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

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Abstract

How do the investment choices of poor entrepreneurs impact their, and their children's, economic well-being? To answer this, we exploit experimental variation in household income linked to a one-time relaxation in household liquidity constraints (Field et al., 2013). Treatment households increase educational investments and, eleven years after the intervention, their children are 34% more likely to attend college. Heterogeneity analysis shows that educational gains only accrue to children with literate parents. In contrast, long-term treatment gains among illiterate adult households accrue on the business margin and are accompanied by *adverse* educational consequences for their children. Specifically, treatment raises household income, but also school drop-out. We estimate positive lifetime earning gains from treatment for all children but treatment income gains for children of literate parents are fourfold larger than those for illiterate parents and this gap exceeds the comparable difference in control group. Our findings highlight household educational investment choices as an underlying driver of opportunity but also a conduit linking parental inequality with with lower intergenerational earning mobility.

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1 Introduction

Can growth opportunities for poor micro-entrepreneurs break the intergenerational transmission of poverty? In theory, interventions which improve the income trajectory of poor entrepreneur households, such as positive liquidity shocks, can increase educational investments and break intergenerational poverty traps (Becker and Tomes, 1986; Galor and Zeira, 1993). However, in reality, households' actual investment responses also reflect household preferences and perceived returns to education (Banerjee, 2004). Poor parents - particularly those with high discount rates - may, for instance, favor investments in household business over child education. Such an outcome may be particularly likely for less educated parents who are more likely to underestimate returns to education.[1] And, finally, even when poor parents invest in schooling, structural or early life factors may mute impacts on children's lifetime earnings.

In this paper we exploit short-run experimental variation in entrepreneur household income to evaluate three questions: First, how do poor parents trade-off investments across children's human capital and business when they experience income gains? Second, do these investment choices vary with parental education? And, finally, how do household investment choices impact household and child income in the longer-run and the evolution of intergenerational educational and earnings mobility?

Our study setting is India — a country with one of the lowest rates of inter-generational educational mobility in the world (Asher et al., 2021). Using survey data collected eleven years post intervention, we revisit the study population 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 eases immediate liquidity constraints (now on, grace period contract). Our treatment increased business investment and, three years post-intervention, household income was 20% higher in the treatment group relative to the control group (Field et al., 2013).[2]

In addition to household business and income measures, our eleven year follow-up also measured educational and socio-economic outcomes for all children, including those that have left the household. Arguably, if parents divert income gains away from enterprises to invest in children's human capital, then longer-run evaluations of programs that yield short-run income gains could

[1] Existing 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), while the evidence on the relationship between parental education and parental beliefs is mixed (Brown, 2006; Boneva and Rauh, 2018; Attanasio et al., 2020).

[2] Multiple papers show that credit contracts that allow borrowers to better match business cash-flows to repayment enable more profitable investment decisions and have 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 some 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). Increased flexibility of contracts have been shown to raise business profits (Barboni and Agarwal, 2018; Battaglia et al., 2021; Aragón et al., 2020) and and household income (Battaglia et al., 2021; Czura, 2015).

underestimate true program returns. This is particularly salient for inter-generational impacts, since households that invest in child schooling are precisely those likely to experience the greatest poverty reduction across generations.

Our first set of findings relate to educational investments and outcomes. Among children who were school aged at the time of the trial, those 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 both school fees and after-school tutoring by about 25%. Overall, the increase in education spending represents 9.2% of the increase in household income. Reflecting these investment patterns, children in treatment households are 9.8 percentage points more likely to attend college, which amounts to a 34% 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 and attainment grow in inverse proportion to child's age at baseline.[3]

Turning to the role of parental beliefs, we hypothesize that perceived returns to schooling vary with parental education. For this reason, our pre-analysis plan specified that we will examine intergenerational mobility based on parental education. Our preferred measure of parental education is parental literacy.[4]Some suggestive evidence comes from the nationally representative National Family health survey for 2015. In Figure 1 we plot incidence of sons attending college as a function of parental literacy. Across all wealth quintiles we see significantly higher educational attainment rates for sons coming from literate households.

Comparing households where both parents are illiterate to those where atleast one parent is literate, we show that treatment effects on educational expenditure and college attainment occur in literate households where treatment increases college attendance by 15.7 percentage points. In contrast, children with illiterate parents are more likely to drop out of school - specifically, they are 13.9 percentage points less likely to complete secondary schooling compared to their control counterparts.

Do differences in educational investment reflect limited business (and, therefore, income) treatment gains among illiterate households in the short-run? To examine this possibility, 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 0.22 s.d higher on this index. Both illiterate and literate households report gains though the magnitude is larger for the

[3]Our results are consistent with evidence from the United States that increasing college attendance requires investment early in a child's educational career (Carneiro and Heckman, 2002; Chetty et al., 2016).

[4]We prefer parental literacy since it is clearly defined and previous studies have shown that maternal literacy programs have a direct on child education (Banerji et al., 2017). However, since our pre-analysis plan did not specify parental literacy as the primary measure, we show that our results are robust to alternative measures of parental education.

former. 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 are no longer present among literate households. Consistent with the idea that investments in business and children's education are often 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 turn to the evolution of household income. In our setting adult sons typically co-reside with their parents. Consistent with treatment differences in schooling and business outcomes, by 2018 treatment effects on household income diverge - illiterate treatment households report approximately 26% higher monthly income than control counterparts. This estimate is comparable to observed income gains in 2010. In contrast, we no longer observe a treatment difference in income among literate households.

Finally, we evaluate the evolution of children's income and inter-generational transmission of inequality. At the time of our 2018 survey, a higher share of treatment children were still in school. Assuming that treatment induced children with highest earning potential to continue to college, estimated treatment effects on child income among those who have completed school are biased downward. Such a 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 don't see any treatment gains for daughters. Next, 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 estimate accounts for potential gains in child earnings aside from educational attainment. This allows us to account for the fact that children in illiterate households (who don't go to college) benefit from treatment by inheriting larger businesses. The net present value of pooled lifetime returns for treatment children is 4,659 US dollars. While the returns are positive for both children of illiterate and literate parents, the treatment gains for children of literate parents are four times that for illliterate parents. The earnings gap between children from literate and illiterate households, in turn, increases from 13% in control to 31% in treatment and points to likely increase in income inequality in the next generation.

We extend an experimental literature that has demonstrated how interventions to alleviate credit constraints can yield persistent household income gains (Balboni et al., 2020; Banerjee et al., 2020) by linking investment choices to inter-generational outcomes. Experimental evidence on human capital investments associated with short-run income gains comes mostly 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. Consistent with our findings, this literature highlights that impacts depend on how parents — especially those running enterprises — resolve certain 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. The period considered by these papers is, however, relatively short.[5] The differences in investment patterns by parental education observed in our setting is consistent with these papers and, more broadly, with a growing body of evidence showing that parental perceptions of returns vary with own level of education and, importantly, predicts households' schooling investment decisions (Jensen, 2010; Attanasio and Kaufmann, 2014; Sequeira et al., 2016; Dizon-Ross, 2019).

A growing body of literature investigates whether credit constraints for human capital accumulation can engender an inter-generational poverty trap. Much of this research is concerned with developed country contexts (Carneiro and Heckman, 2002; Dahl and Lochner, 2012; Bulman et al., 2016), though a handful of recent studies evaluate whether subsidies for school fees increase educational attainment (Angrist et al., 2006; Duflo et al., 2017). Two recent studies find that lifting credit constraints through students loans and scholarships increases college enrollment in Chile and Colombia (Solis, 2017; Londoño-Vélez et al., 2020). To the best of our knowledge, Blattman et al. (2020) is the only paper so far that provides experimental evidence on the long-run effect of a cash transfer on child outcomes. In contrast to our results, Blattman et al. (2020) does not find effect on educational outcomes. This might be because their study was conducted in a rural setting with less opportunities for educational investments or because their sample was much younger at the point of the intervention.

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

How did treatment-induced short-run income gains alter household investment choices and longer-run economic outcomes? 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.

[5] 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 micro-entrepreneurs 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).

2.1 Context

The grace period microfinance experiment (now on, GP treatment) was implemented with 845 low-income microfinance clients in Kolkata in 2007. Each client received an individual liability loan ranging from Rs. 6,000-10,000. 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.[6] Relative to the regular contract, the grace period contract, by encouraging higher return (but also higher risk) business investments, increased household income by 19.5% three years after loan disbursement. Consistent with the existing literature, Field et al. (2013) estimate a high monthly return to capital of 13% for these clients.

How did treated clients invest these additional resources in the medium-run? At baseline the median client was 33 years old and married with two school-aged children (aged 5-18). Further, 97% of client households received income from micro-enterprises, with roughly half reporting some income diversification (measured as having at least one wage earner). We now turn to evidence that suggests that clients had valuable business and educational investments.

Business investments Throughout our study period, micro-enterprises remain a primary income-generating activity for client households. In 2018, 85% of respondents reported at least one family-run enterprise. Despite high returns to capital, credit constraints continued to prevent profitable business investments. In 2012, clients report that only 34% of businesses were opened with sufficient resources on hand. When asked what they would do with an additional Rs. 20,000 at the start of their business, clients continued to respond 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 in the medium run remained an attractive option, especially relative to the returns to human capital investments that take longer to realize. Households also had reason to use income gains is to insure their businesses against idiosyncratic and systemic risks. For instance, between 2012 and 2018, 50% of our businesses shut down in the control group with respondents citing household illnesses as the cause for 27% of these closures. In terms of systemic risk, the nation-wide microfinance crisis led to massive negative liquidity shock between 2010 and 212: between 2010 and 2012, the number of control group households that shut atleast one business increased from 38.6 to 60.8 percent.

Education investments The 2007-2018 period was an era of rapidly broadening educational opportunity, proliferating school expenditures and rising educational attainment across India.[7]

[6] 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.

[7] Between 2007 and 2014, annual school investment (tuition and tutoring) per child nearly tripled from roughly US$36 to US$100 (NSSO 2015). Grade progression and advancement to secondary and tertiary schooling dramat-

Particularly noteworthy was rising college education rates in urban India. Montenegro and Patrinos (2014) estimated that completing college led to 21% higher earnings across India and Rani (2014) estimated a 24% rate of return to college in urban areas. In our control sample, college education is associated with 13% higher monthly earnings among children aged 19 or older. Reflecting the idea that college is a stepping stone towards upward mobility via higher-skilled employment, 70% of college graduate children in the control sample engage in salaried work (76% for sons).[8] College education also substantially improves daughters' marriage market outcomes (Adams-Prassl and Andrew, 2019) – daughters in the control group who attend post-secondary school are 48 percentage points more likely to marry someone with a college degree.

Entry into low cost public tertiary education in India is based on grade 12 exam scores (Sekhri, 2020). Private schooling and after-school tutoring (largely at the secondary school-level) are important ways of improving exam performance (Kingdon, 2020; Berry and Mukherjee, 2019): 64% of control group children who attend private secondary school go on to college compared to 30% of those who attend public school. At baseline, 92% of sample children report some private after-school tutoring which involved supplementary instructions in some (or all) academic subjects.[9] Among secondary school graduates, an additional Rs 100,000 of after-school tutoring increases college attendance by 36 percentage points.

But private education investments are costly. In 10th grade, households spend on average Rs 4,900 per child on school expenditures and after-school tutoring, amounting to 5% of average household income in 2010.[10] On average, spending on after-school tutoring is 64% higher than total school expenditures.

Using the 2014-15 nationally representative National Family Health Survey data, Figure 2 plots urban school completion rates by birth year cohort: The x-axis plots the year at which a respondent turned 18 with shaded blue 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 also 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. Yet, rates of

ically increased: Between 2000 and 2010, the proportion of children who completed primary school rose from 71% to 97%, and tertiary enrollment doubled from 9% to 18% (UN 2014).

[8] 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).

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

[10] 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.

secondary schooling and college education for young cohorts remain relatively low.

