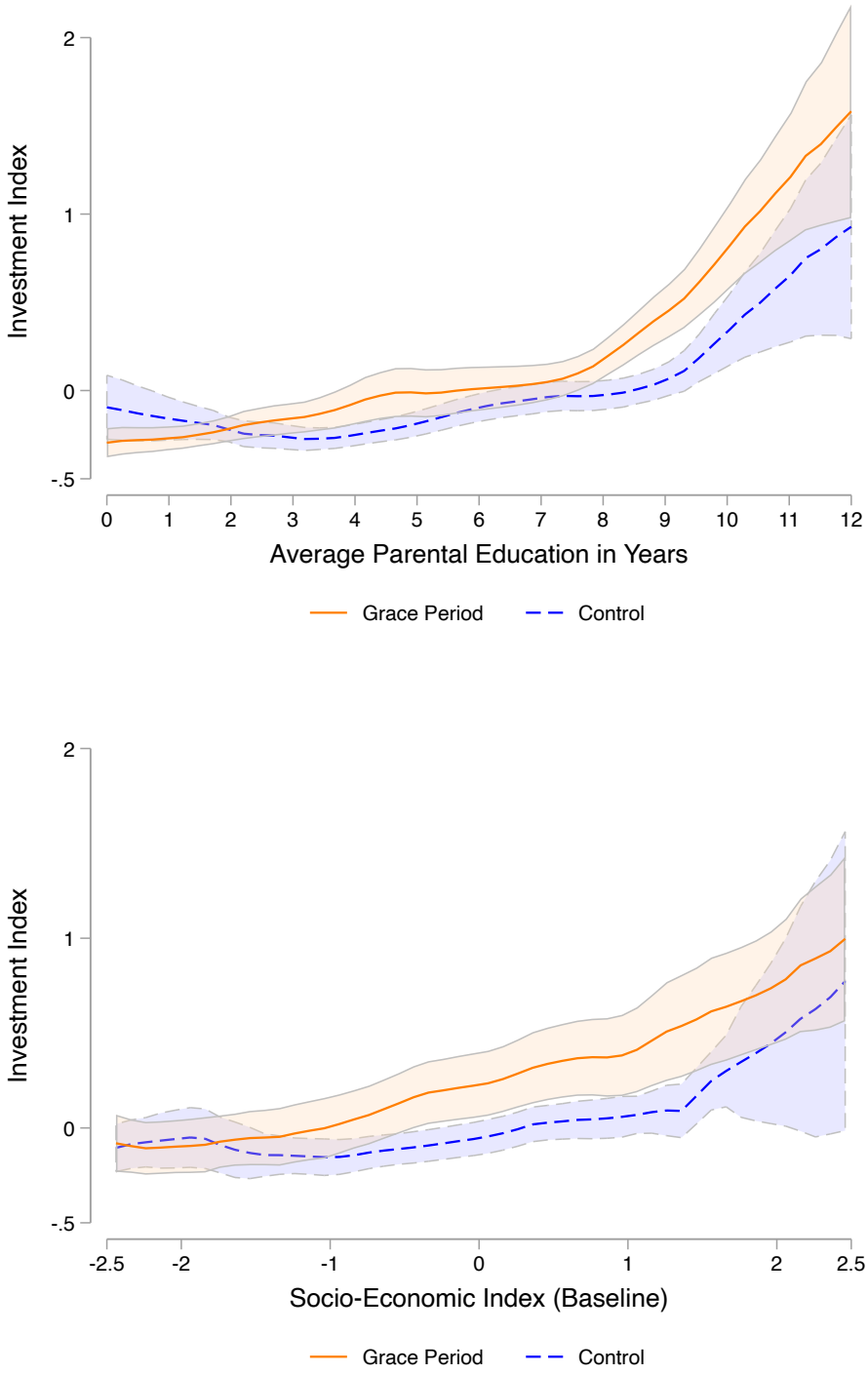
Notes: The sample is restricted to the control group. The blue bars show the distribution of child age at baseline based on the full child roster in the 2018 survey. The green bars shows the share of children enrolled in secondary school at the point of the 2018 survey. The brown bars shows the share of children enrolled in college at the point of the 2018 survey. The blue bars correspond to the left y-axis and the green and brown bars correspond to the right y-axis.

Figure A2: Histogram of Parental Education



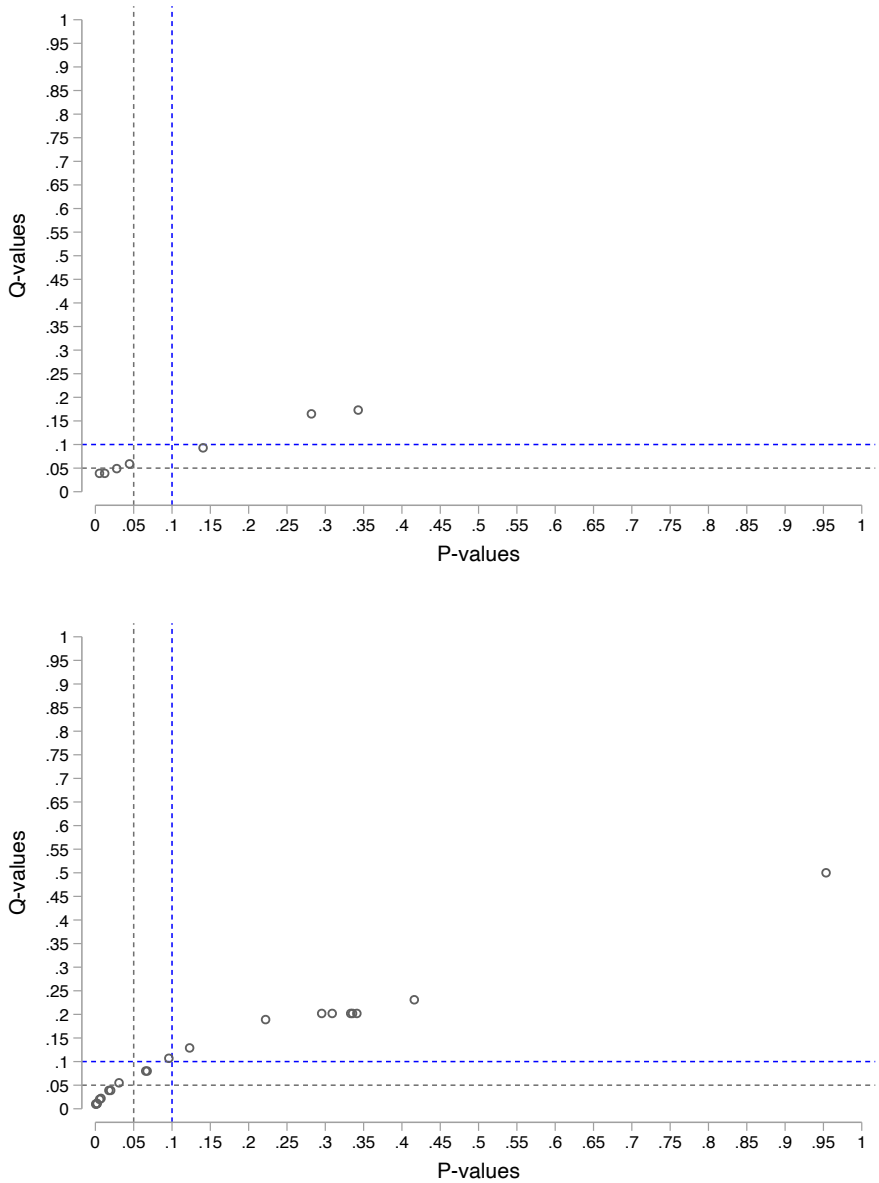
Notes: The histogram is based on the full household sample. Households with at least one parent that attended college are included in the last bar.

Figure A3: Investment Index by Parental Education and Baseline Wealth



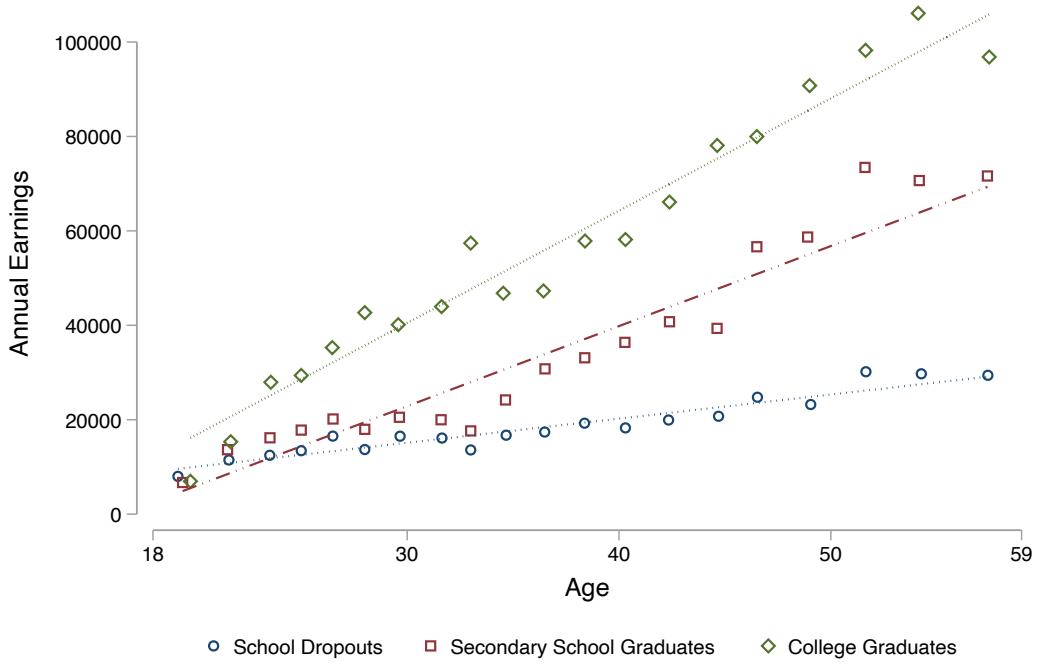
Notes: The figures plots local regressions. The sample consists of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey. The shaded areas in the figure correspond to 90 percent confidence intervals that are not adjusted for clustering. Average parental education in years is top-coded at 12 years and the socio-economic index is top-coded at 95%. See Data Appendix for detailed variable definitions.

