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. Treatment households spend more on private secondary schooling and after-school tutoring for their children and, subsequently, these children are 34% more likely to attend college. The observed educational patterns are consistent with poor households facing a trade-off between investing in their enterprise and in their children's human capital. Consequently, average gains in children's education are also accompanied by greater educational inequality across treatment households.

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1 Introduction

A growing body of experimental evaluations demonstrate that development programs designed to alleviate household liquidity constraints can 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. 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 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).

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.[1] Our treatment 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 our current analysis, we use surveys conducted in 2012 and 2018 to show that income gains persisted and were passed on to children via investments in human capital accumulation.

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 and we cannot reject equality of treatment effects across time. It appears that treatment households maintained relatively higher incomes in part by using a buffer stock of savings to overcome negative shocks.[2] In 2010, treatment group households report 48% higher formal savings and there continues to be a statistically significant savings gap between treatment and control households in 2018, eleven years post-intervention. Treatment households are also less likely to close businesses after the 2010 microfinance crisis, which reduced the supply of microcredit (Breza and Kinnan, 2020).

[1] 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. Flexible credit interventions include: a grace period before repayment begins (Field et al., 2013); seasonal repayment moratoriums or option to reschedule some repayments (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). Increased flexibility of contracts have been shown to raise business profits (Barboni and Agarwal, 2020; Battaglia et al. 2019; Araganon et al., 2020); and household income (Battaglia et al. 2019; Czura, 2015).

[2] We do not detect longer-run treatment differences in weekly business profits, but treatment enterprises remain larger in terms of capital.

Did these household-level income gains influence investments in children's education? At baseline (in 2007), 89% of households in our study population had completed fertility and 52% had at least one school-aged child (i.e. between 7-17, now on "school-age households"). 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 shift their educational outcomes. By 2018, we observe a 9.4 percentage point increase in college enrollment rate for treatment households with similar effects across sons and daughters. We do not observe treatment effects on years of K-12 schooling but treatment households were more more than twice as likely to enroll their children in private secondary school and increased spending on both school fees and after-school tutoring by about 25%. Overall, we find that treatment children score 0.2 standard deviations higher on an aggregate investment index, which includes primary, secondary, and tertiary spending outcomes as sub-components. Younger children — who had more exposure to the treatment — benefit more: treatment effects on educational investment and attainment grow in inverse proportion to child's age at baseline.[3] We are able to measure these long-run educational outcomes because our survey elicits information on all children, including those who have already left the household.

Our intervention eased household credit constraints. As several studies note, while associated increases in household wealth increase the ability to pay for education expenses (income effect), higher returns to business activities also raise the opportunity cost of schooling (substitution effect) and the resolution of this trade-off differs along the parental skill distribution.[4] Consistent with the presence of such a trade-off, the 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 with parents more likely to cite family circumstances — such as financial constraints and school not being worthwhile — as reasons for children's dropout from school. Long-run income effects are concentrated among lower-education households, and the treatment causes these families to use more child labor in their enterprises. We find no evidence that households invested in education as a means of bringing higher skilled labor into the enterprise.

Our findings suggest that households can successfully translate economic gains from positive liquidity shocks to break the inter-generational transmission of poverty. However, the extent to which this occurs remains dependent on parental circumstances. Using income gains and intervention cost estimates from Field et al. (2013), we find that the grace period is highly cost

[3] 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).

[4] 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. For rural settings, evidence on how rainfall-induced income shocks impact educational attainment is mixed (Jensen, 2000; Björkman-Nyqvist, 2013; Zimmermann, 2020).

effective in raising household income. However, because our treatment effects on children's education are concentrated within households of more highly-educated parents, our intervention leads to a decrease in relative educational mobility within our sample.

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).[5] Our study instead 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.

2.1 Context

Entrepreneurship, Financial Inclusion and Economic Mobility in Urban India

Our study, and subsequent sample tracking between 2007 and 2018, occurred during a period of overall strong economic growth coupled with significant urbanization and occupational diversification (World Bank, 2020). Among urban poor households, self employment in micro-enterprises

[5] Bouguen et al. (2019) reviews randomized controlled trials which evaluate long-run impacts in development economics, with a focus on cash transfer and child health programs.

remained an important primary income source.[6] These urban micro-entrepreneurs rely heavily on loans from micro-finance institutions (MFIs), a sector which grew by a factor of six between 2008 and 2017.[7] A large contraction in this sector, between 2010 and 2012, negatively impacted microentrepreneurs throughout the country including those in our study. In particular, in October 2010 (after completion of our 2010 survey), the MFI 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.[8] 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.

For self-employed urban households, 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). Yet, inter-generational economic mobility remains low in India (Asher et al., 2020). Given this, we now turn to a discussion of India's education system, given that higher education, especially college attendance, remains 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, and transition to higher secondary school after class 10. Upper secondary statewide exams at class 12 determine options for tertiary (college) education.

In Figure 1, we plot school completion rates in urban India by birth year cohort using National Family Health Survey (NFHS) data from 2014-15 and highlight three trends:[9] First, primary schooling is now nearly universal in urban India and also among children in our sample. Second, while secondary and tertiary education rates - while higher - remain far from universal: roughly, half the millenials have gone to secondary school and only a third have college education. Third,

[6] According to 2004-2005 and 2011-2012 Indian Human Development Survey rounds, roughly a quarter of urban households report self-employment as primary source of income.

[7] In 2008-2009, Indian MFIs 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.

[8] 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.

[9] The National Family Health Surveys (NFHS), also known as Demographic and Health Surveys (DHS), are nationally representative, household-level surveys carried out in developing countries. We use NFHS Round 4 data. Turning to the graph, the x-axis plots 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.

the gender gap in attainment has almost closed in urban areas across all three educational achievement levels. Educational patterns for our study population are consistent with these trends: while parents in our sample have relatively low educational attainment, with less than 1% having attended any post-secondary school (Appendix Table A1, Panel B), 28% of children in the control group attend at least some college.

The last few decades have also been marked by an explosive rise in private schooling and after-school tutoring in urban India (Kingdon, 2020; Berry and Mukherjee, 2019). By 2015, 42% of children in urban areas attend private school (Kingdon, 2020). In our sample and more broadly, private schools are associated with better educational outcomes.[10] 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 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

[10] 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.

completed college earn 13% more per month than children without a college degree.

2.2 Experimental Design and Data

Our study population comes from an experiment conducted in 2007: In conjunction with a local MFI we identified 169 new five-member loan groups (845 women) in low-income neighborhoods of Kolkata (Field et al., 2013). Each woman 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 MFI 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.[11] See Field et al. (2013) for details of the experimental design.

The grace period treatment leads to a 20% gain in household income and large gains in business investment and profits three years post-intervention (Field et al., 2013). 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.

To examine persistence of economic impacts and spillovers to children, we augment the baseline survey (2007) and follow-up survey data in 2008 and 2010 with two additional survey waves conduced in 2012, and 2018.

Tracking and Attrition analysis

In Appendix Table A3 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 747 out of 845 households from the baseline sample. Nineteen clients died between baseline and our final follow-up survey but in all but one case we were able to interview another household member. These eighteen households are included in our main analysis sample of 747 households.[12] A tracking rate of 88% 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,

[11] 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.

[12] In the period between baseline and our final follow-up, 51 clients 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.

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 A2 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

Appendix Section B describes the full set of outcomes and control variables; here, we focus on key outcomes.

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.[13] 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 services." We calculate business capital from the sum of raw materials, inventory, and assets.[14] 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.[15]

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

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

[14] 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.

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

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. 314 of the 747 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.[16] 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.

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 also

[16] 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.

include a control for whether the interview was conducted with a non-client household member.[17] 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.[18] Standard errors are clustered at the loan group level.

For child i we estimate child-level regressions as:

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

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.[19] 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 these children completed their pre-college education in the period between baseline and our final follow-up[20]; 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 A1).

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 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.[21]

[17] Our main analysis sample includes 18 households in which the client died between baseline and our final follow-up survey. For these households, we conducted the final follow-up survey with a different household member.

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

[19] 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.

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

[21] 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.

3 Results

3.1 Household economic well-being

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.[22] In measuring household income five- and eleven-years post-intervention, we find that economic outcomes have declined across sample households but that treatment effects persist. Compared to 2010, log income for the control group dropped slightly in 2012 and had declined 35% by 2018. This drop in household income is partly driven by changes in household composition and demographics, as clients and their spouses age out of the labor force. Median income per capita only declined by 11% over the same period. Yet we continue to see positive treatment effects of the grace period contract: 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. In column 2 of Table 1, we again examine the medium- and long-term impacts of treatment on household income, but include an interaction term for whether a household has any children aged 7-18 at baseline. In 2007, 53% of our sample households had at least one school-going age child. These households will serve as our primary sample for analysis of children's outcomes. Results shown in column 2 indicate that treatment gains are similar for households with and without school-aged children.

The treatment income gap may have been sustained in part because treatment households were able to build up a buffer stock of savings which increased their resilience to shocks. As shown in column 3 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.[23] Treatment households also respond differently to the economic downturn caused by the 2010 microfinance crisis: while both treatment and control clients report a sharp increase in borrowing problems post-crisis, treatment households are less likely to report having to pawn something to make ends meet or needing to lay off workers. We do not observe any difference in savings behavior between households with and without school-age children at baseline (column 4 of Table 1).

[22] 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.

[23] 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).

The majority of households in both the treatment and control groups own at least one business at every follow-up survey round (column 5-6 Table 1). We therefore examine how the grace period intervention impacted business outcomes across the study period. In columns 7-8 of Table 1, we describe medium- and long-term treatment impacts on enterprise profits. The coefficient on grace period replicates the result shown in Field et al. (2013): three years post-intervention (in 2010) we observe large gains in profits among treatment group households. We find, though, that the treatment group's gains in profits are no longer detectable by 2012 or in 2018.[24] We do, however, see sustained differences in the size of businesses. In columns 3-4 of Appendix Table A5, 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.[25] Moreover, 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 (Appendix Table A5 columns 7-10).[26] 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 (Appendix Table A5 columns 5-6). None of these outcomes differ by whether the household had a school-age child at baseline.

3.2 Next-generation 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 treatment leads to increases in children's educational investments: children in the treatment group who were school-going age at baseline report higher spending at the secondary and tertiary level and an increased likelihood of attending college.

[24] 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.

[25] 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.

[26] 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 **??** results are shown only for 2012.

Figure 2 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 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 2, 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 panel contains a separate regression based on Specification 2. In Panel A of Table 2, we show outcomes for the full child sample. We first examine treatment impacts for our main educational outcome: college attendance. Because a large proportion of our child analysis sample is still in college or of college-going age, we define "attended college" in column 1 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. In column 2 of Table 2, 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. In column 3, we show that treatment continues to have an impact on enrollment for children of school-going age at the time of the survey.

In columns 4-7 of Table 2, 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 (column 7).[27] Conversely, treatment children who were 7-17 at baseline report significantly higher expenditures across measures of secondary schooling investments (column 6) and college spending (column 5). Sub-components of the secondary school investment index include spending on school expenses (admissions fees, school fees, uniforms, and text books) as well as expenses on after-school tutoring. The index also includes an indicator variable for whether children attended a private secondary

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

school. We find that treatment children are more than double as likely to attend private school and that treatment parents spend 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 (columns 2-4 of Table A7). Overall, we find that treatment children score 0.2 standard deviations higher on an aggregate investment index, which includes primary, secondary, and tertiary spending outcomes as sub-components (column 4 of Table 2).

Consistent with the child-level investment results, we also find that treatment households report higher total expenditures on education. In Table A6, 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).[28] 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.