Finally, recall that Figure 1 showed that across wealth quintiles we see significantly higher educational attainment rates for sons coming from literate households (also using NFHS data). In our study sample we similarly see that parental education matters — children of literate parents are 123% more likely to have attended college. Patterns in data from our study sample mirror national trends: as shown in Table A7, educational investments and outcomes are correlated with both parental literacy and wealth among control group households.

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

2.2 Conceptual Framework

The GP treatment increased client income in the short run Field et al. (2013). We now develop a simple framework in order to interpret medium-run treatment differences in investment choices, household income and children's socio-economic outcomes. Motivated by the discussion in subsection 2.1, we assume that the main investment trade-off is between investing in children's education versus continued business investment.

If households perceive the discounted returns to educational investments as exceeding those from continued business investment, then we anticipate positive treatment effects on educational attainment. Treatment effects on business outcomes may decline in the medium run. Treatment effects on household income may also be depressed in the medium run since income returns to education would not have yet been realized.

In contrast, if households perceive the (discounted) returns to enterprise investment as higher than those to educational investment, then we anticipate continued positive treatment impacts on business outcomes. Correspondingly, treatment impacts on household income should persist. Additionally, if capital and labor are complements in production, then school drop out may also increase among treated households, since enterprises in our sample typically rely on household workers.[11]

As discussed earlier, a growing body of literature suggests that perceived returns to education increase with parental education and this also holds in nationally representative data from India. Overall, education levels among our client sample are low but still characterized by significant variation: At least one parent is illiterate in 18% of the households.[12] Consistent with survey data from Kolkata (Chakravarty and Agarwal, 2021) and other settings (Brown, 2006; Boneva and Rauh, 2019; Boneva et al., 2021), we hypothesize that illiterate households have lower (perceived) returns to children's schooling. Illiterate parents may be less equipped to provide complimentary inputs to facilitate their children's human capital accumulation, such as help with schoolwork

[11] Only 16% of enterprises reported any non-household employees at baseline.
[12] Appendix Figure A2 shows the distribution of parental education.

(Banerji et al., 2017). They may also find it harder to assess their children's ability (Dizon-Ross, 2019) or navigate the school system because they have limited exposure to successful students via friend and family networks (Sequeira et al., 2016). Our analysis, therefore, also examines whether differences in parental educational attainment are associated with differential investment responses to the income shock induced by the grace period.

Given this framework, our empirical analysis proceeds as follows. In Sections 3.1 and 3.1 we examine average impacts on schooling investments and then investigate whether treatment effects vary with parental literacy. In Section 3.2, we ask whether treatment impacts on business investments persist. We also evaluate the hypothesis that medium-run economic gains will be more pronounced among illiterate households and that treatment may induce school dropout among households in this sample. As a precursor to our analysis, we now describe our data collection and key economic and child outcomes of interest.

2.3 Data: Surveys and Outcomes

Our primary data source for longer run education, business and income outcomes comes from our 2018 survey. In examining persistence, we compare these outcomes to data from the 2010 survey that is evaluated by Field et al. (2013)).[13]

A. Tracking and Attrition analysis

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; **?**).[14] 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.

B. Samples and Outcomes

We consider multiple outcomes, some at the child-level (child sample) and at the household-level (household sample). Here, we describe our main outcome variables, both outcome indices and their components.

Child sample and educational outcomes The 2018 survey asked clients to report educational attainment and socio-economic outcomes (including residence, income, occupation and marital status) for all their children, including those living outside of the household. At this point, 314 of the 747 tracked clients reported at least one child living outside of the household. In our

[13] In the appendix, we also show intermediary outcomes based on data from a 2012 business survey conducted with our client sample.

[14] Between baseline and final survey 51 clients moved city, 6 could not be located, and 16 were not surveyed due to illness. For 18 of 19 dead clients, we interviewed another household member. 24 clients refused consent.

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.

We focus analysis on the "school-age" sample, i.e. children aged 7-17 years at baseline: they are old enough to have completed K-12 schooling by 2018 and young enough such that short-run treatment income gains could have impacted schooling investments (Appendix Figure A1 plots age distribution and enrollment status in 2018 by child age for the control group). Roughly half of households in our study sample has at least one child in this group (Appendix Table A2). Children younger than 7 years in 2007 and children older than 17 years in 2007 form the "young child" and the "old child" sample respectively.

Our primary educational outcomes are college attendance (or completion), secondary school completion, and schooling expenditures. We create an investment index which 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. When a child dropped out of school before completing secondary school, we asked about the dropout reasons. We categorize the dropout reasons into family, child, and marriage factors.

Our pre-analysis plan specified that we will examine intergenerational mobility based on parental education.[15] Our preferred indicator is our baseline literacy measure, which captures whether both parents can read and write.[16]

Household sample and enterprise outcomes Our enterprise outcome variables are from household surveys administered in 2010 and 2018. Our primary outcomes of interest are profits, capital, and labor. We asked clients to report on profits for up to five household enterprises; 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 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.

[15] While our pre-analysis plan specified parent and child health as additional outcomes of interest, we were unable to collect child health outcomes 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 analysis by whether client had completed fertility at baseline as 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 education investment index are concentrated among households where the client had a higher self-reported level of financial control, but no differences for other education outcomes.

[16] Appendix Table A3 compares literate and illiterate households along several baseline variables. Among households with at least one child aged 7-17 years at baseline, literate have more years of schooling, higher education and renovation expenses, and a higher socio-economic index. They are also more likely to have a savings account and a drain in the neighborhood. Literate households have fewer children at the point of the 2018 survey.

Finally, we combine these three outcomes into a business index. In order to assess households' trade-off between investing in schooling versus the enterprise, we focus our analysis on the sample of households with school-age children.

Household and child earnings The 2010 and 2018 surveys measured household income with the same question: "During the past 30 days, how much total income did your household earn?"[17] In 2018, we also collect information on children's income and other economic outcomes. Assessing treatment effects for these 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 at the time of our 2018 survey. 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. Our work outcomes include an indicator variable for whether the child worked in the past 30 days and, for daughters, whether the child reports being a housewife.

Finally, we construct three child labor measures. First, an indicator variable for whether a child dropped out of school early and started work before age 18. Second, we use the 2012 household roster and create a child labor indicator variable which is equal to one if a child below age 18 engaged in any work activity in the previous 30 days. Third, we create an indicator variable which is equal to one if a child is listed in a business employee roster from the 2012 survey 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 to reduce the number of individual hypotheses that we test. 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 which control for the expected share of rejections that are Type I errors — the false discovery rate (FDR) — for two outcome families. need to update The first family is comprised of 26 tests and includes all household-level economic outcomes and child-level education and socio-economic outcomes for the pooled school-age sample (Panel A of Tables 1, 3, 4, and 5). The second family is comprised of 78 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, 4, and 5).

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.

[17] We follow Field et al. (2013) and top-code household 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 cut-off point for our main child sample, which includes children aged 7-17 years at baseline. Treatment effects are concentrated in the main child sample with suggestive evidence that younger children — those with more exposure to the treatment — benefited more. Consistent with 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 expected educational status in 2018: as discussed in Section 2, the "school-age" sample includes children aged 7-17 years at baseline. The sample of above-17 children at baseline form the "old child" sample: 94% of these children had completed schooling at baseline. Finally, for children under 7 years at baseline ("young child" sample), we consider primary school outcomes. In 2018, 78% of control group children in the young child sample are still in secondary school.

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

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

Outcomes of interest Y_{ihg} include educational investment and attainment, as measured in the 11-year follow-up survey. T_g indicates whether the individual was in a treatment loan group, θ_g are stratification dummies for treatment group batch. Standard errors are clustered by loan group.

Panel A of Table 1 considers results for the school-aged child sample. The GP treatment led parents to substantially increase educational investments: treatment children score 0.20 standard deviations higher (significant at the 1% level) on an aggregate investment index (column 1). We find small and statistically insignificant treatment effects on primary-school investment (column 2). However, the secondary schooling investment index is 0.25 standard deviations higher for treatment children and statistically significant at the 1% level (column 3). Index sub-components include school expenses (admissions fees, school fees, uniforms, and textbooks) as well as after-school tutoring expenses. It also includes an indicator variable for whether children attended a private secondary school. Appendix Table A4 considers index components: Treatment children are more than twice as likely to attend private school. Their parents also spend an extra Rs. 3268.7

on secondary school tuition fees per child and Rs. 6464 on after-school tutoring per child when compared to their control group counterparts. Finally, treatment parents report 0.15 standard deviations higher college expenditures, significant at the 10% level (column 4).[18]

Increased human capital investments map onto enhanced schooling attainment for treatment children. Column (5), Panel A Table 1 shows that, relative to control children in same age group, children in treatment households are 9.8 percentage points more likely to have completed, or be currently enrolled in, college (now on, attend college). This amounts to a 34% increase in likelihood of college attendance compared with similarly aged control group children. As a benchmark, Duflo et al. (2017) 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 (columns 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 in control households (columns 1-4). Among children in control group, 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 0.24 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 college education impacted by treatment. We do not observe treatment effects on any educational outcome for the Old Child sample (panel C) and 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 school levels.

We now examine how our results on educational outcomes are impacted by FDR corrections to account for multiple hypothesis testing. Appendix Figures A4 and A5 plot the sharpened q-values against p-values for the first outcomes family (outcomes for the pooled school-age sample) and second outcomes family (outcomes for the school-age sample by parental education), respectively. In Panel A of the figures, we plot all of the tests with their corresponding q-values. In Panel B, we restrict analysis to p-values below 0.2 and in the text report effects for p-values below 0.1. In Panel A of Table 1, four coefficients are statistically significant at least at the 10% level. After the FDR correction, treatment effects for three of those outcomes — the overall investment index,

[18]We also find similar results when we look at household education expenditures in the past 30 days.

secondary school investment index, and college attendance — all remain significant at the 5% level. The q-value for college spending is 0.161.

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

Earlier, we argued that, relative to illiterate parents, we anticipate that literate parents 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). Further, treatment increases college attendance by 15.7 percentage points and secondary school completion by 12.1 percentage points among children of literate parents. This translates into a 0.5 years of additional schooling. Results on schooling completion and college attendance 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]

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. There are 12 results from Panel A of Table 2 which are statistically significant at the 10 percent level or lower (including joint tests reported at the bottom of the table). After the FDR correction, all but one of these coefficients continues to be statistically significant. The q-value on the grace period treatment effect for college spending (column 4) is 0.133.

Since our pre-analysis plan did not specify parental education cutoffs, we examine whether our

[19]We also see significant declines in years of schooling

main findings from our heterogeneity analysis hold for alternate measures of parental education. In Appendix Table A6, we show that our heterogeneity results hold under alternate specifications: 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, 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 separately estimate outcomes from 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 on 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-components: weekly enterprise profits, business capital and labor. Panel A of Table 3 consider

[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.

household enterprise outcomes in 2010 for the full sample. Columns (1)-(4) show that treatment households have 0.19 standard deviations higher score on business index and this captures gains in business profits and capital. In the short run, treatment led to income gains across the pooled sample. On average, treatment households have 0.22 standard deviations higher scores on the business index than control group households. We see no impact on 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 87% 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 Panel 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 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 0.28 increase in business index score and literate households see no effect on business outcomes (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, 0.64 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.[23] For children of literate parents — or, in other words, for children who are more

[21] Appendix Table A9 shows the results for alternative measures of parental education. For 3 of the 4 measures, the results are qualitatively similar. 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. do we want to discuss this in more detail?