Figure A4: Corrections for Multiple Hypothesis Testing



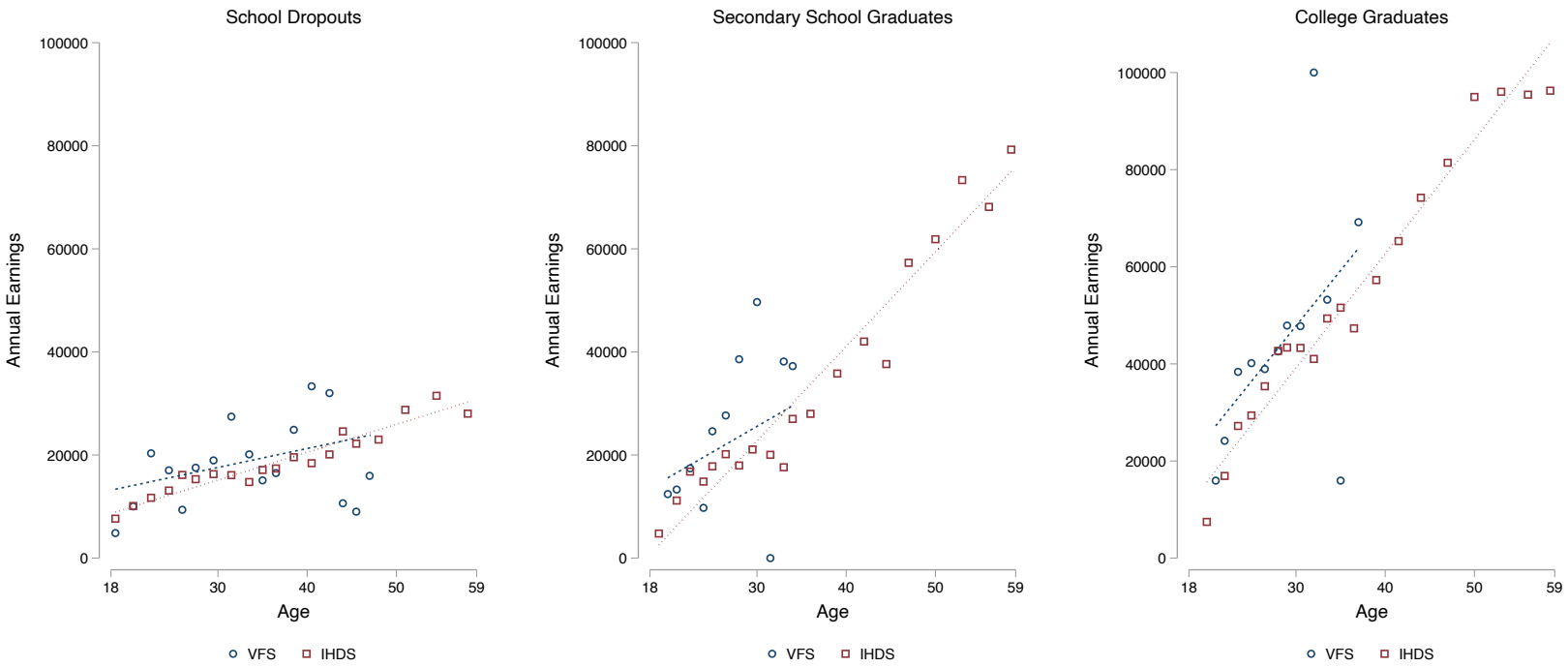
Notes: The figures plots sharpened q-values against unadjusted p-values. The upper figure shows the corrections for the first outcome family and the lower figure shows the corrections for the second outcome family. The first family is comprised of 7 tests and includes household-level economic outcomes and child-level education and socio-economic outcomes for the pooled school-age sample (Panel A of Tables 1, 3 and 4). The second family is comprised of 21 tests and includes the same set of outcomes but from our heterogeneity analysis by parental education for the school-aged sample (Panel A of Table 2 and Panel B of Tables 3 and 4). Sharpened q-values are calculated by on the approach developed by Benjamini et al. (2006) and described in Anderson (2008).

Figure A5: Age-Earning Curves in the Indian Human Development Survey-2



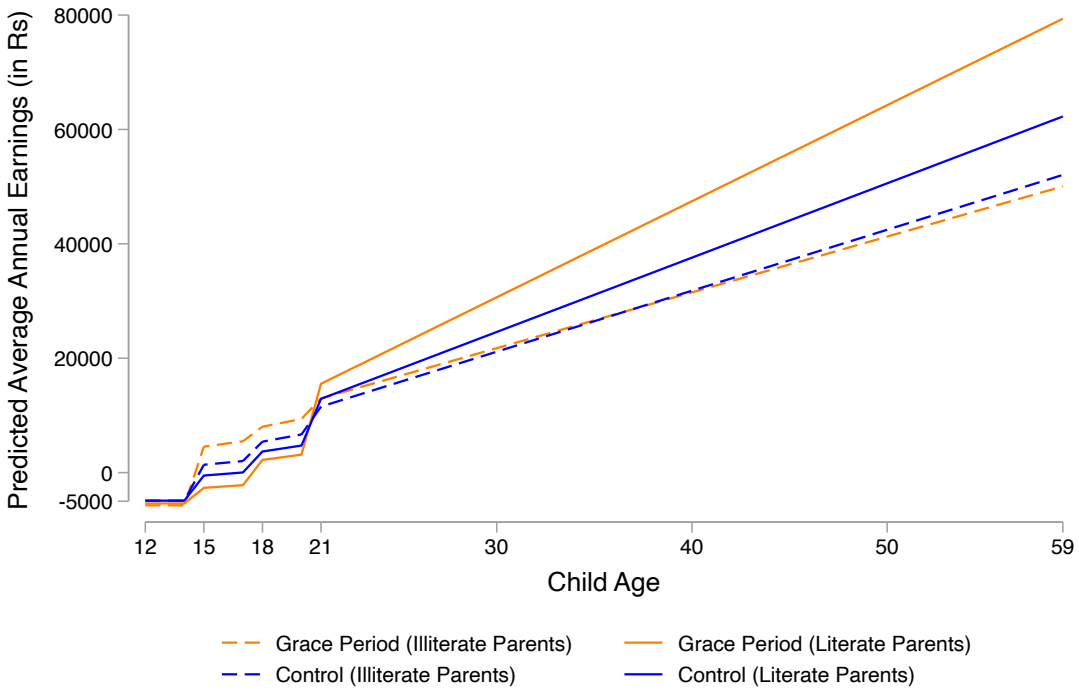
Notes: The figures plots binned scatter plots with annual earnings on the y-axis and age at the x-axis for secondary school dropouts, secondary school graduates, and college graduates in urban India. The data is obtained from the India Human Development Survey-2 (IHDS-2). The sample consists of all household members aged 18-59 years who are not enrolled in school at the point of the survey and who live in urban areas. The dots correspond to binned means and the dashed lines correspond to fitted lines based on linear regressions.

Figure A6: Age-Earning Curves in IHDS and VFS



Notes: The figures plots binned scatter plots with annual earnings on the y-axis and age at the x-axis for or secondary school dropouts, secondary school graduates, and college graduates. The blue figures are based on the VFS sample and the red figures are based on the IHDS-2 sample. The IHDS sample consists of all household members aged 18-59 years who are not enrolled in school at the point of the survey and who live in urban areas. The dots correspond to binned means and the dashed lines correspond to fitted lines based on linear regressions.

Figure A7: Predicted Child Earnings Based on Educational and Economic Returns



Notes: The figure plots predicted child earnings by child age and treatment group based on case 2 in our child welfare analysis (see section 4). Linear age-earning curves for secondary school dropouts, secondary school graduates, and college graduates are estimated based on the urban households in the Indian Human Development Survey-2 (see Appendix Figure A5). We assume that secondary school dropouts start to work at 15, secondary school graduates at 18, and college graduates at 21. Secondary school and college completion rates for the treatment and control group at based on the 2018 enrollment status for the school-age child sample. The orange lines correspond to the treatment group and the blue lines correspond to the control group. The solid lines correspond to children of literate parents and the dashed lines correspond to children of illiterate parents. Information on secondary school and college costs are obtained from the control group. See Appendix Table A14 for inputs to welfare analysis.