In column 8 of Table 2, we present treatment impacts on an education performance index, which is a standardized aggregate of the following sub-components: grade (A, B, or C) on Grade 10 exams and Grade 12 exams and whether the child chose the science track in upper secondary school.[29] Students can choose between science, business, and liberal arts tracks and the science track requires the highest grades on Grade 10 exams. We cannot reject that the treatment has no differential effect on performance outcomes.[30]

Heterogeneity by Gender In Panel B of Table 2, we present education outcomes for sons and daughters separately. Among households in the control group, daughters are as likely as sons to attend college and have, on average, half a year more of K-12 schooling. Control group families do not disproportionately spend more on schooling for their sons at either the primary, secondary, or tertiary levels. Across all measures of schooling attainment, investment, and performance, we cannot reject that the treatment effects are equivalent for sons and daughters.

[28] We did not collect expenditure data in 2010 but show results from baseline (2007) and the one year follow-up survey (2008).

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

[30] 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.

Our results are consistent with broader trends in urban India. 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.[31]

Robustness We present two types of robustness checks on the impacts of the wealth shock on children's education outcomes. First, in Appendix Table A12, we replicate Table 2 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. Second, in Panel C of Table 2 we examine treatment effects on education outcomes for a placebo group: children 18 and older, almost all of whom had completed their education by the time of the intervention.[32] We cannot reject the null hypothesis of no treatment effects on investment, attainment, and performance outcomes for this group.

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

How can we interpret treatment impacts on education within the context of a model of parental investments? As a starting point, we document two key trends among control group households.

Children's schooling outcomes are positively correlated with those of their parents In Figure 3, we plot local polynomial regressions of college attendance and the schooling investment index on parents' highest year of education by treatment and control groups. To classify parental education, we take the highest level of schooling completed by either the mother or father.[33] Among households in the control group, both college attendance and investments increase steadily with parental education. In Appendix Table A13, we further regress children's education on parental education and find that it is 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.

[31] 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.

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

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

Household wealth is positively correlated with children's educational attainment We construct a socio-economic index using baseline data on household asset ownership and use it as a proxy for baseline wealth.[34] In Figure 4, we plot a local polynomial regression of college attendance on socio-economic status and find a positive relationship between baseline wealth and educational attainment. 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, and then plateaus again. In Table A13, we further regress children's education on baseline wealth and again find that it is positively and significantly correlated with educational investments and attainment.

These observed trends among control group households motivate a simple two-period model of educational investments under credit constraints. We formally describe the model in Section D of the Appendix, but lay out the main intuition here. 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. 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 yields two testable predictions:

Prediction 1 Treatment impacts on children's education are concentrated among children of more educated parents.

Consistent with our first prediction, we observe in Figure 3 that treatment effects on college attendance and the schooling investment index are largest among the most highly educated households. In Table 3, we examine the same educational outcomes shown in Table 1, but do so separately for households in each of three parental education groups: (a) households where no parent has more than primary education (19%); (b) households where at least one parent has some secondary education (67%); and, (c) households where at least one parent has upper secondary education (14%). In line with our model's prediction, we find that treatment effects on

[34] 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. The index uses the first component of a principal component analysis

children's educational investment and attainment are concentrated among more highly educated households. We cannot reject the null hypothesis that there is no treatment effect of the grace period on children's years of K-12 schooling and college attendance for children in the low education group and, in fact, the coefficients on these outcomes are negative in magnitude. Meanwhile, treatment leads to about an extra 1.5 years of K-12 schooling for children of parents with upper secondary school or at least some secondary school. Treatment impacts on college attendance are similarly concentrated among children of higher-educated parents, though the coefficient is not statistically significant for children of parents with only lower-secondary education. We observe similar results for the investment index and its sub-components. By contrast, the low-education household children in the treatment group actually report lower educational investments than their counterparts in the control group.

Prediction 2 Treatment impacts on business investments decreases with parental education.

In Table 5, we present treatment impacts on household income by parent education. In the even columns we present results for the full pooled sample and in the odd columns we additionally include interaction terms to separately identify effects for households with and without school-age children at baseline. Our model predicts that high-education parents should have lower income gains due to treatment when they have school-age children. This is because these high-education parents would instead re-invest more of their initial income gains from treatment into their children's schooling. Consistent with our prediction, we find that medium- and long-term income gains from the grace period are concentrated among the lowest-education households. For these households, we observe a 34% gain in income due to treatment in 2018, eleven years post-intervention.

Results on children's self-reported reason for dropout provide further evidence that the grace period led to an increase in the opportunity cost of keeping a low-education household child in school. For any child who reported completing their education before reaching grade 12, we asked the parent why their child left school when they did. We classify reasons for dropout into three categories: family factors (which includes money reasons, a good job opportunity, or feeling that school was not worthwhile); child factors (which includes reporting that the child disliked school or had low test scores); and, dropout for marriage. As shown in columns 1-3 of Table 4, we detect no differences in reason for drop-out among treatment and control group children in households where at least one parent has lower- or upper-secondary education. 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.

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 column 4 of Table 4, we analyze employment patterns by high- and low-education households. 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 A10 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 A17, we regress an index of female empowerment outcomes on treatment.[35] 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).

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

[35]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.

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 **??**).

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.[36] In purchasing power

[36] 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.

parities terms, this gain would be equivalent to $8316.7 PPP.[37] 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%.[38]

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 5). The impacts on college attainment suggest that the grace period intervention successfully shifted the intergenerational dynamics of poverty within our sample.

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

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

[38] 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%.

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[39], 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.

[39]In Appendix Table A18, 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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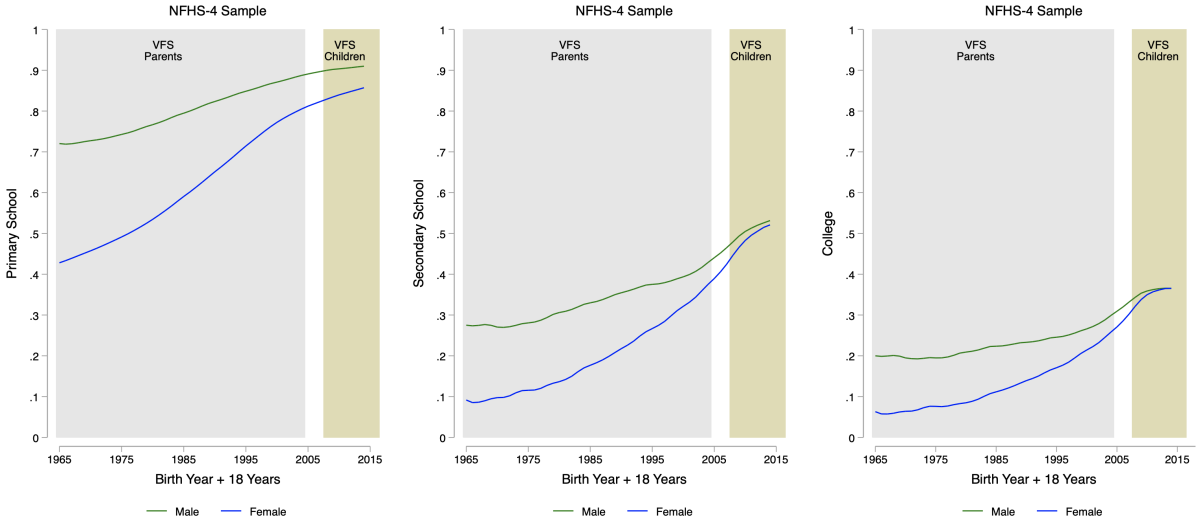
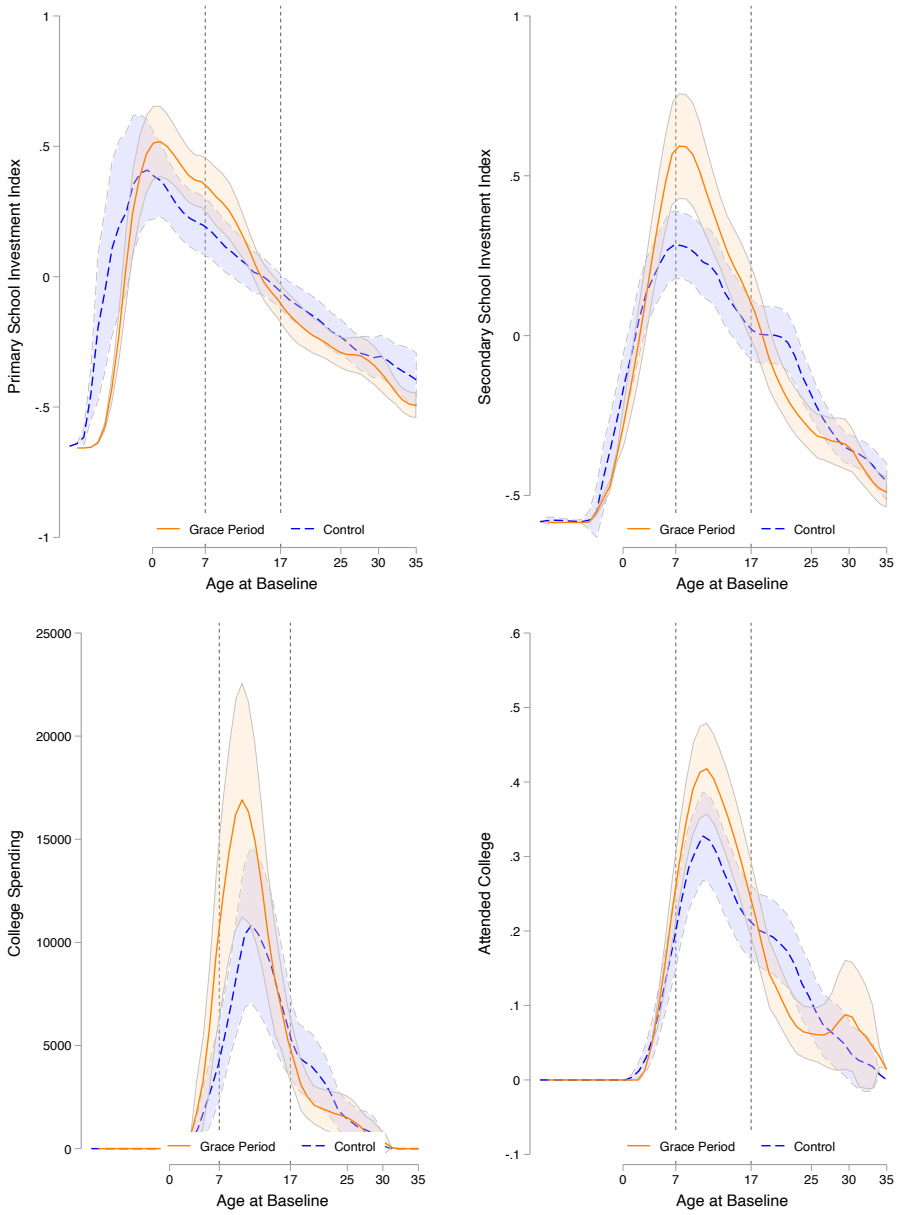
Figure 1: Educational Trends in India NFHS-4 (2015-16)