[23] Since we did not collect information about the perceived returns to schooling, we cannot distinguish whether the patterns by parental education are driven by differences in actual or perceived returns

likely to have higher (perceived) returns to schooling — increased ability to afford educational investments outweighs increased returns in the household business. Meanwhile, for children of illiterate parents, (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. 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 counterparts in the control group and to treatment children of more highly educated parents). In 2012, we collected a detailed module of household and non-household employees in household businesses—it included a listing of all family and non-family workers in each business from inception until the day of the survey. We construct a dummy for whether children below the age of 18 worked in the past 30 days (column 5). We also construct an indicator variable for whether a child was identified as having ever worked in the household enterprise (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.[24]

Lastly, we discuss the results of the FDR corrections. In Panel A of Table 3, results on profits, the business index, and capital are all statistically significant for outcomes in 2010. The difference in profits is robust to the FDR adjustment. The q-value on the business index coefficient is 0.108 and the q-value on the capital coefficient is 0.19. In Panel B, 8 of the 12 outcomes that were statistically significant to be so after the FDR correction. Importantly, the observed divergence in enterprise outcomes between illiterate and literate households in 2018 is robust to the adjustment. Illiterate treatment households have significantly higher profits, number of workers, and business index than illiterate control households (the q-value on the difference in capital is .125) and we cannot reject that the treatment gap for literate households has closed.

[24] 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.

3.3 Alternative channels of influence

Our preferred interpretation for differences in investment decisions by parental literacy is that differences in perceived returns to schooling drive the results. 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 child educational attainment for children aged 25 years or older in our control group. Conditional on child schooling, parent literacy does not have a separate effect on child earnings suggesting that differences in actual returns to education are less likely (not shown). An alternatively explanation could be that more educated parents have lower discount rates. This can influence schooling investment decisions since returns to schooling are usually realized much latter than returns to alternative 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 has 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 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. Another possibility is that with non-aligned spousal preferences, the intervention — which targeted loans to women — may have increased education expenditures by increasing a woman's bargaining power within the household. However, we do not observe treatment impacts on female empowerment in the 2018 survey (not shown).[25]

4 Did household investment choices impact longer-run earnings?

We examine, in turn, earnings evolution at the household and child-level and treatment effects therein.

[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).

4.1 Household earnings

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), Panel A replicates the finding in Field et al. (2013) - in 2010, treated households saw a 16.6 percent increase in household 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.

Panel B column (1) shows that, for the full sample, increase in earnings in 2010 is indistinguishable across literate and illiterate households. But by 2018 we see treatment effects fully diverge depending by parental literacy: 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, but starker, patterns for the school-going sample with the heterogeneity in treatment effects already apparent in 2010 (potentially pointing to differences in investment choices having 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 A12 – with the caveat that many children are still transitioning into the labor market. In 2018, 17% of the school-aged control child sample in was still studying with the number significantly higher for the treatment group (column 1, Panel A). On this outcome, we don't observe significant differences across literate and illiterate households. However, we do observe heterogeneous treatment effects by household literacy for living arrangements - in the literate household sample, treatment children are more likely 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) 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 noisily estimate differences in treatment effects by parental literacy wherein work incidence is higher by treatment for children from illiterate household. Columns (9) and (10) suggest this may reflect differences in type of job - salaried work, which is more common among more educated children, is often associated with longer job search. Labor supply outcomes for the sample of children who were over 18 is consistent with this interpretation (in Panel D).

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

[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.

income among literate households. Meanwhile, illiterate parent households' continued enterprise investments leads to income gains even 11 years post-intervention.

4.2 Child earnings

A. Treatment effects

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 still in school at the time of the 2018 survey, estimated treatment effects on child income are likely biased downwards. Assuming that the 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., 2017). 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 accrued due to educational attainment. That said, reflecting the fact that many children are still studying the estimates are quite noisy. In Panel C, we observe a marginally significant increase in the income of men in the older child sample (aged 18 years or older at baseline) which further supports this argument. Panel D shows no evidence of heterogeneity by household illiteracy for sons. Interestingly, we do see some evidence of heterogeneity among daughters in this sample which is likely linked to marriage market outcomes, in part.

B. Evaluating lifetime treatment gains for children

As a final step, we estimate the inter-generational benefits of the GP treatment by forecasting treatment effects on total 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).[27] 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 IHDS data (Appendix Figure A6), we regress annual earnings against a linear

[27] We rely on IHDS since our control group sample is small and does not span all age groups. Appendix Figure A7 shows the age-earnings IHDS curves are comparable to age-earnings curves in our data.

age term separately for school dropouts, secondary school graduates, and college graduates.[28]

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% and assume no growth in real wages. We obtain average annual costs for the last three years of secondary school, including after-school tutoring, (Rs. 9275) and college (Rs. 4241) from the control group in the 2018 survey.

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 the pooled sample and separately for literate and illiterate households. The first and second row computes these earnings for control and treatment group and row three reports the treatment gain. For the pooled sample, we estimate a treatment-induced increase in the net-present value of lifetime earnings of 2156 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 of -1,800 USD PPP while the earning gains associated with college education mean that children of literate parents experience a large increase in total lifetime earnings of 4027 USD. 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. This is supported by the lack of treatment effects on school performance, even conditional on school completion.

In columns (4)-(6) we reestimate lifetime earnings 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 means of treatment group child earnings in the 2018 survey by the raw means of control group child earnings conditional on educational attainment (Appendix Table A14). We assume that relative economic returns of the treatment is constant over a child's lifetime. Our results are consistent with the idea that unskilled children, defined as children without a college degree, benefit economically from the treatment by inheriting larger household businesses. When we follow this approach, average treatment gains in the net-present value of lifetime earnings increase by 4659 USD PPP for the pooled sample. Column (5) now shows a net gain in lifetime earnings for treatment group children of illiterate households. Figure 5 shows the resulting income trajectories for each group.

The estimates in Table 5 also help forecast whether the treatment amplified the inter-generational earnings gap between literate and illiterate households. We focus on Case 2 and compare the

[28]Panel A of Appendix Table A14 show the corresponding estimates.

earnings gap between literate and illiterate household children across control and treatment. For control households, earnings of children from literate household are 13% higher that children from illiterate households (row A, columns 5 and 6). For treatment households this earnings differential rises to 31% (row B columns 5 and 6). Thus, treatment raises earnings for the next generation but also reduces inter-generational earning mobility. Following (Asher et al., 2021), we calculate bottom half mobility as a measure of inter-generational educational mobility.[29] We find that the treatment decreased bottom half mobility from 0.382 to 0.367.

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 inter-generational 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 grace period contract has an internal rate of return (17.6%) and the cost-benefit ratio (170). By comparison, (Hicks et al., 2020) estimate that deworming has an internal rate of return of 36.7% and (Parker and Vogl, 2021) find that Progresa has a cost-benefit ratio of 2.8.

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 panel structure of the Indian Human Development Survey (IHDS) to examine the relationship between household income in 2005 and children's college attendance in 2012. In Figure A8, we plot a kernel-weighted local polynomial regression of whether a child had attended college in 2012 on household income in 2005.[30] 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 s-shaped relationship between baseline wealth and college attendance in our own sample.

Our results also shed light on the dynamics of educational inequality and on the link between economic and educational mobility. Policymakers may care about inequality effects of interventions, rather than focusing exclusively on average treatment effects (Deaton and Cartwright, 2018).

[29] Bottom half mobility is defined as the expected rank of a child born to a parent in the bottom half of the education distribution.

[30] 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 husbands' home after marriage and would therefore be disproportionately absent from the IHDS household roster in 2012.

Since education gains are concentrated among high-education households, educational inequality increased as a result of treatment among children in our sample. Preventing the emergence of such inequality would potentially require additional interventions, including conditional transfers targeted towards children of low-education parents.

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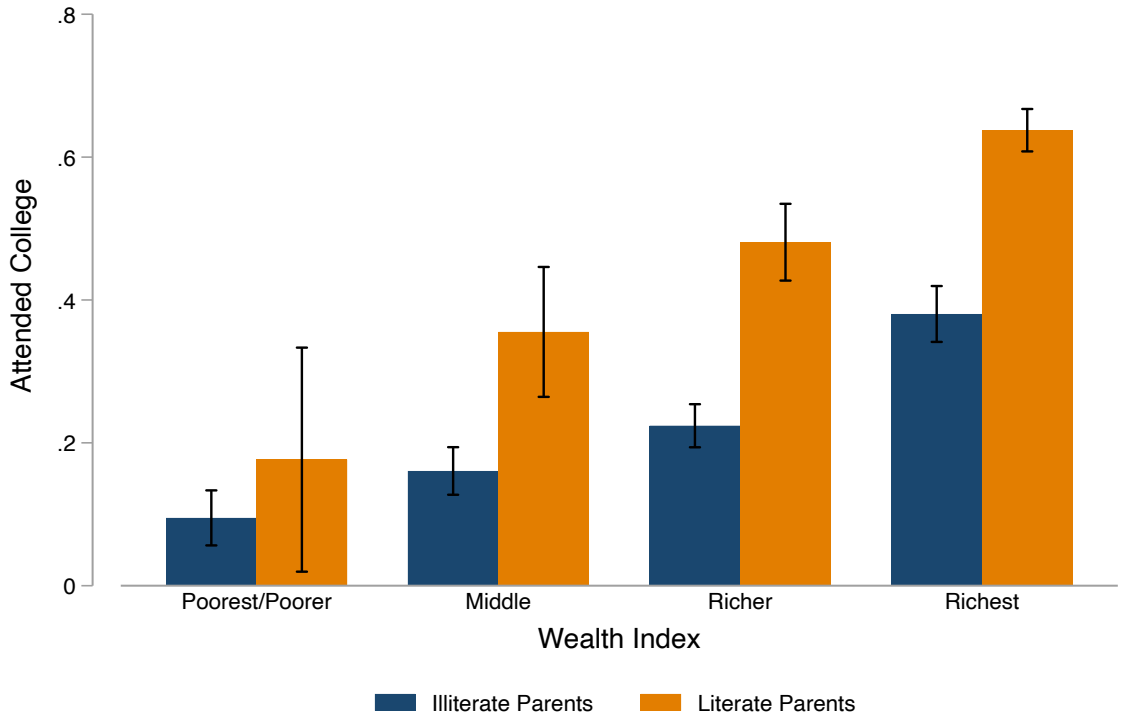
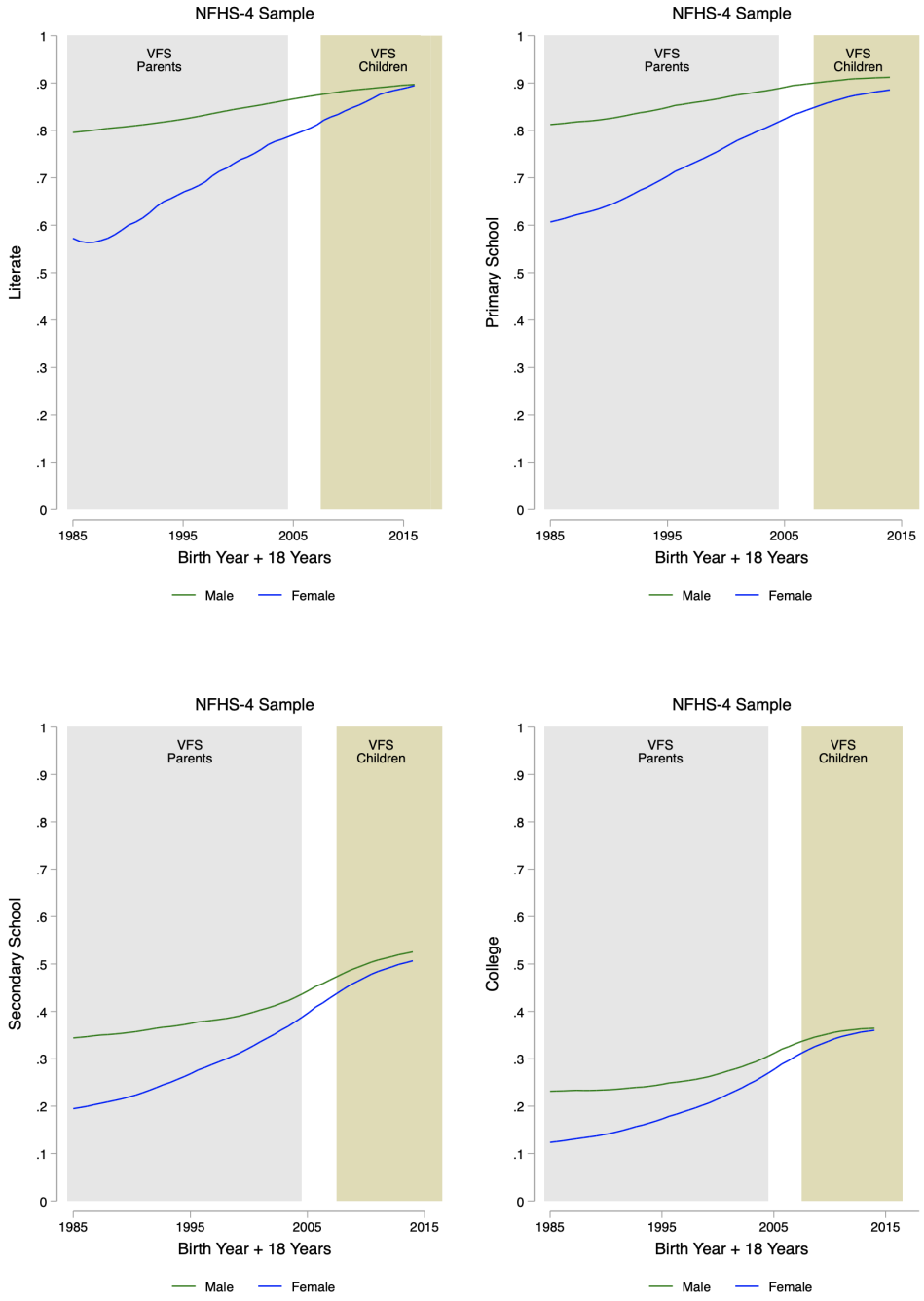
Figure 1: Relationship between College Attendance and Household Wealth by Parental Literacy in NFHS



