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

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Abstract

A randomized control trial in India showed that reducing liquidity constraints among urban microentrepreneurs raised household income two years after the loan was repaid (Field et al., 2013). We present new evidence that the economic benefits persisted and spilled over to the next generation. Relative to control, treatment households report 13% and 8% higher incomes five and eleven years after the intervention. We cannot reject constant treatment effects on income over the decade. Treatment households were more likely to invest in private secondary schooling and after-school tutoring for their children. In the long-run, this translates to a 34% increase in college attendance. Guided by a simple model of parental investment choice, we show that observed educational patterns are consistent with credit market imperfections such that poor households face a trade-off between human and physical capital investments. Consequently, average gains in children's education are also accompanied by greater educational inequality across treatment households.

1 Introduction

A growing body of experimental evaluations examine whether development programs designed to alleviate household liquidity constraints yield income gains and reduce poverty. The studies largely focus on short-run socioeconomic impacts (1-3 years later), though a few recent asset and cash transfer program evaluations report on longer-run outcomes (see, for instance, Bandiera et al. (2017); Banerjee et al. (2020); Blattman et al. (2020)).

Tracking long-term impacts of temporary credit and cash transfer programs can shed light on whether temporarily easing liquidity constraints can reduce structural poverty. If yes, then there is a strong case for providing temporary economic relief to poor households as a way of increasing

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equality of economic opportunity. On the other hand, exposure to shocks could easily counteract short-term income gains and generate convergence in income within a matter of years (Karlan et al., 2019). Establishing whether this is the case has implications for the potential for short-run redistribution schemes to impact long-run poverty or inequality.

Equally, if not more, valuable is determining whether socioeconomic gains associated with such programs persist across generations. Intergenerational impacts of programs matter for welfare both as a measure of redistributive fairness in society but also because low mobility potentially suppresses economic growth via misallocation of resources. India — the setting for our study — has one of the lowest rates of intergenerational mobility in the world (Narayan et al., 2018; Asher et al., 2020). The patterns of intergenerational gains associated with temporary transfer programs can also help us understand economic mechanisms underlying low rates of socioeconomic mobility in much of the world.

In this paper, we revisit the study population of a 2007 field experiment where female microfinance borrowers in the Indian city of Kolkata were randomly assigned to either the classic microfinance contract or to with a repayment schedule that eases capital constraints. The latter immediately increased business investment and, three years post-intervention, household income for treatment group women was 20% higher than for control group women (Field et al., 2013). In this paper, we use surveys conducted in 2012 and 2018 to show that household income gains persisted and were passed on to children via investments in human capital accumulation.

First, we document the income trajectory over the next decade. After five years, treatment households report 13% higher income than households in the control group. In 2018, eleven years post-intervention, treatment households report 8% higher household income than control group households. Though point estimates decrease over time, we cannot reject equality of treatment effects across time. We do not detect longer-run treatment differences in weekly business profits, but treatment enterprises remain larger in terms of capital. We also find that treatment households accumulate a larger stock of savings and are less likely to suffer business closures after the 2010 microfinance crisis in India, which led to a major contraction in the supply of microcredit Breza and Kinnan (2020). We conclude that that treatment households maintained relatively higher incomes in part through using a buffer stock of savings to overcome negative shocks.

Next, we examine educational outcomes for all children born to the client, irrespective of their current residential status. Primary education is close to universal in our setting and so our main interest is in continuation into secondary and tertiary education. Our main sample of interest is households with at least one school-aged child (i.e. between 7-17) in 2007 (what we term "school-age households"). At the time of the intervention, 89% of study population households had completed fertility and 52% of households had school-going children. These children are just old enough that, by the time of the 2018 survey wave, they would have completed secondary schooling. They are also young enough that, through investments, parents could have shifted

their educational outcomes. Children 18 years of age or older at baseline, almost all of whom had completed their education at the time of intervention, form a placebo group.

At baseline, school-age households in treatment and control group report similar educational investments. Beginning a year after intervention, treatment households spend more on their children's education. They are also more likely to send their children to private secondary school and report higher spending on after-school tutoring. By 2018, we observe a 9.4 percentage point increase in the college enrollment rate for treatment households with similar effects across sons and daughters. We also find evidence that younger children — who had more exposure to the treatment — benefited more. We show that treatment effects on educational investment and attainment grow in inverse proportion to the age of the child at baseline. Our results are consistent with evidence from the United States that increasing college attendance requires investments early in a child's educational career (Carneiro and Heckman, 2002; Chetty et al., 2016).

We provide suggestive evidence that credit constraints prevent households from optimally investing in their children's education and that returns to investing vary with parents' own educational attainment. By increasing households' enterprise income, our intervention eased credit constraints on education expenditures and, consistent with our hypothesis, we see that positive impacts on educational attainment are concentrated among children in households where at least one parent has some secondary schooling. In contrast, we observe a slight decrease in attainment for children of lower education parents and an increase in the likelihood they work as a child. We find no evidence that households invested in education as a means of bringing higher skilled labor into the enterprise.

Our study adds to the evidence on the impacts of anti-poverty programs, and in particular credit programs, on outcomes for the next generation. Short-run evaluations of the classic Grameen-style microfinance have found small or no economic impacts on average (Banerjee et al., 2015), though a number of recent 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.[1]If credit programs lead to even short-run income gains, they could have long term impacts through investments in the next generation. However, as several studies note, while associated increases in household wealth increase the ability to pay for education expenses (income effect), increased returns to business activities also increase the opportunity cost of schooling (substitution effect). Thus, the effects of microcredit

[1]In this group, flexible credit interventions can include the following features: a grace period before beginning repayment (Field et al., 2013); seasonal repayment moratoriums or an option to delay a certain number of repayments during the course of the loan tenure (Barboni and Agarwal, 2020; Czura, 2015; Battaglia et al. 2019; Shonchoy and Kurosaki 2014); or, choice of repayment schedule akin to a line of credit (Araganon et al., 2020). Compared to more rigid contracts which mandate repayment at frequent and regular intervals, flexible credit contracts lead to higher business profits (Barboni and Agarwal, 2020; Battaglia et al. 2019; Araganon et al., 2020; Field et al. 2013); increases in household income (Battaglia et al. 2019; Czura, 2015; Field et al. 2013); and, gains in household consumption (Shonchoy and Kurosaki 2014).

on education outcomes is ambiguous ex-ante. Some microfinance studies find an increase in child labor and a decrease in educational attainment (Garlick, 2016; Lakdawala, 2018; Augsburg et al., 2015). In contrast, Attanasio et al. (2015) study a microfinance program in Morocco and find a positive impact on schooling among children of higher-educated parents.[2] Longer term evaluations of cash or asset transfer programs, which include Banerjee et al. (2020), Blattman et al. (2020), and Bandiera et al. (2017), do not find (Blattman et al., 2020), or do not study, impacts on children's education.

Given the importance of increasing enrollment in secondary and tertiary schooling, policymakers and researchers have devoted considerable efforts to identifying interventions which would reduce demand-side constraints to improving educational attainment. These interventions include conditional cash transfers (CCTs), in-kind transfers of school supplies or for transportation, scholarships, or information dissemination. Common to these interventions is the underlying assumption that parents' investment decisions in kids' education not socially optimal, whether because of parents' lack of information, high discount rates, or failure to internalize social spillovers of education (Glewwe and Muralidharan, 2016).[3]

Our study provides suggestive evidence that parents' investment decisions are constrained by access to credit. In doing so, we add to a large body of studies which examines the existence of credit constraints to educational investments in developed country contexts (Carneiro and Heckman, 2002; Dahl and Lochner, 2012; Bulman et al., 2016). Current evidence on credit constraints in schooling in developing countries is relatively scarce, though several studies find that subsidies for school fees increase educational attainment (Angrist et al., 2006; Duflo et al., 2017). While most of these studies investigate primary and secondary school outcomes, two recent studies also find that lifting credit constraints through students loans and scholarships increases college enrollment in Chile and Colombia (Solis, 2017; Londoño-Vélez et al., 2020). We add to this literature by showing that relaxing liquidity constraints leads to long-term increases in educational attainment.

The rest of the paper is organized as follows. Section 2 details the context and describes our data. Section 3 describes our main results. Section 4 presents results of a heterogeneity analysis of impacts on children's educational attainment by parents' level of education, and discusses mechanisms underlying our results. Section 5 concludes.

2 Context and Experiment

This section describes the context, the data we use, and our empirical strategy.

[2] For rural settings, multiple studies examine the effects of rainfall-induced income shocks on educational attainment and find mixed evidence (Jensen, 2000; Björkman-Nyqvist, 2013; Zimmermann, 2020).

[3] See Bouguen et al. (2019) for a review of randomized controlled trials which evaluate long-run impacts in development economics.

2.1 Context

Entrepreneurship and Financial Inclusion in Urban India

Our study and subsequent sample tracking between 2007 and 2018 occurred during a period of, on average, strong economic growth in India coupled with significant urbanization and occupational diversification (World Bank, 2020). According to the 2004-2005 and 2011-2012 rounds of the Indian Human Development Survey, a quarter of poor households living in Indian cities like Kolkata list self-employment as their primary income source.

Small business owners in urban India rely heavily on loans from micro-finance institutions, a sector which grew by a factor of six between 2008 and 2017.[4] However, there was a large contraction in this sector between 2010 and 2012, which negatively impacted microentrepreneurs throughout the country including those in our study. In particular, in October 2010 (after completion of our 2010 survey), the microfinance sector experienced a nation-wide crisis and enormous liquidity shock precipatated by near universal default in the state of Andra Pradesh. Breza and Kinnan (2020) document that the gross-loan portfolio of MFIs in the country dropped by 20% and that it took until 2013 for lending to recover.[5] Economic growth also fell from 10.3% to 6.6% between 2010 and 2011 (Subramanian, 2019). In our 2012 survey, close to a quarter of the sample report increased difficulty borrowing from microfinance institutions.

Inter-generational economic mobility remains low in India (Asher et al., 2020). For lower-income urban households engaged in unskilled labor, upward mobility is epitomized by their children transitioning to salaried employment opportunities, such as holding a government job (Mangal 2020). A survey of parents in Andhra Pradesh finds that when asked "what job would you most like your child to do in the future?", 90% of parents report occupations associated with salaried work (Young Lives, Round 2). There is also broad recognition that education is a key input for this ultimate economic success. College attendance, in particular, is deemed a significant marker of upward mobility among the urban poor.

Education system in India

The Indian education system consists of three levels, with drop-out more marked at transition points between levels. Primary school consists of grades 1 through 4 when children are typically between 5 and 9 years-old. Students then may enter secondary school, which features statewide exams at class 12 that determine options for tertiary (college) education.

In Figure 1, we plot primary, secondary, and tertiary school completion rates in urban areas

[4] In 2008-2009, microfinance institutions in India had Rs. 50.09 billion in loans outstanding with banks, according to the Reserve Bank of India. In 2017-2018, that number was Rs. 323 billion.

[5] Breza and Kinnan (2020) also argue that the crisis propagated to other states because Indian banks across the country held off lending to microfinance institutions, waiting for the crisis to pass.

of India by birth year cohort using National Family Health Survey (NFHS) data.[6,7] We highlight three trends from Figure 1: first, primary schooling is now nearly universal in urban India and also among children in our sample. Second, there is a significant increase in secondary and tertiary education; roughly half the millenials have gone to secondary school and a third have college education. Third, the gender gap in attainment has almost closed in urban areas across all three educational achievement levels. These trends are also present in our sample: while parents in our sample have relatively low educational attainment, with less than 1% having attended any post-secondary school (Appendix Table A2, Panel B), 28% of children in the control group attend at least some college.

We also see significant investment in educational quality. Like much of the developing world, India has experienced an explosive rise in private schooling and after-school tutoring (Kingdon, 2020; Berry and Mukherjee, 2019). By 2015, 42% of children in urban areas of India attend private school (Kingdon, 2020). In our sample and more broadly, private schools are associated with better educational outcomes.[8] For example, 55% of private schools are English rather than Bengali medium and existing research documents large returns to English skills in our setting (Azam et al., 2013). While the median grade on the Class 12 exams for a control group child in public school is a B, for a private school child it is an A. Also, among children in the control group, 64% of those who attend private school go on to college compared to 30% of those who attend public school. But private schooling is costly: the median annual cost of private school among control group children who attend is Rs. 15,600, which amounts to 11% of median income among control group households. 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.

Parents also invest in educational quality via private after-school tutoring for their children. After-school tutoring – mainly for kids in secondary school – is an important feature of India's education system (Berry and Mukherjee, 2019); at baseline, 92% of sample children report some tutoring. Private tutors provide supplementary instructions to students in all academic subjects across primary and secondary schools. In our sample, spending on after-school tutoring is on average 64% higher than total schooling costs, emphasizing how much parents value the supplementary instruction. Existing research documents a positive or mixed effect of private schooling

[6]The National Family Health Surveys (NFHS) are also called the Demographic and Health Surveys (DHS). DHS are nationally representative, household-level surveys carried out in developing countries around the world. We use data from the NFHS Round 4, which is the most recent survey round available and was completed in 2014-2015.

[7]On the x-axis we plot the year at which the respondent turned 18. The shaded blue area represents cohorts of the same age as parents in our sample and the brown shaded area represents cohorts of the same age as children in our sample.

[8]In a review of the literature on private schooling in India, Kingdon (2020) finds an average private-public achievement gap of 0.10 to 0.35 standard deviations across studies.

and after-school instructions on learning outcomes (Dongre and Tewary, 2015; Kingdon, 2020).

Parents' investments in private and after-school tutoring are meant to help their children pass high-stakes college entrance exams. Although public tertiary education in India is low-cost, entry — which is based solely on grade 12 exam scores — is highly competitive (Sekhri, 2020). Many students end up attending private tertiary institutions, which can be very costly. In general, though, there are high returns to tertiary schooling in urban India. Montenegro and Patrinos (2014) estimate that completing college leads to 21% higher earnings across India and Rani (2014) estimates that rates of return to college are 24% in urban areas. We find evidence of the same pattern in our sample: among children aged 19 or older in the control group, those who completed college earn 13% more per month than children without a college degree.

2.2 Experimental Design and Data

Between March and December 2007, Village Financial Services (VFS), a microfinance institution, designated 169 new five-member loan groups (845 women) in low-income neighborhoods of Kolkata for inclusion in the study (Field et al., 2013). Each client received an individual-liability loan, with a modal loan amount of Rs 8,000. Prior to loan disbursement, loan groups were randomized into one of two repayment schedules. Eighty-five groups were assigned to the regular VFS debt contract with repayment in fixed installments starting two weeks after loan disbursement, and 84 groups were assigned an analogous contract that also included a grace period of two months.[9] See Field et al. (2013) for details of the experimental design.

Field et al. (2013) find that the grace period treatment leads to a 20% gain in household income and large gains in business investment and profits three years post-intervention. The authors estimate that marginal returns to capital are between 6 and 13% among entrepreneurs in our setting. They also find that treated households are twice as likely to start a new business and more likely to report making riskier investments. Impacts are concentrated among households with a greater need or preference for flexibility.

In our current analysis, we use five rounds of primary data collection: a baseline survey conducted prior to the rollout of the experiment (2007) and four rounds of follow-up survey data, which were collected in 2008, 2010, 2012, and 2018. We first describe the analysis sample and then outline our data collection and provide key variable definitions.

Tracking and Analysis sample

In Appendix Table A5 we analyze the response rate and composition of respondents across survey rounds. In Panel A, we regress on treatment a dummy that takes on the value one if the household could not be surveyed. At the 11 year follow up, we reach 729 out of 845 households from the

[9]Both groups faced the same interest charges. However, longer debt maturity (55 as opposed to 44 weeks before the full loan amount was due) combined with the same total interest charges implied that grace period clients faced a slightly lower effective interest rate on the loan. Treatment status was assigned within batches of 20 groups, determined by timing of group formation.

baseline sample.[10] A tracking rate of 86% is at par with that of other long-term studies. We find no statistically significant differences in tracking rates across treatment and control groups across the survey rounds.

