NBER WORKING PAPER SERIES

HUMAN CAPITAL INVESTMENT IN THE PRESENCE OF CHILD LABOR

Natalie Bau
Martin Rotemberg
Manisha Shah
Bryce Steinberg

Working Paper 27241
http://www.nber.org/papers/w27241

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2020, Revised March 2021

We are grateful to seminar and conference participants at Ashoka, Dartmouth, Duke, IFPRI, Kent, NBER Children's meetings, Northwestern, NYU, UCSD, NEUDC, and PacDev for insightful comments and suggestions. We would like to thank Eric Edmonds, Michael Gechter, and Abhijeet Singh for helpful advice. We thank Gabriela Rubio and Yiran Xie for their excellent research assistance. Shah and Steinberg gratefully acknowledge funding from NSF grant #1658852. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Human Capital Investment in the Presence of Child Labor
Natalie Bau, Martin Rotemberg, Manisha Shah, and Bryce Steinberg
NBER Working Paper No. 27241
May 2020, Revised March 2021
JEL No. I2,J1,O12

ABSTRACT

Policies that improve early life human capital are a promising tool to alter disadvantaged children's lifelong trajectories. Yet, in many low-income countries, children and their parents face tradeoffs between schooling and productive work. If there are positive returns to human capital in child labor, then children who receive greater early life investments may attend less school. Exploiting early life rainfall shocks in India as a source of exogenous variation in early life investment, we show that increased early life investment reduces schooling in districts with high child labor. These effects persist and are intergenerational, affecting adult household consumption, and lead to a divergence in the next generations' educational outcomes. Our results are robust to instrumenting for child labor prevalence with crop-mix and to the inclusion of a rich set of district-level characteristics. We provide evidence that reductions in educational investment in response to positive early life shocks are total welfare-reducing.

Natalie Bau
Department of Economics
University of California at Los Angeles
Bunche Hall 8283
315 Portola Plaza
Los Angeles, CA 90095
and NBER
nbau@ucla.edu

Martin Rotemberg
New York University
19 W4th St, 6th Floor
New York, NY 10012
mrotemberg@gmail.com

Manisha Shah
Department of Public Policy
University of California, Los Angeles
Luskin School of Public Affairs
3250 Public Affairs Building
Los Angeles, CA 90095-1656
and NBER
manishashah@ucla.edu

Bryce Steinberg
Watson Institute
Brown University
111 Thayer Street
Providence, RI 02912
and NBER
bryce_steinberg@brown.edu

1 Introduction

Policies that increase human capital investment during the critical period between the ages of zero to five, when the developing brain is most plastic (Knudsen et al., 2006), are a promising tool to increase long-term human capital attainment. From 2001 to 2013, the World Bank made 273 investments in early childhood development across 80 different countries, spending 3.4 billion USD (Sayre et al., 2015). The high returns of early life investments may be further amplified by "dynamic complementarities" in the human capital production function, where early skill investments increase the returns to later human capital investments (Cunha and Heckman, 2007), endogenously leading to increases in those investments.

However, the success of these interventions in low-income countries could depend critically on the economic environment. This is because early interventions may also increase the payoff from children or adolescents engaging in market work, working on the family farm, or working in home production. In fact, the prevalence of child labor is still incredibly high in low-income countries. The International Labour Organization estimates that there are approximately 265 million working children in the world—almost 17 percent of the worldwide child population (Ortiz-Ospina and Roser, 2020). As a result, in environments where children have opportunities to work productively, actions taken by parents and children to alter human capital investments in response to positive early life shocks can reduce the positive educational effects of these interventions. While much of the literature on early life investment has focused on the effects of early life investment in high-income countries where child labor is rare, understanding how parents and children respond to positive early life shocks is particularly important in low-income countries, where child labor is common (Bharadwaj et al., 2013; Edmonds, 2007).

In this paper, we exploit variation in early life investment due to rainfall shocks to provide evidence that increased early life investment increases the opportunity cost of schooling by increasing the returns to child labor in rural India. We show that increased early life investment reduces schooling (and increases work) in districts with a high prevalence of child labor. Moreover, we find that these reductions in schooling are most pronounced among girls and lower castes, suggesting that early life investment can increase inequality and harm the most vulnerable in regions with high child labor.

Importantly, these effects are both persistent and intergenerational. To assess the effects of early life investment, we provide some of the first long-run and intergenerational estimates of the effects of early life rainfall shocks, like Maccini and Yang (2009) in Indonesia, but on a broader set of outcomes. We show that positive early life income shocks make adults from high child labor districts worse off on a variety of dimensions. By age 16, individuals in high child labor districts who experience positive rainfall shocks in early childhood score lower on math and reading tests. Households whose heads experienced positive early life income shocks in high child labor districts have lower per capita consumption.

The net negative effect of a positive shock persists across generations. Children of household heads who received a positive early rainfall shock in a high child labor district are more likely to work and less likely to attend school. In contrast, in low child labor districts, individuals who received positive early life shocks have better outcomes on all dimensions. Altogether, these results suggest that the presence of high levels of child labor can eliminate the positive long-term impacts of early life investments and even cause these investments to make children worse off.

One important concern about the validity of these OLS results is that high child labor districts differ from low child labor districts on a variety of dimensions. For example, high child labor might be driven by low school quality, which could in turn attenuate the effects of positive early life shocks. We account for omitted variable bias in two ways.

First, we implement an IV strategy by exploiting an alternative, technological source of variation in the demand for child labor – crop variation – which is primarily driven by regions' agroclimatic features. Children are known to have a comparative advantage in some crops, such as sugar and cotton (Levy, 1985). To choose crop-based instruments in a principled way and maximize our statistical power, we follow Belloni et al. (2012) and use lasso to select instruments for child labor prevalence from a set of measures of the adult employment share of total adult agricultural employment in different disaggregated agricultural industries. The resulting instruments do include sugar and cotton, along with several other crops. Across outcomes, the instrumental variables regressions deliver nearly identical results to OLS.