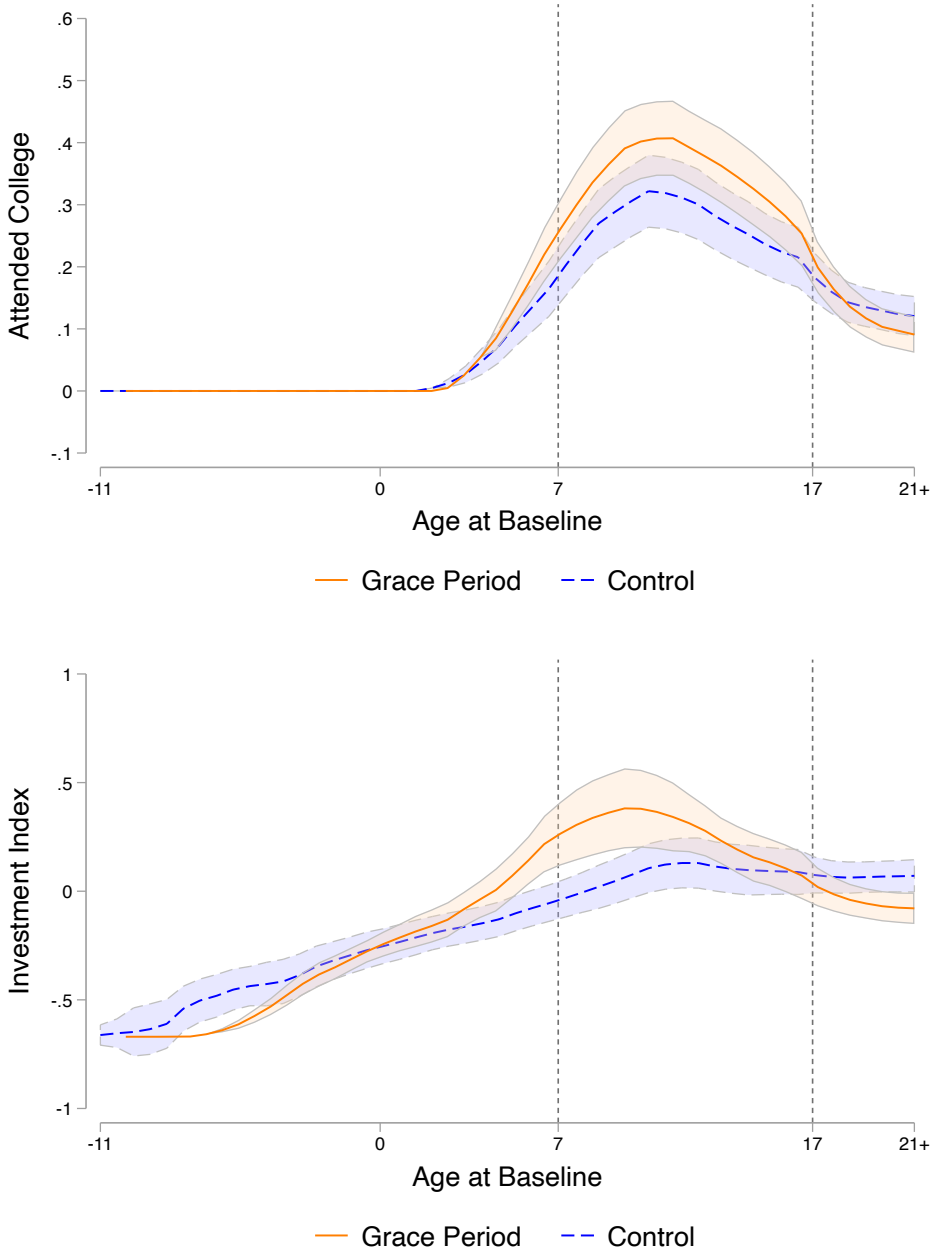
Notes: The figure plots average college attendance rates by wealth quintiles and parental literacy. We combine the lowest two wealth quintiles due to a low number of observations for children of literate parents in these groups. The range plots correspond to 90 percent confidence intervals. The Data comes from the National Family Health Survey-4. We restrict the sample to men aged 19-25 years who live in urban areas.

Figure 2: Educational Trends in India NFHS-4 (2015-16)



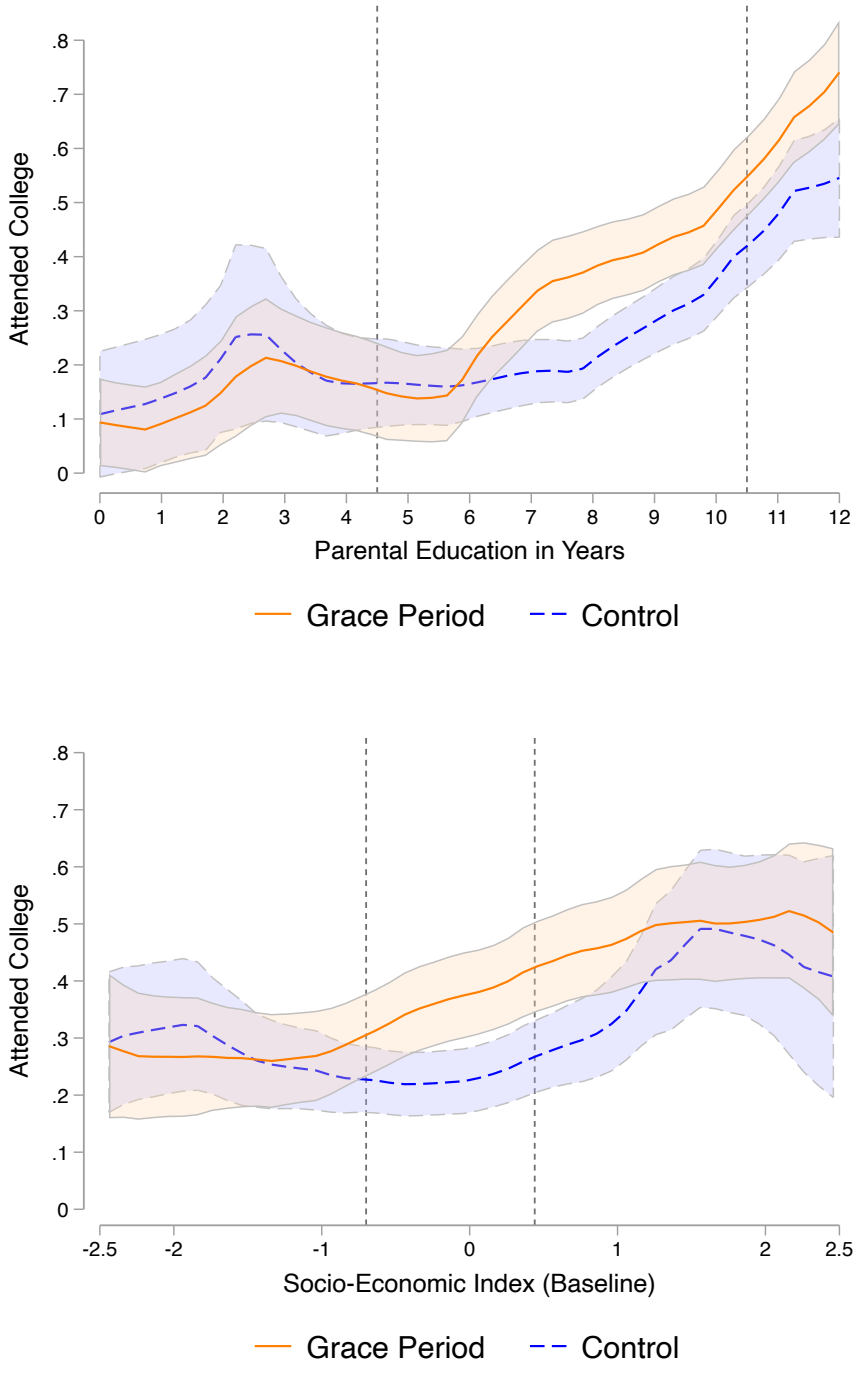
Notes: The figures plot local regressions. Data consists of all household members aged 18-80 urban areas in NFHS-4. The x-axis shows the year in which the person turned 18 years. The green lines correspond to men and the blue lines correspond to women The golden-shared area shows the age range of the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

Figure 3: Childhood Exposure



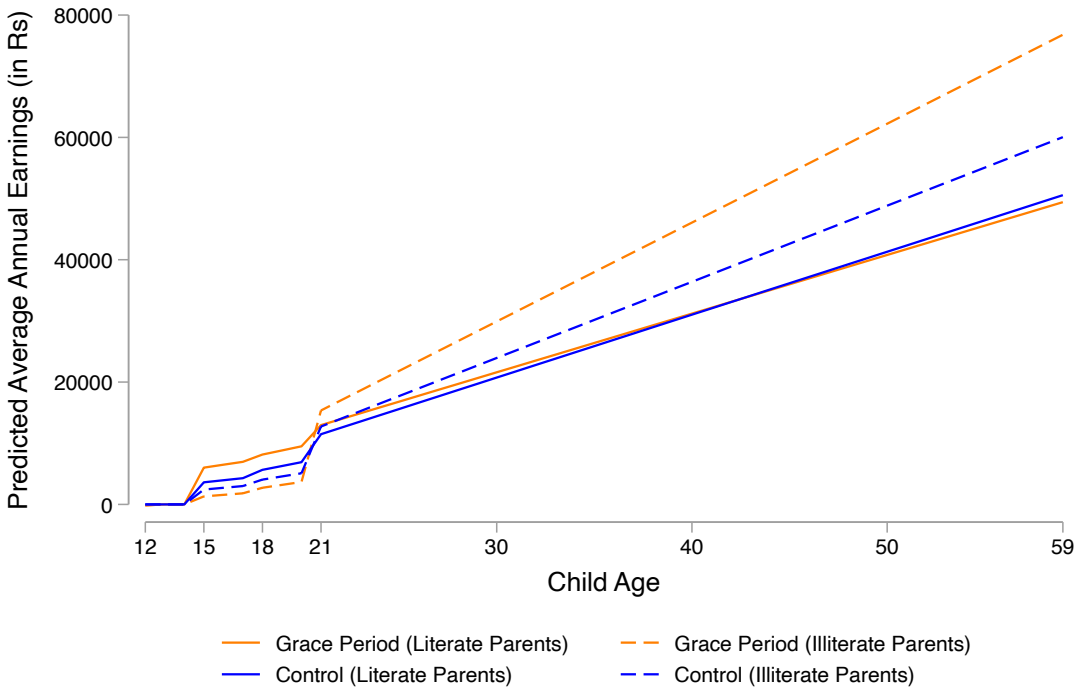
Notes: The figures plots local regressions. The sample consists of all children of the client that were still alive at the time of the 2018 survey. The x-axis shows age at baseline. Negative numbers indicate the number of years that the child was born after baseline. The dotted vertical lines indicate 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 Enrollment 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. See Data Appendix for detailed variable definitions.