In Panel B, we regress the baseline characteristic in each row on treatment; a dummy for whether the household was not surveyed; and, the interaction of the two. In the odd columns we show the coefficient on the interaction and in the even columns the corresponding standard error. Although treatment and control group attriters are similar across most baseline characteristics, we note a few differences. In the 2012 survey, treatment group attriters are from larger households and have lower health expenditures than control group attriters. Also, attriters in the treatment group are more educated than those in the control group. In the 2018 survey, we again observe a difference in household size by attrition. Additionally, treatment group attriters spent more on children's education at baseline.

Appendix Table A3 reports the balance check at baseline for the full sample and for the sample of households with at least one child aged 7-17 at baseline, our primary analysis sample. We are balanced across most variables. Notable exceptions are that treatment clients are less likely to be married and had a higher asset index at baseline. These differences disappear when we restrict the sample to school-age households. Within this subgroup, treatment households were more likely to receive Rs. 10,000 loans. At the bottom of the table we report the p-value of an F-test of whether all the grace period coefficients are jointly equal to zero. In neither the full nor the school-age sample can we reject that this is the case. Nonetheless, we include all variables listed in the balance check as potential lasso controls.

Description of surveys and key variables

All outcomes and control variables used in our analysis are described in Appendix Section B. In the sub-sections below, we describe key outcomes in more detail.

Income and Business Outcomes Figure A2 describes the timing and focus of each of our five rounds of data collection. In 2010, 2012, and 2018, we asked respondents to report on household income.[11] Respondents were asked: "During the past 30 days, how much total income did your household earn?" We follow Field et al. (2013) and top-code household income at the 99.5 percentile. We also asked clients about profits and assets for up to five household businesses. We asked clients to report on profits with the single question, "Can you please 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

[10] In the period between baseline and our final follow-up, 19 clients died and 51 moved to a different city. Twenty-four clients did not consent to the final survey, 6 could not be located, and 16 were not able to be surveyed due to illness or for similar reasons.

[11] Although we did not collect household income in 2007 and 2008, we did ask respondents about household assets and savings.

services." We calculate business capital from the sum of raw materials, inventory, and assets.[12] Our 2010 and 2012 surveys contained business modules which gathered detailed data on the activity of each household enterprise, including number of clients and number and types of goods and services offered by the business.[13]

In order to assess households' response to the MFI crisis, in 2012 we collected in-depth measures of households' resilience to financial shocks. We asked respondents whether any of the following occurred during the period between the 2010 and 2012 surveys: they had ceased operating any household enterprise; they had applied but been rejected for a business or household loan from any formal or informal institution or person; or, they had had difficulty making a loan payment. We also asked whether, in the year prior to the 2012 survey, respondents had asked for financial help from family or friends or had pawned or sold something. We combine these latter three measures into a financial stress index, in which components are standardized using the control group mean and standard deviation.

Children's Education and Socio-Economic Outcomes All survey rounds, except for 2010, asked about the occupation and educational attainment of each person currently residing in the household. We also asked about education, medical, and other household expenditures and, in 2012 and 2018, we assessed health outcomes for household members. In the 2018 follow-up survey, we expanded the scope of our survey to all the client's children, including those living outside the household. Three hundred and four of the 729 clients we located had children living outside of the household at the time of the 2018 survey.

We asked parents to report on the cost of each child's education as well as children's educational attainment and performance. To estimate the cost of each child's schooling, we separately asked about total spending on private after-school tutoring for each grade completed and about total school fees for each grade completed. Parents reported on costs and attainment at primary, secondary, and tertiary schooling levels. Our measures of educational performance are at the secondary school level: we ask parents for their child's letter grade on both their Grade 10 and Grade 12 exams.[14] We also asked which track children chose in upper secondary school (grade 11-12): science, business, or liberal arts. Science track requires the highest grades on children's Grade 10 exams. Following our pre-analysis plan, we combine outcomes into educational investment, attainment, and performance indexes in which sub-components are standardized using the control group mean and standard deviation. For ease of interpretation, we also create separate sub-indexes for investment at the primary and secondary school levels.

[12]The value of raw materials and inventory is computed from survey questions on the value of materials clients currently have in stock and which are used for production. We compute the value of equipment from clients' valuation of their durable assets that are used in their business.

[13]Our 2018 survey, which was focused on children's outcomes, did not include the detailed business module.

[14]We attempted to gather data on children's exact numerical scores on their Grade 10 and 12 exams, but parents had difficulty recalling these numbers.

Finally, we asked parents to report on children's socio-economic outcomes, including occupation, income, and marital status at the time of our 2018 survey. Earnings over the previous 30 days were collected for all adult children, including for those who were in school and working part-time. We also measured whether children's employed was salaried or non-salaried, which is an important marker of upward mobility.

2.3 Empirical Strategy and Analysis Plan

We conduct analysis at both the household-level and at the child-level, and describe each of them in turn.

Our household-level analysis modifies the Field et al. (2013) specification to allow for multiple survey waves as follows

$$Y_{hgt} = \alpha + \beta_0 T_g + \sum_{t=1}^{4} \beta_t (T_g \times W_t) + \alpha_g + \sum_{t=1}^{4} \delta_t W_t + \gamma X_{hg} + \epsilon_{hgt}. \quad (1)$$

Y_{hgt} denotes the outcome of household h which belongs to microfinance group g in survey year t, T_g indicates whether the individual was in a grace period loan group, α_g are stratification dummies for treatment group batch, W_t are survey wave dummies and X_{hg} is a set of baseline control variables selected via the double lasso approach developed by Belloni et al. (2014). We report separate results for households who had at least one child of school-going age (7-17 years) at baseline and those who did not.[15] Standard errors are clustered at the loan group level.

For child i we estimate child-level regressions as:

$$Y_{ihg} = \alpha + \beta T_g + \alpha_g + \gamma X_{ihg} + \epsilon_{ihg}. \quad (2)$$

We use this equation to estimate the effects of a capital shock on educational investment, attainment, and performance, all of which are measured in the 11-year follow-up survey.

Pre-analysis plan Following our pre-analysis plan, we examine average treatment effects for education outcomes as well as heterogeneous treatment effects by child gender and parental education.[16] We did not pre-specify age cut-offs. For the main outcomes, we show non-parametric estimation results for the full child sample. We then use age cutoffs to asses average effects for children mostly likely to be affected by treatment. Our main sample for analysis of children's educational and socio-economic outcomes includes children aged 7-17 years at baseline. Most of

[15] The definition of age-cutoffs is discussed in more detail in the next section.

[16] We also specified that we would examine health outcomes for parents and children. We did not collect health outcomes for children who were not in the household in any survey round, so for the present paper, we exclude health outcomes since a primary goal of the paper is to understand how *all* children were impacted by the intervention. Our pre-analysis plan also specified that we would examine outcomes by whether the client had completed fertility at baseline and by client's decision-making power within the household. We later found that 89% of clients did not have any additional children after baseline, so we do not conduct this heterogeneity analysis. We are still conducting analysis by client's decision-making power.

these children completed their pre-college education in the period between baseline and our final follow-up[17]; in other words, these children are old enough such that we can observe their final level of educational attainment and young enough such that parents' income gains from the grace period intervention could have impacted investments in their schooling. About half of the full household sample has at least one child in our main child sample (Appendix Table A2).

A potential concern with our cut-offs is that 36% of children in the control group aged 7 years at baseline are still in secondary school at the time of the 2018 survey. We thus also conduct robustness checks with different age cut-offs for the main outcomes (Appendix Table A11) and education level-specific age cutoffs (Appendix Table A12). We also show results for children aged 18+ years at baseline as a placebo group. Based on retrospective data in the 2018 survey, 94% of these children had completed schooling at baseline.[18]

3 Results

3.1 Household economic well-being

Persistence

Table 1 considers the medium- to long-term impacts of the grace period contract on household economic outcomes. Column 1 considers household income. As reported in Field et al. (2013), three years after intervention, client households that were randomly assigned to receive the grace period contract report 19.7% higher [SE: 0.075] monthly income.[19] In measuring household income five- and eleven-years post-intervention, we find that economic outcomes have declined across sample households. Compared to 2010, log income for the control group dropped slightly in 2012 (though the difference is not statistically significant) and had declined 35% by 2018. This drop in household income is partly driven by outliers and changes in household composition. Median income per capita only declined by 11% over the same period. Yet positive treatment effects of the grace period contract remain persistent: Five years post-intervention, treatment households still have monthly earnings that are 12.5% higher than those of control group households. In our eleven-year follow-up survey, the treatment group had 7.8% higher income on average. Overall, we cannot reject that the same treatment effect persisted across survey rounds.[20]

The majority of households in both the treatment and control groups own at least one business

[17] Appendix Figure A1 plots the age distribution and enrollment status in 2018 by age for children in the control group.

[18] We do not report separate results for younger children (aged 6 years or less at baseline) since we do not observe the full education trajectory for this subgroup: As of 2018, 78% of children aged 6 years at baseline were still in secondary school in the control group.

[19] In Field et al. (2013), the coefficient reported was 0.195 [SE: 0.08]. In this paper, all controls are chosen using double-post lasso unlike in Field et al. (2013) specification in which a set of controls were selected by the researchers. Results in this paper are robust to using the original set of controls.

[20] Appendix Table A7 further shows that we find similar treatment gains for households with and without children aged 7-18 years at baseline

at every follow-up survey round (column 2 Table 1). We therefore examine how the grace period intervention impacted business outcomes across the study period. In columns 3-5 of Table 1, we show impacts of the grace period on three business outcomes that we tracked consistently across waves: profits, capital, and number of workers. The coefficient on grace period replicates results shown in Field et al. (2013): three years post-intervention (in 2010) we observe large gains in profits and capital among treatment group households, but no impact on enterprise workers. In column 3 we see that the treatment group's gains in profits are no longer detectable by 2012 or in 2018. [21] We do, however, see sustained differences in the size of businesses. In column 4, we find that treatment household enterprises continue to be larger in terms of capital — the value of durable assets and current inventories — in the medium and long-term. In 2012, treatment businesses have 87% more capital than control households, significant at the 5% level. Although this difference is no longer statistically significant in 2018, the point estimate implies that grace period household businesses are still 19% larger.[22] Moreover, in Appendix Table A8, we show that five years post-intervention treatment households report a larger number of clients and that they provide a larger assortment of goods and services.[23] We also observe differences between treatment and control households in terms of the total number of household and non-household employees working in the business.

Resilience to shocks

We find suggestive evidence that the treatment income gap was in part sustained because treatment households were able to build up a buffer stock of savings which increased their resilience to shocks. As shown in column 5 of Table 1, treatment group households have a 48% difference in their level of formal savings in 2010 and there continues to be a statistically significant savings gap between treatment and control households in 2018, eleven years post-intervention.[24]

To explore the idea that treatment households may have used their greater buffer stock of

[21] The divergence in treatment effects for profits and income in 2012 appears to be driven in part by survey timing since the 2012 survey was administered in two parts. 87% of the profit data was collected in October and November 2012. This coincides with the festival season. 46% of control group businesses have high profits during this period according to our seasonality survey module. By contrast, 85% of the income data was collected between December 2012 and January 2013. Only 23% of businesses have high profits during this period. This explanation implies that the grace period especially led to business gains during typically less profitable months. Consistent with this, we find that treatment businesses report a higher frequency of high profit months in the past 12 months, but this difference is not statistically significant. We further find some suggestive evidence that impacts on household income are due in part to diversification into salaried work among households in the treatment group, but this difference is also not statistically significant.

[22] The increased noise in the 2018 survey is driven by a change in the assets module. While we previous asked about the total value of all business assets (2010) or conducted a unprompted listing of assets (2012), we explicitly asked respondents to list all their assets in six asset categories in the 2018 survey.

[23] Data on the number of clients and the number of goods and services were not collected as part of the 2018 survey so in Appendix Table A8 results are shown only for 2012.

[24] In 2010, we asked only about level of savings in a formal savings account. In 2018, we asked about levels of both informal and formal savings and find a statistically significant difference in treatment households' level of overall savings (result not shown).

savings to weather shocks, we examine how households in the sample fared through the 2010 microfinance crisis. Although we do not have exogenous variation in exposure to the MFI crisis, we use a detailed enterprise module collected in 2012 to understand how sample households reacted to the contraction in lending and downturn in economic conditions. We find that 23% of clients report borrowing problems in 2012.[25] As shown in column 1 of Table 2, we find that treatment and control group clients are equally likely to report difficulties in accessing credit.

The grace period intervention led treatment households to respond differently to the liquidity crunch and economic downturn. As seen in columns 3 and 4 of Table 1, both treatment and control households sell off capital and lay off workers: the capital stock of control group households is Rs. 10,096 lower in 2012 than in 2010 and they have 0.62 fewer workers. We cannot reject that treatment and control households downsize their enterprises by the same amount in terms of *levels* but, as a percent of their capital stock, treatment households liquidate a smaller proportion of their businesses. In column 2 of Table 2, we also find that treatment households are 10% less likely to report a business closure in 2012. In our 2012 survey, we asked households whether any of the following had occurred in the previous year: they had problems paying bills; they had to pawn something to make ends meet; or, they had to ask for financial help from a friend or relative. In columns 4-6 we show how treatment and control households answered these questions. Column 3 shows results from a standardized index of these three measures of financial stress. Although the overall index is not statistically significant, treatment households are 37% less likely to report having to pawn something to make ends meet and this result is significant at the five percent level (column 4). In line with this result, we also find that while both treatment and control households had to draw down their savings in 2012, the treatment group had a higher buffer against which to draw (column 5 of Table 1). Having access to these extra savings may have helped guard treatment households against needing to close or liquidate a business.

3.2 Next-generation outcomes

In 2007, 53% of our sample households had at least one school-going age child (7-17). These households serve as our primary sample for analysis of children's outcomes, since children in this group were (i) young enough to be in school at the time of the intervention and (ii) old enough to have completed secondary schooling by 2018 survey.

A. Education expenditures

In Table 3, we report treatment effects on spending on education in the previous 30 days in each survey year for the full sample (column 1) and for households with at least one child aged 7-17 years at baseline (column 2). We did not collect expenditure data in 2010 but show results from baseline (2007) and the one year follow-up survey (2008).

[25] We asked clients: "In the last 24 months, was there ever a time when you wanted to borrow money from a specific source but could not get all the money that I wanted to borrow within 2 weeks?"

Since we include the baseline outcome in this specification, the grace period coefficient serves to check baseline balance in education expenditures. Reassuringly, we do not observe any significant difference in expenditures at baseline across the full treatment and control sample or across the school-age households treatment and control sub-sample.

We begin to observe differences in education expenditures within a year after the intervention. In column 2 we see that treatment households in the school-age children sample spend an extra Rs.200.5 per month in 2008, significant at the 10% level. In 2012, school-age treatment households report 45% higher expenditures on education than school-age control households and this difference is statistically significant at the 1% level. In 2012, we can also detect significant differences in education expenditures in the full sample. On average, 9% of the additional income that school-age households earn in 2012 from the grace period contract is spent on educational investments. By 2018, the difference in expenditures is not statistically significant for the full or school-age samples. This is perhaps unsurprising given that, by 2018, 58% of school-age households have no children in school anymore.

Under the assumption that the grace period only affected education spending by making households wealthier (i.e. we satisfy the exclusion restriction), we can use the grace period as an instrument to estimate the elasticity of education spending with respect to income. Pooling across survey rounds, we estimate an income elasticity of educational expenditure of 1.1 for the full sample and 2.82 for school-age household (Appendix Table A9) [26] [27]

B. Educational outcomes

In Figure 2, we examine treatment effects by age of child at baseline nonparametrically for our main education outcomes. We plot a local polynomial regression of the outcome specified on the y-axis on the age of the child at baseline, by treatment and control. The vertical dotted line represents the cut-off point for our main child sample: children 7-17 years at baseline. Figure 2 shows that households' increased expenditures lead to positive treatment effects on their children's educational investments. The figure also provides evidence that younger children — who had more exposure to the treatment — benefited more. Treatment effects on educational investments grow in inverse proportion to the age of the child at baseline.

Younger children may have benefited more from the grace period contract because it took some time for parents' enterprise investments to yield sufficient wealth accumulation. Alternatively, consistent with Carneiro and Heckman (2002), it is also possible that raising college attendance requires investments early in a child's educational career: to affect the likelihood that a child attends college, it is important to affect the probability that child receives high scores on their

[26] Since information on educational expenditure is missing for 2010, we impute it using the mean of 2008 and 2012 education expenditures.