Table A1: Attrition Check

	Full Household Sample			
	2010 Survey		2018 Survey	
Panel A: Attrition				
	Treat	SE	Treat	SE
	(1)	(2)	(3)	(4)
Attrition	0.003	(0.020)	-0.020	(0.025)
Control Mean	0.089		0.127	
Panel B: Attrition and Baseline Characteristics				
	Attrited x Treat	SE	Attrited x Treat	SE
	(1)	(2)	(3)	(4)
Client's Age	-2.029	(1.966)	0.365	(1.890)
Married	0.097	(0.086)	-0.102	(0.093)
Muslim	-0.007	(0.007)	-0.002	(0.008)
Client's Years of Education	1.239	(0.821)	1.208*	(0.704)
Household Size	0.271	(0.307)	0.746**	(0.294)
Household Shock	0.103	(0.131)	0.172	(0.119)
Household Has a Business (Narrow)	-0.070	(0.087)	-0.055	(0.085)
Owns Home	-0.100	(0.107)	-0.030	(0.090)
Client Has Financial Control	0.039	(0.074)	0.055	(0.068)
No Drain in Neighborhood	-0.031	(0.052)	0.039	(0.078)
Loan Amt 4,000 RPS	-0.006	(0.005)	0.019	(0.021)
Loan Amt 5,000 RPS	-0.073*	(0.038)	-0.023	(0.043)
Loan Amt 6,000 RPS	-0.036	(0.125)	-0.105	(0.089)
Loan Amt 7,000 RPS	-0.000	(0.001)	-0.001	(0.001)
Loan Amt 8,000 RPS	0.074	(0.125)	0.011	(0.110)
Loan Amt 9,000 RPS	-0.001	(0.003)	0.045	(0.042)
Loan Amt 10,000 RPS	0.043	(0.071)	0.055	(0.078)
Socio-Economic Index	0.417	(0.339)	-0.062	(0.301)
Spouse's Age	0.006	(2.433)	0.041	(2.338)
Spouse's Years of Education	-0.126	(1.076)	-0.470	(0.916)
Education Expenditure 2007	-39.598	(99.647)	268.384**	(134.869)
Health Expenditure 2007	-169.917	(258.849)	229.793	(224.769)
Renovation Expenditure 2007	-103.765	(527.110)	458.427	(369.131)

Notes: Panel A reports the grace period coefficient from a regression of an indicator variable for attrition on treatment status at each survey round. Panel B comes from a regression of the baseline characteristic on a grace period indicator, an attrition indicator for the given survey round, and an interaction between the two. The table reports the coefficient on the interaction term. The sample consists of the full household sample. All regressions control for stratification dummies and cluster standard errors by loan group. We do not show the attrition check for the school-age household sample since we only collected a full child roster in the 2018 survey. See Data Appendix for detailed variable definitions. All expenditure variables are top-coded at the 99.5th percentile. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A2: Balance Check

	Full Household Sample					School-Age Household Sample				
	Control		Grace Period			Control		Grace Perid		
	Mean (1)	St. Dev. (2)	Coeff. (3)	St. Err. (4)	N (5)	Mean (6)	St. Dev. (7)	Coeff. (8)	St. Err. (9)	N (10)
Panel A: Original Household-Level Controls										
Client's Age	34.508	[8.406]	-0.637	(0.559)	842	34.259	[5.887]	0.340	(0.608)	380
Married	0.911	[0.286]	-0.046**	(0.022)	843	0.964	[0.187]	-0.010	(0.020)	380
Muslim	0.007	[0.084]	0.014	(0.012)	842	0.010	[0.102]	0.016	(0.015)	380
Client's Years of Education	6.574	[3.591]	-0.149	(0.323)	839	6.135	[3.587]	-0.170	(0.438)	380
Household Size	4.068	[1.420]	0.127	(0.105)	842	4.342	[1.314]	-0.021	(0.145)	380
Household Shock	0.607	[0.489]	0.030	(0.059)	830	0.628	[0.485]	0.018	(0.067)	375
Household Has a Business (Narrow)	0.772	[0.420]	0.014	(0.041)	843	0.777	[0.417]	0.045	(0.050)	380
Owns Home	0.816	[0.388]	-0.011	(0.034)	838	0.854	[0.354]	-0.027	(0.039)	377
Client Has Financial Control	0.838	[0.369]	-0.009	(0.038)	841	0.870	[0.337]	-0.037	(0.044)	379
No Drain in Neighborhood	0.129	[0.335]	-0.022	(0.036)	830	0.126	[0.332]	0.013	(0.045)	375
Loan Amt 4,000 RPS	0.012	[0.108]	0.001	(0.010)	845	0.016	[0.124]	-0.014	(0.011)	381
Loan Amt 5,000 RPS	0.047	[0.212]	-0.014	(0.017)	845	0.047	[0.211]	0.005	(0.027)	381
Loan Amt 6,000 RPS	0.289	[0.454]	-0.056	(0.043)	845	0.301	[0.460]	-0.088*	(0.053)	381
Loan Amt 7,000 RPS	0.002	[0.049]	-0.002	(0.002)	845	0.005	[0.072]	-0.005	(0.005)	381
Loan Amt 8,000 RPS	0.567	[0.496]	0.010	(0.052)	845	0.554	[0.498]	0.009	(0.063)	381
Loan Amt 9,000 RPS	0.000	[0.000]	0.005	(0.005)	845	0.000	[0.000]	0.000	(0.000)	381
Loan Amt 10,000 RPS	0.082	[0.275]	0.056	(0.035)	845	0.078	[0.268]	0.092**	(0.039)	381
Panel B: Additional Household-Level Controls										
Socio-Economic Index	-0.103	[1.347]	0.210*	(0.115)	731	-0.137	[1.167]	0.181	(0.152)	333
Spouse's Age	41.142	[9.084]	-0.085	(0.668)	739	41.000	[6.841]	0.677	(0.712)	363
Spouse's Years of Education	7.715	[3.391]	-0.272	(0.322)	711	7.346	[3.346]	-0.020	(0.389)	350
Number of Children (Still Alive in 2018)	1.798	[1.060]	-0.098	(0.090)	747	2.088	[0.972]	-0.075	(0.110)	381
Impatient	0.505	[0.501]	-0.024	(0.041)	806	0.527	[0.501]	-0.040	(0.057)	363
Education Expenditure 2007	420.569	[540.354]	6.833	(43.282)	841	635.665	[588.191]	11.856	(72.958)	380
Health Expenditure 2007	368.140	[915.473]	37.863	(72.758)	841	303.911	[578.055]	101.277	(102.937)	380
Renovation Expenditure 2007	545.502	[1240.237]	84.322	(129.066)	644	595.572	[1175.597]	159.899	(157.220)	295
Joint Test p-value			0.129					0.465		
Panel C: Child-Level Controls										
Female	0.487	[0.500]	-0.017	(0.027)	1401	0.505	[0.501]	-0.012	(0.045)	544
Child Age	14.265	[9.650]	-1.566**	(0.713)	1401	12.128	[3.188]	-0.219	(0.314)	544
Birth Order	1.762	[0.951]	-0.001	(0.070)	1401	1.791	[0.987]	-0.048	(0.100)	544
Resides with Parents	0.738	[0.440]	-0.002	(0.028)	1401	0.912	[0.284]	0.007	(0.032)	544
Panel D: Heterogeneity Variables										
At Least One Child Aged 7-17 at Baseline	0.520	[0.500]	-0.017	(0.036)	747					
Literate Parents	0.819	[0.385]	-0.066*	(0.034)	725	0.806	[0.397]	-0.056	(0.045)	354
Average Parental Education in Years	7.038	[3.107]	-0.206	(0.295)	839	6.684	[2.995]	-0.222	(0.374)	380
Parent Attended Secondary School	0.875	[0.331]	-0.043	(0.028)	839	0.886	[0.319]	-0.079**	(0.039)	380
Literate Mothers	0.877	[0.329]	-0.045*	(0.027)	810	0.866	[0.341]	-0.042	(0.040)	366
Literate Fathers	0.924	[0.265]	-0.030	(0.025)	711	0.911	[0.286]	-0.034	(0.031)	350

Notes: Columns 3 and 8 report the tests of difference of means where we control for stratification dummies and cluster standard errors by loan group. All variables in Panels A and B come from the baseline survey. The sample in Panels A, B, and D in Columns 1-4 consist of the full household sample and the sample in Panels A, B, D in Columns 5-8 consists of households that have at least one child aged 7-17 years at baseline according to the 2018 survey. The sample in Panel C in Columns 1-4 consist of all children that are still alive at the time of the 2018 survey and the sample in Panel C in Columns 5-8 consist of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey. All expenditure variables are top-coded at the 99.5th percentile. Panel A lists household-level controls used in Field et al. (2013). In household-level regressions, the double-lasso chooses among variables in Panels A and B. In child-level regressions, the double-lasso chooses among variables in Panels A-C. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A3: Comparison of Literate and Illiterate Households in the Control Group