Second, to further rule out the possibility of omitted variable bias, we also control for a battery of other local characteristics, including average incomes, literacy rates, and measures of school quality, as well as household-specific socioeconomic controls and household fixed effects. The inclusion of this rich set of controls has little effect on the results we observe.

Decreased educational attainment and diminished long-run outcomes do not necessarily imply that reduced schooling is welfare-reducing (Baland and Robinson, 2000). It is plausible that the returns to child labor are similar or greater than the returns to schooling in high child labor areas. Efficient households may prefer additional income today to greater income in the future. On the other hand, parents may inefficiently underinvest in the education of their children in response to positive shocks either because they underestimate the size of dynamic complementarities (that is, make mistakes) or because there are incomplete contracting problems between parents and children. Incomplete contracting occurs when imperfectly altruistic parents do not make efficient educational investments because they cannot capture the returns to these investments in the future (Banerjee, 2004; Ashraf et al., 2020; Bau, forthcoming). We examine whether reductions in education are consistent with welfare-maximization in several ways.

First, we allow our estimates to depend on gender and birth order. Since oldest sons traditionally care for parents in their old age, allowing parents to partake in the returns to their education, we expect incomplete contracting problems to be less important for their educational investment. Indeed, we find that for oldest sons' parents reinforce early investments in human capital regardless of child labor prevalence. Second, we examine whether parents are aware that positive early life shocks increase children's cognitive ability. Parents' assessments of the ability of children who experience positive early life shock are significantly higher than their assessments of those who do not, regardless of the prevalence of child labor in the district. This suggests that parents are not simply making mistakes because they are unaware of their children's cognitive skills. Finally, we use a back-of-the-envelope exercise to estimate the maximum discount factor that would justify increasing child labor today at the expense of future consumption if parents are perfectly altruistic. The implied discount factor is lower than social discount factor estimates and is also inconsistent with Indian interest rates over the same period. Altogether, the pattern of results suggest that parents' inefficiently underinvest in children's education in response to positive early life shocks and that at least part of this problem is driven by incomplete contracting.

This paper builds on the literature on human capital investment and dynamic complementarities (Cunha and Heckman, 2008), but takes into account an important feature of developing countries: that children work (Basu and Pham, 1998; Basu, 1999; Edmonds and Pavcnik, 2005). The idea that early and later life educational investments complement each other has been directly tested in several different contexts.[1] In low child labor areas, we find revealed preference evidence of dynamic complementarities, as children who receive greater early life investments endogenously receive more education later on.[2] However, we show that—in the presence of child labor—parents may fail to invest more in education in response to positive early life shocks, even in the presence of dynamic complementarities. Indeed, reduced educational investment in response to positive early life shocks does not rule

[1] See for example Adhvaryu et al. (2018); Aizer and Cunha (2012); Agostinelli and Wiswall (2016); Gilraine (2017); Johnson and Jackson (2019); Malamud et al. (2016); Duque et al. (2020); Rossin-Slater and Wüst (forthcoming) among others. A related literature, primarily in developing countries, estimates the extent to which parents invest unequally in their children in order to reinforce or mitigate early differences in human capital (Behrman et al., 1994; Adhvaryu and Nyshadham, 2016; Dizon-Ross, 2019; Bharadwaj et al., 2018), whether due to dynamic complementarities or other convexities in human capital returns, and finds mixed results.

[2] Akresh et al. (2017) find similar, positive effects of better early life rainfall on later educational investments in Burkino Faso.

out dynamic complementarities in the human capital production function.

Our results contribute to a growing literature on the opportunity cost of schooling in both high-income (Charles et al., 2018; Cascio and Narayan, 2019) and low-income countries (Shah and Steinberg, 2017, forthcoming; Atkin, 2016). This literature has already shown that shocks to the opportunity cost of schooling can reduce human capital investment. We expand on this finding by linking it for the first time with the large literature on the long-run importance of early childhood and even pre-natal investment (Heckman, 2007; Almond and Currie, 2011; Currie and Vogl, 2013; Currie and Rossin-Slater, 2015; Attanasio et al., forthcoming; García et al., 2017). Consistent with this literature, we find that early life circumstances can have a remarkably persistent effect on outcomes throughout adulthood and even into the next generation. Early life investments that increase children's returns to education but correspondingly increase the opportunity cost of schooling can negatively affect long-term outcomes. We provide evidence that children and parents may inefficiently underinvest in education in response to positive early life shocks.

To guide the empirical analysis, Section 2 introduces a theoretical framework for human capital investment and child labor in the presence of dynamic complementarities and derives testable predictions. Section 3 provides further background on child labor in India and describes the data used in the analysis. Section 4 describes both the OLS and the lasso IV empirical strategies, and Section 5 reports the primary results on children's schooling and work using a variety of specifications. Section 6 reports the long-run and intergenerational effects of early life investment on adult outcomes in the presence of child labor. Section 7 reports the results from a series of robustness tests. Section 8 discusses the results and provides evidence that parents are not choosing welfare-maximizing education levels. Section 9 concludes.