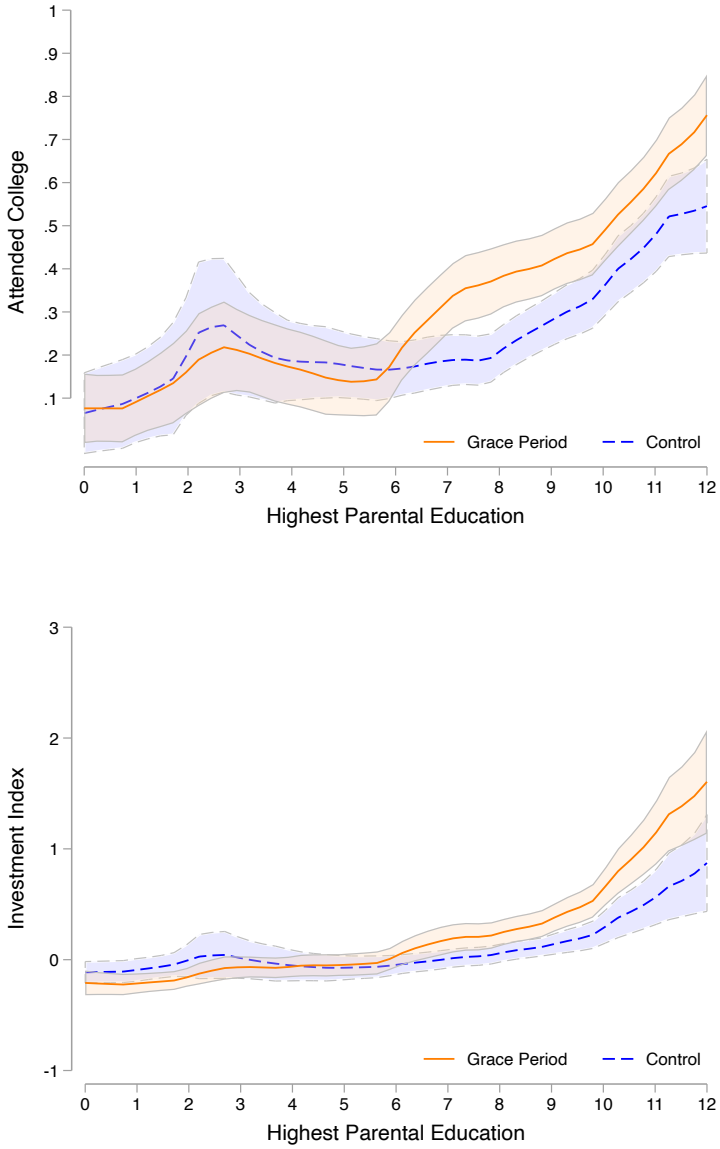
Notes: The figures plot local regressions. Data consists of all household members aged 18-80 urban areas in NFHS-4. The x-axis shows the year in which the person turned 18 years. The golden-shared area shows the age range of the VFS main child sample (aged 7-17 years at baseline) and the grey-shaded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

Figure 2: Childhood Exposure



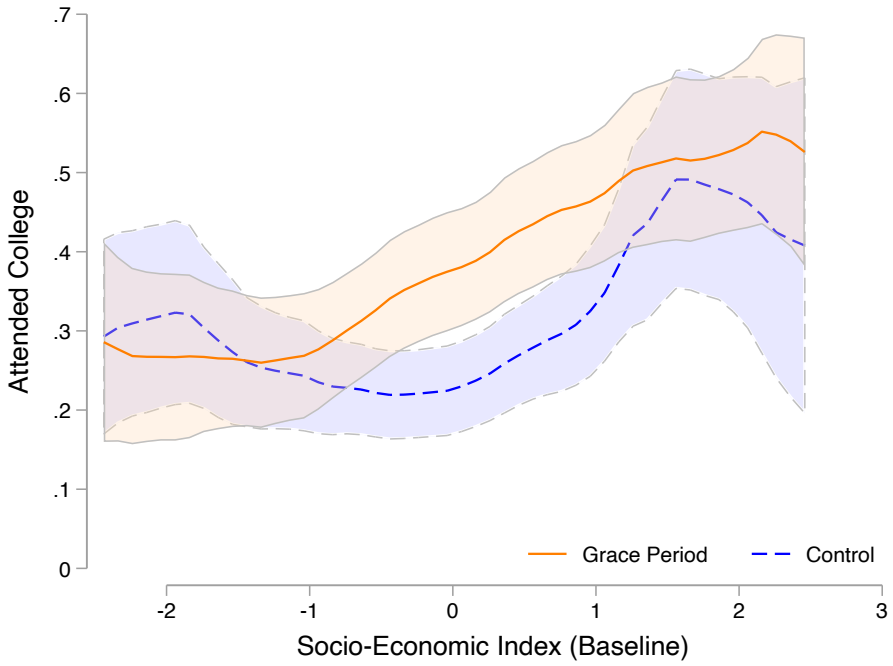
Notes: The figures plots local regressions. Data consists of all children of the client that were still alive at the time of the 2018 survey. The shaded areas in the figure correspond to 90 percent confidence intervals that are not adjusted for clustering. The dotted vertical lines indicate the school-age child sample. See Data Appendix for detailed variable definitions.

Figure 3: College Enrollment by Treatment and Parental Education



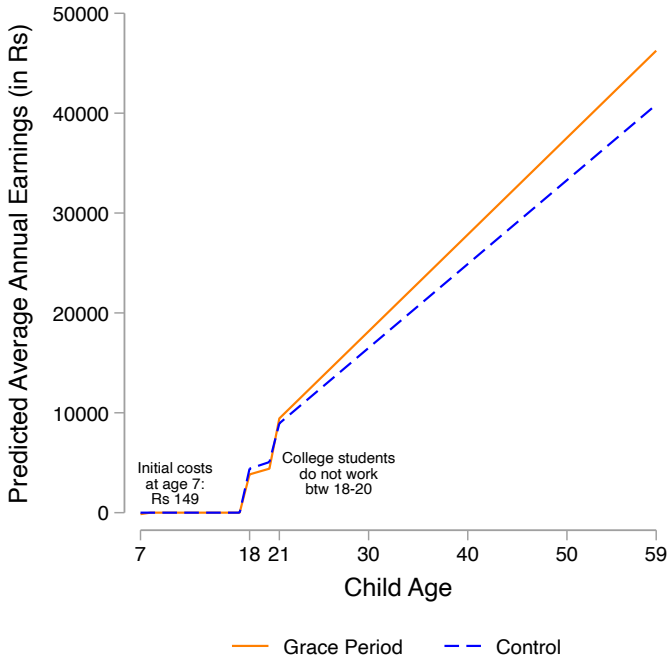
Notes: The figures plots local regressions. Data consists of all children of the client that were still alive at the time of the 2018 survey. The shaded areas in the figure correspond to 90 percent confidence intervals that are not adjusted for clustering. See Data Appendix for detailed variable definitions.

Figure 4: 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 5: 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

	Log Income		Formal Savings		Any Business		Profits	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grace Period	0.197***	0.235**	4531.284**	7201.010**	0.015	0.046	490.296***	322.103
	(0.075)	(0.111)	(1935.065)	(3410.608)	(0.027)	(0.037)	(163.093)	(230.730)
Grace Period × 2012	-0.072	-0.058	-3273.772	-4897.257	-0.023	-0.053	-412.906**	-317.973
	(0.089)	(0.127)	(2169.099)	(3315.630)	(0.029)	(0.046)	(186.651)	(286.038)
Grace Period × 2018	-0.118	-0.168	-2061.425	-4342.442	-0.056	-0.089*	-465.507***	-315.848
	(0.082)	(0.116)	(2238.997)	(3613.043)	(0.035)	(0.051)	(176.626)	(247.135)
Grace Period × School-Age Households		-0.052		-4926.711		-0.060		400.178
		(0.148)		(4197.252)		(0.050)		(348.205)
Grace Period × 2012 × School-Age Households		-0.033		3228.659		0.077		-249.306
		(0.173)		(4273.312)		(0.059)		(381.031)
Grace Period × 2018 × School-Age Households		0.059		4288.567		0.067		-340.907
		(0.146)		(4701.837)		(0.067)		(353.120)
p-val: Grace Period + Grace Period x 2012	0.020	0.012	0.389	0.368	0.726	0.838	0.563	0.984
p-val: Grace Period + Grace Period x 2018	0.102	0.312	0.024	0.075	0.148	0.313	0.758	0.954
p-val: Grace Period + Grace Period x School-Age HHs		0.081		0.351		0.705		0.005
p-val: Grace Period + Grace Period x 2012 GP x School-Age HHs + GP X 2012 x School-Age HHs		0.268		0.759		0.706		0.417
p-val: Grace Period + Grace Period x 2018 GP x School-Age HHs + GP X 2018 x School-Age HHs		0.232		0.169		0.284		0.547
Mean of Control Group 2010	9.016	9.016	9162.708	9162.708	0.850	0.850	1173.808	1173.808
Mean of Control Group 2012	8.981	8.981	6204.137	6204.137	0.901	0.901	1295.439	1295.439
Mean of Control Group 2018	8.668	8.668	5648.049	5648.049	0.853	0.853	851.333	851.333
Observations	2244	2244	2184	2184	2269	2269	2195	2195

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, dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. Income, formal savings, outstanding loans, and profits 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 2: Treatment Effects on Educational Outcomes

					Investment Index Components			
	Attended College	Years of Schooling	Still in School or College	Investment Index	College Spending (Standard-ized)	Secondary School Investment Subindex	Primary School Investment Subindex	Secondary School Perfomance Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: School-Age Child Sample (7-17 Years at Baseline), Pooled								
Grace Period	0.098**	0.193	0.081**	0.200***	0.143	0.258***	0.075	0.017
	(0.038)	(0.228)	(0.038)	(0.070)	(0.088)	(0.080)	(0.070)	(0.065)
Panel B: School-Age Child Sample (7-17 Years at Baseline), Heterogeneity by Gender								
Grace Period	0.094*	0.194	0.091*	0.207**	0.088	0.283**	0.133	0.100
	(0.050)	(0.292)	(0.051)	(0.103)	(0.139)	(0.113)	(0.102)	(0.094)
Grace Period × Female	0.006	-0.086	-0.023	-0.018	0.110	-0.057	-0.129	-0.174
	(0.073)	(0.408)	(0.066)	(0.136)	(0.195)	(0.155)	(0.127)	(0.121)
Female	0.044	0.575*	0.002	0.020	-0.049	0.003	0.054	0.112
	(0.053)	(0.312)	(0.049)	(0.084)	(0.119)	(0.087)	(0.084)	(0.089)
Panel C: Placebo Child Sample (18+ Years at Baseline), Pooled								
Grace Period	0.014	-0.267	0.005	-0.030	-0.002	-0.029	-0.067	-0.047
	(0.024)	(0.279)	(0.005)	(0.060)	(0.069)	(0.058)	(0.066)	(0.088)
p-value: Grace Period + Grace Period x Female	0.073	0.733	0.169	0.040	0.110	0.041	0.962	0.376
Mean of Omitted Group (Panel A)	0.272	9.799	0.176	0.000	-0.000	0.000	0.000	0.000
Mean of Omitted Group (Panel B)	0.267	9.667	0.170	0.015	0.047	0.028	-0.026	-0.026
Mean of Omitted Group (Panel C)	0.126	8.465	0.000	-0.000	0.000	-0.000	-0.000	-0.000
Observations (Panels A-B)	541	543	544	543	531	543	543	543
Observations (Panel C)	492	492	494	492	482	492	492	492

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies, dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The sample in Panels A-B consist of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey. The sample in Panel C consist of children of the client aged 18+ at baseline that are still alive at the time of the 2018 survey. All outcomes are obtained from the 2018 survey. See Data Appendix for detailed variable definitions and Appendix Table A7 for treatment effects on index components. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table 3: Heterogeneous Treatment Effects on Educational Outcomes by Parental Education