	Full Household Sample					School-Age Households				
	Illiterate		Literate			Illiterate		Literate		
	Mean (1)	St. Dev. (2)	Coeff. (3)	St. Err. (4)	N (5)	Mean (6)	St. Dev. (7)	Coeff. (8)	St. Err. (9)	N (10)
Panel A: Household-Level Controls										
Client's Age	37.985	[8.792]	-4.381***	(1.153)	413	34.971	[5.431]	-1.302	(1.031)	187
Married	0.897	[0.306]	0.103***	(0.037)	413	0.971	[0.169]	0.029	(0.028)	187
Muslim	0.029	[0.170]	-0.026	(0.021)	413	0.057	[0.236]	-0.057	(0.039)	187
Client's Years of Education	1.397	[2.776]	6.259***	(0.371)	413	1.514	[2.661]	5.727***	(0.503)	187
Spouse's Years of Education	3.770	[4.137]	4.726***	(0.547)	369	4.000	[4.264]	4.131***	(0.754)	179
Household Size	4.132	[1.434]	-0.002	(0.190)	413	4.429	[1.243]	-0.104	(0.235)	187
Household Shock	0.576	[0.498]	0.045	(0.067)	408	0.559	[0.504]	0.076	(0.095)	185
Household Has a Business (Narrow)	0.721	[0.452]	0.049	(0.060)	413	0.743	[0.443]	0.043	(0.082)	187
Owns Home	0.836	[0.373]	-0.034	(0.051)	412	0.853	[0.359]	-0.012	(0.068)	186
Client Has Financial Control	0.809	[0.396]	0.042	(0.052)	413	0.886	[0.323]	-0.010	(0.061)	187
No Drain in Neighborhood	0.227	[0.422]	-0.123**	(0.055)	408	0.265	[0.448]	-0.175**	(0.080)	185
Loan Amount	7632.353	[1183.272]	-236.249	(161.995)	413	7542.857	[1120.474]	-191.133	(218.397)	187
Socio-Economic Index	-0.495	[1.127]	0.532***	(0.163)	368	-0.482	[0.984]	0.439**	(0.199)	170
Spouse's Age	45.311	[10.148]	-5.048***	(1.382)	369	42.088	[6.824]	-1.557	(1.290)	179
Number of Children (Still Alive in 2018)	2.483	[1.143]	-0.818***	(0.159)	361	2.486	[0.951]	-0.541***	(0.176)	187
Has Savings Account	0.094	[0.294]	0.099**	(0.044)	380	0.030	[0.174]	0.168***	(0.046)	175
Risk Loving	0.500	[0.504]	0.091	(0.068)	406	0.576	[0.502]	0.047	(0.096)	183
Impatient	0.552	[0.501]	-0.057	(0.067)	411	0.559	[0.504]	-0.048	(0.095)	186
At Least One HH Member Is a Wage Worker	0.456	[0.502]	0.067	(0.067)	413	0.486	[0.507]	0.011	(0.095)	187
Education Expenditure 2007	298.847	[388.616]	177.395***	(57.225)	413	452.914	[431.476]	237.968***	(88.333)	187
Health Expenditure 2007	244.489	[485.667]	157.650*	(82.623)	413	216.876	[525.899]	128.165	(101.645)	187
Renovation Expenditure 2007	405.760	[832.258]	218.134	(143.056)	320	350.540	[606.336]	341.221**	(163.522)	150
Panel B: Child-Level Controls										
Female	0.493	[0.502]	-0.008	(0.046)	686	0.525	[0.504]	-0.030	(0.074)	263
Birth Order	2.142	[1.207]	-0.480***	(0.106)	686	1.932	[0.944]	-0.231*	(0.140)	263
Resides with Parents	0.689	[0.464]	0.056	(0.042)	686	0.898	[0.305]	0.018	(0.044)	263

Notes: Columns 3 and 8 report the tests of difference of means. Robust standard errors are shown in brackets. Columns 1-4 consist of the full household sample. Columns 5-8 consists of households that have at least one child aged 7-17 years at baseline according to the 2018 survey. All expenditure variables are top-coded at the 99.5th percentile. Panel A lists household-level controls used in Field et al. (2013). In household-level regressions, the double-lasso chooses among variables in Panels A and B. In child-level regressions, the double-lasso chooses among variables in Panels A-C. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A4: 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	0.055	1127.381	174.251	0.051***	1858.508	4867.620***	1948.090*
	(0.043)	(1056.940)	(824.540)	(0.018)	(1353.826)	(1836.241)	(1045.110)
Panel B: School-Age Child Sample (7-17 Years at Baseline), Heterogeneity by Gender							
Grace Period	0.037	1632.315	1328.386	0.063**	1116.580	6154.425**	1465.640
	(0.056)	(1653.203)	(1043.018)	(0.025)	(2006.738)	(2642.503)	(1669.716)
Grace Period × Female	0.029	-1117.201	-2473.172*	-0.027	1452.541	-2798.642	982.557
	(0.066)	(2157.912)	(1356.762)	(0.033)	(2985.100)	(3436.033)	(2263.455)
Female	-0.037	-601.921	2381.262**	-0.001	-1711.449	1103.024	-578.205
	(0.049)	(1358.350)	(945.089)	(0.018)	(1491.885)	(1902.574)	(1375.393)
Panel C: School-Age Child Sample (7-17 Years at Baseline), Heterogeneity by Parental Literacy							
Grace Period	0.040	946.601	18.704	-0.007	-503.828	2984.863	-2715.032*
	(0.055)	(848.004)	(1578.883)	(0.010)	(1242.226)	(3201.245)	(1479.361)
Grace Period × Literate Parents	0.050	736.302	-110.281	0.090***	4622.931**	2970.610	6439.617***
	(0.075)	(1482.206)	(1736.463)	(0.029)	(2008.214)	(4013.829)	(2197.053)
Literate Parents	0.203***	4578.791***	-301.181	0.027**	4176.386***	1209.396	601.858
	(0.047)	(930.635)	(1334.664)	(0.011)	(1382.509)	(2650.637)	(1569.361)
Panel D: Old Child Sample (18+ Years at Baseline), Pooled							
Grace Period	-0.033	-2021.406*	-98.690	-0.004	-2248.253	-3694.453	3.848
	(0.032)	(1186.367)	(2109.039)	(0.013)	(1514.321)	(3773.609)	(604.215)
Panel E: Young Child Sample (Under 7 Years at Baseline)							
Grace Period	0.045	93.575	369.725				
	(0.054)	(1,316.696)	(581.637)				
Panel A Statistics							
Mean of Omitted Group	0.227	6563.676	8155.801	0.018	10969.469	23411.475	3907.180
Observations	543	518	542	543	513	535	531
Panel B Statistics							
p-value: Grace Period + Grace Period x Female	0.201	0.701	0.283	0.140	0.206	0.160	0.081
Mean of Omitted Group	0.244	7026.675	7171.477	0.022	12246.929	23352.405	4503.718
Observations	543	518	542	543	513	535	531
Panel C Statistics							
p-value: Grace Period + Grace Period x Literate Parents	0.096	0.243	0.920	0.002	0.036	0.012	0.014
Mean of Omitted Group	0.034	3046.764	8424.763	0.000	6973.991	18688.508	2868.457
Observations	543	518	542	543	513	535	531
Panel D Statistics							
Mean of Omitted Group	0.130	8321.492	12822.891	0.022	12157.367	27722.308	2151.340
Observations	492	430	484	492	439	477	482
Panel E Statistics							
Mean of Omitted Group	0.312	5967.569	5203.675				
Observations	341	334	340				

Notes: Standard errors are clustered by loan group and appear in parentheses. All regressions are run on the child level and include stratification, a dummy for whether the client was dead at the point the 2018 survey, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The regressions in Panel C also include a dummy for missing information on parental literacy and an interaction between the dummy for missing information on parental literacy and the grace period variable. The sample in Panels A-C consist of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey. The sample in Panel D consists of children of the client aged 18+ at baseline that are still alive at the time of the 2018 survey. The sample in Panel E consists of children of the client aged 6 years or younger at baseline and that are still alive at the time of the 2018 survey, including children born after baseline if they are at least 5 years old at the point of the 2018 survey. All outcomes are obtained from the 2018 survey. Primary school includes grades 1-4 and secondary school includes grades 5-12. School fees, spending on after-school tutoring, and college spending are top-coded at 99.5% and deflated to 2007 prices using CPI data published by the World Bank. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A5: Robustness Checks for Child Age Cut-Offs

		Investment Index Components				
	Investment Index	Primary School Investment Subindex	Secondary School Investment Subindex	College Spending (Standard-ized)	Attended College	Completed Secondary School
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 6-16 Years at Baseline						
Grace Period	0.181***	0.073	0.194***	0.197**	0.093**	0.039
	(0.069)	(0.070)	(0.071)	(0.090)	(0.038)	(0.040)
Panel B: 6-17 Years at Baseline						
Grace Period	0.194***	0.083	0.217***	0.194**	0.095***	0.050
	(0.068)	(0.071)	(0.074)	(0.086)	(0.035)	(0.038)
Panel C: 6-18 Years at Baseline						
Grace Period	0.162**	0.064	0.178***	0.165**	0.095***	0.051
	(0.067)	(0.070)	(0.069)	(0.077)	(0.035)	(0.037)
Panel D: 7-16 Years at Baseline						
Grace Period	0.186**	0.063	0.228***	0.169*	0.090**	0.027
	(0.073)	(0.072)	(0.077)	(0.094)	(0.042)	(0.043)
Panel E: 7-18 Years at Baseline						
Grace Period	0.185***	0.056	0.232***	0.160*	0.096**	0.046
	(0.070)	(0.072)	(0.074)	(0.084)	(0.037)	(0.040)
Panel F: 8-16 Years at Baseline						
Grace Period	0.153**	0.036	0.213***	0.126	0.075*	-0.008
	(0.077)	(0.078)	(0.079)	(0.096)	(0.044)	(0.046)
Panel G: 8-17 Years at Baseline						
Grace Period	0.164**	0.054	0.242***	0.126	0.084**	0.013
	(0.076)	(0.079)	(0.081)	(0.090)	(0.040)	(0.043)
Panel H: 8-18 Years at Baseline						
Grace Period	0.153**	0.031	0.203***	0.129	0.085**	0.019
	(0.073)	(0.078)	(0.075)	(0.082)	(0.039)	(0.041)

Notes: Standard errors are clustered by loan group and appear in parentheses. All regressions are run on the child level and include stratification, a dummy for whether the client was dead at the point the 2018 survey, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. Each panel shows the results for a different age cutoff to define the school-age child sample. All outcomes are obtained from the 2018 survey. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A6: Robustness Checks for Heterogeneous Treatment Effects on Educational Outcomes by Parental Education