2 Theoretical Framework

To develop testable predictions about the effects of early life human capital investment on education and child labor, we develop a simple, partial equilibrium model. Intuitively, this model brings together the theoretical literature on child labor (e.g. Basu and Pham, 1998), the trade-off between child labor and human capital formation (Basu, 1999; Baland and Robinson, 2000; Dessy, 2000; Hazan and Berdugo, 2002; Ravallion and Wodon, 2000; Beegle et al., 2009), and the literature on dynamic complementarities (Cunha and Heckman, 2007). Doing so allows us to clarify the circumstances under which positive early life human capital investments can reduce schooling, even in the presence of dynamic complementarities in the human capital production function. The model captures the following intuition. If there are dynamic complementarities, increased early life human capital investment positively affects the returns to later schooling investment, incentivizing parents to invest more in later education. This is the standard effect of dynamic complementarities posited by Cunha and Heckman (2007). However, the new feature of our model is that in places where child labor is prevalent, early life investments also affect the child wage, which is the opportunity cost of schooling. Thus, a novel prediction of our model is that this countervailing force attenuates and can even reverse the positive effect of early life investment in schooling. In the latter case, early life investments increase the child wage more than they increase the expected utility the parent derives from the increased return to education, causing schooling and potentially even long-run consumption to fall. Furthermore, if the parent is imperfectly altruistic or she underestimates the size of dynamic complementarities relative to the effect of early life human capital investments on wages, reductions in education due to early life investments will be inefficient and total welfare-reducing.

2.1 Set Up

The decision-maker in the model is a parent, and each parent has one child. The decision-maker is indexed by her child's exogenous educational ability, α, which is distributed according to the function F and her type of district, $d \in \{low, high\}$, which denotes whether a district has high or low child labor. To simplify exposition, at the risk of abusing notation, subscripts for these indices are suppressed when not relevant. There are three periods in the child's life: early life, school age, and adulthood. α becomes observable in period 2, when a child is old enough to attend school. In period 1, the parent decides how much to invest in a child's early life human capital, h. In period 2, the parent makes a discrete decision whether or not to educate the child, $e \in \{0, 1\}$, or have the child work for a wage $w^c_{2,d}(h)$, which depends on h and d.[3] The parent's consumption in the first two periods – when the parent is making human capital investment decisions – is explicitly included in the model. In addition, the parent also places some weight on the child's third period adult utility. This can be thought of as capturing both altruism and a reduced-form representation of the parent's third period consumption, which is determined by the fraction of the child's third period utility that the parent captures as old age support. A parent's preferences in period

[3]The discrete educational investment maps to the fact that children either primarily work or attend school in our data, rather than moving between working and education on a continuum. This is a function of the way data is collected, which asks for a "primary activity" rather than collecting a continuous measure, such as time spent in school and time spent working.

1 are represented by

$$U_1^p(h) = u(c_1^p(y_1, h)) + \rho E\left(\max_e u(c_2^p(y_2, e, h)) + \delta U^c(c_3^c(e, h))\right),$$

where c_1^p and c_2^p are the parent's consumption in periods 1 and 2, c_3^c is the child's adult consumption in period 3, u is the parental utility function, U^c is the child's adult utility, which depends on educational and early life investments, $\delta = \rho\gamma$ is the product of the parent's discount factor ρ and γ, where γ captures both the parent's altruism toward the child and her ability to resolve incomplete contracting problems by extracting utility from the child in the third period, and the expectation is taken over realizations of α. Both u and U^c are assumed to have diminishing marginal returns in consumption.

Similarly, the parent's period 2 utility is given by

$$U_2^p(e, h) = u(c_2^p(y_2, e, h)) + \delta U^c(c_3^c(e, h)).$$

For simplicity, the model abstracts away from borrowing and saving.[4] Then, parental consumption in period 1 is equal to some exogenous income y_1 net the cost of human capital investment h. Parental consumption in period 2 is total income y_2 net the cost of schooling if $e = 1$ or plus the wages from child labor if $e = 0$. Thus,

$$\begin{aligned}
c_1^p &= y_1 - c_h h \\
c_2^p &= y_2 + (1 - e)w_{2,d}^c(h) - c_e e \\
c_3^c &= w_3^c(e, h) + \alpha e
\end{aligned}$$

where c_h is a cost of the human capital investment and c_e is the cost of education. $w_3^c(e, h) + \alpha e$ is what the parent believes to be the child's total adult wage, where the function $w_3^c(e, h)$ allows for a flexible relationship in adult wages between e and h and does not directly depend on d, and the returns to education also depend on exogenous schooling ability α.[5] Parents may have incorrect beliefs about $w_3^c(e, h)$, such that $w_3^c(e, h) \neq \tilde{w}_3^c(e, h)$, where $\tilde{w}_3^c(e, h)$ is the true relationship. Following Cunha and Heckman (2008), parents perceive that there are dynamic complementarities in the adult wage function if $\frac{\partial w_3^c(1,h)}{\partial h} > \frac{\partial w_3^c(0,h)}{\partial h}$. This captures the idea that early life investments in human capital make educational investments more productive.

[4] We relax this assumption in the empirical analysis and explicitly allow for the possibility that households save by showing that the main results are robust to household fixed effects models, which compare siblings who received different shocks.

[5] The fact that adult wages do not depend on d is consistent with the idea that adults can migrate to other labor markets.

Before solving the model, we make two assumptions to simplify exposition. First, we assume that $w^c_{2,low}(h) = 0$, so that if child labor in a district is negligible, child wages are always equal to zero. In places where child labor is high, we assume $\frac{\partial w^c_{2,high}}{\partial h} > 0$. This assumption captures the idea that early life human capital investments increase child wages. We directly test this assumption in subsection 3.4.

2.2 Propositions

We now solve for the parent's equilibrium investment decisions and relate them to changes in first period income y_1.

Proposition 1. *Denote h^* as the parent's equilibrium choice of h. If $w^c_{2,d}(h)$ and $w^c_3(e, h)$ have constant or diminishing marginal returns in h, then $\frac{\partial h^*}{\partial y_1} > 0$ for all d.*

Proof. See Appendix A.

The first proposition simply delivers the classic result that a positive income shock in early life will increase early life human capital investment. The intuition for this prediction is straightforward. When y_1 increases, the marginal utility of first period consumption falls, increasing the parent's incentive to invest in her child's human capital. This proposition is consistent with the previous findings of Shah and Steinberg (2017) and Maccini and Yang (2009), who show that an early life shock increases test scores and weight.