[27] To put this result in perspective, we calculate that households in Haushofer and Shapiro's (2018) rural Kenya sample have income elasticity of 0.7 from a cash drop intervention.

grade 12 exams, which is in turn affected by the child's educational track in upper secondary, which is determined by grade 10 exams, etc. Parents may have therefore felt that it was too late to affect the chances of attending college for older children and chose not to invest in them. Our results are also consistent with Chetty et al. (2016)'s finding from the Moving to Opportunities program in the United States that college attendance and earnings impacts are concentrated among the children who were youngest at the time of their move.

In Table 4, we show average treatment effects on education outcomes for children 7-17 at baseline. The unit of observation is the child and data in this table reflects their educational attainment, performance and expenditures as of 2018. Each row is a separate regression based on Specification 2. In each panel, we also show a summary index which is a standardized aggregate of the outcomes in the panel.[28]

In Panel A, we analyze parents' investments at the primary, secondary, and post-secondary school level for each child. Though point estimates are positive, we do not find statistically significant treatment effects on investment at the primary school level.[29] Conversely, treatment children who were 7-17 at baseline report significantly higher expenditures across all measures of secondary schooling investments and college spending. Treatment children in the target age group are 4.9 [SE: 1.9] percentage points more likely to have attended a private secondary school than target children in the control group. This amounts to a more than doubling in the probability of private school attendance. We asked parents how much they spent on school expenses (admissions fees, school fees, uniforms, and text books) as well as expenses on after-school tutoring. We find that parents with children 7-17 spent an extra Rs.3268.7 [SE:1696.8] on secondary school tuition fees per child and Rs.6464 [SE: 2168.9] on after-school tutoring per child when compared to their counterparts in the control group. Our data do not allow us to distinguish between an increase in expenditures due to children spending more hours with a tutor or because parents select more expensive tutoring. Overall, treatment school children score 0.27 standard deviations [SE: 0.08] higher on the secondary school investment subindex.

Treatment group parents' higher levels of investment in education continue at the post-secondary school level. Children 7-17 in the control group spent an average of Rs. 8241.5 to attend college.[30] Conditional on a child's college attendance, control group households report spending Rs. 27,433 per child on college expenditures. On average, households in the treatment group spend an extra Rs.4786 [SE: 2403.5] per child on college, which amounts to 60% higher

[28] For example, the Investment Index in Panel A is the mean of the standardized values of Private School, Total School Fees, and Total After-School Tutoring for both the primary and secondary school levels, and College Spending.

[29] This is to be expected since most children in this age group would have already completed primary school at baseline.

[30] This is lower than expenditures on secondary school fees and after-school tutoring for two reasons: first, only a fraction of students who attend secondary school will go on to receive a post-secondary school education. Second, total expenditures on secondary school and after-school tuition are accumulated over more years; secondary schooling in India spans grades 5-12, whereas students in general attend only 3 years of college.

expenditures than control group households. The college and secondary school expenditure results are consistent with the concurrent changes in monthly expenditures in Table 3: treatment households with school-age children spent more on their kids' secondary education, after-school tutoring, and college.

In Panel B of Table 4, we analyze years of schooling (K-12) and college attendance for children 7-17 at baseline. Treatment has a positive, but not statistically significant, impact on total years of primary and secondary schooling. Because a large proportion of our child analysis sample is still in college or of college-going age, we define "attended college" in Panel B to mean that the child completed or is currently enrolled in college. Children in treatment households who were 7-17 years of age at baseline are 9.4 [SE: 3.9] percentage points more likely to have gone to college, which amounts to a 34% increase in the likelihood of college attendance when compared with control group children in the same age group. When college and years of education outcome measures are compiled into a standardized index, we find that treatment children in the target age group report educational attainment that is 0.14 standard deviations [SE: 0.08] higher than attainment of children in the control group.

In Panel C of Table 4, we present results on performance outcomes for children who were 7-17 at baseline. We asked parents to recall their children's grades (A, B, or C) on their Grade 10 and Grade 12 exams and to report which track their child chose in upper secondary school (science, business, liberal arts). The science track requires the highest grades on Grade 10 exams. We examine whether treatment group children are more likely to have received an A in Grade 10 or in Grade 12 and whether they chose the science track for upper secondary school.[31] We cannot reject that the treatment has no differential effect any of the three outcomes.[32]

C. Robustness We present three types of robustness checks on the impacts of the wealth shock on children's education outcomes. First, in Appendix Table A11, we replicate Table 4 but vary the age cut-off of the school-age child sample. Changing the upper and lower bound age cut-offs by up to two years only marginally affects the main results.

As a second robustness check, we report treatment effects on primary school and Grade 10 outcomes using education level-specific age cutoffs in Appendix Table A12. This means that we restrict the sample to children born nine years before baseline until one year after baseline for primary school outcomes. For Grade 10 outcomes, we restrict the sample to children aged 5-15 years at baseline. Consistent with our secondary school results, we find positive treatment effects on primary school spending outcomes and private primary school attendance but the coefficients are not statistically significant. For Grade 10 outcomes, we find a statistically significant increase

[31] Children who dropped out before the Grade 12 exams are coded as 0 for the letter grade in that year.

[32] One possible reason for why we do not detect differences in performance is that the grade distinction are a coarse measure of the underlying numerical grade. But during piloting, very few parents could recall the numerical grade.

in after-school tutoring and positive but noisy effects for the remaining outcomes.

As a third robustness check, in Appendix Table A13 we replicate Table 4 but for a placebo group: children 18 and older, almost all of whom had completed their education by the time of the intervention.[33] We cannot reject the null hypothesis of no treatment effects on the investment, attainment, and performance indices of this group.

D. Gender differences

In Table 5, we present education outcomes for sons and daughters separately. Among households in the control group, daughters are as likely as sons to attend private secondary school, complete secondary schooling, and attend college. Control group families do not disproportionately spend more on school fees or after-school tutoring on sons. Treatment group daughters have 0.18 [SE:0.09] standard deviation units higher investment index than control group daughters, while treatment group sons score 0.22 [SE:0.11] standard deviation units higher than sons in the control group. Daughters in the treatment group score 0.12 [SE:0.11] units higher on the education attainment index, compared to 0.15 [SE:0.10] for sons. We cannot reject that the treatment effects are the same for sons and daughters.

In fact, for nine out of the eleven investment, attainment, and performance outcomes, we cannot reject that the treatment effects of our intervention are equivalent for daughters and sons. One exception is science track enrollment, for which we only observe significant increases for sons and reject that the treatment effects are equivalent for both genders. One possibility is catch up: in the control group, girls are nearly twice as likely as boys to pursue the science track.[34]

Over the past several decades, the gender gap in secondary school attainment has drastically decreased across urban areas of India (Figure 1). Even as female labor force participation rates remain stubbornly low, there is evidence that there are important returns to girls' schooling in the marriage market that can push parents to invest in their daughters' education (Adams-Prassl and Andrew, 2019). Within our sample, daughters in the control group who attend post-secondary school are 48 percentage points more likely to marry someone with a college degree.[35]

4 Why did an increase in wealth impact parents' investments in their children's education?

Our findings thus far show that making parents wealthier has important impacts on schooling for the next generation. We now turn to analysis of which types of households were most likely to

[33] Figure 2 shows that children around the cut-off of 17 do not benefit in terms of increased expenditures or educational outcomes

[34] Another exception is after-school tutoring in primary school for which we see an increase for sons but a decrease for daughters.

[35] We cannot examine spousal education as an outcome because treatment impacted daughters' likelihood of attending post-secondary school and many were still enrolled at the time of our follow-up survey. Since women typically delay marriage until after graduation, measures of spousal quality among treatment daughters will be biased by endogenous selection.

use wealth gained due to the grace period intervention to invest in their children's education.

To guide our understanding of which types of households should see larger impacts from treatment on children's schooling, we consider a simple two-period model of educational investments under credit constraints. We assume that in an unmodeled period 0, all parents invest their loans in the business and realize returns. In the first period, parents decide whether to invest in sending their child to college, how much to invest in the business and also how much to borrow or save. In the second period, the household earns an income from their educational investments and investments in the enterprise.

Returns on investments in education are a function of parents' own educational attainment. Parental education could impact children's returns to schooling through either lowering costs, such as costs of helping children with schoolwork, or increasing benefits, such as through expanded access to employment opportunities via peer networks. To classify parental education, we take the highest level of schooling completed by either the mother or father.[36] Similar to Shahe Emran et al. (2020), we then classify a parent as having low education if they had no secondary schooling, so at most, parents in this group had completed primary school. Approximately 15% of our sample households are categorized as low-education households. We assume that short-run gains in income as a result of treatment are realized across the distribution of parental education. We also assume that the only way through which the treatment could increase education is through an increase in wealth and not through, for instance, a change in preferences or household bargaining. We address such alternative explanations in section 4.1 below.

Our model is motivated by two key trends observed among households in the control group: first, children's schooling outcomes are positively correlated with those of their parents. Second, household wealth is positively correlated with children's educational attainment. At baseline, we asked households to report whether they owned a pre-specified series of assets and their value.[37] We create a Socio-Economic Index using the first component of a principal component analysis and use this index as a proxy for baseline wealth. In Table A1, we limit the sample to the control group and regress children's education on baseline wealth and parental education. We see that both variables are positively and significantly correlated with educational investments and attainment. The fact that parental education remains significant even after controlling for wealth implies that it is not just a proxy for whether parents can afford to send children to school.

In Appendix Figure A3, we show further evidence that credit constraints limit educational attainment. We plot a local polynomial regression of college attendance on socio-economic status by treatment and control. We see that the relationship between wealth and college attendance is s-shaped: college attendance is at first flat, then rapidly increases after a certain level of wealth,

[36] In 73% of cases, the father's education is higher or equal to the mother's education.

[37] We asked households whether they owned the following assets: a radio, a cassette player, a camera, a refrigerator, a washing machine, a heater, a television, a VCR, a pressure lamp, a tubewell, a wristwatch, and a clock.

and then plateaus again. Appendix Figure A3 also shows that treatment effects on children's education are largest for medium-wealth households. These trends are consistent with the idea that very wealthy households are not credit constrained and treatment impacts would not be sufficient for very poor households to overcome lump-sum costs of education.

Our model yields two testable predictions[38]: first, the impact of an increase in income on children's education should be concentrated among children of more educated parents. In Table 6, we see that gains in children's school performance and increases in education spending are indeed concentrated among children of highly educated parents. The children of high-education parents in the treatment group score higher on dimensions of educational investments and attainment than the children of high-education parents in the control group: they are 6.3 [SE: 1.4] percentage points more likely to attend private secondary school and spend an extra Rs. 4106.2 [SE: 1901.0] on secondary schooling, Rs. 8824.4 [SE: 4600.0] on after-school tutoring in secondary, and Rs. 6466.5 [SE: 3926.0] on college. We do not observe any increase in investments in primary school but, overall, children of high-education parents in the treatment group score 0.25 standard deviations [SE: 0.09] higher on the investment index. They have 0.39 [SE: 0.22] additional years of schooling and are 11.8 [SE: 9.4] percentage points more likely to attend college. These children score 0.2 standard deviations [SE: 0.08] higher than their control group counterparts on the overall attainment index.

By contrast, the low-education household children in the treatment group actually have lower educational investments and attainment than their counterparts in the control group: they score 0.22 [SE:0.12] standard deviations lower on the secondary school education investment index, significant at the 10% level, and 0.26 [SE:0.2] standard deviations lower on the education attainment index. Although not statistically significant, we see for example that treatment children in low-education households are less likely to complete 10th and 12th grade.

The second prediction of our model is that the effect of income gains on business investments decreases with parental education. In Appendix Table A15, we present the income and profits effects of the treatment by parent education (panel A compares outcomes between treatment and control high-education households and panel B compares outcomes between treatment and control low-education households). Although point estimates indicate that both types of parents benefited from the intervention, low-education households have larger treatment effects.

We also asked the parents of children who left school before completing grade 12 why the child had dropped out and present those results in Panel D of Table 6. We detect no differences in reason for drop-out among treatment and control group children in high-education households. Conversely, we find that treatment children in low-education households are more likely to report leaving school early because of money reasons or because school was not worthwhile, a signal that the the grace period lead to an increase in the opportunity cost of keeping a low-education

[38]See Appendix Section D for a complete derivation of the model.

household child in school.

Finally, we find suggestive evidence that the grace period treatment led low-education households to pull their children out of school to capitalize on new investment opportunities in the business. In 2012, we collected a detailed module of household and non-household employees in household businesses. In Appendix Table A16, we analyze employment patterns by high- and low-education households. In column 1 we show total household employees, in column 2 total non-household employees, and in column 3 we have a dummy for whether a household had at least one child under the age of 18 that was engaging in any work activity at the time of the survey. We see that low-education treatment households are significantly more likely than low-education control group households to use child labor. We do not detect any treatment differences in child labor use among treatment and control high-education households.

As a robustness check, in Appendix Table A14 we split the sample by the median education of the parents. We find that results for high-education parents are robust, although the negative treatment effects on educational outcomes among children in low-education households are less pronounced.

4.1 Alternative Explanations

Our results show that the grace period intervention led to substantial increases in children's educational attainment. We have argued that wealth accumulation is the primary channel through which the grace period impacted parents' investments in their children's education. In this section, we discuss three possible alternative explanations for the observed gains in educational attainment. We start by analyzing whether treatment group parents increased spending on education because the grace period led to higher returns to skilled labor in their enterprises. Second, we explore whether increases in education expenditures could be due to a shift in intra-household bargaining power as a result of treatment. Third, we discuss whether it is feasible that the grace period itself — the two month delay before repayment — directly led parents to increase education expenditures.

Grace period raised returns to skilled labor We showed suggestive evidence that the grace period led children of low-education parents to drop out of school early to work in the household business. This suggests that the grace period contract raised returns to unskilled labor for these households. By the same token, the grace period may have led some treatment households to make business investments which increased the returns to skilled (college-educated) labor in their enterprises. High-education parents, for whom the grace period leads to an increase in spending on their children's schooling, may have invested the intent that these children would then use their expanded skill-set at work in the household enterprise.

Though we cannot rule it out, we find this to be an implausible channel of impact of the grace period on education. First, the business sectors in which our sample respondents work

— tailoring, food preparation, kirana shops, etc — are not those in which clients would need high-skilled labor (indeed as noted earlier, fewer than 1% of parents in our sample had a college education). We find no evidence that treatment group clients are more likely to change business sectors. Second, even if there were returns to high-skilled education that could justify the extra expenditures that treatment group parents incur in secondary and post-secondary education, given that the education trajectories of only younger children were impacted, they would have had to at minimum wait 5 to 6 years before the children could be employed in the enterprise. Last, and most important, we see no evidence that children with college degrees return to work on the parent's household enterprise. We find that only 17% (18% for sons) of college graduates in the placebo group engage in self-employment (and almost always this is work on a different enterprise than the parent's). Instead, 64% of college graduates go on to do salaried work (85% for sons). This suggests that households invest in higher education as a means of transitioning into higher-paid salaried work, rather than increasing the skills of household enterprise workers.

Grace period impacted education expenditures via increasing women's bargaining power Female microfinance clients and their spouses may have different preferences regarding spending decisions on their children's education. If spousal preferences are not aligned, then the intervention — which targeted loans to women — may have led to increased expenditures on schooling by increasing female clients' bargaining power within the household. This effect would be similar to that found in Qian (2008), where the author shows that a reform which created an exogenous rise in sex-specific agricultural income among households in China led to increases in children's education. The author argues that strengthening a woman's bargaining power in the household allows her to express her relative preference for equalizing expenditures on boys' and girls' education. More broadly, research finds that improving a woman's bargaining power can lead to increases in educational expenditures by allowing her to direct household resources to human capital investments, for which she has a stronger preference (see Heath and Jayachandran (2016) for a review of this literature).

In columns 1 and 3 of Table A18, we regress an index of female empowerment outcomes on treatment.[39] In column 2 we show the results for the full sample and in column 5 we limit the sample to households with a child 7-17 at baseline. We do not find changes in female empowerment due to the intervention, which leads us to believe that increased female bargaining power is an unlikely explanation for the observed impacts on education. That we do not find impacts of the grace period on women's empowerment may be explained in part by the fact that loans were often invested in male-operated businesses among households in our sample (Bernhardt et al., 2019).