	School-Age Child Sample (7-17 Years)							
					Investment Index Components			
	Attended College	Years of Schooling	Still in School or College	Investment Index	College Spending (Standardized)	Secondary School Investment Subindex	Primary School Investment Subindex	Secondary School Perfomance Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grace Period	-0.037	-1.054	-0.094	-0.181	-0.126	-0.222*	-0.032	-0.055
	(0.089)	(0.661)	(0.090)	(0.121)	(0.158)	(0.125)	(0.133)	(0.075)
Grace Period × Parents Attended Secondary School	0.132	1.436**	0.186*	0.330***	0.274	0.453***	0.031	0.034
	(0.102)	(0.689)	(0.107)	(0.127)	(0.184)	(0.142)	(0.149)	(0.113)
Grace Period × Parents Attended Higher Secondary School	0.301**	1.506**	0.270*	0.888**	0.507	1.112***	0.511*	0.275
	(0.132)	(0.734)	(0.145)	(0.407)	(0.601)	(0.414)	(0.277)	(0.258)
Parents Attended Secondary School	0.028	-0.335	-0.044	-0.151	-0.062	-0.172*	0.074	0.214***
	(0.076)	(0.579)	(0.075)	(0.099)	(0.138)	(0.097)	(0.118)	(0.072)
Parents Attended Higher Secondary School	0.280***	0.483	0.116	0.452	1.085***	0.425	0.120	0.991***
	(0.106)	(0.622)	(0.105)	(0.285)	(0.421)	(0.295)	(0.197)	(0.214)
p-value: Grace Period + Grace Period x Parents Attended Secondary School	0.058	0.163	0.063	0.033	0.070	0.008	0.990	0.793
p-value: Grace Period + Grace Period x Parents Attended Higher Secondary School	0.008	0.138	0.109	0.064	0.503	0.022	0.047	0.376
Mean of Omitted Group	0.186	9.093	0.186	-0.175	-0.098	-0.150	-0.173	-0.275
Observations	541	543	544	543	531	543	543	543

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies, dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The sample consists of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey. All outcomes are obtained from the 2018 survey. See Data Appendix for detailed variable definitions and Appendix Table A7 for treatment effects on index components. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table 4: Treatment Effects on School Dropout Reasons

	School-Age Child Sample (7-17 Years at Baseline)			Full Household Sample
	School Dropout Reasons			
	Family Factors	Child Factors	Marriage Factors	Child Workers in HH Business
	(1)	(2)	(3)	(4)
Panel A: Pooled				
Grace Period	0.021	-0.015	-0.006	0.079
	(0.039)	(0.037)	(0.027)	(0.052)
Panel B: Heterogeneity by Parental Education				
Grace Period	0.277***	-0.074	0.007	0.316*
	(0.098)	(0.100)	(0.064)	(0.184)
Grace Period × Parents Attended Secondary School	-0.296***	0.084	-0.004	-0.276
	(0.112)	(0.113)	(0.069)	(0.196)
Grace Period × Parents Attended Higher Secondary School	-0.386***	-0.019	-0.081	-0.314
	(0.129)	(0.133)	(0.077)	(0.211)
Parents Attended Secondary School	0.119*	-0.101	-0.069	0.042
	(0.070)	(0.082)	(0.049)	(0.096)
Parents Attended Higher Secondary School	0.050	-0.118	-0.073	-0.007
	(0.096)	(0.103)	(0.061)	(0.119)
p-value: Grace Period + Grace Period x Parents Attended Secondary School	0.704	0.840	0.932	0.496
p-value: Grace Period + Grace Period x Parents Attended Higher Secondary School	0.185	0.253	0.091	0.983
Mean of Omitted Group (Panel A)	0.202	0.210	0.112	0.285
Mean of Omitted Group (Panel B)	0.095	0.310	0.190	0.413
Observations	532	532	532	771

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification, dummies dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. 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: Heterogeneous Treatment Effects on Household Income by Parental Education

	Log Income							
	Pooled		2010		2012		2018	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grace Period	0.258***	0.176	0.201	-0.171	0.262**	0.322*	0.319***	0.438***
	(0.088)	(0.128)	(0.179)	(0.258)	(0.126)	(0.195)	(0.121)	(0.163)
Grace Period × Parents Attended Secondary School	-0.164*	-0.005	-0.038	0.393	-0.177	-0.092	-0.282**	-0.392**
	(0.097)	(0.146)	(0.199)	(0.290)	(0.138)	(0.217)	(0.130)	(0.185)
Grace Period × Parents Attended Higher Secondary School	-0.167	-0.051	0.051	0.801**	-0.195	-0.419	-0.312*	-0.499**
	(0.122)	(0.173)	(0.227)	(0.325)	(0.181)	(0.256)	(0.160)	(0.211)
Grace Period × School-Age Households		0.168		0.678**		-0.079		-0.219
		(0.190)		(0.343)		(0.324)		(0.200)
Grace Period × Parents Attended Secondary School × School-Age Households		-0.265		-0.720*		-0.132		0.220
		(0.216)		(0.391)		(0.344)		(0.248)
Grace Period × Parents Attended Higher Secondary School × School-Age Households		-0.248		-1.458***		0.385		0.358
		(0.259)		(0.452)		(0.421)		(0.260)
p-value: Grace Period + Grace Period x Parents Attended Secondary School	0.049	0.013	0.083	0.113	0.198	0.006	0.536	0.612
p-value: Grace Period + Grace Period x Parents Attended Higher Secondary School	0.315	0.299	0.091	0.004	0.603	0.562	0.944	0.651
p-value: Grace Period + Grace Period x School-Age HHs		0.011		0.041		0.288		0.127
p-value: GP + GP x School-Age HHs + GP x Sec. School + GP x Sec. School x School-Age HHs		0.775		0.126		0.702		0.076
p-value: GP + GP x School-Age HHs + GP x Higher Sec. School + GP x Higher Sec. School x School-Age HHs		0.834		0.009		0.203		0.033
Mean of Omitted Group	8.695	8.735	8.809	9.030	8.821	8.883	8.427	8.307
Observations	2244	2244	749	749	757	757	738	738

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, dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. Income 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.

A. Appendix Tables and Figures: Additional Analysis

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

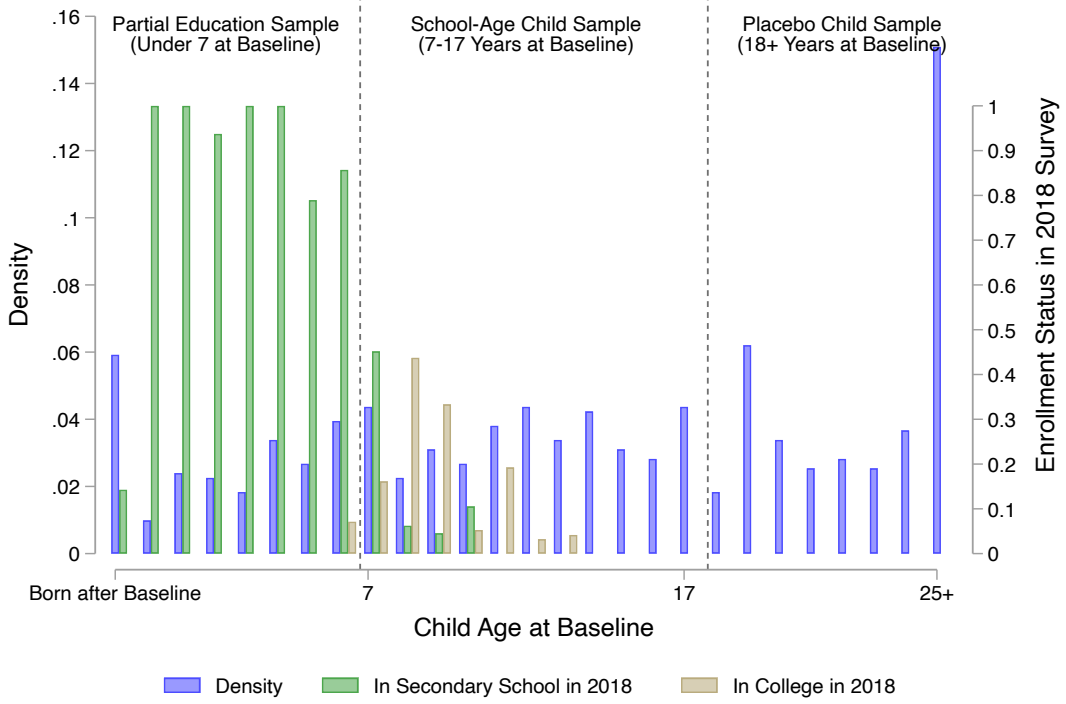


Figure A2: Timeline of Household Surveys

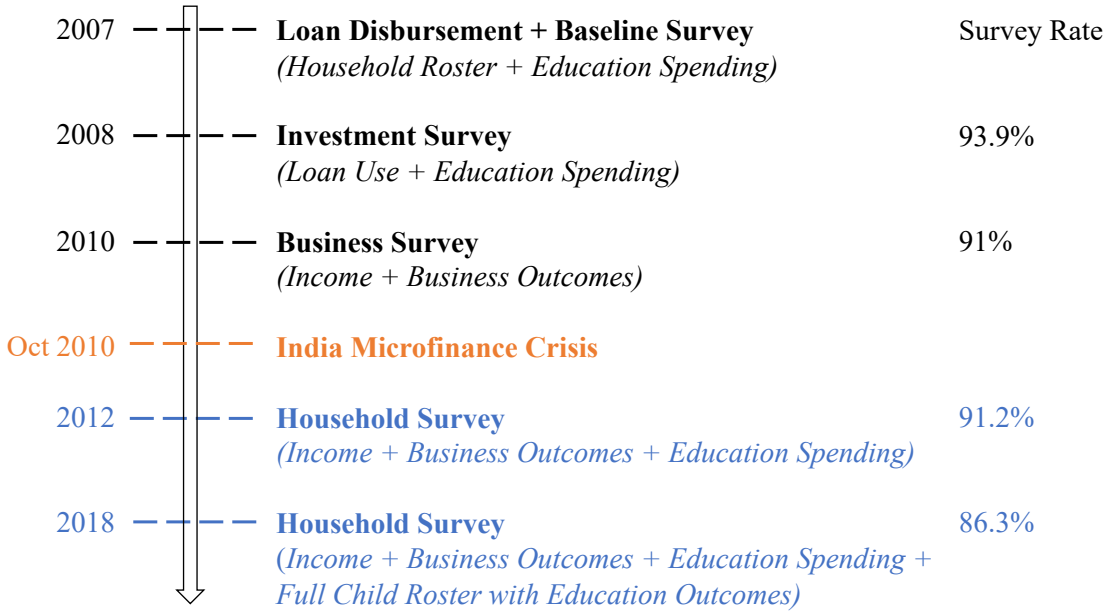
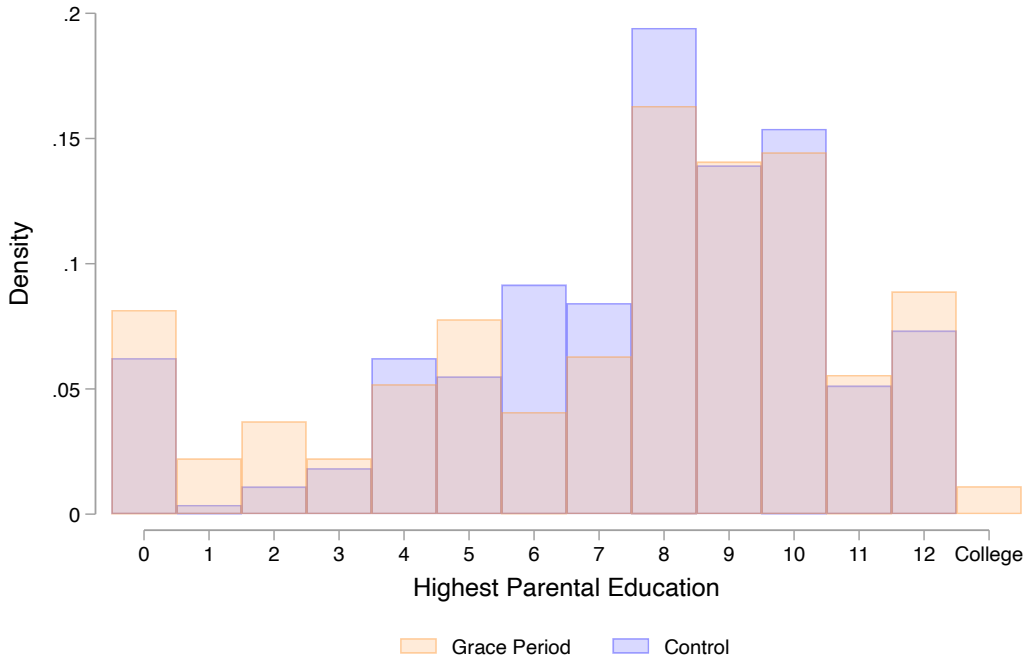
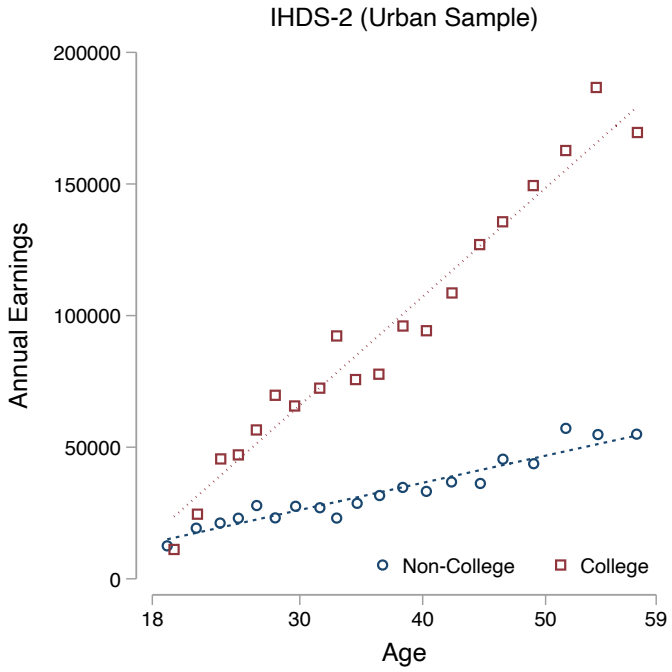