Figure 5: 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. Linear age-earning curves for secondary school dropouts, secondary school graduates, and college graduates are estimated based on the urban IHDS-2 sample (see Appendix Figure A6). 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.

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 (Standardized)	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	-0.049	0.014	0.020
	(0.065)	(0.071)	(0.067)	(0.083)	(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 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 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	Profits	Capital	Number of Workers	Business Index	Profits	Capital	Number of Workers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(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 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 in both panels 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 (1)	2018 (2)	2010 (3)	2018 (4)	Male (5)	Female (6)
Panel A: Pooled						
Grace Period	0.166**	0.071	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					1145.820*	-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 1-2 are run on the household level and the regressions in columns 3-4 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 child sample in Panels A and B (columns 3-4) consist 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. The household sample in Panels A and B (columns 1-2) consists of all households with at least one child aged 7-17 years at baseline according to the 2018 survey. 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.

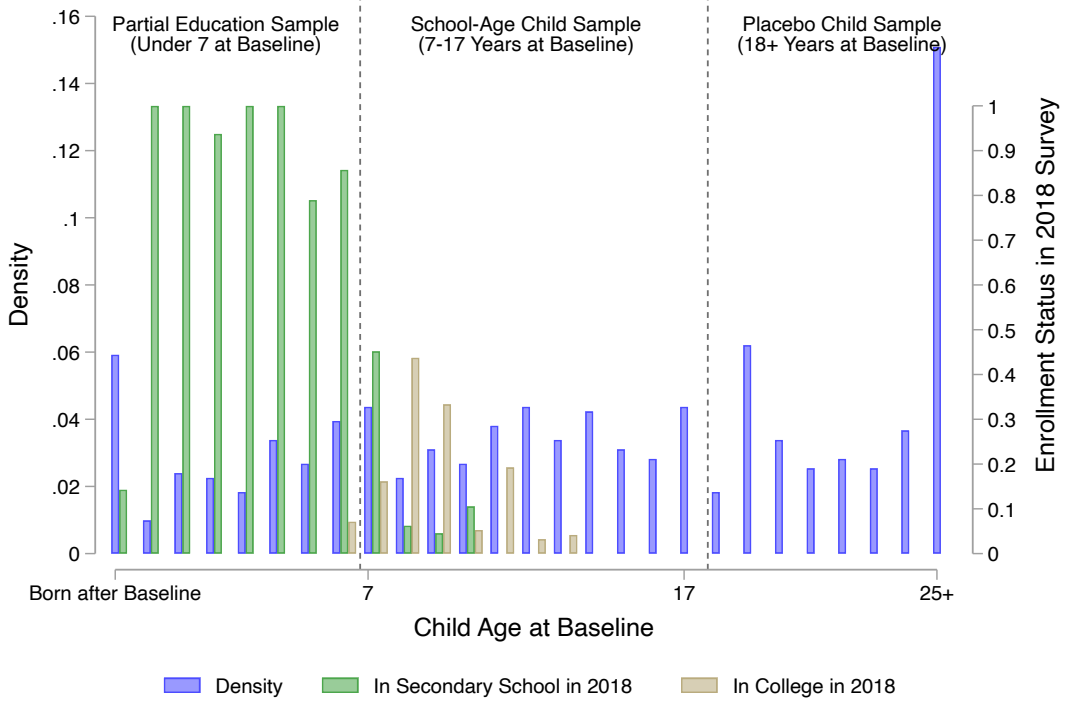
Table 5: Welfare Analysis

	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	307379.8	278756.8	316101.5	307379.8	278756.8	316101.5
(in USD PPP)	(26173.3)	(23736.1)	(26916.0)	(26173.3)	(23736.1)	(26916.0)
B: Net-Present Value of Private Lifetime Earnings (Treatment) in INR	332707.5	257621.3	363395.2	362098.8	295225.0	389526.6
(in USD PPP)	(28330.0)	(21936.4)	(30943.1)	(30832.7)	(25138.4)	(33168.1)
C: Treatment Gains (B-A)	25327.7	-21135.4	47293.7	54719.0	16468.3	73425.1
(in USD PPP)	(2156.7)	(-1799.7)	(4027.1)	(4659.3)	(1402.3)	(6252.1)
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)	170.0		317.4	367.2	110.5	492.8
F: International Rate of Return (in %)	17.6		15.5	28.9	97.5	20.2

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 5 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%.

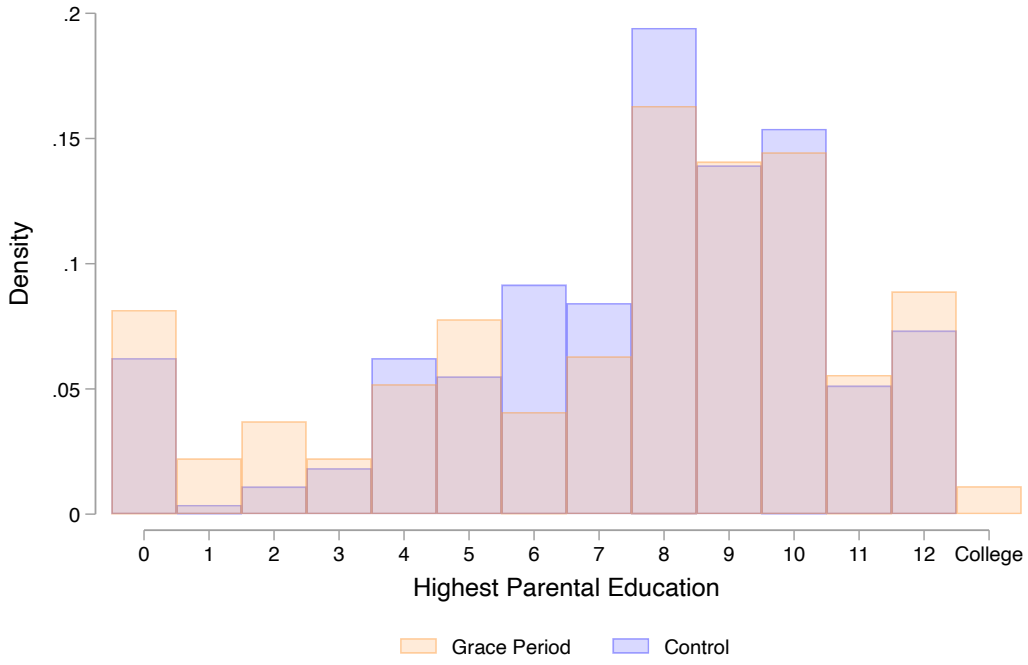
A. Appendix Tables and Figures: Additional Analysis

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



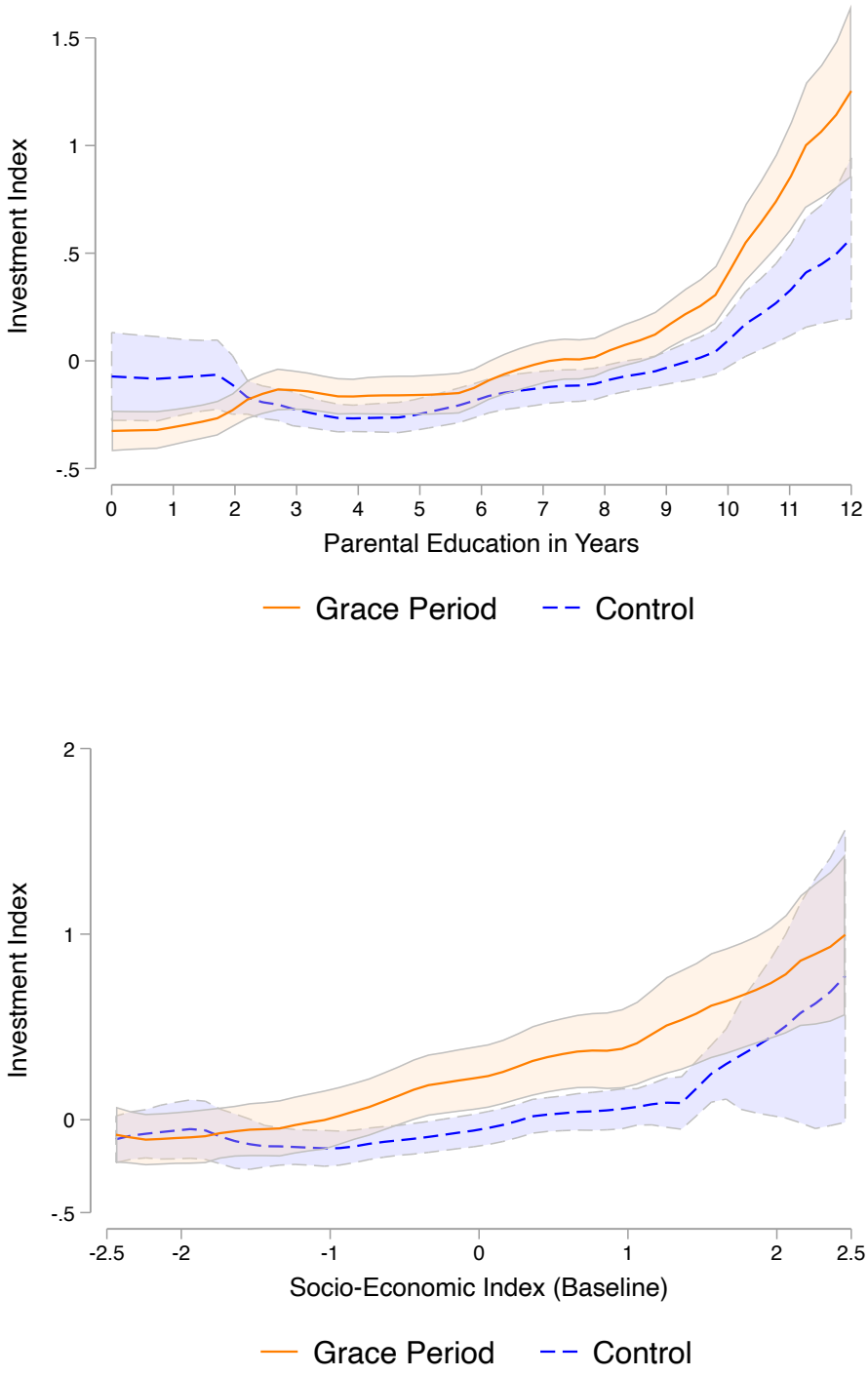
Notes: The blue bars show the distribution of child age at baseline in the control group and correspond to the left y-axis. The green bars shows the share of children enrolled in secondary school at the point of the 2018 survey. The brown bars shows the share of children enrolled in college at the point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