[39]The index consists of whether the client reports having a major say in how much to spend on food, education, clothing, health, home improvements, and recreational activities.

Grace period directly led to investments in education We argue that investments in children's human capital were made possible because the grace period intervention led treatment households to invest productively in their enterprises and experience larger business returns; over time, these increased returns led treatment households to accumulate additional wealth and enabled higher levels of expenditures on education. An alternative explanation for our findings is that the grace period intervention led treatment group households to differentially invest in their children's schooling by directly easing credit constraints on educational expenditures.

Several pieces of evidence run counter to this proposed mechanism: first, credit constraints were eased for both treatment and control households as both types of households were given access to loans in the original intervention. We have no reason to think that deferring loan repayments by two months would have allowed households to differentially reap the returns to children's education, which take years to realize. Moreover, Table 1 in Field et al. (2013) shows that treatment and control households report spending only an average of Rs. 240 of a median Rs. 8000 loan on human capital expenditures. We do not observe a different level of human capital expenditures across treatment and control group households. Even if we restrict our attention to households where children were of schooling age (7-17 years old at baseline), we find similar results (Appendix Table A17).

5 How Valuable Was the Grace Period Intervention? Three Welfare Estimates

We find that the grace period treatment has important impacts on next generation educational outcomes. From a policy perspective, the key trade-off is the relative cost-effectiveness of a grace period credit intervention against other programs which seek to increase household income or children's human capital accumulation in developing countries. To make progress on this question, we calculate three measures of the welfare impact of the intervention: first, we use household-level returns to estimate compute a benefit-cost ratio based on program costs vs. gains in household income. An important limitation of this approach is that estimates do not account for gains in children's future income due to human capital accumulation. We therefore estimate the grace period treatment on children's lifetime earnings using linear approximation of age-earning profiles for college and non-college household members. Finally, policymakers may also want to account for changes to inequality when assessing welfare effects. This requires a measure that distinguishes relative mobility from aggregate gains in education levels. We conclude this section by estimating treatment impact on relative educational mobility.

5.1 Cost-Benefit Analysis of Impacts on Household Income

We calculate benefits of the grace period treatment using our estimate of the average income gain treatment households accrued due to the grace period. As shown in Table 1, we find that

the grace period led to income gains in 2010 (19.6%), 2012 (12.4%), and 2018 (7.8%). In these survey waves, median monthly income in the control group was Rs. 10.000 (2010), Rs. 12.000 (2012), and Rs. 13.000 (2018), respectively. We impute income gains between survey waves using linear interpolation. If we estimate that income gains persist only for the eleven years between baseline and our final follow-up survey—and end immediately thereafter—we would find that treatment group households gained an average of Rs. 77,440 in real terms.[40] In purchasing power parities terms, this gain would be equivalent to $8316.7 PPP.[41] To determine costs of the grace period intervention, we rely on calculations performed by Field et al. (2013). The authors find that the grace period led to an increase of Rs. 148.7 in the average outstanding loan amount within 52 weeks of the loan due date. Using purchasing power parities, this amounts to $11.69 PPP. Together, our estimates of costs and benefits show that the grace period intervention has an average benefit-cost ratio of 564.3 and an internal rate of return of 8,006%.[42]

In comparison, a skills and asset-transfer program in rural Bangladesh costing $1,120 per household increased incomes by 21% after four years, leading to a benefit-cost ratio of 3.2 and an internal rate of return of 22% (Bandiera et al., 2017). We note, though, that although both this study and Bandiera et al. (2017) deal with poor populations, there are important differences between the study settings: ultra-poor interventions target households close to subsistence. In contrast, the median household in our control group sample lives on roughly $4 per person per day. It may be that liquidity shock interventions are best suited as a means of increasing human capital accumulation among urban populations such as ours, where average wealth is higher and where there is relatively easy access to educational institutions. We view this as an important question for future research.

5.2 Analysis of Impact on Child Life-time Earnings

Our cost-benefit estimates do not account for the additional benefits accrued through increased human capital accumulation among children in the treatment group. While it is still too early to calculate the economic returns to the increase in college attendance, an imputation exercise based on age-earnings curves for college and non-college graduates from the 2011-2012 Indian Human Development Survey (Appendix Figure A4) predicts that children aged 7-17 at baseline will have on average a 10% higher income twenty years after the intervention (Figure 3). The impacts on college attainment suggest that the grace period intervention successfully shifted the intergenerational dynamics of poverty within our sample.

[40] We follow Bandiera et al. (2017) and assume a social discount rate of 5%. Annual changes in the consumer price index in India across the study period are obtained from the World Bank.

[41] Purchasing power parities conversion taken for the year 2007 from https://data.oecd.org/conversion/purchasing-power-parities-ppp.htm.

[42] Using the 95% confidence intervals in Table 1, the average benefit-cost ratio ranges from 71.8 to 1057.8 and the internal rate of return from 2056% to 13922%.

5.3 Analysis of Impact on Intergenerational Mobility

Because of small sample sizes in each bin of the parent and child education distributions, we cannot precisely estimate standard mobility measures such as absolute upward mobility ($E[y|x = 25]$) (Chetty et al., 2014). Instead we follow Asher et al. (2020) and estimate bottom-half mobility ($E[y|x \in [0, 50]]$), i.e. the expected rank of a child born to a parent in the bottom half of the education distribution. We find that bottom-half mobility decreases from 0.44 in the control group to 0.42 in the treatment group as a result of the grace period intervention.

Since education gains are concentrated among high-education households, we find that the grace period leads to a *decrease* in relative intergenerational educational mobility within our sample. This suggests that increasing educational attainment for the next generation may require a range of interventions, including conditional transfers targeted towards children of low-education parents.

6 Conclusion

Our results demonstrate how a positive shock to household income can have enduring effects on the next generation through increased human capital investments in children. We are able to estimate these intergenerational treatment effects by collecting panel data on all individuals who were part of the original household—not just those contemporaneously living at home. This type of data collection is relevant to many settings in which researchers are interested in capturing medium- to long-run effects. If this type of intervention causes households to invest in their children's education, then focusing only on financial returns in the short run will miss a key part of the story, necessitating an even longer run follow-up to quantify true impacts. Although the children in our sample are too young for us to measure ultimate marital or occupational outcomes[43], the large difference in college enrollment rates across treatment and control coupled with the high returns to higher education in urban India suggest substantial financial gains for the next generation.

[43] In Appendix Table A19, we present results on treatment effects for socio-economic outcomes: we do not find any impacts on whether children are currently married, working, or on their earnings in the previous 30 days. We note, though, that nearly 20% of control group children are still studying and treatment group children are 7 [SE:3.8] percentage points more likely to report being a student.

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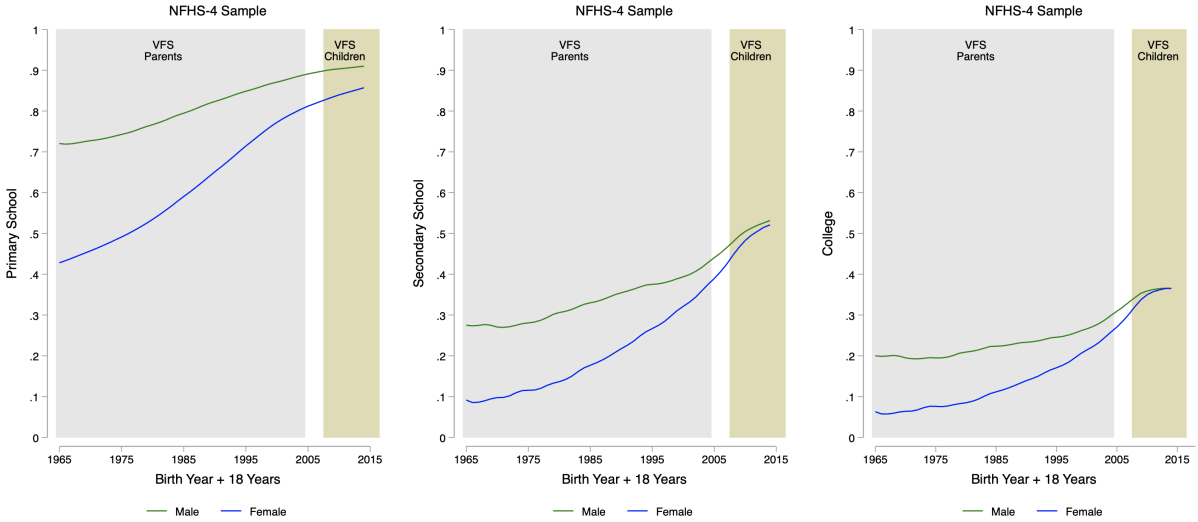
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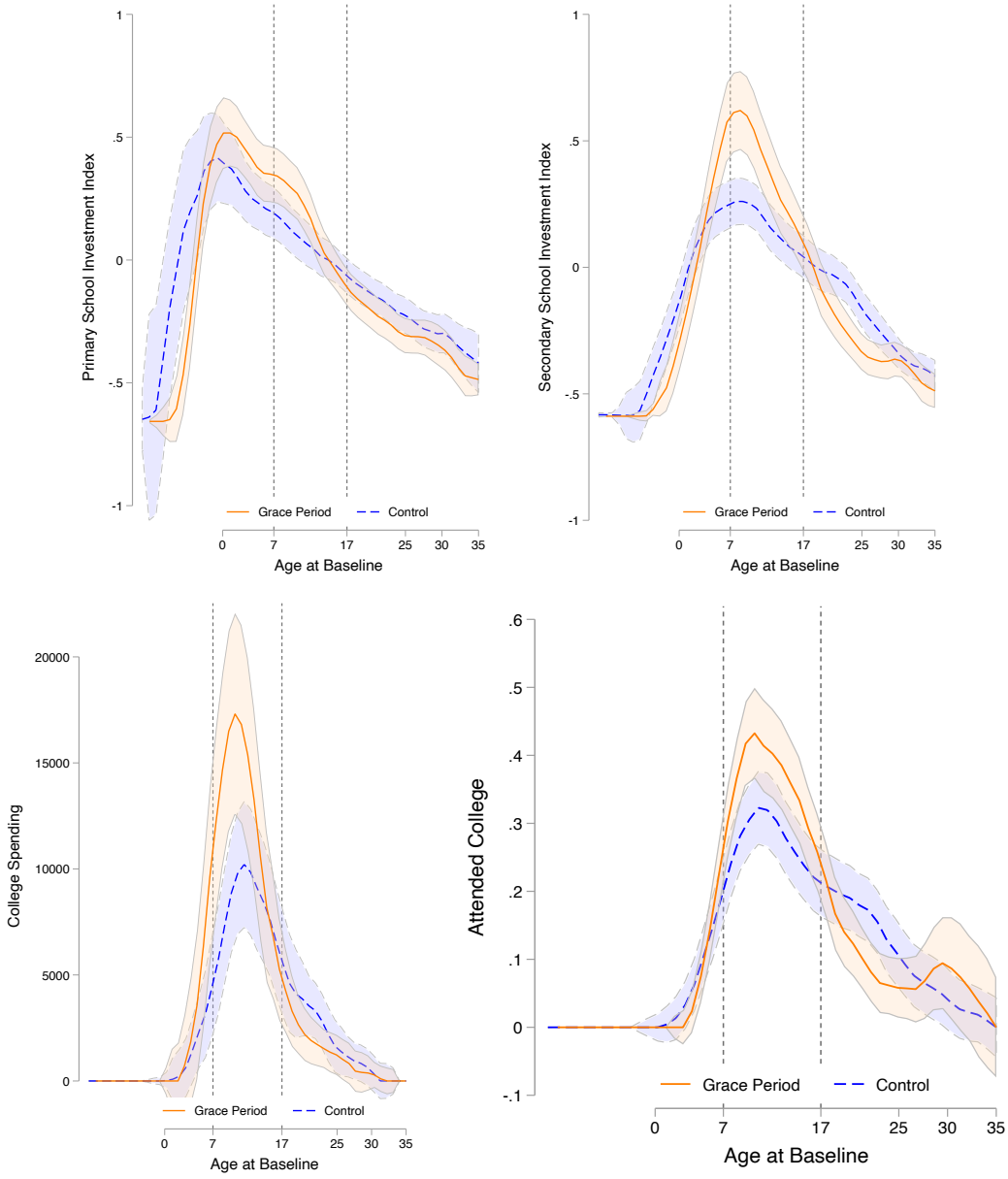
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Figure 1: Educational Trends in India NFHS-4 (2015-16)



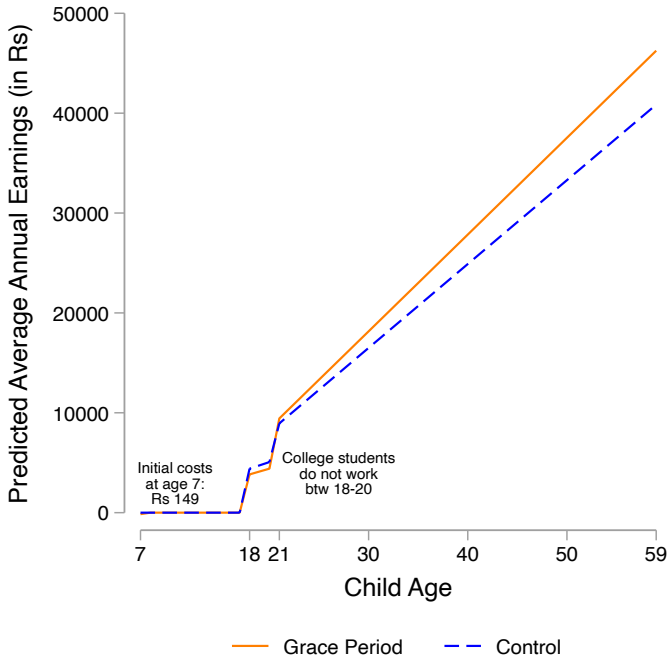
Notes: The figures plot local regressions. Data consists of all household members aged 18-80 urban areas in NFHS-4. The x-axis shows the year in which the person turned 18 years. The golden-shared area shows the age range of the VFS main child sample (aged 7-17 years at baseline) and the grey-shaded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

Figure 2: Childhood Exposure



Notes: The figures plots local regressions. Data consists of all children of the client that were 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 3: Predicted Child Earnings



Notes: The figure plots predicted child earnings by child age and treatment group. The prediction exercise is based on linear age-earning curves for college and non-college graduate in the urban IHDS-2 sample (see Appendix Figure A4). We assume that non-college graduates start to work at age 18 and college graduates start to work at 21. We then use the education-specific constant and age coefficient to predict child earnings. College completion rates for the treatment and control group at based on 2018 enrollment status.