Figure A3: Histogram of Parental Education



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

Figure 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: 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	0.113	0.334	0.141	0.029	0.024	747
Number of Children Aged 0-6 Years at Baseline	0.334	0.585	0.383	0.043	0.045	747
Number of Children Aged 7-17 Years at Baseline	0.736	0.861	0.721	-0.020	0.070	747
Number of Children Aged 18 Years or Above at Baseline	0.728	1.112	0.596	-0.121	0.078	747
At Least One Child Born After Baseline	0.108	0.311	0.128	0.021	0.022	747
At Least One Child Aged 0-6 Years at Baseline	0.275	0.447	0.319	0.041	0.034	747
At Least One Child Aged 7-17 Years at Baseline	0.520	0.500	0.500	-0.017	0.036	747
At Least One Child Aged 18 Years or Above at Baseline	0.383	0.487	0.314	-0.062*	0.035	747
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 A2: 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	34.259	5.887	0.340	0.608	380
Married	0.911	0.286	-0.046**	0.022	843	0.964	0.187	-0.010	0.020	380
Muslim	0.007	0.084	0.014	0.012	842	0.010	0.102	0.016	0.015	380
Client's Years of Education	6.609	3.543	-0.104	0.323	839	6.187	3.517	-0.169	0.431	380
Household Size	4.068	1.420	0.127	0.105	842	4.342	1.314	-0.021	0.145	380
Household Shock	0.607	0.489	0.030	0.059	830	0.628	0.485	0.018	0.067	375
Household Has a Business (Narrow)	0.772	0.420	0.014	0.041	843	0.777	0.417	0.045	0.050	380
Owns Home	0.816	0.388	-0.011	0.034	838	0.854	0.354	-0.027	0.039	377
Client Has Financial Control	0.838	0.369	-0.009	0.038	841	0.870	0.337	-0.037	0.044	379
No Drain in Neighborhood	0.129	0.335	-0.022	0.036	830	0.126	0.332	0.013	0.045	375
Loan Amt 4,000 RPS	0.012	0.108	0.001	0.010	845	0.016	0.124	-0.014	0.011	381
Loan Amt 5,000 RPS	0.047	0.212	-0.014	0.017	845	0.047	0.211	0.005	0.027	381
Loan Amt 6,000 RPS	0.289	0.454	-0.056	0.043	845	0.301	0.460	-0.088*	0.053	381
Loan Amt 7,000 RPS	0.002	0.049	-0.002	0.002	845	0.005	0.072	-0.005	0.005	381
Loan Amt 8,000 RPS	0.567	0.496	0.010	0.052	845	0.554	0.498	0.009	0.063	381
Loan Amt 9,000 RPS	0.000	0.000	0.005	0.005	845	0.000	0.000	0.000	0.000	381
Loan Amt 10,000 RPS	0.082	0.275	0.056	0.035	845	0.078	0.268	0.092**	0.039	381
Panel B: Additional Household-Level Controls										
Socioeconomic Index (PCA)	-0.103	1.347	0.210*	0.115	731	-0.137	1.167	0.181	0.152	333
Spouse's Age	41.142	9.084	-0.085	0.668	739	41.000	6.841	0.677	0.712	363
Spouse's Years of Education	7.783	3.313	-0.285	0.315	708	7.385	3.276	0.039	0.382	348
Number of Children (Still Alive in 2018)	1.798	1.060	-0.098	0.090	747	2.088	0.972	-0.075	0.110	381
Education Expenditure 2007	420.569	540.354	6.833	43.282	841	635.665	588.191	11.856	72.958	380
Health Expenditure 2007	368.140	915.473	37.863	72.758	841	303.911	578.055	101.277	102.937	380
Renovation Expenditure 2007	545.502	1240.237	84.322	129.066	644	595.572	1175.597	159.899	157.220	295
Joint Test p-value			0.096					0.560		
Panel C: Child-Level Controls										
Female	0.487	0.500	-0.017	0.027	1401	0.505	0.501	-0.012	0.045	544
Birth Order	1.774	0.977	-0.008	0.071	1401	1.769	0.975	-0.016	0.105	544
Resides with Parents	0.738	0.440	-0.002	0.028	1401	0.912	0.284	0.007	0.032	544

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 A3: 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)	-0.020	(0.025)
Control Mean	0.071		0.089		0.099		0.127	
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)	0.365	(1.890)
Married	0.004	(0.093)	0.097	(0.086)	-0.016	(0.094)	-0.102	(0.093)
Muslim	0.029	(0.046)	-0.007	(0.007)	0.004	(0.010)	-0.002	(0.008)
Client's Years of Education	2.633**	(1.018)	1.239	(0.821)	1.600**	(0.770)	1.208*	(0.704)
Household Size	-0.490	(0.339)	0.271	(0.307)	0.510*	(0.305)	0.746**	(0.294)
Household Shock	0.052	(0.143)	0.103	(0.131)	0.111	(0.120)	0.172	(0.119)
Household Has a Business (Narrow)	-0.104	(0.086)	-0.070	(0.087)	-0.052	(0.080)	-0.055	(0.085)
Owns Home	-0.126	(0.132)	-0.100	(0.107)	-0.096	(0.104)	-0.030	(0.090)
Client Has Financial Control	0.058	(0.107)	0.039	(0.074)	-0.013	(0.102)	0.055	(0.068)
No Drain in Neighborhood	-0.130**	(0.063)	-0.031	(0.052)	-0.035	(0.075)	0.039	(0.078)
Loan Amt 4,000 RPS	-0.000	(0.006)	-0.006	(0.005)	0.026	(0.029)	0.019	(0.021)
Loan Amt 5,000 RPS	-0.002	(0.053)	-0.073*	(0.038)	-0.051	(0.035)	-0.023	(0.043)
Loan Amt 6,000 RPS	-0.046	(0.139)	-0.036	(0.125)	-0.097	(0.111)	-0.105	(0.089)
Loan Amt 7,000 RPS	0.001	(0.001)	-0.000	(0.001)	0.000	(0.001)	-0.001	(0.001)
Loan Amt 8,000 RPS	0.005	(0.142)	0.074	(0.125)	0.067	(0.128)	0.011	(0.110)
Loan Amt 9,000 RPS	0.002	(0.002)	-0.001	(0.003)	0.028	(0.025)	0.045	(0.042)
Loan Amt 10,000 RPS	0.040	(0.061)	0.043	(0.071)	0.026	(0.069)	0.055	(0.078)
Socioeconomic Index (PCA)	0.346	(0.436)	0.417	(0.339)	0.572	(0.382)	-0.062	(0.301)
Spouse's Age	-2.602	(2.609)	0.006	(2.433)	-1.377	(2.840)	0.041	(2.338)
Spouse's Years of Education	1.005	(0.988)	-0.126	(1.076)	0.451	(1.240)	-0.470	(0.916)
Education Expenditure 2007	-3.739	(223.474)	-39.598	(99.647)	-193.895	(134.048)	268.384**	(134.869)
Health Expenditure 2007	84.105	(105.840)	-169.917	(258.849)	-402.946*	(229.020)	229.793	(224.769)
Renovation Expenditure 2007	-218.324	(242.023)	-103.765	(527.110)	-112.848	(562.797)	458.427	(369.131)

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 A4: 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	-0.027	(0.020)	-0.009	(0.020)	-0.010	(0.018)
Control Mean	0.057		0.052		0.036	
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	-0.489	(2.994)	-4.414**	(2.076)	-2.876	(3.274)
Married	0.076	(0.085)	-0.003	(0.016)	0.025	(0.018)
Muslim	-0.025	(0.018)	-0.003	(0.011)	0.017	(0.015)
Client's Years of Education	1.978	(1.463)	-0.031	(1.824)	0.165	(2.090)
Household Size	-0.794*	(0.423)	0.174	(0.482)	0.790	(0.845)
Household Shock	0.095	(0.255)	0.009	(0.211)	0.086	(0.270)
Household Has a Business (Narrow)	0.099	(0.081)	0.027	(0.224)	0.138	(0.149)
Owns Home	0.164	(0.218)	-0.323	(0.209)	-0.063	(0.252)
Mother Has Financial Control	0.272*	(0.150)	0.084	(0.177)	-0.100	(0.285)
No Drain in Neighborhood	-0.170	(0.118)	0.177	(0.132)	-0.244	(0.148)
Loan Amt 4,000 RPS	-0.003	(0.012)	-0.003	(0.007)	-0.000	(0.006)
Loan Amt 5,000 RPS	0.146	(0.131)	-0.105	(0.093)	0.005	(0.019)
Loan Amt 6,000 RPS	-0.363**	(0.147)	-0.269	(0.200)	-0.194	(0.248)
Loan Amt 7,000 RPS	0.004	(0.006)	0.002	(0.003)	-0.001	(0.002)
Loan Amt 8,000 RPS	-0.067	(0.235)	0.213	(0.245)	0.160	(0.328)
Loan Amt 9,000 RPS	0.000	(0.000)	0.000	(0.000)	0.000	(0.000)
Loan Amt 10,000 RPS	0.283*	(0.148)	0.162	(0.186)	0.030	(0.219)
Socioeconomic Index (PCA)	-0.017	(0.709)	0.659	(0.680)	0.902	(0.736)
Spouse's Age	-0.567	(2.857)	-2.907	(2.730)	-2.670	(3.758)
Spouse's Years of Education	1.066	(1.155)	0.832	(2.086)	1.648	(2.229)
Education Expenditure 2007	221.375	(440.599)	166.141	(305.966)	-716.101**	(301.303)
Health Expenditure 2007	141.103	(195.922)	233.745	(323.916)	-192.244	(164.926)
Renovation Expenditure 2007	-458.746	(485.017)	999.724	(903.313)	-541.067	(444.231)

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 A5: Additional Business Outcomes