Figure A2: Histogram of Parental Education



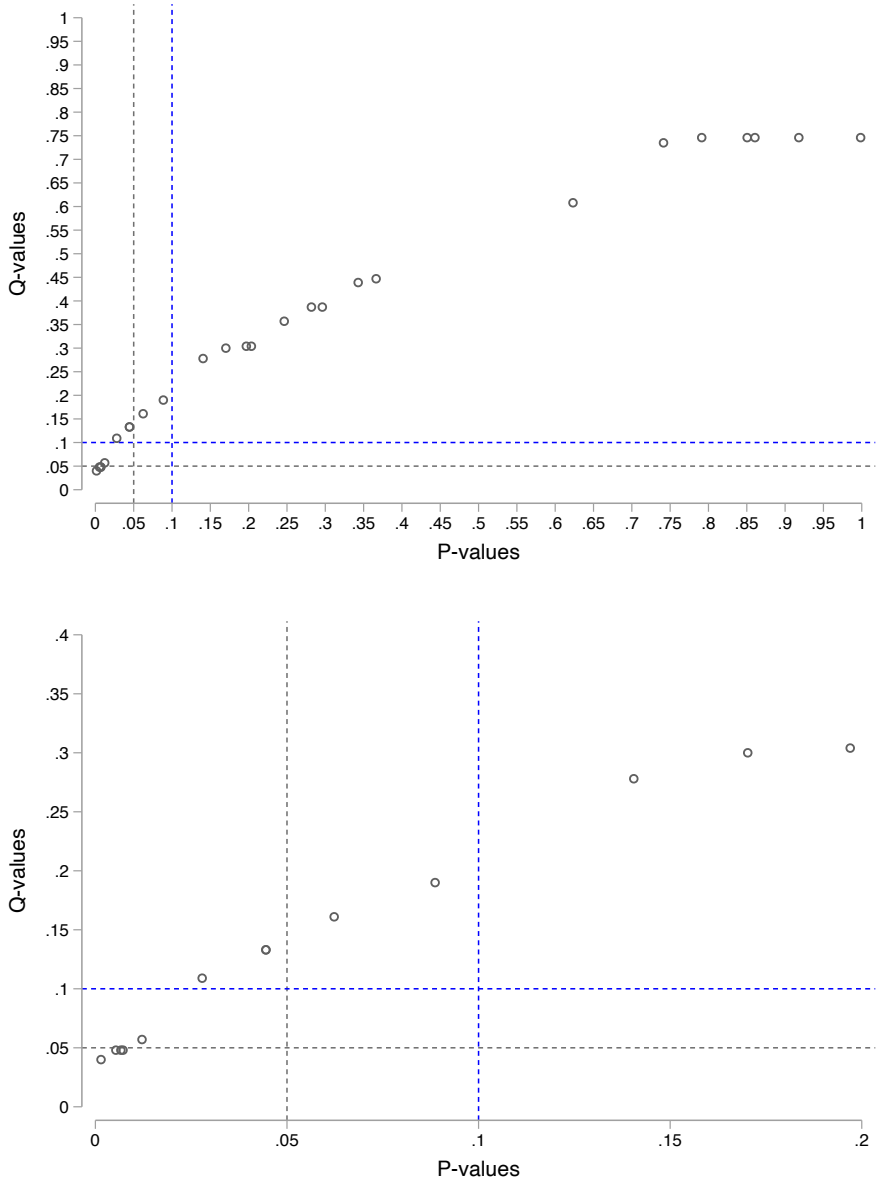
Notes: The histogram is generated on the child level. The sample consists of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey.

Figure A3: Investment Index by Parental Education and Baseline Wealth



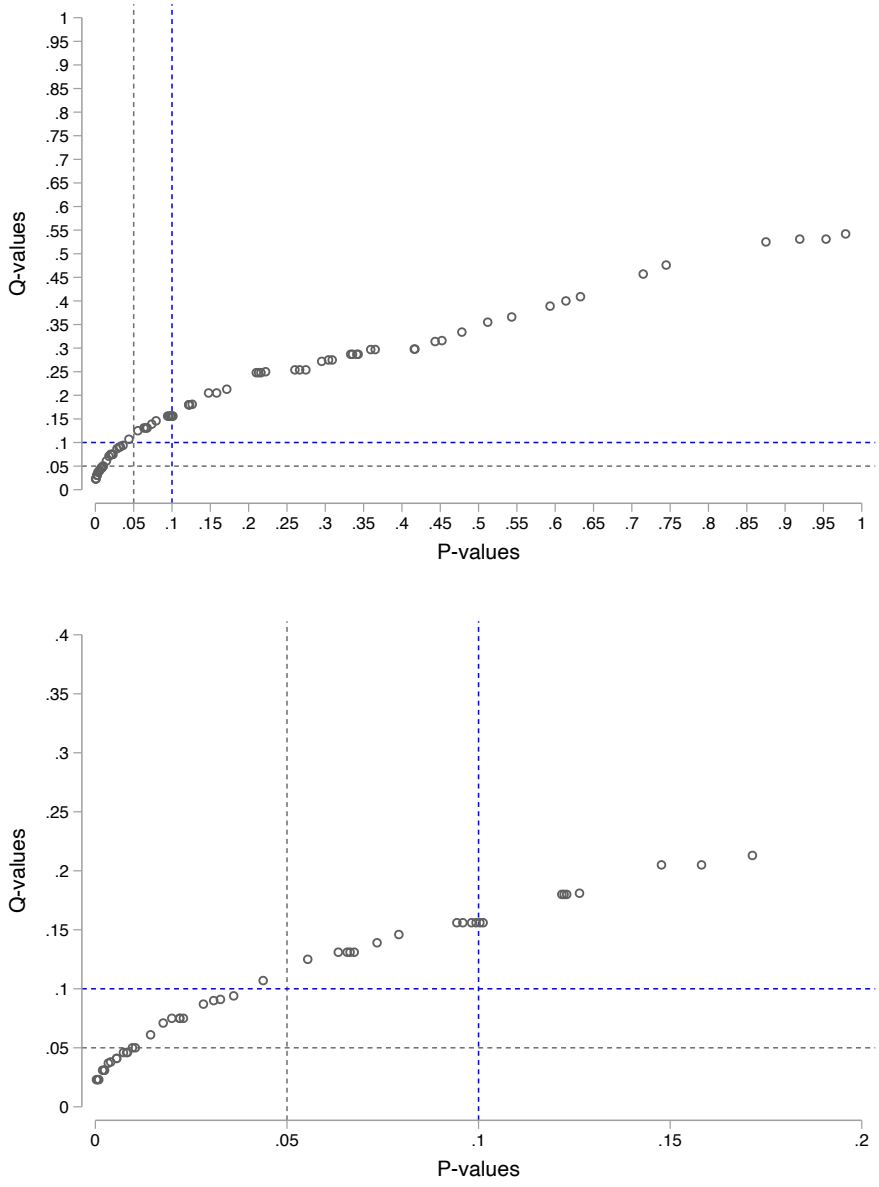
Notes: The figures plots local regressions. The sample consists of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey. The shaded areas in the figure correspond to 90 percent confidence intervals that are not adjusted for clustering. See Data Appendix for detailed variable definitions.

Figure A4: Corrections for Multiple Hypothesis Testing for Pooled Analysis



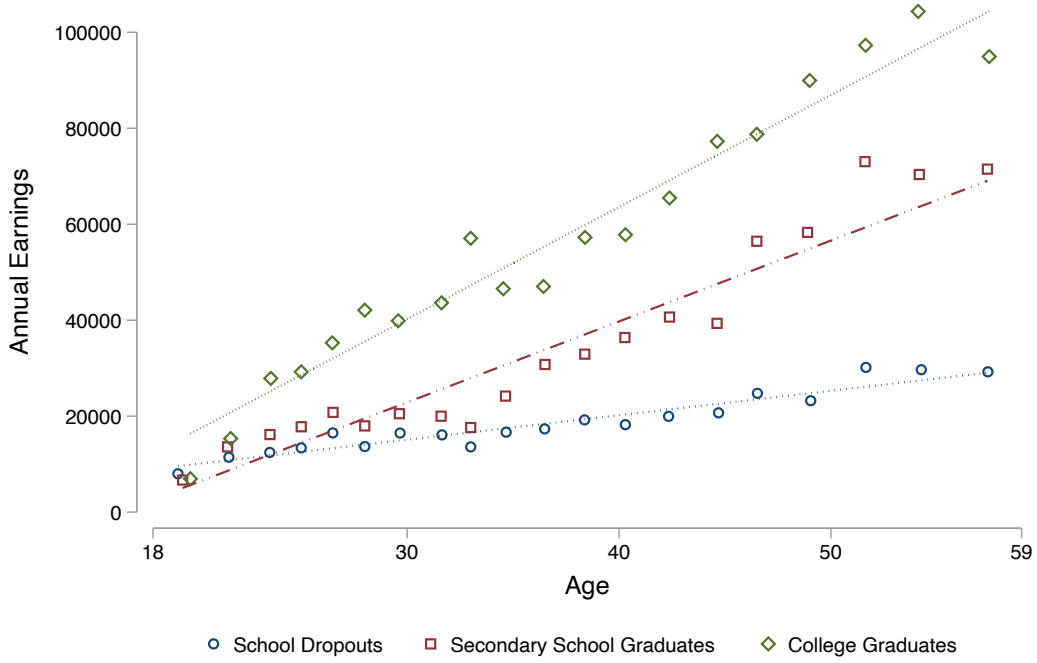
NEED TO UPDATE Notes: The figures plots sharpened q-values against unadjusted p-values. The lower figure only shows tests with an unadjusted p-value of 0.2 or less to improve readability.

Figure A5: Corrections for Multiple Hypothesis Testing for Heterogeneity Analysis



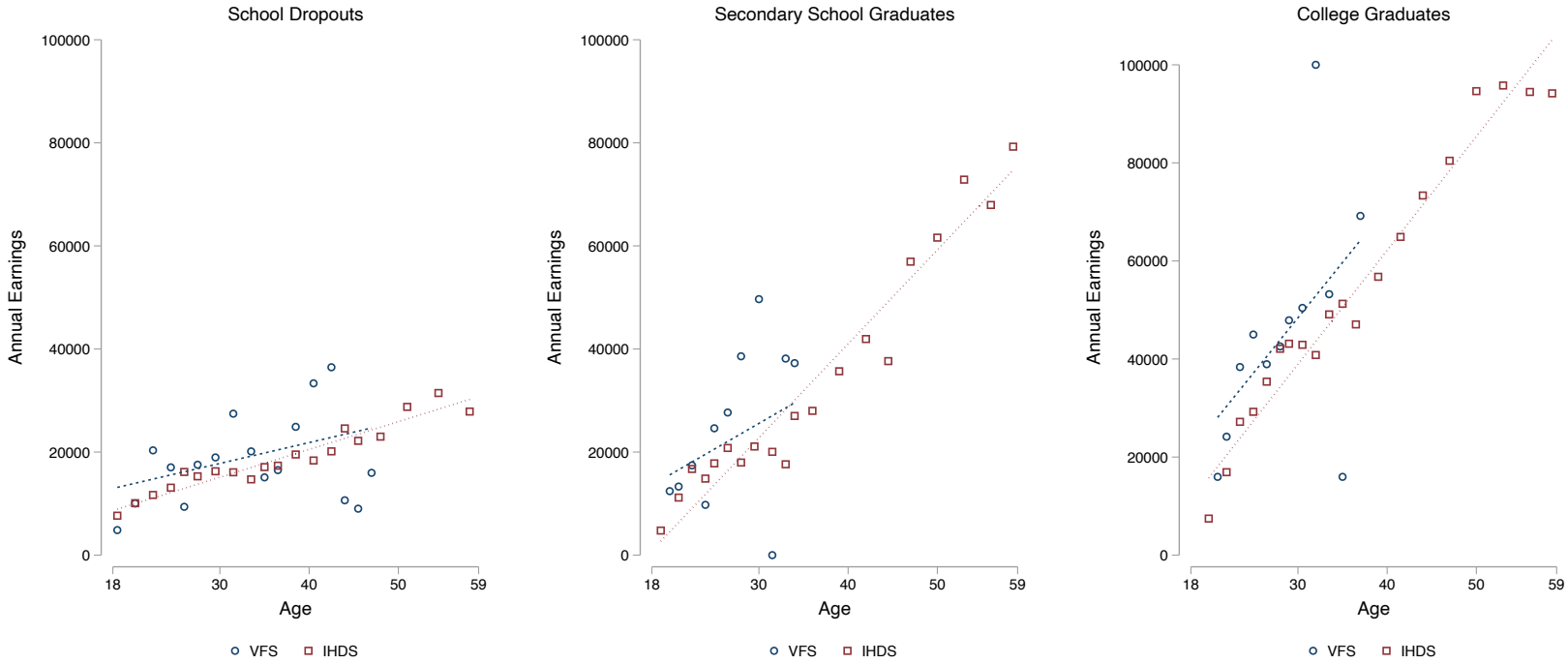
NEED TO UPDATE Notes: The figures plots sharpened q-values against unadjusted p-values. The lower figure only shows tests with an unadjusted p-value of 0.2 or less to improve readability.