Table 1: Household Economic Outcomes

	Full Household Sample					
	Log Income	Any Business	Profits	Capital	Workers	Formal Savings
	(1)	(2)	(3)	(4)	(5)	(6)
Grace Period	0.197***	0.015	490.561***	18165.857***	0.147	4528.854**
	(0.075)	(0.027)	(163.194)	(6525.544)	(0.212)	(1934.686)
Grace Period × 2012	-0.072	-0.023	-417.298**	-4019.552	0.060	-3275.642
	(0.089)	(0.029)	(189.107)	(7493.736)	(0.210)	(2168.948)
Grace Period × 2018	-0.119	-0.056	-465.403***	-4528.911	-0.184	-1949.950
	(0.084)	(0.035)	(176.632)	(15032.809)	(0.240)	(2254.898)
2012	-0.034	0.051**	134.123	-10329.636**	-0.613***	-2969.066**
	(0.058)	(0.023)	(120.279)	(4455.223)	(0.151)	(1336.264)
2018	-0.347***	0.002	-319.514***	43969.531***	0.050	-3513.288***
	(0.053)	(0.022)	(101.767)	(8727.183)	(0.145)	(1187.085)
p-val: Grace Period + Grace Period x 2012	0.019	0.726	0.592	0.012	0.047	0.391
p-val: Grace Period + Grace Period x 2018	0.111	0.148	0.755	0.363	0.808	0.022
Mean of Omitted Group	9.016	0.850	1173.808	26412.013	1.204	9162.708
Observations	2226	2269	2195	2194	2238	2167

Notes: The panel sample consists of the 2010, 2012, and 2018 endline surveys. Standard errors are clustered by loan group and appear in brackets. All regressions include survey wave dummies, stratification dummies, and controls that are chosen using the double-lasso approach. Appendix Table A3 shows the list of potential lasso controls. Income, profits, capital, number of workers, and formal savings are top-coded at 99.5% for each survey round. Income, profits, capital, and formal savings 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 2: Resilience Results in 2012 Survey

	Full Household Sample					
				Index Components		
	Borrowing Problems	Business Closure	Financial Stress Index	Problems with Bill Payments	Pawned/Sold Something	Asked for Financial Help
	(1)	(2)	(3)	(4)	(5)	(6)
Grace Period	0.023	-0.059*	-0.059	0.011	-0.061**	-0.016
	(0.032)	(0.036)	(0.054)	(0.032)	(0.025)	(0.037)
Mean of Control Group	0.233	0.608	-0.000	0.268	0.163	0.323
Observations	769	771	769	769	768	769

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions include survey wave dummies, stratification dummies, and controls that are chosen using the double-lasso approach. Appendix Table A3 shows the list of potential lasso controls. All outcomes are obtained from the 2012 survey. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table 3: Treatment Effects on Education Expenditure

	Education Expenditure	
	Full Household Sample (1)	School-Age Households (2)
Grace Period	-3.430	7.854
	(40.348)	(70.496)
Grace Period × 2008	31.517	202.547*
	(71.737)	(120.213)
Grace Period × 2012	107.009*	213.231**
	(55.229)	(98.194)
Grace Period × 2018	62.031	121.557
	(91.733)	(140.326)
2008	150.349***	114.895
	(51.179)	(70.523)
2012	72.790**	-36.034
	(33.539)	(54.326)
2018	56.950	-214.233**
	(64.499)	(89.407)
p-val: Grace Period + Grace Period x 2008	0.715	0.073
p-val: Grace Period + Grace Period x 2012	0.052	0.011
p-val: Grace Period + Grace Period x 2018	0.464	0.275
Mean of Omitted Group	420.569	635.483
Observations	3094	1443

Notes: The panel sample consists of the 2007 baseline survey and the 2008, 2010, and 2018 endline surveys. Standard errors are clustered by loan group and appear in brackets. All regressions include survey wave dummies, stratification dummies, and controls that are chosen using the double-lasso approach. Appendix Table A3 shows the list of potential lasso controls. Column 1 consists of the full household sample. Column 2 consists of household that have at least one child aged 7-17 years at baseline according to the 2018 survey. Education expenditure in 2007 was collected for the past 12 months and is divided by 12 to calculate monthly expenses. Education expenditure is 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 4: Treatment Effects on Education Outcomes

	Main Child Sample (7-17 Years at Baseline)	
	Control Mean	Grace Period Coeff.
	(1)	(2)
Panel A: Educational Investments		
Investment Index	0.000	0.203***
		[0.071]
Primary School Investment Subindex	-0.000	0.068
		[0.070]
Private School	0.235	0.037
		[0.041]
Total School Fees	5058.566	1018.778
		[853.907]
Total After-School Tutoring	6386.433	249.289
		[658.155]
Secondary School Investment Subindex	0.000	0.266***
		[0.084]
Private School	0.019	0.049***
		[0.019]
Total School Fees	11898.092	3268.683*
		[1696.812]
Total After-School Tutoring	27089.807	6464.033***
		[2168.914]
College Spending	8241.479	4786.013**
		[2403.507]
Panel B: Educational Attainment		
Attainment Index	0.000	0.137*
		[0.075]
Years of Schooling	9.818	0.162
		[0.228]
Attended College	0.278	0.094**
		[0.039]
Panel C: Educational Perfomance		
Performance Index	0.000	0.032
		[0.067]
Grade A in Class 10	0.182	-0.007
		[0.031]
Grade A in Class 12	0.129	0.005
		[0.027]
Science Track	0.068	0.028
		[0.024]
Observations		524

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 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. 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 5: Heterogeneity by Gender

	Main Child Sample (7-17 Years at Baseline)				
	Sons		Daughters		
	Control Mean	Grace Period Coeff.	Control Mean	Grace Period Coeff.	p-val: Difference
	(1)	(2)	(3)	(4)	(5)
Panel A: Educational Investments					
Investment Index	0.016	0.217**	-0.016	0.181*	0.794
		[0.105]		[0.093]	
Primary School Investment Subindex	-0.026	0.139	0.027	-0.016	0.229
		[0.103]		[0.086]	
Private School	0.248	0.038	0.221	0.026	0.863
		[0.055]		[0.051]	
Total School Fees	5309.048	1479.554	4806.080	473.111	0.565
		[1276.973]		[1161.621]	
Total After-School Tutoring	5503.576	1086.330	7276.031	-806.329	0.092
		[789.928]		[922.592]	
Secondary School Investment Subindex	0.025	0.300***	-0.026	0.222**	0.615
		[0.116]		[0.113]	
Private School	0.023	0.061**	0.015	0.035	0.417
		[0.025]		[0.025]	
Total School Fees	13155.440	1512.982	10630.605	4750.169**	0.354
		[2435.169]		[2383.205]	
Total After-School Tutoring	26369.618	8160.675***	27826.875	4415.864	0.338
		[2914.432]		[2958.424]	
College Spending	9555.267	4210.958	6875.556	6358.151**	0.673
		[3759.731]		[3205.050]	
Panel B: Educational Attainment					
Attainment Index	-0.039	0.148	0.040	0.117	0.817
		[0.097]		[0.107]	
Years of Schooling	9.654	0.273	9.985	0.048	0.579
		[0.298]		[0.318]	
Attended College	0.271	0.095*	0.285	0.089	0.933
		[0.051]		[0.057]	
Panel C: Educational Perfomance					
Performance Index	-0.022	0.113	0.023	-0.061	0.164
		[0.095]		[0.089]	
Grade A in Class 10	0.173	0.007	0.191	-0.023	0.615
		[0.043]		[0.043]	
Grade A in Class 12	0.135	0.019	0.123	-0.012	0.569
		[0.040]		[0.037]	
Science Track	0.053	0.068**	0.084	-0.016	0.078
		[0.034]		[0.033]	
Observations		266		258	

Notes: The estimates in Columns 2 and 4 come from a single regression where we regress the outcome on the treatment indicator, the treatment indicator interacted with gender, and the gender indicator. The coefficients are used to calculate separate treatment effects for each gender. Column 5 reports the p-value for the intervention term. Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 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. 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 6: Heterogeneity by Parental Education

	Main Child Sample (7-17 Years at Baseline)				
	Low Education (≤ Class 4)		High Education (> Class 4)		
	Control Mean	Grace Period Coeff.	Control Mean	Grace Period Coeff.	p-val: Difference
	(1)	(2)	(3)	(4)	(5)
Panel A: Educational Investments					
Investment Index	-0.179	-0.138	0.035	0.255***	0.002
		[0.107]		[0.086]	
Primary School Investment Subindex	-0.178	-0.027	0.035	0.074	0.479
		[0.127]		[0.079]	
Private School	0.070	0.014	0.267	0.037	0.808
		[0.080]		[0.047]	
Total School Fees	3401.951	-795.631	5382.000	1233.976	0.097
		[966.276]		[977.868]	
Total After-School Tutoring	6627.907	824.174	6339.236	-10.646	0.619
		[1548.298]		[701.313]	
Secondary School Investment Subindex	-0.154	-0.220*	0.030	0.366***	0.000
		[0.117]		[0.095]	
Private School	0.000	-0.018	0.023	0.063***	0.004
		[0.014]		[0.024]	
Total School Fees	9860.976	-2893.662	12299.639	4106.161**	0.004
		[1900.639]		[1917.858]	
Total After-School Tutoring	22281.905	-4971.433	28020.369	8824.428***	0.005
		[4599.533]		[2427.958]	
College Spending	5772.093	-1759.678	8737.664	6466.463**	0.097
		[3925.819]		[2856.619]	
Panel B: Educational Attainment					
Attainment Index	-0.241	-0.261	0.047	0.200***	0.025
		[0.201]		[0.077]	
Years of Schooling	9.093	-0.899	9.959	0.389*	0.049
		[0.647]		[0.224]	
Attended College	0.186	-0.076	0.295	0.118***	0.051
		[0.094]		[0.042]	
Panel C: Educational Perfomance					
Performance Index	-0.275	-0.061	0.054	0.042	0.386
		[0.089]		[0.080]	
Grade A in Class 10	0.023	-0.009	0.213	-0.007	0.975
		[0.039]		[0.038]	
Grade A in Class 12	0.048	-0.050	0.145	0.010	0.234
		[0.039]		[0.032]	
Science Track	0.023	0.004	0.077	0.030	0.582
		[0.040]		[0.028]	
Panel D: Dropout Reasons					
Money Reasons	0.071	0.164**	0.134	-0.042	0.008
		[0.075]		[0.034]	
School Was Not Worthwhile	0.024	0.134**	0.088	-0.011	0.038
		[0.063]		[0.026]	
Disliked School	0.167	-0.029	0.088	-0.031	0.983
		[0.094]		[0.031]	
Low Test Scores	0.143	-0.020	0.097	-0.026	0.946
		[0.091]		[0.029]	
Observations		99		424	

Notes: The estimates in Columns 2 and 4 come from a single regression where we regress the outcome on the treatment indicator, the treatment indicator interacted with high parental education, and the high parental education indicator. The coefficients are used to calculate separate treatment effects for each parental education group. Column 5 reports the p-value for the intervention term. High parental education is defined as having at least one parent that attended secondary school. Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 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. 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.

A. Appendix Tables and Figures: Additional Analysis

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

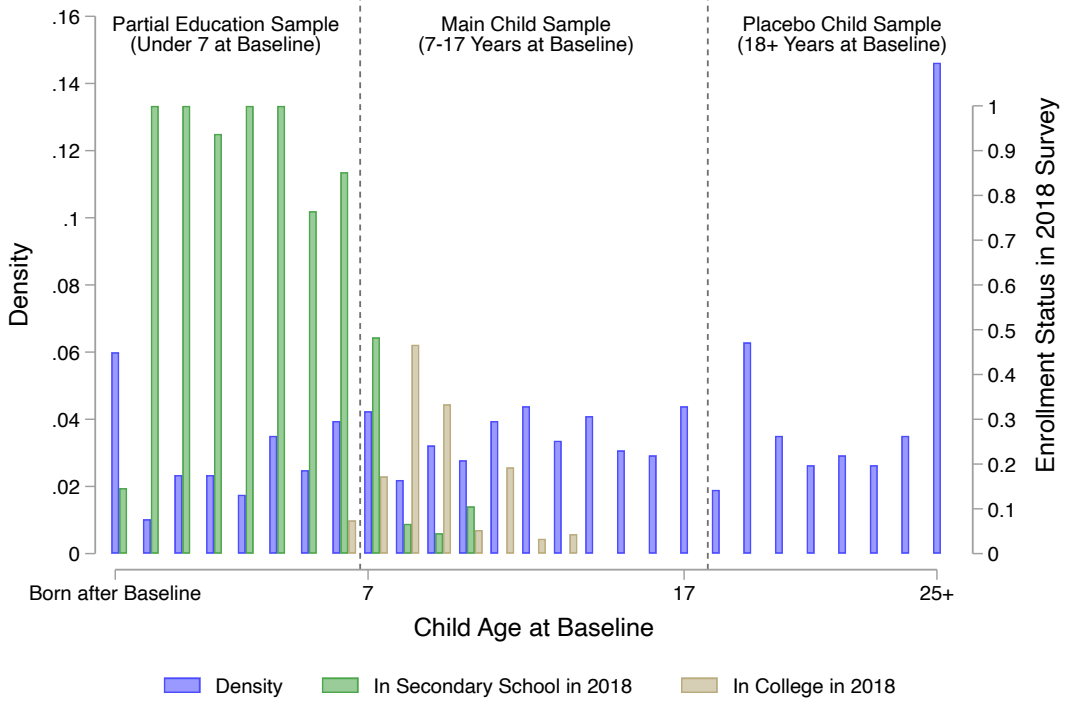


Figure A2: Timeline of Household Surveys

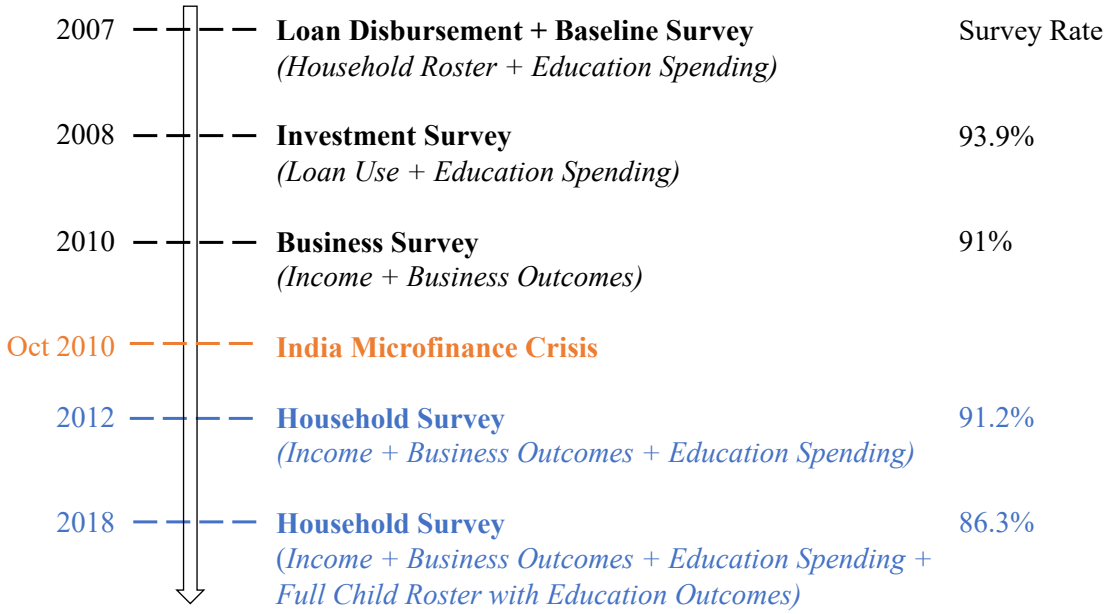
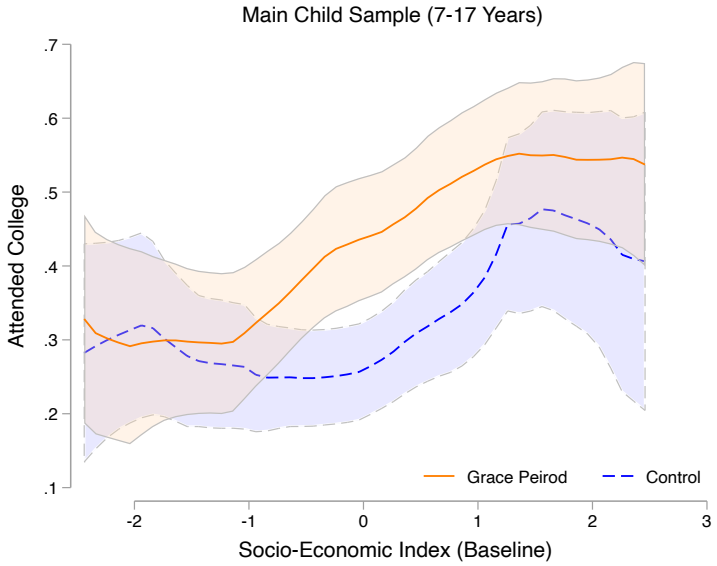
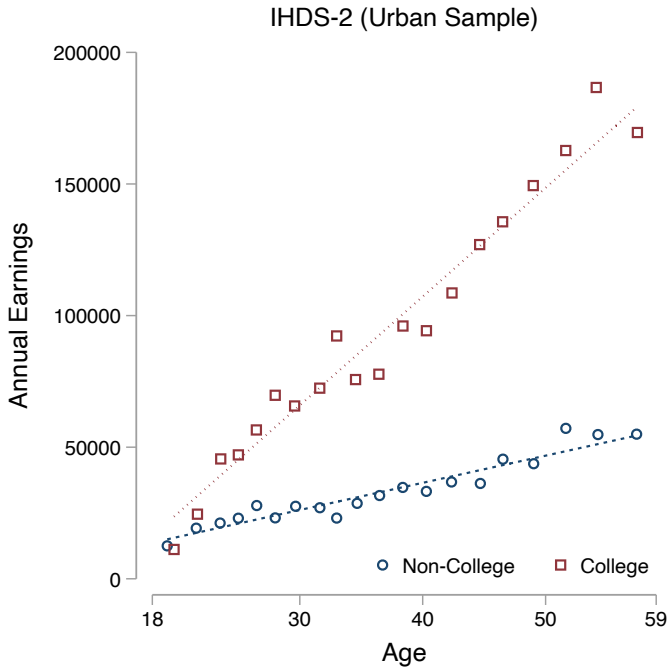


Figure A3: College Enrollment by Treatment and Baseline Wealth



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. All outcomes are obtained from 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: Age-Earning Curves by College Completion (IHDS)



Notes: The figures plots binscatter graphs with annual earnings on the y-axis and age at the x-axis for college and non-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. The dots correspond to binned means and the dashed lines correspond to fitted lines based on linear regressions.