	Business Closure		Capital		Number of Workers		Number of Products/Services		Number of Clients	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Grace Period	-0.075**	-0.020	18165.857***	22363.721**	0.147	0.279	4.660***	7.129**	237.238*	56.065
	(0.033)	(0.050)	(6525.544)	(10294.083)	(0.212)	(0.382)	(1.529)	(2.870)	(124.361)	(117.929)
Grace Period × 2012	0.014	-0.074	-4019.552	-2189.319	0.055	-0.187	-0.920	-3.173	162.399	475.058***
	(0.043)	(0.061)	(7493.736)	(10680.216)	(0.210)	(0.365)	(2.242)	(4.134)	(139.541)	(169.164)
Grace Period × 2018	0.011	-0.097	-4528.911	-14184.583	-0.184	-0.166				
	(0.049)	(0.067)	(15032.809)	(20874.202)	(0.240)	(0.389)				
Grace Period × School-Age Households		-0.082		-6113.466		-0.276		-4.026		444.962
		(0.068)		(13770.946)		(0.478)		(3.444)		(295.540)
Grace Period × 2012 × School-Age Households		0.149*		-3891.727		0.446		3.636		-569.305**
		(0.084)		(15209.783)		(0.450)		(4.701)		(252.326)
Grace Period × 2018 × School-Age Households		0.188**		18887.453		0.019				
		(0.089)		(31242.050)		(0.488)				
p-val: Grace Period + Grace Period x 2012	0.092	0.064	0.012	0.017	0.057	0.586	0.083	0.273	0.001	0.001
p-val: Grace Period + Grace Period x 2018	0.111	0.028	0.363	0.673	0.808	0.587				
p-val: Grace Period + Grace Period x School-Age HHs		0.030		0.077		0.989		0.085		0.053
p-val: Grace Period + Grace Period x 2012		0.626		0.259		0.082		0.226		0.034
GP x School-Age HHs + GP X 2012 x School-Age HHs										
p-val: Grace Period + Grace Period x 2018		0.825		0.389		0.488				
GP x School-Age HHs + GP X 2018 x School-Age HHs										
Mean of Control Group 2010	0.386	0.386	26412.013	26412.013	1.204	1.204	5.393	5.393	397.251	397.251
Mean of Control Group 2012	0.608	0.608	16316.272	16316.272	0.592	0.592	7.461	7.461	376.773	376.773
Mean of Control Group 2018	0.663	0.663	70656.144	70656.144	1.248	1.248				
Observations	2284	2284	2194	2194	2238	2238	1538	1538	1513	1513

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, dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. Capital, number of workers, number of products and services, and number of clients are top-coded at 99.5% for each survey round. Capital is 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 A6: Treatment Effects on Household Expenditures

	Education Expenditures		Health Expenditures		Renovation Expenditures	
	(1)	(2)	(3)	(4)	(5)	(6)
Grace Period	0.545	-56.525	51.619	-68.527	34.056	-125.897
	(44.549)	(47.485)	(73.565)	(103.150)	(156.994)	(201.648)
Grace Period × 2008	34.924	-68.321	11.625	-21.994	-24.223	399.136
	(71.902)	(73.257)	(95.381)	(152.435)	(194.444)	(284.952)
Grace Period × 2012	170.636**	55.004	357.687*	567.791*	-161.899	-186.001
	(80.233)	(95.401)	(202.551)	(300.525)	(283.007)	(363.102)
Grace Period × 2018	138.320	-6.284	32.985	186.212	-30.287	539.020
	(183.558)	(225.859)	(221.400)	(350.914)	(964.078)	(1336.744)
Grace Period × School-Age Households		76.055		190.514		207.919
		(78.400)		(134.099)		(243.633)
Grace Period × 2008 × School-Age Households		246.801*		28.381		-728.197*
		(126.173)		(179.591)		(378.106)
Grace Period × 2012 × School-Age Households		285.407*		-440.970		57.056
		(163.713)		(373.715)		(596.782)
Grace Period × 2018 × School-Age Households		300.009		-250.312		-974.414
		(347.573)		(447.482)		(1927.934)
p-val: Grace Period + Grace Period x 2008	0.651	0.144	0.467	0.400	0.951	0.235
p-val: Grace Period + Grace Period x 2012	0.038	0.988	0.020	0.065	0.634	0.348
p-val: Grace Period + Grace Period x 2018	0.435	0.782	0.695	0.734	0.997	0.758
p-val: Grace Period + Grace Period x School-Age HHs		0.780		0.217		0.695
p-val: Grace Period + Grace Period x 2008 GP x School-Age HHs + GP X 2008 x School-Age HHs		0.026		0.014		0.062
p-val: Grace Period + Grace Period x 2012 GP x School-Age HHs + GP X 2012 x School-Age HHs		0.005		0.294		0.912
p-val: Grace Period + Grace Period x 2018 GP x School-Age HHs + GP X 2018 x School-Age HHs		0.233		0.827		0.796
Mean of Control Group 2007	420.569	420.569	368.140	368.140	545.502	545.502
Mean of Control Group 2008	571.818	571.818	386.914	386.914	480.719	480.719
Mean of Control Group 2012	793.838	793.838	776.542	776.542	716.621	716.621
Mean of Control Group 2018	1096.340	1096.340	1573.364	1573.364	2932.786	2932.786
Observations	3112	3112	3146	3146	2733	2733

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, dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. Expenditure outcomes in 2007 were collected for the past 12 months and are divided by 12 to calculate monthly expenses. All expenditure 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 A7: Treatment Effects on Educational Subindex Components

		Secondary School Investment Subindex Components			Primary School Investment Subindex Components			Performance Subindex Components		
	College Spending	Private School	Total School Fees	Total After-School Tutoring	Private School	Total School Fees	Total After-School Tutoring	Science Track	Grade A in Class 12	Grade A in Class 10
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: School-Age Child Sample (7-17 Years at Baseline), Pooled										
Grace Period	3609.967	0.049***	3058.329*	6032.322***	0.056	962.125	100.324	0.021	0.001	-0.010
	(2207.946)	(0.018)	(1618.797)	(2092.776)	(0.042)	(824.025)	(629.154)	(0.023)	(0.026)	(0.030)
Panel B: School-Age Child Sample (7-17 Years at Baseline), Heterogeneity by Gender										
Grace Period	2205.343	0.059**	1334.333	7710.826***	0.043	1351.505	1092.719	0.063*	0.016	0.005
	(3500.999)	(0.024)	(2380.390)	(2851.347)	(0.055)	(1252.685)	(759.751)	(0.034)	(0.040)	(0.043)
Grace Period × Female	2776.050	-0.021	3302.178	-3624.128	0.026	-858.201	-2157.138**	-0.086*	-0.032	-0.031
	(4919.252)	(0.032)	(3423.852)	(3815.701)	(0.066)	(1721.238)	(1081.988)	(0.046)	(0.054)	(0.058)
Female	-1228.925	-0.004	-2152.384	2447.340	-0.030	-274.844	2193.901***	0.048	0.008	0.046
	(2989.981)	(0.018)	(1842.506)	(2403.786)	(0.048)	(1118.485)	(800.230)	(0.033)	(0.038)	(0.044)
Panel C: School-Age Child Sample (7-17 Years at Baseline), Heterogeneity by Parental Education										
Grace Period	-3169.302	-0.009	-4074.153**	-5964.518	0.003	-1171.149	599.580	0.000	-0.049	-0.010
	(3991.301)	(0.011)	(1917.502)	(4848.549)	(0.083)	(948.036)	(1556.478)	(0.040)	(0.035)	(0.037)
Grace Period × Parents Attended Secondary School	6910.404	0.038	6575.503***	12333.624**	0.008	920.552	-455.754	0.014	0.027	-0.009
	(4627.864)	(0.025)	(2343.626)	(5230.935)	(0.099)	(1038.518)	(1702.865)	(0.046)	(0.047)	(0.058)
Grace Period × Parents Attended Higher Secondary School	12779.258	0.209**	14889.953*	22138.631**	0.306**	8989.531**	-1120.224	0.044	0.172	0.026
	(15148.076)	(0.085)	(8622.805)	(9883.400)	(0.146)	(4441.694)	(2396.054)	(0.105)	(0.110)	(0.117)
Parents Attended Secondary School	-1560.467	0.002	-1503.514	-4663.174	0.103	311.957	257.808	0.018	0.062	0.141***
	(3474.998)	(0.008)	(1874.272)	(4247.252)	(0.069)	(813.888)	(1357.531)	(0.028)	(0.041)	(0.040)
Parents Attended Higher Secondary School	27340.193***	0.115**	10513.533*	-879.018	0.204*	3246.822	-2075.225	0.233***	0.281***	0.476***
	(10613.074)	(0.056)	(6351.760)	(8038.113)	(0.116)	(2851.104)	(1747.925)	(0.081)	(0.080)	(0.091)
Panel D: Placebo Samples (18+ Years at Baseline), Pooled										
Grace Period	-22.634	-0.004	-1191.625	-591.442	-0.033	-708.589	253.998	-0.013	0.010	-0.030
	(672.182)	(0.013)	(844.986)	(2006.996)	(0.032)	(472.430)	(648.074)	(0.014)	(0.020)	(0.028)
p-value: Grace Period + Grace Period x Female	0.110	0.115	0.043	0.150	0.187	0.660	0.226	0.477	0.649	0.519
p-value: Grace Period + Grace Period x Parents Attended Secondary School	0.070	0.172	0.106	0.014	0.835	0.765	0.845	0.570	0.473	0.634
p-value: Grace Period + Grace Period x Parents Attended Higher Secondary School	0.503	0.017	0.197	0.055	0.005	0.065	0.769	0.650	0.233	0.884
Mean of Omitted Group (Panel A)	8233.308	0.018	11812.938	26898.731	0.227	5049.416	6369.235	0.070	0.129	0.179
Mean of Omitted Group (Panel B)	9411.579	0.022	13058.504	26297.895	0.244	5286.142	5457.254	0.052	0.133	0.170
Mean of Omitted Group (Panel C)	5772.093	0.000	9860.976	22281.905	0.070	3401.951	6627.907	0.023	0.048	0.023
Mean of Omitted Group (Panel D)	2437.023	0.022	6724.668	15370.878	0.130	3045.191	4015.985	0.026	0.033	0.078
Observations (Panels A-C)	531	543	513	535	543	518	542	543	542	542
Observations (Panel D)	482	492	439	477	492	430	484	492	492	491

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification, dummies dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. 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 A8: Treatment Effects on School Dropout Components

	School-Age Child Sample (7-17 Years at Baseline)					
	School Dropout Reasons					
	Family Factor Components			Child Factor Components		
	Money Reasons	School Was Not Worthwhile	Good Work Opportunity	Disliked School	Low Test Scores	Had to Repeat Grade
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Pooled						
Grace Period	-0.015	0.022	0.006	-0.021	-0.015	0.025
	(0.034)	(0.024)	(0.014)	(0.029)	(0.030)	(0.022)
Panel B: Heterogeneity by Parental Education						
Grace Period	0.147**	0.128**	0.037	-0.023	-0.027	0.040
	(0.075)	(0.061)	(0.024)	(0.091)	(0.091)	(0.028)
Grace Period × Parents Attended Secondary School	-0.205**	-0.105	-0.037	0.010	0.016	0.002
	(0.082)	(0.070)	(0.027)	(0.096)	(0.093)	(0.041)
Grace Period × Parents Attended Higher Secondary School	-0.136	-0.222**	-0.046	-0.081	0.002	-0.069
	(0.088)	(0.088)	(0.050)	(0.121)	(0.097)	(0.042)
Parents Attended Secondary School	0.113**	0.048	0.011	-0.106	-0.052	0.057***
	(0.056)	(0.039)	(0.011)	(0.081)	(0.080)	(0.021)
Parents Attended Higher Secondary School	-0.011	0.055	0.031	-0.063	-0.093	0.027
	(0.061)	(0.069)	(0.032)	(0.103)	(0.078)	(0.031)
able$_e du_d ropout_c hild_{71} 7_b yparentedu$ $MeanofOmittedGroup(PanelA)$	0.135	0.075	0.015	0.097	0.101	0.049
Mean of Omitted Group (Panel B)	0.071	0.024	0.000	0.167	0.143	0.000
Observations	532	532	532	532	532	532