Building on Proposition 1, the next set of propositions describe the key empirical results in the paper – that early life shocks increase education rates in places with low child labor and have smaller positive or even negative effects on education rates in places with high child labor. Proposition 2 delivers a standard result in the dynamics complementarities literature.

Proposition 2. *Denote $\lambda_d(y_1)$ to be the share of children educated in a district of type d given y_1. $\frac{\partial \lambda_{low}(y_1)}{\partial y_1} > 0$ only if $\frac{\partial w^c_3(1,h)}{\partial h} > \frac{\partial w^c_3(0,h)}{\partial h}$.*

Proof. See Appendix A.

This proposition captures the fact that, in low child labor places, increased h only positively affects the parent's educational decisions through its effect on the returns to later life educational investments. Therefore, if an early life shock increases educational investments in low child labor markets, this is evidence in favor of dynamic complementarities.

The remaining propositions introduce the novel predictions of this paper. Proposition 3a shows that the standard dynamic complementarity results can be reversed by opportunity costs. In high child labor markets, positive early life investments can have *negative* effects, despite their potential positive effect on the returns to education due to dynamic complementarities. Proposition 3b (presented in Appendix A) describes the conditions under which opportunity cost effects are not strong enough to reverse the positive effect of early life investment on education but nonetheless dampen that positive effect.

Proposition 3a. *If* $\frac{\partial w^c_{2,high}(h^*(y_1))}{\partial h}$ *is sufficiently great, then* $\frac{\partial \lambda_{high}(y_1)}{\partial y_1} < 0$.

Proof. See Appendix A.

Proposition 3a shows that when the effect on parental utility of the increase in child wages due to an increase in y_1 is sufficiently large in high child labor places, it outweighs the effect of the increase in the returns to education (weighted by the parents' altruism and discount factor). Then, positive income shocks that increase early life investments can lead to reduced education.

Finally, our last two propositions consider some plausible circumstances under which these reductions in education will be inefficient. These sources of inefficiency in educational investment appear in other work (for example, Banerjee (2004) on intergenerational incomplete contracting and Jensen (2010) on systematic under-estimation of the returns to schooling). Our contribution is showing that, in conjunction with the existence of child labor, these forces can cause increased early life investments to have perverse effects and reduce total welfare. Additionally, modeling these sources provides us with tests for whether reductions in schooling in response to increases in early life human capital in high child labor districts are inefficient. We view an educational investment decision as inefficient if it does not maximize total welfare $W_2(e;\alpha,h)$, which is the sum of the parent's and child's utilities (equivalent to setting $\gamma = 1$ in $U^p_2(e,h)$).

Proposition 4a. *If* $\gamma < 1$ *or* $\frac{\partial w^c_3(h,1)}{\partial h} < \frac{\partial \tilde{w}^c_3(h,1)}{\partial h}$ *(where* $w^c_3(h,0) = \tilde{w}^c_3(h,0)$*), then an increase in* y_1 *may inefficiently reduce education.*

Proof. See Appendix A.

Proposition 4a captures two intuitive circumstances under which the reductions in education due to the increase in y_1 (under Proposition 3a) may be inefficient. The first case $\gamma < 1$ captures the idea that an imperfectly altruistic parent who cannot perfectly contract with her child to capture the returns to her investments during childhood will underweight the increase in a child's utility in the future relative to the increase in consumption today. Thus, an increase in y_1 will reduce the parent's returns to educating the child, even though the increase in y_1 increases the returns to education for total household utility. The second case $\frac{\partial w^c_3(h,1)}{\partial h} < \frac{\partial \tilde{w}^c_3(h,1)}{\partial h}$ captures the idea that dynamic complementarities are hard to observe and even a perfectly altruistic parent may underestimate them. Thus, the parent will underestimate the increase in the returns to education for a child's adult wages due to an increase in y_1 relative to the increase in the child wages, again leading the reduction in education to be inefficient.

The final proposition focuses on the case where $\frac{\partial w_3^c(h,1)}{\partial h} = \frac{\partial \tilde{w}_3^c(h,1)}{\partial h}$ and where an increase in y_1 increases the range of children for whom educational investment is efficient.

Proposition 4b. *If* $\frac{\partial w_3^c(h,1)}{\partial h} = \frac{\partial \tilde{w}_3^c(h,1)}{\partial h}$ *and* $\frac{\partial W_2(1;\alpha^*,h)}{\partial h} > \frac{\partial W_2(0;\alpha^*,h)}{\partial h}$, *then there exists a value* $\bar{\gamma}$ *such that for* $\gamma > \bar{\gamma}$, $\frac{\partial \alpha^*}{\partial h} < 0$.

Proof. See Appendix A.

This proposition focuses on the case where an increase in h increases the net value of educating the marginal child more than it increases the value of the child working. In this case, it would be efficient for the marginal parent to respond to an increase in h by investing in education. If the parent is sufficiently altruistic or sufficiently able to contract with the child to capture the return to her investment ($\gamma > \bar{\gamma}$), the value of α needed for a child to be educated will fall and the marginal child will be educated. In other words, if γ is sufficiently high, the parent values the child's adult utility enough that she responds to the increase in the returns to education by efficiently increasing educational investment. This is easy to see in the extreme case where $\gamma = 1$ and a parent is either perfectly altruistic or perfectly able to contract with her child. In that case, education levels are never inefficient, and there will never be an inefficient reduction in educational investment in response to an increase in h. This proposition indicates that if there is incomplete contracting between parents and children, we should see heterogeneity in the response to early income shocks across parent-child pairs with different underlying incomplete contracting problems.

3 Background and Data

We now describe the datasets used in this paper. We next report basic facts about child labor in India and provide evidence that early life rainfall does indeed shock early life human capital. Moreover, this effect is similar in both high and low child labor districts. Finally, we show that, consistent with the mechanisms in the theoretical framework, greater human capital (in the form of both height and lagged test scores) is associated with higher child wages and makes children more likely to work in response to increased labor demand.