Table A1: Correlational Evidence from Control Group

	Control Group Children (7+ Years at Baseline)						
		Index Components				Index Components	
	Investment Index	College Spending	Secondary School Investment Subindex	Primary School Investment Subindex	Attainment Index	Attended College	Years of Schooling
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Socioeconomic Index	0.078**	945.970	0.085**	0.055**	0.075**	0.018	0.295***
	(0.034)	(857.140)	(0.033)	(0.025)	(0.029)	(0.014)	(0.096)
Highest Parental Education (in Years)	0.056***	1163.004***	0.051***	0.033***	0.090***	0.031***	0.290***
	(0.011)	(311.971)	(0.010)	(0.009)	(0.012)	(0.005)	(0.044)
Mean	-0.000	5729.115	-0.000	-0.000	-0.000	0.211	9.227
Observations	466	452	466	466	466	465	466

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. Robust standard errors appear in brackets. 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 A2: Summary Statistics

	Full Household Sample					
	Control		Grace Period			
	Mean (1)	St. Dev. (2)	Mean (3)	Coeff (4)	St. Err. (5)	N (6)
Panel A: Household Demographics						
Number of Children Born After Baseline	0.114	0.335	0.144	0.032	0.024	729
Number of Children Aged 0-6 Years at Baseline	0.330	0.577	0.391	0.054	0.046	729
Number of Children Aged 7-17 Years at Baseline	0.731	0.855	0.707	-0.026	0.071	729
Number of Children Aged 18 Years or Above at Baseline	0.720	1.096	0.595	-0.115	0.078	729
At Least One Child Born After Baseline	0.108	0.311	0.130	0.023	0.022	729
At Least One Child Aged 0-6 Years at Baseline	0.274	0.447	0.326	0.048	0.035	729
At Least One Child Aged 7-17 Years at Baseline	0.521	0.500	0.497	-0.019	0.036	729
At Least One Child Aged 18 Years or Above at Baseline	0.385	0.487	0.312	-0.066*	0.036	729
Panel B: Parental Education						
Mother's Education: Completed Primary	0.812	0.391	0.739	-0.070**	0.033	839
Mother's Education: Completed Secondary	0.054	0.227	0.070	0.015	0.020	839
Mother's Education: Attended College	0.005	0.069	0.005	-0.000	0.005	839
Father's Education: Completed Primary	0.894	0.308	0.850	-0.041	0.029	708
Father's Education: Completed Secondary	0.114	0.318	0.118	0.005	0.027	708
Father's Education: Attended College	0.011	0.104	0.015	0.004	0.008	708

Notes: Column 4 reports the tests of difference of means where we control for stratification dummies and cluster standard errors by loan group. Data on the number of children in each age range at baseline come from the 2018 survey and data on parental education come from the 2007 baseline 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 A3: Balance Check

	Full Household Sample					School-Age Households				
	Control		Grace Period			Control		Grace Perid		
	Mean (1)	St. Dev. (2)	Coeff. (3)	St. Err. (4)	N (5)	Mean (6)	St. Dev. (7)	Coeff. (8)	St. Err. (9)	N (10)
Panel A: Original Household-Level Controls										
Client's Age	34.508	8.406	-0.637	0.559	842	34.319	5.943	0.219	0.622	370
Married	0.911	0.286	-0.046**	0.022	843	0.963	0.190	-0.009	0.020	370
Muslim	0.007	0.084	0.014	0.012	842	0.011	0.103	0.016	0.015	370
Client's Years of Education	6.609	3.543	-0.104	0.323	839	6.202	3.524	-0.133	0.437	370
Household Size	4.068	1.420	0.127	0.105	842	4.335	1.328	-0.013	0.151	370
Household Shock	0.607	0.489	0.030	0.059	830	0.640	0.481	0.013	0.066	365
Household Has a Business (Narrow)	0.772	0.420	0.014	0.041	843	0.777	0.418	0.047	0.051	370
Owns Home	0.816	0.388	-0.011	0.034	838	0.856	0.352	-0.024	0.039	367
Client Has Financial Control	0.838	0.369	-0.009	0.038	841	0.872	0.335	-0.038	0.044	369
No Drain in Neighborhood	0.129	0.335	-0.022	0.036	830	0.124	0.330	0.018	0.046	365
Loan Amt 4,000 RPS	0.012	0.108	0.001	0.010	845	0.011	0.103	-0.007	0.009	371
Loan Amt 5,000 RPS	0.047	0.212	-0.014	0.017	845	0.048	0.214	-0.000	0.026	371
Loan Amt 6,000 RPS	0.289	0.454	-0.056	0.043	845	0.298	0.459	-0.084	0.053	371
Loan Amt 7,000 RPS	0.002	0.049	-0.002	0.002	845	0.005	0.073	-0.006	0.005	371
Loan Amt 8,000 RPS	0.567	0.496	0.010	0.052	845	0.559	0.498	0.004	0.063	371
Loan Amt 9,000 RPS	0.000	0.000	0.005	0.005	845	0.000	0.000	0.000	0.000	371
Loan Amt 10,000 RPS	0.082	0.275	0.056	0.035	845	0.080	0.272	0.093**	0.040	371
Panel B: Additional Household-Level Controls										
Socioeconomic Index (PCA)	-0.103	1.347	0.210*	0.115	731	-0.137	1.179	0.197	0.155	326
Spouse's Years of Education	7.783	3.313	-0.285	0.315	708	7.345	3.296	0.124	0.385	338
Number of Children (Still Alive in 2018)	1.781	1.056	-0.087	0.091	729	2.080	0.981	-0.075	0.114	371
Education Expenditure 2007	420.569	540.354	6.833	43.282	841	635.483	593.120	11.331	74.050	370
Health Expenditure 2007	368.140	915.473	37.863	72.758	841	308.949	584.586	87.802	105.051	370
Renovation Expenditure 2007	545.502	1240.237	84.322	129.066	644	597.334	1187.270	174.918	160.151	288
Joint Test p-value			0.115					0.677		
Panel C: Child-Level Controls										
Female	0.480	0.500	-0.009	0.027	1360	0.496	0.501	-0.002	0.046	524
Birth Order	1.768	0.980	-0.000	0.073	1360	1.777	0.982	-0.031	0.109	524
Resides with Parents	0.765	0.425	-0.014	0.027	1360	0.943	0.232	0.005	0.025	524

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 Panel 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. Education, health, and renovation expenditure was collected for the past 12 months and is divided by 12 to calculate monthly expenses. All expenditure variables are top-coded at the 99.5th percentile. Panel A lists household-level controls used in Field et al. (2013). See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A4: Attrition Check

	Full Household Sample							
	2008 Survey		2010 Survey		2012 Survey		2018 Survey	
Panel A: Attrition								
	Treat	SE	Treat	SE	Treat	SE	Treat	SE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Attrition	-0.016	(0.018)	0.003	(0.020)	-0.019	(0.022)	-0.026	(0.027)
Control Mean	0.071		0.089		0.099		0.151	
Panel B: Attrition and Baseline Characteristics								
	Attrited x Treat	SE	Attrited x Treat	SE	Attrited x Treat	SE	Attrited x Treat	SE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Client's Age	-2.902	(2.235)	-2.029	(1.966)	-2.821	(2.225)	0.713	(1.567)
Married	0.004	(0.093)	0.097	(0.086)	-0.016	(0.094)	-0.082	(0.082)
Muslim	0.029	(0.046)	-0.007	(0.007)	0.004	(0.010)	-0.003	(0.007)
Client's Years of Education	2.633**	(1.018)	1.239	(0.821)	1.600**	(0.770)	0.819	(0.691)
Household Size	-0.490	(0.339)	0.271	(0.307)	0.510*	(0.305)	0.552**	(0.273)
Household Shock	0.052	(0.143)	0.103	(0.131)	0.111	(0.120)	0.104	(0.109)
Household Has a Business (Narrow)	-0.104	(0.086)	-0.070	(0.087)	-0.052	(0.080)	-0.022	(0.081)
Owns Home	-0.126	(0.132)	-0.100	(0.107)	-0.096	(0.104)	-0.067	(0.087)
Client Has Financial Control	0.058	(0.107)	0.039	(0.074)	-0.013	(0.102)	0.057	(0.068)
No Drain in Neighborhood	-0.130**	(0.063)	-0.031	(0.052)	-0.035	(0.075)	-0.030	(0.074)
Loan Amt 4,000 RPS	-0.000	(0.006)	-0.006	(0.005)	0.026	(0.029)	-0.000	(0.023)
Loan Amt 5,000 RPS	-0.002	(0.053)	-0.073*	(0.038)	-0.051	(0.035)	-0.001	(0.041)
Loan Amt 6,000 RPS	-0.046	(0.139)	-0.036	(0.125)	-0.097	(0.111)	-0.115	(0.092)
Loan Amt 7,000 RPS	0.001	(0.001)	-0.000	(0.001)	0.000	(0.001)	-0.000	(0.001)
Loan Amt 8,000 RPS	0.005	(0.142)	0.074	(0.125)	0.067	(0.128)	0.037	(0.110)
Loan Amt 9,000 RPS	0.002	(0.002)	-0.001	(0.003)	0.028	(0.025)	0.038	(0.035)
Loan Amt 10,000 RPS	0.040	(0.061)	0.043	(0.071)	0.026	(0.069)	0.042	(0.069)
Socioeconomic Index (PCA)	0.346	(0.436)	0.417	(0.339)	0.572	(0.382)	0.025	(0.288)
Spouse's Years of Education	1.005	(0.988)	-0.126	(1.076)	0.451	(1.240)	-0.827	(0.849)
Education Expenditure 2007	-3.739	(223.474)	-39.598	(99.647)	-193.895	(134.048)	235.599**	(114.710)
Health Expenditure 2007	84.105	(105.840)	-169.917	(258.849)	-402.946*	(229.020)	246.114	(197.211)
Renovation Expenditure 2007	-218.324	(242.023)	-103.765	(527.110)	-112.848	(562.797)	342.992	(300.441)

Notes: The sample consists of all households. 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. All regressions control for stratification dummies and cluster standard errors by loan group. 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: Attrition Check for School-Age Households

	School-Age Households					
	2008 Survey		2010 Survey		2012 Survey	
Panel A: Attrition						
	Treat	SE	Treat	SE	Treat	SE
	(1)	(2)	(3)	(4)	(5)	(6)
Attrition	-0.028	(0.020)	-0.009	(0.021)	-0.004	(0.017)
Control Mean	0.059		0.053		0.032	
Panel B: Attrition and Baseline Characteristics						
	Attrited x Treat	SE	Attrited x Treat	SE	Attrited x Treat	SE
	(1)	(2)	(3)	(4)	(5)	(6)
Mother's Age	-0.477	(3.005)	-4.408**	(2.086)	-2.453	(3.602)
Married	0.078	(0.086)	-0.003	(0.016)	0.021	(0.019)
Muslim	-0.026	(0.019)	-0.003	(0.011)	0.015	(0.015)
Mother's Years of Education	1.921	(1.475)	-0.036	(1.829)	-0.956	(1.884)
Household Size	-0.790*	(0.425)	0.174	(0.484)	0.950	(0.891)
Household Shock	0.100	(0.256)	0.006	(0.213)	0.126	(0.282)
Household Has a Business (Narrow)	0.100	(0.081)	0.027	(0.225)	0.164	(0.165)
Owns Home	0.163	(0.217)	-0.325	(0.209)	-0.211	(0.189)
Mother Has Financial Control	0.271*	(0.150)	0.083	(0.178)	-0.044	(0.297)
No Drain in Neighborhood	-0.174	(0.121)	0.175	(0.131)	-0.256	(0.165)
Loan Amt 4,000 RPS	0.001	(0.010)	-0.002	(0.007)	-0.000	(0.006)
Loan Amt 5,000 RPS	0.148	(0.136)	-0.105	(0.093)	0.007	(0.019)
Loan Amt 6,000 RPS	-0.358**	(0.147)	-0.268	(0.199)	-0.113	(0.254)
Loan Amt 7,000 RPS	0.004	(0.006)	0.002	(0.003)	-0.000	(0.002)
Loan Amt 8,000 RPS	-0.079	(0.235)	0.211	(0.243)	0.084	(0.345)
Loan Amt 9,000 RPS	0.000	(0.000)	0.000	(0.000)	0.000	(0.000)
Loan Amt 10,000 RPS	0.285*	(0.149)	0.163	(0.186)	0.023	(0.240)
Socioeconomic Index (PCA)	-0.034	(0.712)	0.654	(0.680)	0.576	(0.634)
Father's Years of Education	1.072	(1.155)	0.829	(2.084)	1.666	(2.386)
Education Expenditure 2007	217.402	(442.946)	164.792	(306.076)	-832.550**	(324.021)
Health Expenditure 2007	142.424	(198.804)	234.581	(324.664)	-218.309	(178.839)
Renovation Expenditure 2007	-457.183	(485.781)	998.615	(903.997)	-577.686	(497.823)

Notes: The sample consists of household that have at least one child aged 7-17 years at baseline according to the 2018 survey. 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. All regressions control for stratification dummies and cluster standard errors by loan group. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A6: Attrition Reasons in 2018

	Attrition Breakdown for 2018			
	Full Household Sample			
	Dead	Migrated	Refused	Other
	(1)	(2)	(3)	(4)
Grace Period	-0.008	-0.016	-0.009	0.007
	(0.010)	(0.017)	(0.013)	(0.012)
Mean of Omitted Group	0.026	0.068	0.033	0.024
Observations	845	845	845	845

Notes: The sample consists of all households. Standard errors are clustered by loan group and appear in brackets. All regressions are run on the household level and include stratification dummies. The 'other'-outcome in column 8 includes clients that could not be located and clients that were unsurveyable due to illness. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A7: Economic Outcomes for School-Age Households

	School-Age Households				
	Log Income	Profits	Capital	Workers	Formal Savings
	(1)	(2)	(3)	(4)	(5)
Grace Period	0.195*	603.525**	18591.156*	-0.012	1711.012
	(0.109)	(241.634)	(10191.822)	(0.273)	(2334.775)
Grace Period × 2012	-0.052	-339.481	-6592.282	0.291	-495.855
	(0.128)	(256.072)	(11149.262)	(0.267)	(2911.829)
Grace Period × 2018	-0.087	-496.793**	5180.081	-0.119	1088.597
	(0.110)	(239.573)	(22477.986)	(0.306)	(2953.600)
2012	-0.092	4.701	-8416.365	-0.650***	-3462.122*
	(0.097)	(117.452)	(7400.576)	(0.205)	(1970.115)
2018	-0.329***	-342.064***	49421.819***	0.227	-3125.248*
	(0.077)	(117.282)	(13473.411)	(0.211)	(1785.219)
p-val: Grace Period + Grace Period x 2012	0.097	0.163	0.238	0.069	0.545
p-val: Grace Period + Grace Period x 2018	0.103	0.328	0.335	0.531	0.080
Mean of Omitted Group	9.054	1218.822	29322.042	1.174	8809.229
Observations	1061	1040	1042	1064	1061