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification, dummies dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. 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: Additional Results on School Dropout Reasons

	Dropout Reasons								
		Family Factor Components				Child Factor Components			
	Family Factors	Money Reasons	School Was Not Worthwhile	Good Work Opportu-nity	Child Factors	Disliked School	Low Test Scores	Had to Repeat Grade	Marriage Factors
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: School-Age Child Sample (7-17 Years at Baseline), Heterogeneity by Gender									
Grace Period	0.035	-0.025	0.034	0.003	-0.032	-0.027	-0.020	0.032	0.008
	(0.057)	(0.049)	(0.040)	(0.023)	(0.057)	(0.048)	(0.042)	(0.032)	(0.011)
Grace Period × Female	-0.031	0.020	-0.026	0.005	0.032	0.011	0.009	-0.013	-0.028
	(0.070)	(0.059)	(0.049)	(0.027)	(0.071)	(0.050)	(0.050)	(0.044)	(0.057)
Female	-0.103**	-0.057	-0.053	-0.015	-0.140***	-0.102***	-0.057	-0.009	0.221***
	(0.050)	(0.040)	(0.032)	(0.018)	(0.050)	(0.038)	(0.038)	(0.028)	(0.041)
Panel B: Placebo Samples (18+ Years at Baseline), Pooled									
Grace Period	0.000	0.026	0.023	-0.020	0.056	0.038	-0.015	0.001	-0.058*
	(0.053)	(0.057)	(0.036)	(0.013)	(0.043)	(0.029)	(0.036)	(0.027)	(0.030)
p-value: Grace Period + Grace Period x Female	0.925	0.905	0.768	0.565	0.984	0.490	0.753	0.530	0.716
Mean of Omitted Group (Panel A)	0.241	0.150	0.105	0.023	0.278	0.150	0.128	0.053	0.000
Mean of Omitted Group (Panel B)	0.444	0.305	0.120	0.042	0.232	0.097	0.127	0.058	0.197
Observations (Panel A)	532	532	532	532	532	532	532	532	532
Observations (Panel B)	474	474	474	474	474	474	474	474	474

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification, dummies dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. 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: Heterogeneous Treatment Effects on Educational Outcomes by Median Parental Education

	School-Age Child Sample (7-17 Years)							
					Investment Index Components			
	Attended College	Years of Schooling	Still in School or College	Investment Index	College Spending (Standard-ized)	Secondary School Investment Subindex	Primary School Investment Subindex	Secondary School Perfomance Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grace Period	0.003	-0.705*	-0.002	-0.043	-0.047	-0.003	-0.052	-0.143**
	(0.056)	(0.384)	(0.053)	(0.082)	(0.087)	(0.097)	(0.082)	(0.073)
Grace Period × Above-Median Parental Education	0.164**	1.395***	0.127	0.405***	0.365**	0.460***	0.201	0.273**
	(0.077)	(0.446)	(0.080)	(0.135)	(0.183)	(0.145)	(0.127)	(0.132)
Above-Median Parental Education	0.182***	0.205	0.108**	0.124	0.307***	0.069	0.152	0.324***
	(0.052)	(0.350)	(0.051)	(0.089)	(0.118)	(0.082)	(0.094)	(0.095)
p-value: Grace Period + Grace Period x Above-Median Parental Education	0.002	0.007	0.028	0.001	0.034	0.000	0.129	0.219
Mean of Omitted Group	0.160	9.302	0.123	-0.188	-0.167	-0.158	-0.144	-0.188
Observations	541	543	544	543	531	543	543	543

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies, dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. The sample consists of children of the client aged 7-17 at baseline that are still alive at the time of the 2018 survey. 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 A11: 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 A12: Robustness Checks

					Investment Index Components			
	Attended College	Years of Schooling	Still in School or College	Investment Index	College Spending (Standard-ized)	Secondary School Investment Subindex	Primary School Investment Subindex	Secondary School Perfomance Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: 6-16 Years at Baseline								
Grace Period	0.096**	0.192	0.057	0.182***	0.205**	0.189***	0.068	0.035
	(0.038)	(0.212)	(0.041)	(0.069)	(0.093)	(0.073)	(0.068)	(0.063)
Panel B: 6-17 Years at Baseline								
Grace Period	0.098***	0.190	0.052	0.185***	0.196**	0.220***	0.076	0.025
	(0.035)	(0.209)	(0.039)	(0.065)	(0.089)	(0.073)	(0.067)	(0.061)
Panel C: 6-18 Years at Baseline								
Grace Period	0.097***	0.153	0.050	0.170***	0.148*	0.194***	0.056	0.032
	(0.035)	(0.202)	(0.037)	(0.064)	(0.081)	(0.069)	(0.066)	(0.060)
Panel D: 7-16 Years at Baseline								
Grace Period	0.093**	0.238	0.089**	0.191**	0.143	0.240***	0.064	0.028
	(0.042)	(0.236)	(0.041)	(0.075)	(0.092)	(0.079)	(0.070)	(0.067)
Panel E: 7-18 Years at Baseline								
Grace Period	0.096**	0.155	0.076**	0.184***	0.142	0.244***	0.053	0.026
	(0.038)	(0.220)	(0.037)	(0.069)	(0.087)	(0.074)	(0.069)	(0.064)
Panel F: 8-16 Years at Baseline								
Grace Period	0.074*	0.128	0.098**	0.145*	0.131	0.209***	0.039	-0.030
	(0.044)	(0.250)	(0.038)	(0.079)	(0.098)	(0.080)	(0.077)	(0.073)
Panel G: 8-17 Years at Baseline								
Grace Period	0.085**	0.072	0.082**	0.158**	0.132	0.240***	0.054	-0.035
	(0.040)	(0.237)	(0.035)	(0.073)	(0.092)	(0.078)	(0.077)	(0.070)
Panel H: 8-18 Years at Baseline								
Grace Period	0.085**	0.052	0.077**	0.148**	0.133	0.207***	0.030	-0.023
	(0.040)	(0.228)	(0.034)	(0.071)	(0.089)	(0.073)	(0.076)	(0.068)

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies, dead client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. All outcomes are obtained from the 2018 survey. See Data Appendix for detailed variable definitions and Appendix Table A7 for treatment effects on index components. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A13: Correlational Evidence from Control Group

	Control Group Children (7+ Years at Baseline)						
				Investment Index Components			
	Attended College	Years of Schooling	Investment Index	College Spending (Standard-ized)	Secondary School Investment Subindex	Primary School Investment Subindex	Secondary School Perfomance Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Socioeconomic Index	0.017	0.280***	0.077**	0.044	0.084***	0.054**	0.042*
	(0.013)	(0.093)	(0.033)	(0.041)	(0.032)	(0.024)	(0.023)
Highest Parental Education (in Years)	0.031***	0.295***	0.057***	0.057***	0.052***	0.033***	0.056***
	(0.005)	(0.044)	(0.011)	(0.015)	(0.010)	(0.009)	(0.011)
Mean	0.203	9.159	-0.000	-0.000	0.000	0.000	0.000
Observations	483	484	484	470	484	484	484

Notes: Sample consists of all children of the client in the control group aged 7 years or older at baseline that are still alive at the time of the 2018 survey. All regressions include dead client 2018 survey dummies. Robust standard errors appear in 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 A14: Heterogeneous Treatment Effects on Household Business Outcomes by Parental Education

	Any Business		Profit		Business Closure		Capital		Workers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Grace Period	0.062	0.039	89.739	-399.649	0.041	0.003	12261.534	-5078.927	0.257	-0.137
	(0.045)	(0.076)	(256.247)	(401.283)	(0.067)	(0.090)	(17792.116)	(19703.168)	(0.303)	(0.524)
Grace Period × Parents Attended Secondary School	-0.090*	-0.068	210.598	631.121	-0.107	-0.067	4114.443	22550.553	-0.219	0.279
	(0.049)	(0.080)	(275.890)	(430.976)	(0.073)	(0.100)	(19840.391)	(21299.685)	(0.344)	(0.577)
Grace Period × Parents Attended Higher Secondary School	-0.044	0.024	-76.806	517.922	-0.145	-0.167	10409.737	39042.890	-0.141	0.444
	(0.069)	(0.100)	(363.500)	(528.684)	(0.094)	(0.128)	(23815.458)	(30801.052)	(0.383)	(0.608)
Grace Period × School-Age Households		0.040		907.203*		0.087		31875.280		0.728
		(0.087)		(514.615)		(0.144)		(22650.393)		(0.644)
Grace Period × Parents Attended Secondary School × School-Age Households		-0.035		-775.866		-0.095		-32236.529		-0.924
		(0.096)		(569.780)		(0.154)		(28238.725)		(0.704)
Grace Period × Parents Attended Higher Secondary School × School-Age Households		-0.148		-1176.039*		0.046		-55292.537		-1.137
		(0.120)		(671.086)		(0.189)		(39347.816)		(0.796)
p-value: Grace Period + Grace Period x Parents Attended Secondary School	0.216	0.360	0.005	0.116	0.037	0.169	0.060	0.044	0.778	0.494
p-value: Grace Period + Grace Period x Parents Attended Higher Secondary School	0.719	0.327	0.957	0.690	0.096	0.058	0.140	0.144	0.694	0.457
p-value: Grace Period + Grace Period x School-Age HHs		0.101		0.110		0.378		0.219		0.100
p-value: GP + GP x School-Age HHs + GP x Sec. School + GP x Sec. School x School-Age HHs		0.978		0.195		0.682		0.350		0.358
p-value: GP + GP x School-Age HHs + GP x Higher Sec. School + GP x Higher Sec. School x School-Age HHs		0.538		0.105		0.844		0.220		0.283
Mean of Omitted Group	0.876	0.876	1285.978	1285.978	0.451	0.451	29679.288	29679.288	1.136	1.136
Observations	2145	2145	2072	2072	2160	2160	2074	2074	2114	2114

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, client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. Profits, capital, and number of workers are top-coded at 99.5% for each survey round. Profits and capital are deflated to 2007 prices using CPI data published by the World Bank. See Data Appendix for detailed variable definitions. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table A15: Heterogeneous Treatment Effects on Educational Expenditures by Parental Education

	Educational Expenditures							
	Pooled		2008		2012		2018	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Grace Period	-4.755	-139.667*	76.867	-24.172	-18.699	-148.461	-52.877	-184.373
	(51.368)	(77.698)	(88.262)	(90.997)	(91.584)	(96.723)	(91.955)	(176.683)
Grace Period × Parents Attended Secondary School	96.371	128.846	-16.197	-72.295	124.800	164.277	179.855	252.224
	(67.239)	(91.256)	(109.841)	(102.498)	(106.237)	(113.319)	(113.330)	(203.221)
Grace Period × Parents Attended Higher Secondary School	-42.274	-14.049	-123.710	-79.859	22.331	81.387	-45.582	-102.157
	(142.543)	(152.625)	(166.876)	(201.445)	(159.725)	(186.789)	(314.217)	(336.470)
Grace Period × School-Age Households		234.359**		108.125		265.121		246.461
		(108.437)		(191.683)		(186.703)		(191.871)
Grace Period × Parents Attended Secondary School × School-Age Households		-25.151		204.601		-71.002		-148.433
		(137.479)		(225.662)		(202.786)		(234.872)
Grace Period × Parents Attended Higher Secondary School × School-Age Households		-1.545		4.299		-30.852		145.909
		(320.268)		(366.038)		(355.213)		(650.395)
p-value: Grace Period + Grace Period x Parents Attended Secondary School	0.050	0.849	0.402	0.196	0.040	0.807	0.134	0.540
p-value: Grace Period + Grace Period x Parents Attended Higher Secondary School	0.729	0.269	0.736	0.573	0.978	0.653	0.737	0.329
p-value: Grace Period + Grace Period x School-Age HHs		0.202		0.610		0.460		0.409
p-value: GP + GP x School-Age HHs + GP x Sec. School + GP x Sec. School x School-Age HHs		0.393		0.511		0.402		0.392
p-value: GP + GP x School-Age HHs + GP x Higher Sec. School + GP x Higher Sec. School x School-Age HHs		0.680		0.959		0.653		0.957
Mean of Omitted Group	317.711	303.505	261.731	130.853	381.575	260.863	314.710	505.225
Observations	2271	2271	793	793	733	733	745	745