3.1 Data Sources and Measurement

This paper utilizes a variety of datasets from India, which we describe in Table 1. The summary statistics for our main outcomes of interest are reported in Appendix Table A1.

Table 1: Data Sources

Data Source	Type	Years	Variables Used
National Sample Survey (NSS) Schedule 1	Repeated Cross-Section	2004-2012	consumption
National Sample Survey (NSS) Schedule 10	Repeated Cross-Section	2004-2012	avg. child labor primary activity consumption
National Sample Survey (NSS) Migration Survey	Cross-Section	2007	out-migration
Annual Status of Education Report (ASER)	Repeated Cross-Section	2005-2009	dropout math and reading scores
India Human Development Survey (IHDS)	HH Panel	2005 and 2012	child wages anthropometrics math scores
University of Delaware Gridded Rainfall Data	District Panel	1970-2008	rain shocks
Unified District Information System (DISE)	Cross-Section	2005	education quality measures

Notes: This table reports the different data sources (and key variables from those data sources) used in this paper.

National Sample Survey, Schedule 10: Main Outcomes and District Controls. We use the National Sample Survey (NSS) to measure the main outcomes of interest: school attendance, child work, and adult consumption. The NSS is a repeated cross section of an average of 100,000 Indian households a year, conducted by the Indian government. We use rounds 60, 61, 62, 64, 66, and 68 (2004, 2004-5, 2005-6, 2007-8, 2009-10, 2011-12) in our analysis. We also restrict the sample to include only rural households, since the mechanism through which rainfall shocks affect rural incomes (increased crop yields) is well-established in the literature. In contrast, wages in urban areas are less likely to be affected by rainfall shocks. We assume that children are born in their district of residence, since cross-district migration in India is low (Topalova, 2007; Munshi and Rosenzweig, 2016), particularly for children (Kone et al., 2018).

The survey asks for the "primary activity" of each member in the household and includes categories for school attendance, wage labor, salaried work, domestic work, and so on. We define a child as "attending school" if her primary activity is listed as attends school and "working" if her primary activity is any form of wage/salary labor, work with or without pay at a "home enterprise" (usually a farm, but the data also includes other small family businesses), or domestic chores.[6]

We also use this primary activity variable to generate a district-level measure of child

[6]These categories comprise most of the primary activities of children under 18, though there are other categories that are omitted, such as too young/infirm for work (typically the very old and very young), and "other," which includes begging and prostitution.

labor, our key explanatory variable. To create that measure for a round t, we calculate the share of survey years a district is above the median for share of children reporting working (leaving out own-survey year t). To create our pool of instruments for child labor, we also exploit the fact that the NSS asks respondents for their "principal industry." For a survey in round t, our pool of potential instruments is the share of adults working in agriculture who report working in each disaggregated agricultural industry, leaving out own-survey year t.

The NSS Schedule 10 captures consumption by asking households to provide a consumption diary over the past week. We sum over expenditures in the different categories to arrive at a household consumption measure. We follow Deaton (1997) and calculate per capita consumption in households with both adults and children by weighting children (who have lower consumption needs) as one-third or one-half of an adult.

To control for potential differences between high and low child labor districts, we also use the NSS 10 to generate additional district- and household-level control variables. Our district-level controls consist of the average wage for adult wage-earners in the district, the share of adults who work for wages, the share of male and female adults who are literate, the share who graduated primary school, and the share who have graduated from secondary school. Our household-level control variables include a control for whether the male head of the household is literate, whether he earns a wage, whether he graduated primary and secondary school, household size, and total number of children in the household.

National Sample Survey, Schedule 1: Additional Measures of Consumption. We also supplement the NSS Schedule 10 with the NSS Schedule 1 (Household Consumer Expenditure) from the same rounds and years. This survey consists of a different cross-section of households and contains a detailed consumption module. We primarily use measures of total household consumption but additionally calculate the share of consumption on food (to back out welfare using the logic of Engel curves) and if the household uses a poor fuel source (coal, firewood, dung, or other). The survey additionally asks children how many lunches they had at school in the past month, which we use as an intensive margin measure of school attendance.

National Sample Survey, Migration Survey. In addition to the NSS Schedule 10 and 1, we also draw on a special migration survey conducted as part of the NSS in 2006-2007. Unlike other rounds of the NSS, this survey asked households about all members who had ever left the household, including information on those members' age. This allows us to create a dataset that includes both migrants and non-migrants and evaluate the effect of early life shocks on migration and to test whether migration is differential across high and low child labor districts.

India Human Development Survey. For additional data on child labor wages and their correlation with measures of child human capital, we turn to the India Human Development Survey (IHDS), a panel dataset that was implemented in 2005 and 2012. The IHDS is a nationally representative, multi-topic panel survey of 41,554 households in 1503 villages and 971 urban neighborhoods across India. This survey measures child height, weight, and cognitive abilities, and these data allow us to test the assumption that children with higher human capital earn higher wages in the market. We mainly use the IHDS 2012 for the analyses, but to look at the effects of lagged test scores, we augment it with the IHDS 2005.

Annual Status of Education Report: Test Scores and Dropout. We further supplement the IHDS and the NSS with data from ASER, the Annual Status of Education Report, which includes test scores in math and reading for a large cross-section of children from almost every rural district in India, including those who are out of school, from 2005–2014. Pratham, an Indian NGO, began collecting ASER test score data in all rural districts in India in 2005. Data is collected from approximately 500,000 children per year regardless of whether they are out of school every November. The math and writing sections of the ASER survey ask each child four progressively more difficult questions. The math section covers recognizing numbers 1-9, recognizing numbers 10-99, subtracting, and dividing. The writing section covers recognizing letters, recognizing words, reading a paragraph, and reading a story. In each section, we calculate the sum of correct questions.