Notes: The panel sample consists of the 2010, 2012, and 2018 endline surveys for households that have 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 brackets. All regressions include survey wave dummies, stratification dummies, and controls that are chosen using the double-lasso approach. Appendix Table A3 shows the list of potential lasso controls. Income, profits, capital, number of workers, and formal savings are top-coded at 99.5% for each survey round. Income, profits, capital, and formal savings 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 A8: Additional Business Results in 2012 Survey

	Full Household Sample	
	Number of Clients	Number of Goods and Services Provided
	(1)	(2)
Grace Period	387.754***	4.050*
	(123.492)	(2.193)
Mean of Control Group	376.773	7.461
Observations	744	769

Notes: The sample consists of all households. Standard errors are clustered by loan group and appear in brackets. All regressions include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 shows the list of potential lasso controls. Both outcomes are top-coded at 99.5% for each survey round. 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: IV Results

	Full Household Sample		School-Age Households	
	Income (IHS)	Education Exp. (IHS)	Income (IHS)	Education Exp. (IHS)
	(1)	(2)	(3)	(4)
Grace Period	0.141***		0.156**	
	(0.041)		(0.063)	
Income (IHS)		1.104		2.818
		(1.497)		(1.875)
Specification	OLS	2SLS	OLS	2SLS
F-Stat	11.571		6.143	
Mean of Omitted Group	9.579	4.613	9.597	4.696
Observations	2095	2095	1021	1021

Notes: The panel sample consists of the 2010, 2012, and 2018 endline surveys. 2010 education expenditure was imputed using the arithmetic mean of 2008 and 2012 education expenditure. Income and educational expenditure are transformed using the inverse hyperbolic sine. Columns 1-2 consists of the full sample and columns 2-3 consist of households that have 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 brackets. All regressions include survey wave dummies and stratification dummies Income and educational expenditure 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 A10: Heterogeneity by Exposure

	Main Child Sample (7-17 Years at Baseline)				
	7-11 Years		12-17 Years		
	Control Mean	Grace Period Coeff.	Control Mean	Grace Period Coeff.	p-val: Difference
	(1)	(2)	(3)	(4)	(5)
Panel A: Educational Investments					
Investment Index	0.069	0.378***	-0.051	0.046	0.022
		[0.113]		[0.088]	
Primary School Investment Subindex	0.082	0.191	-0.061	-0.047	0.088
		[0.117]		[0.080]	
Private School	0.250	0.092	0.224	-0.012	0.132
		[0.057]		[0.050]	
Total School Fees	5777.091	2133.049	4498.014	-35.361	0.242
		[1600.524]		[884.654]	
Total After-School Tutoring	7220.541	759.988	5777.316	-321.261	0.355
		[1023.103]		[735.779]	
Secondary School Investment Subindex	0.070	0.464***	-0.052	0.115	0.024
		[0.123]		[0.101]	
Private School	0.018	0.072**	0.020	0.029	0.213
		[0.029]		[0.023]	
Total School Fees	12623.273	6561.969***	11324.209	75.071	0.031
		[2399.401]		[2067.243]	
Total After-School Tutoring	31283.636	9622.282***	23993.691	2987.123	0.104
		[3326.275]		[2647.114]	
College Spending	8368.148	9451.170**	8149.664	970.156	0.116
		[4107.233]		[3115.145]	
Panel B: Educational Attainment					
Attainment Index	0.078	0.223*	-0.057	0.055	0.227
		[0.116]		[0.090]	
Years of Schooling	9.902	0.630*	9.757	-0.140	0.042
		[0.330]		[0.271]	
Attended College	0.333	0.097	0.237	0.083	0.855
		[0.063]		[0.051]	
Panel C: Educational Perfomance					
Performance Index	0.034	0.081	-0.025	-0.014	0.463
		[0.102]		[0.087]	
Grade A in Class 10	0.170	0.050	0.191	-0.053	0.111
		[0.048]		[0.042]	
Grade A in Class 12	0.134	0.048	0.126	-0.032	0.159
		[0.044]		[0.035]	
Science Track	0.098	-0.003	0.046	0.051*	0.234
		[0.037]		[0.029]	
Observations		234		290	

Notes: The estimates in Columns 2 and 4 come from a single regression where we regress the outcome on the treatment indicator, the treatment indicator interacted with the age group indicator, and the age group indicator. The coefficients are used to calculate separate treatment effects for each age group. Column 5 reports the p-value for the intervention term. Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 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. All outcomes are obtained from the 2018 survey. See Data Appendix for detailed variable definitions. School fees, after-school tutoring, and college spending are top-coded at 99.5%. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A11: Robustness Checks

	Grace Period Coeff. for Different Age Cutoffs				
	6-17 Years	8-17 Years	6-18 Years	7-18 Years	8-18 Years
Panel A: Educational Investments					
Investment Index	0.219***	0.199**	0.203***	0.214***	0.182**
	[0.078]	[0.087]	[0.075]	[0.080]	[0.083]
Primary School Investment Subindex	0.070	0.050	0.052	0.047	0.030
	[0.067]	[0.073]	[0.066]	[0.068]	[0.072]
Private School	0.054	0.019	0.038	0.017	-0.006
	[0.039]	[0.042]	[0.038]	[0.040]	[0.041]
Total School Fees	784.318	649.285	625.099	834.125	397.085
	[800.739]	[796.157]	[763.182]	[811.091]	[754.219]
Total After-School Tutoring	301.420	473.063	335.943	289.548	503.318
	[634.931]	[710.588]	[629.050]	[645.921]	[692.335]
Secondary School Investment Subindex	0.230***	0.226***	0.193**	0.231***	0.198**
	[0.080]	[0.081]	[0.077]	[0.079]	[0.078]
Private School	0.043**	0.048**	0.042**	0.048**	0.045**
	[0.020]	[0.019]	[0.020]	[0.019]	[0.020]
Total School Fees	2537.017	2098.404	2362.401	2700.305*	1614.426
	[1595.962]	[1712.916]	[1525.399]	[1589.172]	[1478.087]
Total After-School Tutoring	5014.305**	5485.712**	4877.651**	6272.315***	5326.460***
	[2157.219]	[2170.286]	[2052.384]	[2075.389]	[2070.346]
College Spending	5078.841**	3871.920	3826.311*	4541.953*	3673.819
	[2234.765]	[2531.446]	[1974.460]	[2332.604]	[2289.797]
Panel B: Educational Attainment					
Attainment Index	0.154**	0.125	0.141**	0.139*	0.117
	[0.072]	[0.081]	[0.070]	[0.076]	[0.080]
Years of Schooling	0.192	0.067	0.168	0.169	0.082
	[0.211]	[0.240]	[0.206]	[0.225]	[0.236]
Attended College	0.094***	0.079*	0.095***	0.093**	0.083**
	[0.035]	[0.041]	[0.035]	[0.039]	[0.040]
Observations	574	465	608	558	499

Notes: Each columns corresponds to a set of regressions with a different age cutoff. Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 shows the list of potential lasso controls. 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 A12: Robustness Checks with Alternative Education Outcomes and Age Groups

	Control Mean	Grace Period Coeff.
	(1)	(2)
Panel A: Primary School (Children Age at Baseline: -1 – 9 Years)		
Private Primary School	0.288	0.082
		[0.053]
Total Primary School Fees	7799.215	892.076
		[1394.762]
Total Primary School After-School Tutoring	8004.842	396.602
		[811.370]
Observations		416
Panel B: Secondary School, until Grade 10 Children Age at Baseline: 5 – 15 Years)		
Private Secondary School (Grades 5-10)	0.031	0.028
		[0.018]
Total School Fees (Grades 5-10)	10650.467	1752.164
		[1462.915]
Total After-School Tutoring (Grades 5-10)	20524.370	3543.965**
		[1521.483]
Observations		523

Notes: The sample in Panel A consists of children of the client aged -1-9 at baseline that are still alive at the time of the 2018 survey. A negative age refers to the number of years the child was born after baseline. The sample in Panel B consists of children of the client aged 5-15 at baseline that are still alive at the time of the 2018 survey. Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 shows the list of potential lasso controls. All outcomes are obtained from the 2018 survey. Primary and 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 A13: Placebo Results

	Placebo Child Sample (18+ Years at Baseline)	
	Control Mean	Grace Period Coeff.
	(1)	(2)
Panel A: Educational Investments		
Investment Index	0.000	-0.038
		[0.058]
Primary School Investment Subindex	-0.000	-0.067
		[0.067]
Private School	0.135	-0.032
		[0.034]
Total School Fees	3100.529	-747.259
		[486.386]
Total After-School Tutoring	4032.362	290.078
		[661.092]
Secondary School Investment Subindex	0.000	-0.046
		[0.055]
Private School	0.023	-0.004
		[0.013]
Total School Fees	6932.306	-1748.876**
		[859.604]
Total After-School Tutoring	15840.988	-1227.307
		[2094.424]
College Spending	2533.730	-158.599
		[682.878]
Panel B: Educational Attainment		
Attainment Index	-0.000	-0.040
		[0.068]
Years of Schooling	8.552	-0.232
		[0.273]
Attended College	0.131	0.014
		[0.024]
Panel C: Educational Perfomance		
Performance Index	0.000	-0.048
		[0.089]
Grade A in Class 10	0.081	-0.031
		[0.029]
Grade A in Class 12	0.035	0.010
		[0.021]
Science Track	0.027	-0.014
		[0.014]
Observations		479

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 shows the list of potential lasso controls. The sample consists of children of the client aged 18 years or older 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 A14: Heterogeneity by Median Parental Education

	Main Child Sample (7-17 Years at Baseline)				
	Low Education (≤ Class 7)		High Education (> Class 7)		
	Control Mean	Grace Period Coeff.	Control Mean	Grace Period Coeff.	p-val: Difference
Panel A: Educational Investments					
Investment Index	-0.162	0.043	0.107	0.376***	0.015
		[0.089]		[0.104]	
Private Secondary School	0.000	0.010	0.031	0.071***	0.023
		[0.019]		[0.025]	
Total Secondary School Fees	9093.500	1550.968	13780.369	4491.460*	0.332
		[1977.568]		[2395.286]	
Total After-School Tutoring	23020.971	-1016.598	29776.282	11226.370***	0.008
		[3204.413]		[3079.474]	
College Spending	4052.596	-1042.632	11088.824	8672.222**	0.041
		[2193.220]		[3957.519]	
Panel B: Educational Attainment					
Attainment Index	-0.186	-0.143	0.123	0.319***	0.002
		[0.122]		[0.091]	
Completed Grade 10	0.533	-0.097	0.635	0.187***	0.000
		[0.072]		[0.041]	
Completed Grade 12	0.371	-0.107*	0.472	0.140***	0.003
		[0.064]		[0.054]	
Attended College	0.162	0.028	0.354	0.151***	0.128
		[0.059]		[0.054]	
Years of Education	9.676	-0.575	10.994	0.882***	0.010
		[0.474]		[0.334]	
Panel C: Educational Perfomance					
Performance Index	-0.187	-0.077	0.123	0.096	0.177
		[0.075]		[0.102]	
Grade A in Class 10	0.086	-0.047	0.245	0.025	0.242
		[0.034]		[0.048]	
Grade A in Class 12	0.077	-0.035	0.164	0.020	0.290
		[0.033]		[0.040]	
Science Track	0.029	-0.013	0.094	0.056	0.129
		[0.026]		[0.038]	
Panel D: Dropout Reasons					
Money Reasons	0.146	0.034	0.110	-0.030	0.384
		[0.064]		[0.035]	
School Was Not Worthwhile	0.058	0.100**	0.090	-0.039	0.008
		[0.043]		[0.029]	
Disliked School	0.107	0.026	0.097	-0.060*	0.158
		[0.053]		[0.033]	
Low Test Scores	0.155	-0.033	0.071	-0.016	0.798
		[0.059]		[0.028]	
Observations		210		313	

Notes: The estimates in Columns 2 and 4 come from a single regression where we regress the outcome on the treatment indicator, the treatment indicator interacted with high parental education, and the high parental education indicator. The coefficients are used to calculate separate treatment effects for each parental education group. Column 5 reports the p-value for the intervention term. High parental education is defined as having at least one parent that attended class 8. Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 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. 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 A15: Household Economic Results by Parental Education

	School-Age Households	
	Log Income (1)	Profits (2)
Panel A: High Education Households		
Grace Period	0.130	358.873*
	(0.118)	(217.649)
Grace Period × 2012	-0.016	11.116
	(0.138)	(238.185)
Grace Period × 2018	-0.056	-317.994
	(0.122)	(214.172)
2012	-0.118	-40.765
	(0.103)	(119.334)
2018	-0.345***	-332.264***
	(0.084)	(119.281)
Panel B: Low Education Households		
Grace Period	0.495*	1276.760*
	(0.264)	(660.573)
Grace Period × 2012	-0.278	-1953.823**
	(0.291)	(850.095)
Grace Period × 2018	-0.237	-1228.427
	(0.276)	(796.284)
2012	0.091	352.873
	(0.210)	(410.977)
2018	-0.225	-379.841
	(0.205)	(280.174)
Mean of Omitted Group (High Edu)	9.097	1238.174
Observations (High Edu)	893	875
Mean of Omitted Group (Low Edu)	8.754	1076.901
Observations (Low Edu)	165	162
p-val: Grace Period + Grace Period x 2012 (High Edu)	0.195	0.096
p-val: Grace Period + Grace Period x 2018 (High Edu)	0.317	0.734
p-val: Grace Period + Grace Period x 2012 (Low Edu)	0.339	0.147
p-val: Grace Period + Grace Period x 2018 (Low Edu)	0.086	0.869
p-val: Grace Period x High. Edu	0.130	0.116
p-val: Grace Period x High. Edu + Grace Period x High. Edu x 2012	0.496	0.113
p-val: Grace Period x High. Edu + Grace Period x High. Edu x 2018	0.126	0.098

Notes: The panel sample consists of the 2010, 2012, and 2018 endline surveys for households that have 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 brackets. All regressions include survey wave dummies, stratification dummies, and controls that are chosen using the double-lasso approach. Appendix Table A3 shows the list of potential lasso controls. Panel A and Panel B come from separate regression. High education households are defined as households in which either the client or her spouse attended secondary school. The bottom three p-values come from a single regression in which we interact the grace period and year dummies and their interactions with a high parental education indicator. Both outcomes 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 A16: Child Labor

	School-Age Households		
	Number of HH Employees (1)	Number of Non-HH Employees (2)	Any Child Labor (3)
Grace Period	0.244	0.204	0.114***
	(0.233)	(0.201)	(0.042)
Grace Period × High Edu. Parents	-0.181	-0.098	-0.127**
	(0.270)	(0.210)	(0.050)
High Edu. Parents	-0.043	-0.113	0.118***
	(0.174)	(0.172)	(0.045)
p-val: Grace Period + Grace Period x High Edu	0.580	0.062	0.589
Mean of Omitted Group	0.182	0.227	0.000
Observations	356	356	359

Notes: The sample consists of household that have 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 brackets. All regressions include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 shows the list of potential lasso controls. The number of household and non-household workers 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 A17: Self-Reported Loan Use

	Full Household Sample				School-Age Households			
		Non-Business Expenditure				Non-Business Expenditure		
	Total Business Expenditure	Total	Education	Health	Total Business Expenditure	Total	Education	Health
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grace Period	400.969**	-276.878	-49.350	17.283	468.748*	-321.874	-79.418	-16.951
	(186.103)	(176.824)	(47.106)	(67.822)	(239.331)	(217.994)	(71.765)	(64.147)
Mean of Omitted Group	6278.807	1119.347	119.588	101.878	6467.388	995.676	129.787	99.931
Observations	845	845	845	845	371	371	371	371

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the household level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 shows the list of potential lasso controls. All outcomes are obtained from the 2008 survey. Columns 1-4 consist of the full household sample. Column 5-8 consists of household that have at least one child aged 7-17 years at baseline according to the 2018 survey. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A18: Alternative Mechanisms

	Full Household Sample		School-Age Households	
	Empowerment Index	Migrated	Empowerment Index	Migrated
	(1)	(2)	(3)	(4)
Grace Period	-0.014	0.014	0.038	0.015
	(0.058)	(0.021)	(0.085)	(0.028)
Mean of Omitted Group	-0.000	0.097	0.029	0.074
Observations	729	729	371	371