		Investment Index Components				
	Investment Index	Primary School Investment Subindex	Secondary School Investment Subindex	College Spending (Standard-ized)	Attended College	Completed Secondary School
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Parental Education in Years						
Grace Period	-0.192	-0.071	-0.104	-0.077	0.054	-0.076
	(0.178)	(0.148)	(0.163)	(0.230)	(0.077)	(0.081)
Grace Period × Average Parental Education in Years	0.067**	0.025	0.065**	0.044	0.009	0.021*
	(0.033)	(0.024)	(0.030)	(0.043)	(0.011)	(0.011)
Average Parental Education in Years	0.054**	0.044**	0.056**	0.080***	0.042***	0.029***
	(0.025)	(0.018)	(0.022)	(0.031)	(0.008)	(0.009)
Panel B: Parent Attended Secondary School						
Grace Period	-0.172	-0.027	-0.210	-0.120	-0.091	-0.184*
	(0.141)	(0.143)	(0.140)	(0.158)	(0.098)	(0.105)
Grace Period × Parent Attends Secondary School	0.434***	0.128	0.562***	0.389**	0.231**	0.277**
	(0.153)	(0.160)	(0.155)	(0.197)	(0.102)	(0.109)
Parent Attends Secondary School	-0.190	0.038	-0.219**	0.115	-0.048	-0.110
	(0.120)	(0.128)	(0.110)	(0.148)	(0.076)	(0.081)
Panel C: Mother's Literacy						
Grace Period	-0.141	-0.027	0.056	-0.375**	-0.137*	-0.148*
	(0.108)	(0.139)	(0.093)	(0.179)	(0.070)	(0.082)
Grace Period × Literate Mother	0.440***	0.130	0.255**	0.654***	0.281***	0.243***
	(0.138)	(0.151)	(0.130)	(0.221)	(0.079)	(0.092)
Literate Mother	0.083	0.081	0.033	-0.113	-0.013	-0.008
	(0.104)	(0.114)	(0.085)	(0.169)	(0.056)	(0.073)
Panel D: Father's Literacy						
Grace Period	-0.099	-0.038	-0.056	-0.079	0.056	-0.148*
	(0.122)	(0.132)	(0.127)	(0.198)	(0.088)	(0.085)
Grace Period × Literate Father	0.387**	0.139	0.378***	0.308	0.059	0.231**
	(0.151)	(0.157)	(0.145)	(0.229)	(0.098)	(0.102)
Literate Father	0.216*	0.252**	0.036	0.120	0.117*	0.034
	(0.116)	(0.123)	(0.105)	(0.199)	(0.069)	(0.073)
Panel A Statistics						
Mean of Omitted Group	-0.043	0.087	-0.146	-0.067	0.105	0.263
Observations	543	543	543	531	541	543
Panel B Statistics						
p-value: Grace Period + Grace Period x Parent Attends Secondary School	0.003	0.232	0.000	0.020	0.002	0.040
Mean of Omitted Group	-0.155	-0.144	-0.124	-0.109	0.186	0.372
Observations	543	543	543	531	541	543
Panel C Statistics						
p-value: Grace Period + Grace Period x Literate Mother	0.002	0.221	0.002	0.018	0.003	0.047
Mean of Omitted Group	-0.190	-0.179	-0.247	-0.010	0.211	0.342
Observations	543	543	543	531	541	543
Panel D Statistics						
p-value: Grace Period + Grace Period x Literate Father	0.003	0.245	0.001	0.042	0.018	0.089
Mean of Omitted Group	-0.235	-0.278	-0.172	-0.100	0.129	0.355
Observations	543	543	543	531	541	543

Notes: Standard errors are clustered by loan group and appear in parentheses. All regressions are run on the child level and include stratification, a dummy for whether the client was dead at the point the 2018 survey, a dummy for missing information on parental literacy, an interaction between the dummy for missing information on parental literacy and the grace period variable, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The sample in each panel consists of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey. Average parental education in years is top-coded at 12 years. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A7: Correlations Between Education Outcomes and Household Characteristics in the Control Group

	Control Group Children (7+ Years at Baseline)						
		Investment Index Components					
	Investment Index	Primary School Investment Subindex	Secondary School Investment Subindex	College Spending (Standard-ized)	Attended College	Completed Secondary School	Child Income in 2018 (Conditional)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Literate Parents	0.254***	0.191***	0.265***	0.125	0.133***	0.161***	208.851
	(0.057)	(0.057)	(0.053)	(0.082)	(0.035)	(0.043)	(259.639)
Socio-Economic Index	0.133***	0.089***	0.128***	0.094**	0.027**	0.030**	
	(0.035)	(0.023)	(0.033)	(0.044)	(0.014)	(0.015)	
Completed College							1816.553***
							(455.980)
Mean for Illiterate Parents	-0.239	-0.177	-0.243	-0.126	0.098	0.188	1676.501
Observations	484	484	484	470	483	484	375

Notes: Robust standard errors appear in parentheses. The sample consists of all children of the client in the control group aged 7 years or older at baseline that are still alive at the time of the 2018 survey. The sample in column 7 is restricted to children who completed their education. All regressions include a dummy for whether the client was dead at the point the 2018 survey and a dummy for missing information on parental literacy. The regressions in columns 1-6 also include a dummy for missing information on the socio-economic index. The socio-economic index is top-coded at 99.5%. All outcomes are obtained from the 2018 survey. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A8: Treatment Effects on Household Enterprise Outcomes in 2012

	Business Index (1)	Index Components: Profits (2)	Capital (3)	Number of Workers (4)	Log Income (5)
Panel A: Full Household Sample, Pooled					
Grace Period	0.133**	73.850	13844.148**	0.187*	0.113**
	(0.057)	(134.390)	(5620.797)	(0.104)	(0.053)
Panel B: Full Household Sample, Heterogeneity by Parental Literacy					
Grace Period	0.009	-66.276	-4481.067	0.170	0.231**
	(0.132)	(314.056)	(10601.337)	(0.257)	(0.100)
Grace Period × Literate Parents	0.175	203.262	23972.137*	0.047	-0.174
	(0.142)	(342.780)	(12586.522)	(0.276)	(0.112)
Literate Parents	-0.036	34.932	-6422.848	-0.047	-0.162*
	(0.118)	(298.072)	(9099.488)	(0.204)	(0.094)
Panel C: School-Age Household Sample, Pooled					
Grace Period	0.147*	177.791	8949.434	0.259*	0.121
	(0.087)	(204.803)	(9031.862)	(0.147)	(0.087)
Panel D: School-Age Household Sample, Heterogeneity by Parental Literacy					
Grace Period	0.161	-93.567	6443.468	0.573**	0.253
	(0.123)	(300.290)	(11264.569)	(0.279)	(0.173)
Grace Period × Literate Parents	-0.014	344.477	4530.803	-0.410	-0.250
	(0.157)	(365.808)	(16482.207)	(0.335)	(0.189)
Literate Parents	0.194*	165.327	13444.488	0.365**	-0.192
	(0.103)	(293.582)	(9513.886)	(0.185)	(0.148)
Panel A Statistics					
Mean of Omitted Group	-0.000	1295.439	16316.272	0.621	8.981
Observations	771	768	755	767	757
Panel B Statistics					
p-value: Grace Period + Grace Period x Literate Parents	0.004	0.373	0.004	0.056	0.345
Mean of Omitted Group	0.033	1306.493	20649.657	0.650	8.903
Observations	771	768	755	767	757
Panel C Statistics					
Mean of Omitted Group	0.000	1277.197	20653.508	0.549	8.975
Observations	369	367	360	366	361
Panel D Statistics					
p-value: Grace Period + Grace Period x Literate Parents	0.164	0.305	0.345	0.356	0.975
Mean of Omitted Group	-0.124	1204.453	11163.718	0.294	8.900
Observations	369	367	360	366	361

Notes: Standard errors are clustered by loan group and appear in parentheses. All regressions include survey wave dummies, stratification dummies, a dummy for whether the client was dead at the point the 2018 survey, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The regressions in Panels B and D also include a dummy for missing information on parental literacy and an interaction between the dummy for missing information on parental literacy and the grace period variable. The sample in Panels A and B consists of all households. The sample in Panels C and D consists of all households with at least one child aged 7-17 years at baseline according to the 2018 survey. Profits, capital, and the number of workers are top-coded at 99.5% for each survey round. Profits and capital are deflated to 2007 prices using CPI data published by the World Bank All outcomes are obtained from the 2012 survey. Income was obtained from a separate follow up survey in 2012. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A9: Robustness Checks for Heterogeneous Treatment Effects on Enterprise Outcomes by Parental Education