Figure A6: Age-Earning Curves in IHDS



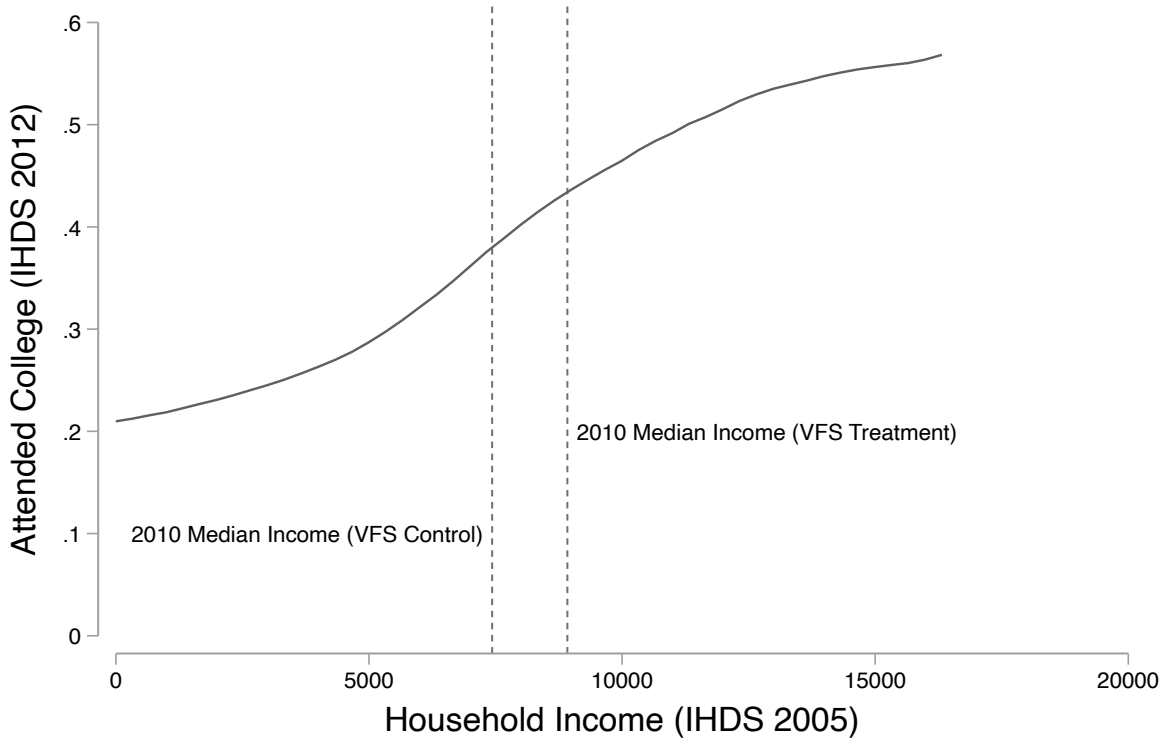
Notes: The figures plots binscatter graphs with annual earnings on the y-axis and age at the x-axis for secondary school dropouts, secondary school graduates, and college graduates in urban India. The data is obtained from the India Human Development Survey Wave 2 and the sample consists of all household 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 A7: Age-Earning Curves in IHDS vs VFS



Notes: The figures plots binscatter graphs 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 sample. The dots correspond to binned means and the dashed lines correspond to fitted lines based on linear regressions.

Figure A8: Relationship between College Attendance and Household Income in IHDS



Notes: The figure plots local regressions. Data comes from the Indian Human Development Survey (IHDS). 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 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)
Socioeconomic Index (PCA)	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	St. Dev.	Coeff.	St. Err.	N	Mean	St. Dev.	Coeff.	St. Err.	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(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										
Socioeconomic Index (PCA)	-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
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			0.107					0.562		
Panel C: Child-Level Controls										
Female	0.487	[0.500]	-0.017	(0.027)	1401	0.505	[0.501]	-0.012	(0.045)	544
Birth Order	1.774	[0.977]	-0.008	(0.071)	1401	1.769	[0.975]	-0.016	(0.105)	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
High Education Parents	0.875	[0.331]	-0.043	(0.028)	839	0.886	[0.319]	-0.079**	(0.039)	380
Above-Median Education Parents	0.682	[0.466]	-0.017	(0.039)	839	0.668	[0.472]	-0.023	(0.055)	380

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. 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 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
Socioeconomic Index (PCA)	-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

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	0.050***	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	0.042	1632.315	1328.386	0.060**	1116.580	6154.425**	1465.640
	(0.056)	(1653.203)	(1043.018)	(0.024)	(2006.738)	(2642.503)	(1669.716)
Grace Period × Female	0.023	-1117.201	-2473.172*	-0.021	1452.541	-2798.642	982.557
	(0.067)	(2157.912)	(1356.762)	(0.032)	(2985.100)	(3436.033)	(2263.455)
Female	-0.028	-601.921	2381.262**	-0.004	-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	-0.008	-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	0.089***	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	0.020*	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	-3879.990	-394.601
	(0.032)	(1186.367)	(2109.039)	(0.013)	(1514.321)	(3756.239)	(669.352)
Panel E: Young Child Sample (Under 7 Years at Baseline)							
Grace Period	0.045	267.161	394.888				
	(0.054)	(1,256.940)	(624.056)				
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	0.219	0.701	0.283	0.109	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, dummies dead client 2018 survey dummies, 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-C consists 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 consist 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. School fees, after-school tutoring, and college spending are top-coded at 99.5%. 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	0.170**	0.056	0.232***	0.160*	0.096**	0.046
	(0.071)	(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	0.086**	0.019
	(0.073)	(0.078)	(0.075)	(0.082)	(0.040)	(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 dummies, dead client 2018 survey dummies, 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 and Appendix Table A4 for treatment effects on index components. * 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 (Standardized)	Attended College	Completed Secondary School
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Parental Education in Years						
Grace Period	-0.305	-0.204	-0.251	-0.181	0.008	-0.138
	(0.217)	(0.175)	(0.190)	(0.279)	(0.098)	(0.096)
Grace Period × Parental Education in Years	0.070**	0.038	0.073***	0.051	0.014	0.026**
	(0.031)	(0.024)	(0.028)	(0.042)	(0.012)	(0.011)
Parental Education in Years	0.021	0.011	0.043**	0.067**	0.034***	0.023***
	(0.022)	(0.018)	(0.019)	(0.029)	(0.009)	(0.009)
Panel B: Parent Attended Secondary School						
Grace Period	-0.172	-0.027	-0.210	-0.268	-0.091	-0.184*
	(0.141)	(0.143)	(0.140)	(0.180)	(0.098)	(0.105)
Grace Period × Parent Attends Secondary School	0.434***	0.128	0.562***	0.534**	0.231**	0.277**
	(0.153)	(0.160)	(0.155)	(0.216)	(0.102)	(0.109)
Parent Attends Secondary School	-0.190	0.038	-0.219**	-0.144	-0.048	-0.110
	(0.120)	(0.128)	(0.110)	(0.173)	(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	0.018	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, dummies dead client 2018 survey dummies, 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. 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: Correlational Evidence from Control Group

	Control Group Children (7+ Years at Baseline)					
		Investment Index Components				
	Investment Index	Primary School Investment Subindex	Secondary School Investment Subindex	College Spending (Standardized)	Attended College	Completed Secondary School
	(1)	(2)	(3)	(4)	(5)	(6)
Literate Parents	0.254***	0.191***	0.265***	0.125	0.133***	0.161***
	(0.057)	(0.057)	(0.053)	(0.082)	(0.035)	(0.043)
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)
Mean	-0.000	-0.000	-0.000	0.000	0.203	0.314
Observations	484	484	484	470	483	484

Notes: 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. All regressions include dead client 2018 survey dummies. Robust standard errors appear in parentheses. 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 Survey

		Index Components			
	Business Index	Profits	Capital	Number of Workers	Log Income
	(1)	(2)	(3)	(4)	(5)
Panel A: Full Household Sample, Pooled					
Grace Period	0.133**	73.850	13546.167**	0.187*	0.113**
	(0.057)	(134.390)	(5601.287)	(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