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the household level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 shows the list of potential lasso controls. 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 A19: Treatment Effects on Socio-Economic Outcomes

	Main Child Sample (7-17 Years at Baseline)	
	Control Mean	Grace Period Coeff.
	(1)	(2)
Married	0.449	-0.026
		[0.040]
Married as Child	0.129	-0.025
		[0.031]
Resides with Parents	0.621	0.027
		[0.036]
Currently a Student	0.182	0.070*
		[0.038]
Currently in College	0.103	0.082***
		[0.030]
Full-Time Employed/Self-Employed	0.460	0.019
		[0.036]
Unemployed	0.057	0.000
		[0.023]
Worked (Past 30 Days)	0.519	-0.001
		[0.039]
Engaged in Salaried Work (Past 30 Days)	0.249	-0.028
		[0.039]
Engaged in Daily Wage Work (Past 30 Days)	0.092	-0.005
		[0.028]
Engaged in Self-Employment (Past 30 Days)	0.199	0.016
		[0.038]
Days Worked (Past 30 Days)	11.732	-0.753
		[0.932]
Hours Worked (Past 7 Days)	21.581	-2.490
		[1.973]
Child Income (Past 30 Days)	3278.700	117.977
		[474.648]
Observations		524

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 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. All outcomes are obtained from the 2018 survey. Child income and hours and days worked 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 A20: Treatment Effects on Socio-Economic Outcomes by Gender

	Main Child Sample (7-17 Years at Baseline)				
	Sons		Daughters		
	Control Mean	Grace Period Coeff.	Control Mean	Grace Period Coeff.	p-val: Difference
	(1)	(2)	(3)	(4)	(5)
Married	0.212	0.015	0.687	-0.071	0.208
		[0.050]		[0.056]	
Married as Child	0.015	-0.006	0.244	-0.048	0.502
		[0.019]		[0.061]	
Resides with Parents	0.917	-0.023	0.321	0.076	0.167
		[0.044]		[0.058]	
Currently a Student	0.173	0.090*	0.191	0.048	0.521
		[0.051]		[0.049]	
Currently in College	0.106	0.110***	0.101	0.050	0.286
		[0.040]		[0.042]	
Full-Time Employed/Self-Employed	0.742	-0.024	0.176	0.061	0.278
		[0.056]		[0.051]	
Unemployed	0.091	-0.049	0.023	0.052**	0.011
		[0.034]		[0.026]	
Worked (Past 30 Days)	0.774	-0.008	0.260	0.009	0.829
		[0.058]		[0.052]	
Engaged in Salaried Work (Past 30 Days)	0.374	0.008	0.123	-0.070*	0.307
		[0.066]		[0.039]	
Engaged in Daily Wage Work (Past 30 Days)	0.183	-0.016	0.000	0.007	0.670
		[0.052]		[0.014]	
Engaged in Self-Employment (Past 30 Days)	0.252	-0.015	0.146	0.055	0.337
		[0.059]		[0.045]	
Days Worked (Past 30 Days)	17.889	-0.518	5.672	-0.709	0.922
		[1.470]		[1.220]	
Hours Worked (Past 7 Days)	35.722	-3.255	7.551	-1.578	0.686
		[3.419]		[2.150]	
Child Income (Past 30 Days)	5484.350	549.071	1142.520	-277.301	0.342
		[791.264]		[441.708]	
Housewife			0.611	-0.153***	
				[0.053]	
Observations		266		258	

Notes: The estimates in Columns 2 and 4 come from a single regression where we regress the outcome on the treatment indicator, the treatment indicator interacted with gender, and the gender indicator. The coefficients are used to calculate separate treatment effects for each gender. Column 5 reports the p-value for the intervention term. Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 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. All outcomes are obtained from the 2018 survey. Child income and hours and days worked 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 A21: Treatment Effects on Socio-Economic Outcomes by Parental Education

	Main Child Sample (7-17 Years at Baseline)				
	Low Education (≤ Class 4)		High Education (> Class 4)		
	Control Mean	Grace Period Coeff.	Control Mean	Grace Period Coeff.	p-val: Difference
	(1)	(2)	(3)	(4)	(5)
Married	0.581	0.156*	0.423	-0.071	0.030
		[0.094]		[0.043]	
Married as Child	0.256	-0.079	0.105	-0.022	0.471
		[0.075]		[0.031]	
Resides with Parents	0.512	-0.082	0.643	0.049	0.123
		[0.074]		[0.042]	
Currently a Student	0.186	-0.078	0.181	0.100**	0.071
		[0.085]		[0.044]	
Currently in College	0.116	-0.047	0.101	0.097***	0.064
		[0.073]		[0.032]	
Full-Time Employed/Self-Employed	0.349	0.174**	0.482	-0.021	0.051
		[0.086]		[0.042]	
Unemployed	0.047	-0.028	0.059	0.008	0.479
		[0.043]		[0.026]	
Worked (Past 30 Days)	0.419	0.121	0.538	-0.029	0.130
		[0.087]		[0.044]	
Engaged in Salaried Work (Past 30 Days)	0.071	0.087	0.283	-0.047	0.094
		[0.068]		[0.045]	
Engaged in Daily Wage Work (Past 30 Days)	0.119	0.017	0.087	-0.015	0.690
		[0.077]		[0.028]	
Engaged in Self-Employment (Past 30 Days)	0.238	-0.008	0.192	0.019	0.732
		[0.073]		[0.042]	
Days Worked (Past 30 Days)	9.024	2.042	12.254	-1.375	0.107
		[1.825]		[1.066]	
Hours Worked (Past 7 Days)	16.366	-0.484	22.590	-2.881	0.576
		[3.843]		[2.199]	
Child Income (Past 30 Days)	2089.024	671.823	3512.081	113.612	0.440
		[588.150]		[537.050]	
Observations		99		424	

Notes: The estimates in Columns 2 and 4 come from a single regression where we regress the outcome on the treatment indicator, the treatment indicator interacted with high parental education, and the high parental education indicator. The coefficients are used to calculate separate treatment effects for each parental education group. Column 5 reports the p-value for the intervention term. High parental education is defined as having at least one parent that attended secondary school. Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 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. All outcomes are obtained from the 2018 survey. Child income and hours and days worked 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 A22: Treatment Effects on Socio-Economic Outcomes for Placebo Group

	Placebo Child Sample (18+ Years at Baseline)	
	Control Mean	Grace Period Coeff.
	(1)	(2)
Married	0.903	-0.024
		[0.031]
Married as Child	0.212	-0.031
		[0.033]
Resides with Parents	0.365	-0.010
		[0.040]
Currently a Student	0.000	0.005
		[0.005]
Currently in College	0.000	0.005
		[0.005]
Full-Time Employed/Self-Employed	0.669	-0.006
		[0.033]
Unemployed	0.012	-0.008
		[0.007]
Worked (Past 30 Days)	0.654	-0.005
		[0.035]
Engaged in Salaried Work (Past 30 Days)	0.253	0.006
		[0.036]
Engaged in Daily Wage Work (Past 30 Days)	0.089	-0.010
		[0.030]
Engaged in Self-Employment (Past 30 Days)	0.340	0.006
		[0.046]
Days Worked (Past 30 Days)	14.332	0.968
		[0.829]
Hours Worked (Past 7 Days)	27.167	2.612
		[2.378]
Child Income (Past 30 Days)	5114.851	909.857
		[595.317]
Observations		479

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 shows the list of potential lasso controls. The sample consists of children of the client aged 18 years or older at baseline that are still alive at the time of the 2018 survey. All outcomes are obtained from the 2018 survey. Child income and hours and days worked 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 A23: Treatment Effects on Socio-Economic Outcomes for Placebo Group by Gender

	Placebo Sample (18+ Years at Baseline)				
	Sons		Daughters		
	Control Mean	Grace Period Coeff.	Control Mean	Grace Period Coeff.	p-val: Difference
	(1)	(2)	(3)	(4)	(5)
Married	0.833	-0.032	0.983	-0.010	0.678
		[0.051]		[0.022]	
Married as Child	0.022	0.026	0.430	-0.095	0.094
		[0.027]		[0.067]	
Resides with Parents	0.638	-0.082	0.057	0.084**	0.027
		[0.062]		[0.042]	
Currently a Student	0.000	0.001	0.000	0.011	0.308
		[0.001]		[0.011]	
Currently in College	0.000	0.001	0.000	0.011	0.308
		[0.001]		[0.011]	
Full-Time Employed/Self-Employed	0.978	0.024	0.320	-0.047	0.363
		[0.018]		[0.074]	
Unemployed	0.022	-0.024*	0.000	0.010	0.037
		[0.013]		[0.008]	
Worked (Past 30 Days)	0.949	-0.007	0.320	-0.005	0.978
		[0.031]		[0.074]	
Engaged in Salaried Work (Past 30 Days)	0.407	0.005	0.082	0.020	0.852
		[0.058]		[0.045]	
Engaged in Daily Wage Work (Past 30 Days)	0.154	-0.013	0.017	-0.008	0.939
		[0.054]		[0.017]	
Engaged in Self-Employment (Past 30 Days)	0.437	0.026	0.231	-0.022	0.562
		[0.054]		[0.069]	
Days Worked (Past 30 Days)	23.422	0.644	4.618	0.779	0.944
		[0.914]		[1.581]	
Hours Worked (Past 7 Days)	45.713	3.569	7.337	1.077	0.588
		[3.676]		[2.951]	
Child Income (Past 30 Days)	9173.077	1579.475	808.163	257.131	0.254
		[1076.058]		[474.974]	
Housewife			0.680	0.037	
				[0.074]	
Observations		264		215	

Notes: The estimates in Columns 2 and 4 come from a single regression where we regress the outcome on the treatment indicator, the treatment indicator interacted with gender, and the gender indicator. The coefficients are used to calculate separate treatment effects for each gender. Column 5 reports the p-value for the intervention term. Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies and controls that are chosen using the double-lasso approach. Appendix Table A3 shows the list of potential lasso controls. The sample consists of children of the client aged 18 years or older at baseline that are still alive at the time of the 2018 survey. All outcomes are obtained from the 2018 survey. Child income and hours and days worked 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.

B. Data Appendix

Household-Level Outcome Variables

- *Income:* obtained from the following survey question: "During the past 30 days, how much total income did your household earn?".
- *Any Business:* indicator variable is that is equal to one if the household at at least one operational business at the time of the survey
- *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.
- *Formal Savings:* all savings the household has inside bank accounts.
- *Borrowing Problems:* obtained from the following survey question:"In the last 24 months, was there ever a time when you wanted to borrow money from a specific source but could not get all the money that I wanted to borrow within 2 weeks?"
- *Business Closure:* indicator variable that is equal to one if a client reported having closed a household business that was operating at the time of loan disbursement.
- *Financial Stress Index:* standardized index that consists of the following variables: problems with bill payments, pawned/sold something, asked for financial help. See Appendix C for details regarding the construction of standardized indices.
- *Problems with Bill Payments:* indicator variable that is equal to one if there was ever a time in the past year in which the client could not pay gas, electricity or telephone bills or the mortgage or rent on time.
- *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
- *Asked for Financial Help:* indicator variable that is equal to one if there was ever a time in the past year in which the client asked for financial help from friends or family.
- *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.
- *Number of Household Employees:* the sum of the number of household employees 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 Non-Household Employees:* the sum of the number of non-household employees 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.

- *Any Child Labor:* indicator variable that is equal to one if at least one household member below 18 years engaged in a work activity (salaried work, daily wage work, or self-employment).

- *Total Business Expenditure:* consists of business spending on inventory and raw materials, business equipment, and operating costs.

- *Empowerment Index:* . standardized index that consists of the following variables: clients has major say in how much to spend on food, education, clothing, health, home improvements, and recreational activities.

- *Migrated:* is an indicator variable that is equal to one if the household's address changed between the 2012 and 2018 survey.

Child-Level Outcome Variables

- *Investment Index:* standardized index that consists of the following variables: private secondary school, total secondary school fees, total after-school tutoring, college spending.

- *Private Secondary School:* indicator variable that is equal to one if the child attended at least one year of private secondary school for grades 5 to 12.

- *Total Secondary School Fees:* obtained from the following survey 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 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 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)?"

- *Attainment Index:* standardized index that consists of the following variables: 10th grade completion, 12 grade completion, college attendance, and total years of education.

- *Completed Grade 10 :* indicator variable that is equal to one if the child completed grade 10.

- *Completed Grade 12:* indicator variable that is equal to one if the child completed grade 12.

- *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.
- *Years of Education:* years of education of the child. We use the following imputations: completed vocational school = highest completed primary/school class plus 2 years, graduate degree or engineer diploma = 15 years, medical/engineering degree = 16 years, postgraduate degree = 17 years, currently in college = 13 years.
- *Performance Index:*
- *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.
- *Married:* child is married at the point of the 2018 survey.
- *Married as Child:* child was less than 18 years old at the time of marriage. he value is equal to zero if the child is not married.
- *Resides with Parents:* child is part of the household roster in the 2018 survey.
- *Currently a Student:* child is attending an educational institution at the point of the survey.
- *Currently in College:* child is attending an educational institution at the point of the survey.
- *Full-Time Employed/Self-Employed:* child's occupation is full-time employed/self-employed.
- *Unemployed:* child is currently unemployed according to the occupation module.
- *Worked (Past 30 Days:* indicator variable that is equal to one the child engaged in a work activity (salaried work, daily wage work, or self-employment) in the past 30 days.
- *Child Income:* total child income in the past 30 days.

Control Variables

- *Mother'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.
- *Mother'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.
- *Father'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.

C. 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.

D. Theoretical Model

This model is based on a standard model of educational investments under credit constraints (see e.g. Lochner and Monge-Naranjo (2012)) but adds business investments and a binary education investment decision for which the returns depend on parental education.

Consider a two-period-lived parent who decides whether to send her child to college and how much to invest into the household business in the first period and receives returns in the second. Preferences are

$$U = u(c_1) + \beta u(c_2) \tag{3}$$

where c_1 is consumption in period 1, c_2 is consumption in period 2, β is a discount factor, and $u(\cdot)$ is strictly increasing and concave and satisfies standard Inada conditions.

Each parent is endowed with wealth W. If the child attends college (h), the parent pays costs τ, which includes foregone wages and tuition fees, in period 1. Returns to college are increasing with parental education (e). The parent also decides how much to invest into the household business b. In period 2, child earnings are $w + hew_h$ and business profits are $f(b)$, where $f(\cdot)$ is positive, strictly increasing, and concave. In period 1, parents can borrow or save d at a gross interest rate R. Consumption levels in each period are

$$c_1 = W - h\tau - b + d \tag{4}$$

$$c_2 = w + hew_h + f(b) - Rd \tag{5}$$

Without credit constraints, the parent maximizes equation 3 subject to equations 4 and 5. Optimal business investment equate its marginal return with that of financial assets.

$$f(b^*)' = R \tag{6}$$

Parents will send their children to college if

$$ew_h > R\tau \tag{7}$$

For simplicity, we assume that there are only two types of parents: high educated (e_h) and low educated (e_l) with $e_h > \frac{R\tau}{w_h} > e_l$. This implies that only high educated parents would ever send their child to college.

Unconstrained optimal borrowing $d(W, e)$ smooths consumption over time, satisfying the Euler equation.

$$u'[W - h^*\tau - b^* + d^*] = \beta R u'[w + h^* ew_h + f(b^*) - Rd^*] \tag{8}$$

Let us now consider a borrowing constraint. Assume that there is an exogenously specified upper limit on the amount of debt that individuals can accumulate, such that $d < \bar{d}$ with $0 \leq \bar{d} < \infty$. The equation $d^* = \bar{d}$ implicitly defines a threshold level of assets $W_{\min}(e)$ determining who is constrained $[W < W_{\min}(e)]$ and who is unconstrained $[W \geq W_{\min}(e)]$.

Assuming that at least some parents are constrained, we have the following predictions:

Prediction 1: The effect of an additional parental wealth on child college attendance increases with parental educational $\frac{\frac{\partial h}{\partial W}}{\partial e} > 0$.

Prediction 2: The effect of an additional parental wealth on business investments decreases with parental educational $\frac{\frac{\partial b}{\partial W}}{\partial e} < 0$.