	Full Household Sample				Schol-Age Household Sample			
	2010 Survey		2018 Survey		2010 Survey		2018 Survey	
	Business Index (1)	Log Income (2)	Business Index (3)	Log Income (4)	Business Index (5)	Log Income (6)	Business Index (7)	Log Income (8)
Panel A: Parental Education in Years								
Grace Period	0.370**	0.216	0.138	0.278**	0.767***	0.513**	0.181	0.136
	(0.148)	(0.174)	(0.144)	(0.118)	(0.247)	(0.244)	(0.172)	(0.143)
Grace Period × Average Parental Education in Years	-0.023	-0.006	-0.014	-0.029*	-0.077**	-0.045	-0.016	-0.005
	(0.020)	(0.022)	(0.018)	(0.015)	(0.032)	(0.031)	(0.023)	(0.019)
Average Parental Education in Years	0.019	0.030*	0.002	0.031***	0.046**	0.047**	0.006	0.032**
	(0.013)	(0.017)	(0.014)	(0.012)	(0.020)	(0.022)	(0.016)	(0.014)
Panel B: Parent Attended Secondary School								
Grace Period	0.373**	0.183	0.102	0.319***	0.757**	0.451*	0.181	0.229
	(0.189)	(0.173)	(0.149)	(0.117)	(0.344)	(0.256)	(0.180)	(0.154)
Grace Period × Parent Attends Secondary School	-0.197	-0.014	-0.070	-0.286**	-0.612*	-0.312	-0.124	-0.149
	(0.201)	(0.187)	(0.153)	(0.124)	(0.363)	(0.275)	(0.198)	(0.166)
Parent Attends Secondary School	0.116	0.151	0.057	0.200**	0.270**	0.258	0.086	0.208*
	(0.088)	(0.141)	(0.132)	(0.093)	(0.112)	(0.202)	(0.146)	(0.122)
Panel C: Mother's Literacy								
Grace Period	0.161	0.184	0.262	0.355***	0.612*	0.621**	0.556**	0.369**
	(0.190)	(0.197)	(0.162)	(0.136)	(0.343)	(0.246)	(0.219)	(0.158)
Grace Period × Literate Mother	0.045	-0.036	-0.249	-0.320**	-0.435	-0.492*	-0.543**	-0.308*
	(0.205)	(0.214)	(0.171)	(0.144)	(0.356)	(0.264)	(0.234)	(0.171)
Literate Mother	-0.008	0.085	0.081	0.202*	0.227*	0.442**	0.281***	0.265**
	(0.107)	(0.151)	(0.118)	(0.121)	(0.128)	(0.187)	(0.105)	(0.133)
Panel D: Father's Literacy								
Grace Period	0.224	-0.015	0.078	0.089	0.187	-0.060	0.119	-0.032
	(0.184)	(0.218)	(0.228)	(0.154)	(0.265)	(0.300)	(0.138)	(0.206)
Grace Period × Literate Father	0.017	0.222	-0.050	-0.044	0.081	0.264	-0.047	0.141
	(0.194)	(0.220)	(0.239)	(0.160)	(0.293)	(0.315)	(0.161)	(0.219)
Literate Father	0.062	-0.101	0.017	-0.005	0.096	-0.182	0.184*	0.109
	(0.078)	(0.155)	(0.173)	(0.099)	(0.114)	(0.244)	(0.100)	(0.150)
Panel A Statistics								
Mean of Omitted Group	0.096	8.920	0.056	8.457	-0.197	8.653	-0.165	8.587
Observations	769	749	708	738	363	351	358	378
Panel B Statistics								
p-value: Grace Period + Grace Period x Parent Attends Secondary School	0.013	0.029	0.575	0.508	0.206	0.206	0.536	0.240
Mean of Omitted Group	-0.109	8.809	-0.052	8.423	-0.244	8.754	-0.093	8.530
Observations	769	749	708	738	363	351	358	378
Panel C Statistics								
p-value: Grace Period + Grace Period x Literate Mother	0.004	0.059	0.833	0.518	0.123	0.253	0.881	0.407
Mean of Omitted Group	0.004	8.855	-0.062	8.408	-0.207	8.629	-0.218	8.482
Observations	769	749	708	738	363	351	358	378
Panel D Statistics								
p-value: Grace Period + Grace Period x Literate Father	0.002	0.011	0.659	0.404	0.039	0.069	0.434	0.132
Mean of Omitted Group	-0.066	9.080	0.019	8.662	-0.107	9.224	-0.148	8.634
Observations	769	749	708	738	363	351	358	378

Notes: Standard errors are clustered by loan group and appear in parentheses. All regressions include survey wave dummies, stratification dummies, a dummy for whether the client was dead at the point the 2018 survey, a dummy for missing information on parental literacy, an interaction between the dummy for missing information on parental literacy and the grace period variable, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The sample in columns 1-4 consists of all households. The sample in columns 5-8 consists of all households with at least one child aged 7-17 years at baseline according to the 2018 survey. The business index components consists of profits, capital, and the number of workers. Average parental education in years is top-coded at 12 years. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A10: Treatment Effects on School Dropout Reasons

	School-Age Child Sample (7-17 Years at Baseline)					
	Secondary School Dropout Reasons					
	Family Factors	Child Factors	Marriage Factors	Started to Work Below 18 and Dropout	Worked in 2012 (below 18 Only)	Ever Worked for HH Business (until 2012)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Pooled						
Grace Period	0.024	-0.016	-0.006	-0.027	0.022	-0.006
	(0.039)	(0.037)	(0.027)	(0.033)	(0.038)	(0.034)
Panel B: Heterogeneity by Parental Literacy						
Grace Period	0.178**	-0.012	0.094*	0.037	0.092	0.075
	(0.089)	(0.083)	(0.051)	(0.077)	(0.103)	(0.070)
Grace Period × Literate Parents	-0.221**	-0.017	-0.132**	-0.101	-0.089	-0.110
	(0.100)	(0.088)	(0.060)	(0.080)	(0.102)	(0.080)
Literate Parents	0.069	-0.159**	0.030	0.025	-0.088	0.044
	(0.059)	(0.067)	(0.039)	(0.058)	(0.067)	(0.053)
p-value: Grace Period + Grace Period x Literate Parents	0.309	0.455	0.238	0.055	0.930	0.355
Mean of Omitted Group (Panel A)	0.202	0.210	0.112	0.201	0.088	0.121
Mean of Omitted Group (Panel B)	0.161	0.321	0.089	0.220	0.143	0.103
Observations	532	532	532	544	229	529

Notes: Standard errors are clustered by loan group and appear in parentheses. All regressions are run on the child level and include stratification dummies, a dummy for whether the client was dead at the point the 2018 survey, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The regressions in Panel B also include a dummy for missing information on parental literacy and an interaction between the dummy for missing information on parental literacy and the grace period variable. The sample consist of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey. The sample in column 5 is restricted to school-age children who where younger than 18 years at baseline. Family factors consists of the following reasons: money reasons, a good job opportunity, or feeling that school was not worthwhile. Child factors consist of the following reasons: child disliked school or had low test scores. Marriage factors include marriage- and pregnancy-related reasons. The outcomes in columns 1-4 are obtained from the 2018 survey and the outcomes in columns 5-6 are obtained from the 2012 survey. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A11: Robustness Checks for Heterogeneous Treatment Effects on School Dropout by Parental Education

	School-Age Child Sample (7-17 Years at Baseline)					
	Secondary School Dropout Reasons					
	Family Factors	Child Factors	Marriage Factors	Started to Work Below 18 and Dropout	Worked in 2012 (below 18 Only)	Ever Worked for HH Business (until 2012)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Parental Education in Years						
Grace Period	0.186**	-0.049	0.031	0.071	0.176*	0.104
	(0.087)	(0.094)	(0.054)	(0.078)	(0.105)	(0.064)
Grace Period × Average Parental Education in Years	-0.027**	0.004	-0.006	-0.017*	-0.021*	-0.018**
	(0.011)	(0.012)	(0.007)	(0.010)	(0.012)	(0.009)
Average Parental Education in Years	-0.001	-0.027***	-0.008	-0.008	-0.011	0.010*
	(0.009)	(0.009)	(0.005)	(0.007)	(0.007)	(0.006)
Panel B: Parent Attended Secondary School						
Grace Period	0.254***	-0.075	0.006	0.091	0.328***	0.107
	(0.093)	(0.100)	(0.064)	(0.079)	(0.095)	(0.067)
Grace Period × Parent Attends Secondary School	-0.283***	0.062	-0.020	-0.154*	-0.360***	-0.137*
	(0.105)	(0.111)	(0.068)	(0.085)	(0.103)	(0.070)
Parent Attends Secondary School	0.109	-0.104	-0.070	0.029	0.071	0.105***
	(0.067)	(0.080)	(0.049)	(0.061)	(0.047)	(0.032)
Panel C: Mother's Literacy						
Grace Period	0.232**	-0.072	0.086	0.002	0.054	0.073
	(0.101)	(0.108)	(0.067)	(0.077)	(0.143)	(0.099)
Grace Period × Literate Mother	-0.262**	0.063	-0.108	-0.048	-0.035	-0.097
	(0.107)	(0.110)	(0.075)	(0.085)	(0.142)	(0.107)
Literate Mother	0.081	-0.137*	0.034	0.049	-0.147	0.025
	(0.073)	(0.083)	(0.050)	(0.064)	(0.096)	(0.069)
Panel D: Father's Literacy						
Grace Period	0.210*	-0.063	0.053	0.082	0.252**	0.083
	(0.112)	(0.114)	(0.081)	(0.114)	(0.099)	(0.067)
Grace Period × Literate Father	-0.222*	0.050	-0.067	-0.138	-0.271**	-0.083
	(0.119)	(0.122)	(0.086)	(0.116)	(0.108)	(0.070)
Literate Father	0.110	-0.175**	-0.054	0.006	0.064	0.075**
	(0.068)	(0.089)	(0.068)	(0.072)	(0.049)	(0.033)
Panel A Statistics						
Mean of Omitted Group	0.105	0.421	0.316	0.211	0.000	0.000
Observations	532	532	532	544	229	529
Panel B Statistics						
p-value: Grace Period + Grace Period x Parent Attends Secondary School	0.495	0.753	0.623	0.074	0.452	0.405
Mean of Omitted Group	0.095	0.310	0.190	0.186	0.000	0.047
Observations	532	532	532	544	229	529
Panel C Statistics						
p-value: Grace Period + Grace Period x Literate Mother	0.447	0.792	0.476	0.213	0.601	0.499
Mean of Omitted Group	0.167	0.333	0.083	0.211	0.214	0.132
Observations	532	532	532	544	229	529
Panel D Statistics						
p-value: Grace Period + Grace Period x Literate Father	0.778	0.753	0.604	0.106	0.647	1.000
Mean of Omitted Group	0.100	0.333	0.167	0.226	0.000	0.033
Observations	532	532	532	544	229	529