NEED TO UPDATE Notes: Panel A consists of households that were surveyed across all survey rounds (2010, 2012, and 2018) and Panel B consists of households that had at least one child aged 7-17 years at baseline according to the 2018 survey. Standard errors are clustered by loan group and appear in parentheses. All regressions include survey wave dummies, stratification dummies, dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. Profits and capital are top-coded at 99.5% for each survey round 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 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	0.323**	0.109	0.044	0.236*	0.764***	0.501*	0.081	0.095
	(0.165)	(0.186)	(0.159)	(0.129)	(0.278)	(0.265)	(0.188)	(0.155)
Grace Period × Parental Education in Years	-0.014	0.009	-0.000	-0.020	-0.067**	-0.037	-0.001	0.001
	(0.019)	(0.021)	(0.017)	(0.014)	(0.031)	(0.030)	(0.022)	(0.018)
Parental Education in Years	0.009	0.020	-0.002	0.026**	0.037**	0.036*	-0.003	0.027**
	(0.012)	(0.015)	(0.014)	(0.011)	(0.018)	(0.020)	(0.015)	(0.013)
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*	0.625**	0.556**	0.369**
	(0.190)	(0.197)	(0.162)	(0.136)	(0.343)	(0.249)	(0.219)	(0.158)
Grace Period × Literate Mother	0.045	-0.036	-0.249	-0.320**	-0.435	-0.498*	-0.543**	-0.308*
	(0.205)	(0.214)	(0.171)	(0.144)	(0.356)	(0.266)	(0.234)	(0.171)
Literate Mother	-0.008	0.085	0.081	0.202*	0.227*	0.445**	0.281***	0.265**
	(0.107)	(0.151)	(0.118)	(0.121)	(0.128)	(0.185)	(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	0.254	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 are run on the child level and include stratification, dummies dead client 2018 survey dummies, 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. 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	0.020	-0.016	-0.006	-0.027	0.014	-0.008
	(0.037)	(0.037)	(0.027)	(0.033)	(0.040)	(0.035)
Panel B: Heterogeneity by Parental Literacy						
Grace Period	0.202**	-0.012	0.094*	0.037	0.106	0.084
	(0.094)	(0.083)	(0.051)	(0.077)	(0.109)	(0.072)
Grace Period × Literate Parents	-0.255**	-0.017	-0.132**	-0.101	-0.113	-0.123
	(0.105)	(0.088)	(0.060)	(0.080)	(0.108)	(0.082)
Literate Parents	0.060	-0.159**	0.030	0.025	-0.123*	0.027
	(0.062)	(0.067)	(0.039)	(0.058)	(0.068)	(0.053)
p-value: Grace Period + Grace Period x Literate Parents	0.199	0.455	0.238	0.055	0.850	0.297
Mean of Omitted Group (Panel A)	0.202	0.210	0.112	0.201	0.104	0.121
Mean of Omitted Group (Panel B)	0.161	0.321	0.089	0.220	0.185	0.103
Observations	532	532	532	544	247	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 dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The sample consists of children of the client aged 7-17 at baseline that are still alive at the time of 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 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	0.251**	-0.042	0.018	0.102	0.242**	0.125
	(0.107)	(0.109)	(0.065)	(0.084)	(0.111)	(0.078)
Grace Period × Parental Education in Years	-0.032**	0.002	-0.004	-0.018**	-0.028**	-0.018**
	(0.013)	(0.013)	(0.008)	(0.009)	(0.012)	(0.009)
Parental Education in Years	0.001	-0.023**	-0.010*	-0.006	-0.005	0.008
	(0.009)	(0.009)	(0.006)	(0.007)	(0.006)	(0.005)
Panel B: Parent Attended Secondary School						
Grace Period	0.276***	-0.075	0.006	0.091	0.295***	0.132*
	(0.098)	(0.100)	(0.064)	(0.079)	(0.107)	(0.072)
Grace Period × Parent Attends Secondary School	-0.318***	0.062	-0.020	-0.154*	-0.330***	-0.172**
	(0.108)	(0.111)	(0.068)	(0.085)	(0.114)	(0.077)
Parent Attends Secondary School	0.108	-0.104	-0.070	0.029	0.047	0.106***
	(0.070)	(0.080)	(0.049)	(0.061)	(0.054)	(0.034)
Panel C: Mother's Literacy						
Grace Period	0.268**	-0.072	0.086	0.006	0.108	0.081
	(0.109)	(0.108)	(0.067)	(0.079)	(0.141)	(0.102)
Grace Period × Literate Mother	-0.306***	0.063	-0.108	-0.053	-0.105	-0.109
	(0.115)	(0.110)	(0.075)	(0.086)	(0.140)	(0.109)
Literate Mother	0.074	-0.137*	0.034	0.046	-0.165*	-0.002
	(0.078)	(0.083)	(0.050)	(0.065)	(0.093)	(0.071)
Panel D: Father's Literacy						
Grace Period	0.164	-0.046	0.085	-0.021	0.219*	0.064
	(0.105)	(0.110)	(0.077)	(0.105)	(0.117)	(0.078)
Grace Period × Literate Father	-0.170	0.021	-0.111	-0.037	-0.266**	-0.058
	(0.112)	(0.116)	(0.083)	(0.108)	(0.123)	(0.081)
Literate Father	0.136	-0.174**	-0.023	-0.028	0.107	0.051
	(0.085)	(0.086)	(0.063)	(0.076)	(0.106)	(0.048)
Panel A Statistics						
Mean of Omitted Group	0.105	0.421	0.316	0.211	0.000	0.000
Observations	532	532	532	544	247	529
Panel B Statistics						
p-value: Grace Period + Grace Period x Parent Attends Secondary School	0.321	0.753	0.623	0.074	0.438	0.274
Mean of Omitted Group	0.095	0.310	0.190	0.186	0.043	0.047
Observations	532	532	532	544	247	529
Panel C Statistics						
p-value: Grace Period + Grace Period x Literate Mother	0.320	0.792	0.476	0.211	0.936	0.441
Mean of Omitted Group	0.167	0.333	0.083	0.211	0.235	0.132
Observations	532	532	532	544	247	529
Panel D Statistics						
p-value: Grace Period + Grace Period x Literate Father	0.885	0.526	0.346	0.074	0.213	0.835
Mean of Omitted Group	0.100	0.333	0.167	0.226	0.063	0.033
Observations	532	532	532	544	247	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 dead client 2018 survey dummies, 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. 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	0.283**	0.110*	0.345***	0.081	0.357*	0.180
	(0.127)	(0.058)	(0.105)	(0.077)	(0.192)	(0.126)
Grace Period × Impatient	-0.144	-0.024	-0.298**	-0.060	-0.216	-0.177
	(0.173)	(0.084)	(0.134)	(0.103)	(0.224)	(0.155)
Impatient	-0.041	0.009	0.054	0.005	-0.024	-0.010
	(0.101)	(0.055)	(0.077)	(0.079)	(0.095)	(0.106)
Panel B: Heterogeneity by Parental Literacy and Socio-Economic Index						
Grace Period	-0.082	-0.053	0.143	0.278*	0.360	0.419**
	(0.115)	(0.068)	(0.151)	(0.152)	(0.243)	(0.167)
Grace Period × Literate Parents	0.439***	0.208***	0.084	-0.300*	-0.247	-0.460**
	(0.118)	(0.074)	(0.163)	(0.163)	(0.258)	(0.182)
Grace Period × Socio-Economic Index	0.038	0.015	-0.033	0.007	-0.105	0.013
	(0.101)	(0.034)	(0.060)	(0.045)	(0.100)	(0.074)
Literate Parents	0.122	0.056	-0.014	0.075	0.190*	0.225***
	(0.087)	(0.055)	(0.082)	(0.093)	(0.102)	(0.087)
Socio-Economic Index	0.125	0.007	0.051	0.054*	0.115**	0.058
	(0.086)	(0.024)	(0.035)	(0.032)	(0.055)	(0.054)
Panel A Statistics						
p-value: Grace Period + Grace Period x Literate Parents	0.178	0.166	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	0.717	0.360	0.662
Mean of Omitted Group	-0.236	0.169	-0.031	-0.082	-0.198	-0.192
Observations	543	541	769	708	363	358

NEED TO UPDATE Notes: Panel A consists of households that were surveyed across all survey rounds (2010, 2012, and 2018) and Panel B consists of households that had at least one child aged 7-17 years at baseline according to the 2018 survey. Standard errors are clustered by loan group and appear in parentheses. All regressions include survey wave dummies, stratification dummies, dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. Profits and capital are top-coded at 99.5% for each survey round 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 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	0.078**	0.021	-0.022	0.006	-0.145***	-0.019	0.008	0.069*	0.000	0.057
	(0.038)	(0.036)	(0.039)	(0.035)	(0.054)	(0.054)	(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	0.053	-0.138*	0.194**	0.202**	0.041	0.053	-0.063	0.104	-0.049	0.111
	(0.063)	(0.082)	(0.087)	(0.078)	(0.126)	(0.114)	(0.131)	(0.079)	(0.082)	(0.079)
Grace Period × Literate Parents	0.037	0.219**	-0.286***	-0.257***	-0.225	-0.068	0.134	-0.054	0.084	-0.074
	(0.077)	(0.091)	(0.101)	(0.087)	(0.145)	(0.158)	(0.157)	(0.089)	(0.096)	(0.092)
Literate Parents	0.121***	0.028	-0.044	-0.028	0.034	0.131	0.106	0.055	0.040	0.081
	(0.046)	(0.058)	(0.064)	(0.054)	(0.099)	(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	0.002
	(0.005)	(0.039)	(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	0.038	0.009	0.028	0.278**	0.184	-0.032	-0.109*	-0.039	-0.043
	(0.002)	(0.071)	(0.048)	(0.048)	(0.110)	(0.130)	(0.075)	(0.062)	(0.052)	(0.099)
Grace Period × Literate Parents	0.007	-0.067	-0.056	-0.092	-0.358**	-0.096	0.077	0.179**	0.115	0.053
	(0.007)	(0.093)	(0.063)	(0.072)	(0.145)	(0.136)	(0.101)	(0.075)	(0.072)	(0.123)
Literate Parents	0.001	0.017	-0.041	-0.055	0.202**	0.204**	0.115*	-0.099*	0.075	-0.231***
	(0.002)	(0.054)	(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	0.067	0.046	0.043	0.151	0.008	0.844	0.417	0.219	0.520	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	0.594	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 1, 3, and 6 is restricted to male children and the sample in the other columns is restricted to female children. 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	0.58	0.52
Secondary School Graduates	0.15	0.10
College Graduates:	0.27	0.37
Illiterate Parents Sample:		
Secondary School Dropouts	0.66	0.78
Secondary School Graduates	0.17	0.10
College Graduates:	0.17	0.12
Literate Parents Sample:		
Secondary School Dropouts	0.56	0.41
Secondary School Graduates	0.14	0.11
College Graduates:	0.31	0.47
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 survey. 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 Wave 2 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. 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.

B. Data Appendix

Household-Level Outcome Variables

- *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?".
- *Income from Salaried work:* In the 2010 survey, the outcome is obtained from the following survey question: "During the past 30 days, how much total income did your household earn from salaried activities?". In the 2012 and 2018 surveys, the outcomes is the sum across all household members from the following survey question: "How much income did (NAME) earn from fixed salaried work over the last 30 days?"
- *Profit:* 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.
- *Business Closure:* indicator variable that is equal to one if a client reported having closed at least one household business that was operating at the time of loan disbursement.
- *Business Start:* indicator variable that is equal to one if a client reported having started at least one household business since loan disbursement. We code business re-openings as simultaneous business closure and business start.
- *Formal Savings:* all savings the household has inside bank accounts.
- *Oustanding Loans:* sum of all outstanding loans of the households at the point of the survey.
- *Number of Clients:* the sum of the number of clients per week across all household businesses that were operational at the time of the survey. It is equal to zero if the household has no operational businesses.
- *Number of Products and Services:* the sum of the number of products and services across all household businesses that were operational at the time of the survey. It is equal to zero if the household has no operational businesses.
- *Pawned/Sold Something:* indicator variable that is equal to one if there was ever a time in the past year in which the client pawned or sold something
- *Education Expenditures:* In the 2008, 2012, and 2018 surveys, the outcome is obtained from the following survey question: "How much did your household spend on educational expenses during the past 30 days?". In the baseline survey, the outcome is obtained by summing up household expenses on three educational categories in the past 12 months.
- *Health Expenditures:* In the 2008, 2012, and 2018 surveys, the outcome is obtained from the following survey question: "How much did your household spend on medical treatment

expenses during the past 30 days?". In the baseline survey, the outcome is obtained by summing up household expenses on ten medical treatment categories in the past 12 months.

- *Renovation Expenditures:* In the 2008 and 2012 surveys, the outcome is obtained from the following survey question: "How much did your household spend on renovation expenses during the past 30 days?". In the 2018 survey, the outcome is obtained from the following survey question: "How much did your household spend on household renovations and damage expenses during the past 30 days?". In the baseline survey, the outcome is obtained by asking about renovation expenses in the past 12 months.

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.
- *Years of K-12 Schooling:* years of K-12 schooling of the child.
- *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 coping 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 coping 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)?"
- *Performance Index:* standardized index that consists of the following variables: grade A in class 10, grade A n class 12, and science track.
- *Grade A in Class 10:* indicator variable that is equal to one if the child received an overall A grade in the Grade 10 exams. It is equal to zero if the child dropped out before grade 10.
- *Grade A in Class 12:* indicator variable that is equal to one if the child received an overall A grade in the Grade '1 exams. It is equal to zero if the child dropped out before grade 12.
- *Science Track:* indicator variable that is equal to one if the child was enrolled in the science track in grades 11 to 12. It is equal to zero if the child dropped out before grade 12.
- *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 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 Attended 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 surevy.

- *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/Daily Wage Work:* indicator variable that is equal to one if the respondent answered "yes" to at least one of the following questions: "Did (NAME) engage in self-employment in the last 30 days?" and "Did (NAME) work for a daily wage job 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.
- *Socioeconomic 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. .
- *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.
- *High Education Parents:* indicator variable that is equal to one if at least one parent attended class 5 or higher.
- *Parental Education in Years:* years of K-12 schooling for the parent with the highest level of educational attainment.
- *High Education Mother:* indicator variable that is equal to one if the mother of the child attended class 5 or higher.
- *High Education Father:* indicator variable that is equal to one if the father of the child attended class 5 or higher.
- *Client Has Financial Control:* indicator variable that is equal to one if the client answered "yes" to the following survey question at baseline: "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?".
- *Death in Household:* indicator variable that is equal to one if at least one household member died between baseline and the corresponding survey year.
- *School-Age Households:* indicator variable that is equal to one if the household at at least one child aged 7-17 years at baseline according to the 2018 survey.

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.