Unified District Information System for Education: Educational Quality. To obtain measures of educational quality at the district-level, we draw on the 2005 round of the Unified District Information System for Education (DISE), which was developed by India's National University for Educational Planning and Administration. These data allow us to observe the percent of schools with single classrooms and teachers, the percent with student-teacher ratios greater than 60, the percent of primary schools with boys and girls toilets, the percent with blackboards, the percent without buildings, and the average number of textbooks per school at the district-level, all of which we use as controls for school quality.

Yearly Gridded Rainfall: Variation in Human Capital. The data on rainfall shocks come from the University of Delaware Gridded Rainfall Data for 1970-2008. Following the earlier literature (Shah and Steinberg, 2017; Jayachandran, 2006), we define a "rainfall shock" as equal to one if rain is in the top 20th percentile for the district, -1 if it is in the bottom 20th percentile, and 0 otherwise.[7] We form an aggregate early life rainfall shock measure denoted ELR by summing over the shocks when the child is in utero (age=-1), in her first year of life (year of birth), and in her second year of life. Thus, the aggregate shock variable ranges from -3 to +3. We match this data to children in all the other datasets using their birth year and district.

3.2 Background on Child Labor in India

Officially, child labor for children aged 14 and under has been banned in India since 1986. However, the ban covers only certain industries and has not been well-enforced.[8] Moreover, the main employers of child labor, agriculture and family-run businesses, were exempted from the ban. Beyond the various exemptions, the ban itself may have increased child labor through negative income effects (Bharadwaj et al., 2013).

Overall, child and adolescent labor are common in India, as is the case in many low-income countries. According to our data from the National Social Sample Schedule 10, 9% of children aged 5–17 reported working as their primary activity, while 26% of individuals 15–17 did so. UNICEF (2011) estimates that 28 million children in India aged 5-14 are engaged in work.[9] Figure 1 shows the variation in the percent of children 5-17 who report working as their primary activity across Indian districts using the NSS data from 2004. The most common industries for these children are agriculture and domestic duties, and children both work in the labor market for pay and part-time at home or on family farms. Within the Indian context, Shah and Steinberg (2017) show that child labor responds to productivity shocks, suggesting that wages are an important determinant of whether children work.

3.3 Early Life Rainfall and Human Capital

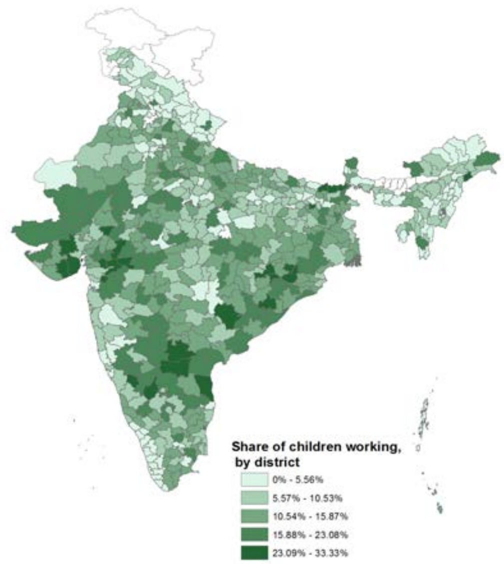
Before turning to our main empirical strategies, in this subsection and the next subsection, we provide evidence on two key preliminaries. In this subsection, we document the link between early life rainfall and early life human capital investment, which we will exploit for identification. In the next subsection, we examine whether children with greater human

[7] In India, though flooding can happen, more rain is almost always better for crop yields. See Jayachandran (2006) and Santangelo (2019) for more discussion of the direct relationship between rainfall and crop yields.

[8] Industries where child labor is banned include occupations involving the transport of passengers, catering establishments at railway stations, ports, foundries, handling of toxic or inflammable substances, handloom or power loom industry, and mines. Processes banned included hand-rolling cigarettes, making or manufacturing matches, explosives, shelves, and soap, construction, automobile repairs, and the production of garments (Bharadwaj et al., 2013).

[9] For domestic work to count under this definition, a child must be engaged in domestic work for over 28 hours per week.

Figure 1: Child Labor Prevalence by District, 2004



Notes: This figure shows a map of the districts of India, shaded by the prevalence of child labor, which is defined as the proportion of children aged 5-17 who report working in the market, in domestic work, or for a home enterprise as their primary activity. Source: NSS Schedule 10 (2004).

capital appear to have a greater opportunity cost of schooling, consistent with the key mechanisms in the theoretical framework.

To test the implications of the model, we use early life rainfall shocks as a proxy for shocks to early life human capital. The existing literature provides a strong argument that positive rainfall shocks increase yields, increasing parental wages, as shown by Jayachandran (2006) and Kaur (2019). Intuitively, and as indicated by Proposition 1, higher parental wages should lead to higher early life investment. Maccini and Yang (2009) and Shah and Steinberg (2017) provide evidence that this is the case. Parental investments could take many forms, including increased nutrition for the child or for pregnant or breastfeeding mothers, increased medical care during infancy, and more parental time spent fostering development.

Figure 2 provides evidence for a positive relationship between early life rainfall and early life human capital investment. The first panel of the figure shows the relationship between the aggregate early life rainfall shock and height for children and adolescents aged 5 to 17 in the IHDS 2012 separately for districts with above and below median child labor.[10] This

[10]For our IHDS analysis, a district is defined as having above median child labor if it had an above median share of children working in the NSS round 68, the most proximate NSS round to the IHDS 2012.

figure plots the relationship using residual variation after conditioning on age and district fixed effects. There is a clear positive relationship between early life rain and height in childhood, which is indicative of increased health investments for children who experienced higher early life rain. This effect is not differential for high and low child labor districts, indicating that Proposition 1 holds in both cases and that differences between the effects of early life rainfall on medium and long-term outcomes across districts are unlikely to be driven by differences in the effects of early life rainfall on human capital investment.