Notes: Standard errors are clustered by loan group and appear in parentheses. All regressions are run on the child level and include stratification dummies, a dummy for whether the client was dead at the point the 2018 survey, a dummy for missing information on parental literacy, an interaction between the dummy for missing information on parental literacy and the grace period variable, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The sample consist of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey. The sample in column 5 is restricted to school-age children who where younger than 18 years at baseline. The outcomes in columns 1-4 are obtained from the 2018 survey and the outcomes in columns 5-6 are obtained from the 2012 survey. Average parental education in years is top-coded at 12 years. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A12: Mechanisms

	School-Age Child Sample (7-17 Years at Baseline)		Full Household Sample		School-Age Household Sample	
	Investment Index (1)	Attended College (2)	Business Index 2010 (3)	Business Index 2018 (4)	Business Index 2010 (5)	Business Index 2018 (6)
Panel A: Heterogeneity by Discount Rate						
Grace Period	0.277**	0.104**	0.345***	0.081	0.357*	0.180
	(0.115)	(0.053)	(0.105)	(0.077)	(0.192)	(0.126)
Grace Period × Impatient	-0.124	-0.004	-0.298**	-0.060	-0.216	-0.177
	(0.161)	(0.079)	(0.134)	(0.103)	(0.224)	(0.155)
Impatient	-0.001	0.027	0.054	0.005	-0.024	-0.010
	(0.102)	(0.055)	(0.077)	(0.079)	(0.095)	(0.106)
Panel B: Heterogeneity by Parental Literacy and Socio-Economic Index						
Grace Period	-0.091	-0.055	0.140	0.275*	0.353	0.416**
	(0.116)	(0.068)	(0.151)	(0.151)	(0.244)	(0.166)
Grace Period × Literate Parents	0.447***	0.209***	0.090	-0.295*	-0.238	-0.460**
	(0.118)	(0.074)	(0.164)	(0.163)	(0.260)	(0.181)
Grace Period × Socio-Economic Index	0.026	0.013	-0.043	0.001	-0.119	0.008
	(0.103)	(0.033)	(0.057)	(0.045)	(0.099)	(0.076)
Literate Parents	0.117	0.056	-0.018	0.070	0.185*	0.220**
	(0.088)	(0.055)	(0.082)	(0.093)	(0.102)	(0.087)
Socio-Economic Index	0.134	0.007	0.055	3.264***	0.124**	0.068
	(0.090)	(0.024)	(0.036)	(0.663)	(0.057)	(0.058)
Panel A Statistics						
p-value: Grace Period + Grace Period x Literate Parents	0.122	0.086	0.573	0.783	0.258	0.972
Mean of Omitted Group	0.017	0.263	-0.027	0.000	0.019	0.012
Observations	543	541	769	708	363	358
Panel B Statistics						
p-value: Grace Period + Grace Period x Literate Parents	0.001	0.002	0.003	0.746	0.351	0.640
Mean of Omitted Group	-0.236	0.169	-0.031	-0.082	-0.198	-0.192
Observations	543	541	769	708	363	358

Notes: Standard errors are clustered by loan group and appear in parentheses. The regressions in columns 1-2 are run on the child level and the regressions in columns 3-6 are run on the household level. All regressions include stratification dummies, a dummy for whether the client was dead at the point the 2018 survey, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The regressions in Panel A also include a dummy for missing information on discount rates and an interaction between the dummy for missing information on discount rates and the grace period variable. The regressions in Panel B also include a dummy for missing information on parental literacy, a dummy for missing information on the socio-economic index, and interactions between both dummies for missing information and the grace period variable. The socio-economic index is top-coded at 99.5%. The child sample in columns 1-2 consists of children of the client aged 7-17 at baseline that are still alive and have completed schooling at the time of the 2018 survey. The household sample in columns 3-4 consists of all households and the household sample in columns 5-6 consists of all households with at least one child aged 7-17 years at baseline according to the 2018 survey. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A13: Treatment Effects on Children's Socio-Economic Outcomes

					Daughters Only			Conditional on School Completion		
	Still in School / College	Still in Household	Married	Any Children	Housewife	Spouse with Secondary School	Spouse with College	Any Work	Salaried Work	Self Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: School-Age Child Sample (7-17 Years at Baseline), Pooled										
Grace Period	0.058*	0.017	-0.015	0.010	-0.145***	-0.021	0.018	0.069*	0.000	0.057
	(0.034)	(0.033)	(0.037)	(0.033)	(0.051)	(0.053)	(0.068)	(0.036)	(0.042)	(0.041)
Panel B: School-Age Child Sample (7-17 Years at Baseline), Heterogeneity by Parental Literacy										
Grace Period	0.041	-0.120	0.165*	0.181**	0.043	0.053	-0.063	0.104	-0.048	0.111
	(0.054)	(0.079)	(0.085)	(0.077)	(0.126)	(0.114)	(0.131)	(0.079)	(0.080)	(0.079)
Grace Period × Literate Parents	0.035	0.191**	-0.243**	-0.226***	-0.233	-0.068	0.134	-0.054	0.078	-0.074
	(0.065)	(0.089)	(0.096)	(0.087)	(0.143)	(0.158)	(0.157)	(0.089)	(0.095)	(0.092)
Literate Parents	0.072*	0.016	-0.027	-0.015	0.080	0.131	0.106	0.055	0.052	0.081
	(0.042)	(0.058)	(0.063)	(0.057)	(0.093)	(0.084)	(0.117)	(0.071)	(0.067)	(0.064)
Panel C: Old Child Sample (18+ Years at Baseline), Pooled										
Grace Period	0.005	-0.002	-0.024	-0.031	0.027	0.109*	-0.016	-0.000	0.028	0.004
	(0.005)	(0.042)	(0.030)	(0.032)	(0.072)	(0.063)	(0.054)	(0.034)	(0.036)	(0.045)
Panel D: Old Child Sample (18+ Years at Baseline), Heterogeneity by Parental Literacy										
Grace Period	0.001	0.036	0.009	0.028	0.278**	0.184	-0.032	-0.109*	-0.039	-0.043
	(0.002)	(0.074)	(0.048)	(0.048)	(0.110)	(0.130)	(0.075)	(0.062)	(0.052)	(0.099)
Grace Period × Literate Parents	0.007	-0.049	-0.056	-0.092	-0.358**	-0.096	0.077	0.179**	0.115	0.053
	(0.007)	(0.095)	(0.063)	(0.072)	(0.145)	(0.136)	(0.101)	(0.075)	(0.072)	(0.123)
Literate Parents	0.001	0.071	-0.041	-0.055	0.202**	0.204**	0.115*	-0.099*	0.075	-0.231***
	(0.002)	(0.056)	(0.040)	(0.044)	(0.103)	(0.094)	(0.065)	(0.059)	(0.054)	(0.075)
Panel A Statistics										
Mean of Omitted Group	0.176	0.619	0.449	0.309	0.609	0.900	0.225	0.556	0.279	0.189
Observations	544	544	543	543	270	153	153	428	425	424
Panel B Statistics										
p-value: Grace Period + Grace Period x Literate Parents	0.072	0.057	0.061	0.229	0.003	0.844	0.417	0.219	0.570	0.454
Mean of Omitted Group	0.102	0.525	0.542	0.390	0.710	0.833	0.125	0.491	0.212	0.135
Observations	544	544	543	543	270	153	153	428	425	424
Panel C Statistics										
Mean of Omitted Group	0.000	0.363	0.907	0.810	0.685	0.815	0.185	0.652	0.243	0.338
Observations	494	494	492	492	223	186	186	493	484	480
Panel D Statistics										
p-value: Grace Period + Grace Period x Literate Parents	0.296	0.823	0.238	0.217	0.386	0.106	0.525	0.119	0.131	0.860
Mean of Omitted Group	0.000	0.303	0.934	0.855	0.526	0.655	0.069	0.711	0.158	0.507
Observations	494	494	492	492	223	186	186	493	484	480

Notes: Standard errors are clustered by loan group and appear in parentheses. All regressions are run on the child level and include stratification dummies, a dummy for whether the client was dead at the point the 2018 survey, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The regressions in Panels B and D also include a dummy for missing information on parental literacy and an interaction between the dummy for missing information on parental literacy and the grace period variable. The sample in Panels A and B consist of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey. Children that are under age 7 at baseline are excluded from these panels because they have not reached age 18 at the point of the 2018 survey. The samples in Panels C and D consists of children of the client aged 18+ at baseline that are still alive at the time of the 2018 survey. The sample in columns 5-7 is restricted to female children and the sample in columns 8-10 is restricted to children who completed schooling at the time of the 2018 survey. All outcomes are obtained from the 2018 survey. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A14: Inputs to Welfare Analysis