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, client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. Expenditure outcomes in 2007 were collected for the past 12 months and are divided by 12 to calculate monthly expenses. All expenditure 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: Heterogeneous Treatment Effects on Other Expenditures by Parental Education

	Health Expenditures		Renovation Expenditures	
	(1)	(2)	(3)	(4)
Grace Period	46.388	265.787	390.420	194.688
	(373.677)	(531.879)	(1279.923)	(1179.974)
Grace Period × Parents Attended Secondary School	45.020	-181.682	-647.546	-322.756
	(390.232)	(534.801)	(1353.500)	(1345.077)
Grace Period × Parents Attended Higher Secondary School	279.881	11.955	-539.340	498.516
	(433.542)	(608.278)	(1626.279)	(1718.344)
Grace Period × School-Age Households		-391.883		332.022
		(761.737)		(2547.720)
Grace Period × Parents Attended Secondary School × School-Age Households		417.214		-473.482
		(789.577)		(2680.118)
Grace Period × Parents Attended Higher Secondary School × School-Age Households		484.434		-2218.360
		(848.476)		(3225.314)
p-value: Grace Period + Grace Period x Parents Attended Secondary School	0.451	0.625	0.517	0.839
p-value: Grace Period + Grace Period x Parents Attended Higher Secondary School	0.129	0.356	0.874	0.569
p-value: Grace Period + Grace Period x School-Age HHs		0.813		0.809
p-value: GP + GP x School-Age HHs + GP x Sec. School + GP x Sec. School x School-Age HHs		0.573		0.935
p-value: GP + GP x School-Age HHs + GP x Higher Sec. School + GP x Higher Sec. School x School-Age HHs		0.547		0.602
Mean of Omitted Group	1313.893	1313.893	1542.243	1542.243
Observations	2159	2159	1963	1963

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, client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. Expenditure outcomes in 2007 were collected for the past 12 months and are divided by 12 to calculate monthly expenses. All expenditure 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 A17: Alternative Mechanisms

	Empowerment Index		Migrated	
	(1)	(2)	(3)	(4)
Grace Period	-0.006	-0.048	0.009	0.021
	(0.058)	(0.080)	(0.021)	(0.032)
Grace Period × School-Age Households		0.091		-0.022
		(0.122)		(0.043)
p-val: Grace Period + Grace Period x School-Age HHs		0.624		0.975
Mean of Omitted Group	-0.000	-0.000	0.100	0.100
Observations	747	747	747	747

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the household level and include stratification dummies, client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 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 A18: Treatment Effects on Socio-Economic Outcomes

	Full-Time Employed/Self-Employed			Income			Married			Housewife
	Pooled	Male	Female	Pooled	Male	Female	Pooled	Male	Female	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Main Child Sample (7-17 Years at Baseline), Pooled										
Grace Period	0.009	-0.028	0.058	27.154	749.101	-308.759	-0.022	0.013	-0.071	-0.144***
	(0.036)	(0.056)	(0.051)	(464.661)	(820.526)	(455.768)	(0.039)	(0.047)	(0.056)	(0.054)
Panel B: Main Child Sample (7-17 Years at Baseline), Heterogeneity by Parental Education										
Grace Period	0.181**	0.168	0.185**	612.388	1743.060*	-94.443	0.154	0.172	0.168	0.052
	(0.087)	(0.126)	(0.092)	(595.442)	(1029.725)	(363.433)	(0.101)	(0.128)	(0.116)	(0.112)
Grace Period × Parents Attended Secondary School	-0.207**	-0.213	-0.184	-807.907	-1335.593	-151.981	-0.214*	-0.165	-0.258*	-0.172
	(0.102)	(0.150)	(0.113)	(717.720)	(1350.452)	(528.468)	(0.111)	(0.141)	(0.136)	(0.135)
Grace Period × Parents Attended Higher Secondary School	-0.211	-0.179	-0.066	-865.552	-1363.260	-164.329	-0.262*	-0.286*	-0.367*	-0.459***
	(0.147)	(0.213)	(0.198)	(1621.412)	(2506.779)	(2279.317)	(0.146)	(0.166)	(0.215)	(0.175)
Parents Attended Secondary School	0.096	0.032	0.111*	666.564	245.840	823.637**	-0.074	-0.069	-0.069	0.039
	(0.066)	(0.119)	(0.057)	(491.671)	(972.403)	(404.061)	(0.079)	(0.090)	(0.105)	(0.088)
Parents Attended Higher Secondary School	0.053	-0.154	0.202	2200.904*	1720.103	2929.310**	-0.216**	-0.106	-0.328**	-0.139
	(0.105)	(0.173)	(0.125)	(1145.345)	(2055.767)	(1309.552)	(0.107)	(0.109)	(0.161)	(0.157)
Panel C: Placebo Samples (18+ Years at Baseline), Pooled										
Grace Period	-0.003	0.024*	-0.049	957.499*	1599.151	166.860	-0.024	-0.027	-0.011	0.027
	(0.033)	(0.013)	(0.073)	(577.706)	(1038.414)	(395.017)	(0.030)	(0.052)	(0.021)	(0.072)
p-value: Grace Period + Grace Period x Parents Attended Secondary School	0.572	0.521	0.987	0.683	0.646	0.551	0.214	0.905	0.210	0.106
p-value: Grace Period +	0.788	0.941	0.490	0.875	0.876	0.911	0.250	0.247	0.262	0.002
Mean of Omitted Group (Panel A)	0.460	0.746	0.181	3292.181	5492.600	1239.552	0.449	0.209	0.681	0.609
Mean of Omitted Group (Panel B)	0.349	0.722	0.080	2089.024	4844.118	137.500	0.581	0.278	0.800	0.720
Mean of Omitted Group (Panel C)	0.667	0.979	0.315	5133.649	9211.009	776.471	0.907	0.839	0.984	0.685
Observations (Panels A-B)	543	273	270	514	253	261	543	273	270	270
Observations (Panel C)	492	269	223	379	190	189	492	270	222	223

Notes: Standard errors are clustered by loan group and appear in brackets. All regressions are run on the child level and include stratification dummies, client 2018 survey dummies, and controls that are chosen using the double-lasso approach. Appendix Table A2 shows the list of potential lasso controls. Income is 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.
- *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.
- *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.
- *Child Workers:* number of client's children in the household in 2012 that worked at some point for a household business.
- *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

- *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 Schooling:* years of K-12 schooling of the child.

- *Investment Index:* standardized index that consists of the following variables: college spending, secondary school investment subindex, and primary school investment subindex.
- *Secondary School Investment Subindex:* standardized index that consists of the following variables: private secondary school, total secondary school fees, and total secondary school after-school tutoring.
- *Primary School Investment Subindex:* standardized index that consists of the following variables: private primary school, total primary school fees, and total primary school after-school tutoring.
- *Private School:* indicator variable that is equal to one if the child attended at least one year of private primary school for grades 1 to 4 or at least one year of private secondary school for grades 5 to 12 respectively.
- *Total Secondary School Fees:* obtained from the following textit question: "How much were/are the total school fees for (CHILD) in class X (including textbooks, uniforms, school fees, admission fees etc.)?". The question was explicitly asked for grades 1, 10 and 12 and whenever the child changed a school. For the remaining classes, we impute the value by coping the value from the class below. The value is zero if the child did not complete the corresponding class. We then compute total primary school fees by summing all fees for grades 1 to 4 and total secondary school fees by summing all fees for grades 5 to 12.
- *Total After-School Tutoring:* obtained from the following survey question: "How much did you spend in total on private tuition for (CHILD) in class X?". The question was explicitly asked for grades 1, 10 and 12 and whenever the child changed a school. For the remaining classes, we impute the value by coping the value from the class below. The value is zero if the child did not complete the corresponding class. We then compute total primary school after-school tutoring by summing all tutoring costs for grades 1 to 4 and total total secondary school after-school tutoring by summing all tutoring costs for grades 5 to 12.
- *College Spending:* obtained from the following survey question: "How much did (CHILD) spend in total until now on all post-secondary schooling (excluding living costs such as board or food)?"
- *Performance Index:* standardized index that consists of the following variables: grade A in class 10, grade A n class 12, and science track.
- *Grade A in Class 10:* indicator variable that is equal to one if the child received an overall A grade in the Grade 10 exams. It is equal to zero if the child dropped out before grade 10.
- *Grade A in Class 12:* indicator variable that is equal to one if the child received an overall A grade in the Grade '1 exams. It is equal to zero if the child dropped out before grade 12.
- *Science Track:* indicator variable that is equal to one if the child was enrolled in the science track in grades 11 to 12. It is equal to zero if the child dropped out before grade 12.
- *Dropout Reasons:* obtained from the following survey question: "Why did (NAME) stop attending school?" This question was asked for all children that did not complete grade 12. Multiple choices were allowed. The value is equal to zero if the child completed grade 12.
- *Married:* child is married at the point of the 2018 survey.

- *Currently a Student:* 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.
- *Child Income:* total child income in the past 30 days.
- *Housewife:* child's occupation is housewife.

Control Variables

- *Client's Age:* age of the client in years at baseline.
- *Married:* indicator variable that is equal to one if the client was married at baseline.
- *Muslim:* indicator variable that is equal to one if the head of the household is Muslim.
- *Client's Years of Education:* years of education of client at baseline.
- *Household Size:* number of household members at baseline.
- *Household Shock:* dummy for birth, death, or heavy rain in the last 30 days.
- *Household Has a Business (Narrow) :* indicator variable that is equal to one if the household reported to have at least one business in operation at baseline, excluding businesses formed either during 30 days prior to or after loan group formation.
- *Owns Home:* indicator variable that is equal to one if the household owned the home at baseline.
- *Mother Has Financial Control:* obtained from the following survey question: "If a close relative like your parents or siblings fell sick and needed money, would you be able to lend money to that relative, if you had the extra money?".
- *No Drain in Neighborhood:* indicator variable that is equal to one if the neighborhood had no drain at baseline.
- *Loan Amount:* VFS loan amount given to client.
- *Socioeconomic Index:* consists of the first component of a principal component analysis of whether the household had owned a radio, cassette player, camera, refrigerator, washing machine, heater, television, VCR, pressure lamp, tube well, wristwatch, or clock for longer than one year.
- *Spouse's Age:* years of education of the client's spouse at baseline.
- *Spouse's Years of Education:* years of education of the client's spouse at baseline.
- *Number of Children (Still Alive in 2018):* total number of children of the client at baseline that are still alive in 2018. This variable is constructed based on age variables in the child roster in the 2018 survey. The age variable is missing if the child was not alive in the 2018 survey. .
- *Birth Order:* birth order of the child.
- *Resides with Parents:* indicator variable that is equal to one if the child was part of the household roster at baseline.

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 $\frac{\frac{\partial b}{\partial W}}{\partial e} < 0$.