Similarly, panel B of Figure 2 shows the relationship between math and reading test scores in the ASER data, respectively, and early life rain. Here, we restrict the sample to children aged 5-7 in order to minimize the impact of endogenous, later-life schooling investments on test scores. Again, there is a positive relationship between test scores and early life rain in both high and low child labor districts. Appendix Table A2 reports analogous regressions of height and math and reading scores on the aggregate early life rainfall measure by high and low child labor districts, reporting both OLS and IV results. We will discuss both of these regression specifications in more detail in Section 4. Aggregate rainfall shocks are associated with improved early life human capital, and this effect is not differential across high and low child labor districts.

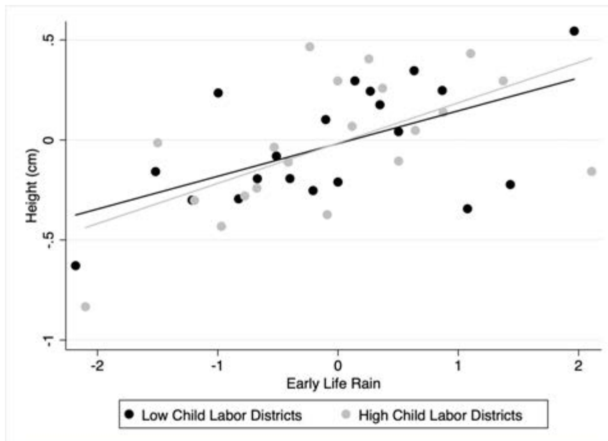
3.4 Human Capital and Child Wages

We evaluate whether increased human capital increases the value of children's labor, a key assumption of the model, in two ways. First, we report the results of hedonic regressions, which provide new evidence that childhood human capital is positively associated with child wages. Second, using the exogenous shock to early life human capital provided by early life rainfall, we show that when the demand for child labor is greater, children with higher values of early life rain are more likely to work.

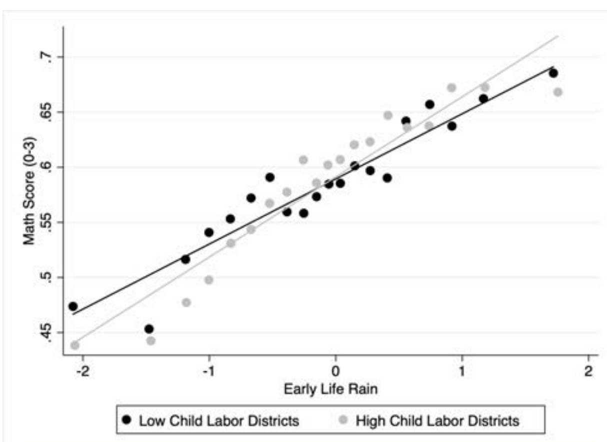
Table 2 reports results from hedonic regressions of child wages (conditional on working for pay) in the IHDS 2012 on height and lagged test scores. The sample controlling for lagged test scores only includes children aged 15-17 in 2012, since lagged test scores are only available for children 8-11 in the 2005 survey. In both cases, we find a strong positive association. Consistent with Case and Paxson (2008), height is positively correlated with wages, while a child who answers one more math question correctly receives a 6% higher wage. While we caution that these regressions cannot be interpreted as causal measures of the effect of human capital on the opportunity cost of schooling, both because they do not include children who do not work for pay and because human capital does not vary exogenously, these descriptive results are consistent with greater human capital increasing the opportunity cost of schooling. The same mechanisms are likely to be important for

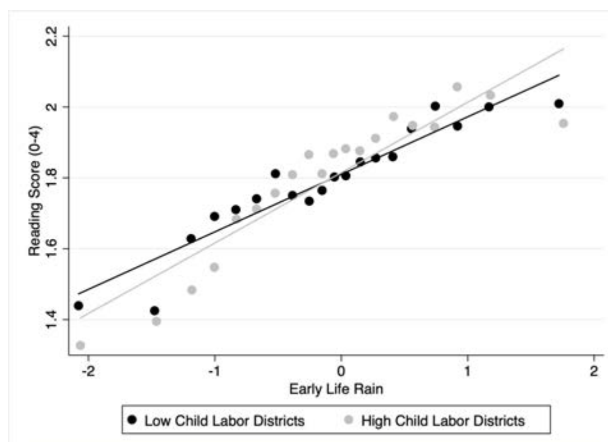
Figure 2: Effect of Early Life Shocks on Height and Early Test Scores

(a) Height Age 5-17 (IHDS)



(b) Test Scores Age 5-7 (ASER)





Notes: Each panel plots the relationship between residual outcomes (y-axis) and residual early life shocks, separately by high (above median) and low (below median) child labor districts. The outcome in Panel (a) is height in cm in the IHDS, for 5-17 year-olds. The outcomes in Panel (b) are math and reading scores from the ASER 2005-2014, for 5-7 year-olds. Math scores range from 0-3 and reading scores range from 0-4. Residuals are calculated after controlling for fixed effects for district and age. A district is defined as above median if it had above the median level of child labor in the NSS Round 68 (the most proximate NSS round to the IHDS 2012).
Source: IHDS (2012) and ASER (2005-2014).

Table 2: Hedonic Wage Regressions for Children

	(1)	(2)	(3)
	ln(wage)		
Height (cm)	0.0069***		0.0064**
	(0.0021)		(0.0030)
Lagged Math Score		0.057***	0.054**
		(0.022)	(0.024)
Mean Outcome	2.53	2.56	2.54
Number Districts	270	235	211
Number Observations	1,307	766	604

Notes: This table reports the descriptive relationship between size, cognitive skill, and ln(wage) conditional on working, controlling for age and gender fixed effects. Standard errors, clustered at the district level, are reported in parentheses. Wages and height are from the IHDS II (2012), while lagged test score data is from the IHDS I (2005).

children who work without wages (e.g. on family farms).[11] While the econometrician cannot observe opportunity costs in these cases since wages are not observed, it is still likely that the opportunity cost grows with human capital.