Panel A: Age-Earning Curves (IHDS)		
School Dropouts, Intercept	-199.5	
School Dropouts, Age Coefficient	510.8	
Secondary School Graduates, Intercept	-28019.8	
Secondary School Graduates, Age Coefficient	2379.0	
College Graduates, Intercept	-30884.0	
College Graduates, Age Coefficient	2379.0	
	Control	Treatment
Panel B: Schooling Costs (VFS)		
Average Annual Schooling Costs (Class 7-9)	4904.4	
Average Annual Schooling Costs (Class 10-12)	9274.8	
Average Annual College Costs	4241.4	
Panel C: Educational Attainment by Treatment Group (VFS)		
Pooled Sample:		
Secondary School Dropouts	0.55	0.49
Secondary School Graduates	0.16	0.11
College Graduates:	0.29	0.40
Illiterate Parents Sample:		
Secondary School Dropouts	0.64	0.77
Secondary School Graduates	0.18	0.11
College Graduates:	0.18	0.12
Literate Parents Sample:		
Secondary School Dropouts	0.52	0.38
Secondary School Graduates	0.15	0.12
College Graduates:	0.33	0.50
Panel D: Child Income by Educational Attainment and Treatment Group (VFS)		
Income of School Dropouts	1241.0	1480.1
Income of Secondary School Graduates	1871.4	2139.3
Income of College Graduates	2906.6	2957.3

Notes: School dropout is defined as not having completed grade 12. Secondary school graduates are children who completed grade 12 but did not attend college. College graduates are children who completed college or are attending college at the point of the 2018survey. Panel A shows the the estimates from regressing household member income against age for different levels of educational attainment. The sample comes from the India Human Development Survey-2 and consists of all household members aged 18-59 years who are not enrolled in school at the point of the survey and live in urban areas. Panel B shows average annual schooling costs for control group children aged 7-17 years at baseline in the VFS sample. Average schooling costs contain school fees and after-school tuition and are based on children who completed secondary school at the point of the 2018 survey. Average college costs are based on children who completed college at the point of the 2018 survey. Panel C shows the share of children who are school dropouts, secondary school graduates and college graduates at the point of the 2018 survey. We drop children that are still in secondary school at the point of the 2018 survey and treat children that are still in college at the point of the 2018 survey as college graduates. Panel D shows raw means of 2018 child income for each level of educational attainment by treatment group. The sample is restricted to children aged 7-17 years at baseline who are not enrolled in school or college at the point of the 2018 survey. Income is top-coded at 99.5% and deflated to 2007 prices using CPI data published by the World Bank.

Table A15: Welfare Analysis with a Social Discount Rate of 10%

	Case 1: Educational Returns Only			Case 2: Educational & Economic Returns		
	Pooled	Illiterate Parents	Literate Parents	Pooled	Illiterate Parents	Literate Parents
	(1)	(2)	(3)	(4)	(5)	(6)
A: Net-Present Value of Private Lifetime Earnings (Control) in INR	102328.2	95683.7	104608.3	102328.2	95683.7	104608.3
(in USD PPP)	(8713.2)	(8147.5)	(8907.4)	(8713.2)	(8147.5)	(8907.4)
B: Net-Present Value of Private Lifetime Earnings (Treatment) in INR	108892.9	92824.4	115177.7	119362.4	107151.9	124077.5
(in USD PPP)	(9272.2)	(7904.0)	(9807.4)	(10163.7)	(9124.0)	(10565.2)
C: Treatment Gains (B-A)	6564.7	-2859.3	10569.4	17034.1	11468.2	19469.1
(in USD PPP)	(559.0)	(-243.5)	(900.0)	(1450.5)	(976.5)	(1657.8)
D: Cost of Treatment	149	149	149	149	149	149
(in USD PPP)	(12.7)	(12.7)	(12.7)	(12.7)	(12.7)	(12.7)
E: Benefit-Cost Ratio (C/D)	44.1		70.9	114.3	77.0	130.7

Notes: The table shows the results of two welfare calculations on the child level. In the first case (columns 1-3), we only account for income gains through differences in educational attainment. In the second case (columns 1-3), we also allow the treatment to affect child income separately from educational attainment. Columns 2 and 5 show the results for children of literate parents and columns 3 and 6 show the results for children of literate parents. See Section 4 for a detailed discussion and Appendix Table A14 for inputs to welfare analysis. The net present value calculation assumes a social discount rate of 10%. Incomes are deflated to 2007 prices using CPI data published by the World Bank and converted to 2007 USD PPP based on conversation tables published by the OECD.

B. Data Appendix

Household-Level Outcome Variables

- *Business Index:* standardized index that consists of the following variables: profits, capital, and number of workers.
- *Profits:* obtained from the following survey question: "Can you please tell us the average weekly profit you have now or when your business was last operational?".
- *Capital:* value (Rs) of raw materials and inventory plus equipment across all businesses in operation at the time of the survey.
- *Workers:* sum of all household and non-household workers across all household businesses in operation at the time of the survey.
- *Income:* In the 2010 and 2018 survey, the outcome is obtained from the following survey question: "During the past 30 days, how much total income did your household earn?". In the 2012 survey, the outcome is obtained from the following survey question: "What is the average income for the whole household per month now?".

Child-Level Outcome Variables

- *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.
- *Completed Secondary School:* indicator variable that is equal to one if the child completed grade 12.
- *Investment Index:* standardized index that consists of the following variables: college spending, secondary school investment subindex, and primary school investment subindex.
- *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.
- *Primary School Investment Subindex:* standardized index that consists of the following variables: private primary school, total primary school fees, and total primary 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 for grades 1 to 4 or at least one year of private secondary school for grades 5 to 12 respectively.
- *Total Secondary School Fees:* obtained from the following 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. 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 fees by summing all fees for grades 1 to 4 and total secondary school fees by summing all 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 total secondary school after-school tutoring by summing all tutoring costs for grades 5 to 12.
- *College Spending:* obtained from the following survey question: "How much did (CHILD) spend in total until now on all post-secondary schooling (excluding living costs such as board or food)?"
- *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. Family factors consists of the following reasons: money reasons, a good job opportunity, or feeling that school was not worthwhile. Child factors consist of the following reasons: child disliked school or had low test scores. Marriage factors include marriage- and pregnancy-related reasons.
- *Started to Work Below 18 and Dropout:* indicator variable that is equal to one if the child started to work before he/she was 18 years old and did not complete grade 12. The age at which the child started to work is obtained by combining the answers to the following survey questions: "At what age did (NAME) leave the last school he/she attended?" and "How long after graduating/leaving school did (NAME) find that job? (in months)".
- *Worked in 2012 (below 18 only).* indicator variable that is equal to one if the child either engaged in any salaried work, self-employment, or daily wage work in the past 30 days in the 2012 survey. The outcome is only defined for children who were part of the household roster and below 18 years at the point of the 2012 survey.
- *Ever Worked for HH Business (until 2012):* indicator variable that is equal to one if the child was listed in the employee roster of any household business in the 2012 survey. The employee rosters include past and current employees of the household business.
- *Still in School or College:* Child is attending secondary school or college at the point of the 2018 survey.
- *Child Income (Conditional):* is obtained by summing the child's income from salaried work, self-employment, and daily wage work in the past 30 days. The outcome is missing if the child is still in school or college at the point of the 2018 survey.
- *Married:* child is married at the point of the 2018 survey.
- *Any Children:* child has at least one child herself.
- *Spouse with Secondary School (Conditional:)* indicator variable that is equal to one if the spouse of the daughter attended at least one year of secondary school. This outcome is only defined for daughters who are married at the point of the 2018 survey.
- *Spouse with College (Conditional:)* indicator variable that is equal to one if the spouse of the daughter attended college. This outcome is only defined for daughters who are married 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)?".
- *Any Work:* indicator variable that is equal to one if the child either engaged in any salaried work, self-employment, or daily wage work in the past 30 days.
- *Any Salaried Work:.* obtained from the following survey question: "Did (NAME) get a fixed salary from an employer in the last 30 days?".
- *Any Self-Employment* indicator variable that is equal to one if the respondent answered "yes" to the following questions "Did (NAME) engage in self-employment in the last 30 days?".

Control Variables

- *Client's Age:* age of the client in years at baseline.
- *Married:* indicator variable that is equal to one if the client was married at baseline.
- *Muslim:* indicator variable that is equal to one if the head of the household is Muslim.
- *Client's Years of Education:* years of education of client at baseline.
- *Household Size:* number of household members at baseline.
- *Household Shock:* dummy for birth, death, or heavy rain in the last 30 days.
- *Household Has a Business (Narrow) :* 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.
- *Owns Home:* indicator variable that is equal to one if the household owned the home at baseline.
- *Mother 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?".
- *No Drain in Neighborhood:* indicator variable that is equal to one if the neighborhood had no drain at baseline.
- *Loan Amount:* VFS loan amount given to client.
- *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.
- *Spouse's Age:* years of education of the client's spouse at baseline.
- *Spouse's Years of Education:* years of education of the client's spouse at baseline.

- *Number of Children (Still Alive in 2018):* total number of children of the client at baseline that are still alive in 2018. This variable is constructed based on age variables in the child roster in the 2018 survey. The age variable is missing if the child was not alive in the 2018 survey. .
- *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.

Heterogeneity Analysis

- *Female:* indicator variable that is equal to one if the child is female.
- *Literate Parents:* indicator variable that is equal to one if both parents can read and write.
- *Parental Education in Years:* average years of schooling of parents.
- *Parent Attended Secondary School:* indicator variable that is equal to one if at least one parent attended class 5 or higher.
- *Literate Mother:* indicator variable that is equal to one if the mother can read and write.
- *Literate Father:* indicator variable that is equal to one if the father can read and write.
- *Impatient:* indicator variable that is equal to one if the client has a discount rate above the median.

Construction of Standardized Indices

1. If a component value in a index is missing and therefore cannot be standardized, we replace it with the relevant treatment group's average, 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).
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.