We next show that children with greater early life human capital (due to higher values of early life rain) are more likely to work in response to an exogenous shock to the demand for child labor. Our exogenous shock to demand comes from the combination of the timing of the NSS surveys and the harvest and sowing periods for crops grown in different districts. We ascertain when a crop has high labor demand by matching crop-state level calendars to the crops from the NSS. To measure shocks to the demand for labor in a district, we calculate the share of adults in the NSS who report working in a crop that at the time of the survey was in its harvesting or sowing period. We are then interested in the coefficient on the interaction between this measure and early life rain, which we expect to be positive when the outcome is an indicator variable for whether a child works. Our identification strategy controls for district-month-year fixed effects and exploits the variation in exposure to rainfall shocks and whether households in a given round are surveyed during a harvest or sowing period. Appendix Table A4 reports the results. Higher values of early life rain lead children to work more in response to higher demand for labor and attend school less, consistent with the model's assumption that greater early life human capital increases the opportunity cost of schooling.

[11] In Appendix Table A3, we also examine whether early life rain directly affects measures of child wages. While precision is an issue, as is selection into working, the point estimates are consistent with early life rain positively affecting child wages, particularly in high child labor districts.

4 Empirical Strategy

The theoretical framework predicts that the effects of early human capital investments on later schooling investment will depend upon the opportunity cost of children's time. In subsection 3.3, we established that rainfall shocks experienced in utero and in the first two years of life provide exogenous variation in the stock of early human capital. In this section, we outline the empirical strategy for the remainder of the paper. We then estimate the different effects of early life rainfall shocks on subsequent schooling investment, as well as later-life and intergenerational outcomes, by the prevalence of child labor.

OLS Strategy. In the primary OLS specifications, we estimate the following regression

$$y_{idmtag} = \beta_1 ELR_{dta} + \beta_2 ELR_{dta} \times CL_{dt} + \tau_{dmt} + \tau_a + \tau_g + \epsilon_{idmtag} \tag{1}$$

where y_{idmtag} is an outcome measure (i.e. working, being in school, test scores, consumption) for individual i in district d in month m and year t at age a of gender g, ELR_{dta} is individual i's early life aggregate rainfall shock, CL_{dt} is a measure of child labor in district d in year t, τ_{dmt} is a district-month-year fixed effect, τ_a is an age fixed effect, and τ_g is a gender fixed effect. As mentioned above, CL_{dt} is a variable for the share of rounds (leaving out round t) that an above median share of the district's children work. β_2 is the differential effect of early life rainfall in districts with above median share of child labor. We also report β_1 estimated for all districts without a child labor interaction term to capture the average effect of the aggregate early rainfall shock, and we report $\beta_1 + \beta_2$, the total effect of a positive early rainfall shock in a district that always has above median child labor.

District-month-year and age fixed effects ensure that the estimates are identified from within-district and within-cohort variation. Thus, fixed differences across districts (such as those that experience drought more often than others) will not drive the results. The identifying assumption for β_1 is that, conditional on country-level changes in rainfall patterns over time, deviations from district-level average rainfall are not associated with other time-varying district-level characteristics that may affect children's outcomes. This is the standard identifying assumption from Shah and Steinberg (2017) and Maccini and Yang (2009).

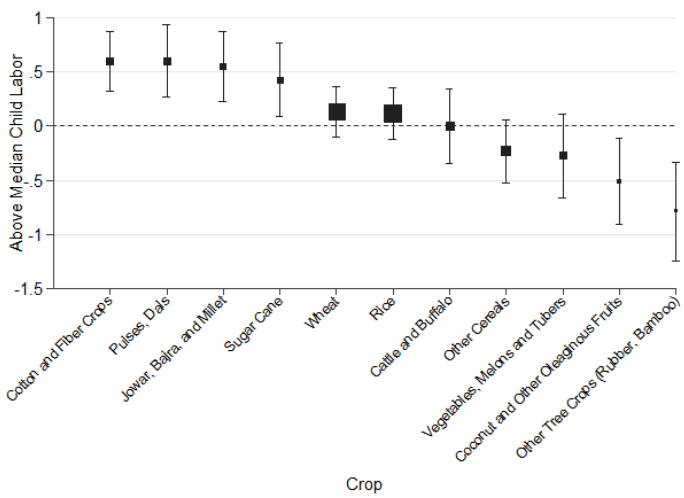
Interpreting β_2 as capturing the interaction between child labor and early life rainfall shocks requires the additional assumption that there is no important district-level characteristic associated with child labor that also leads rainfall to have different effects in high and low child labor districts. Because this assumption is strong, we introduce a complementary instrumental variables identification strategy below. Additionally, Section 7 will be devoted to a series of tests that increase our confidence that β_2 is driven by child labor itself rather than other district characteristics.

One additional concern for the estimation of both β_1 and β_2 is that there is a direct long-run effect of early life rainfall shocks on individuals' outcomes (e.g. if families use the windfall to buy investment assets), which may not be fully controlled for by district/time fixed effects. In an appendix specification, to account for this, we also estimate models with household fixed effects. These models compare children in the same household who were born in different years and control for any household-level unobservables, such as savings or migration decisions.

IV Strategy. The OLS strategy will be biased if districts with high child labor are different in other ways (e.g. poorer) and if these unobserved factors lead early life rainfall to have different effects. To address this concern, we propose an IV strategy that exploits technological variation in children's comparative advantage in working. Children have a relative advantage in some crops given the nature of the tasks associated with planting, weeding, and harvesting. For example, cotton is known as a child labor crop because it is low to the ground and very lightweight (Levy, 1985). Crop-mix across regions in India is mainly driven by agroclimatic conditions, such as average temperatures and rainfall, as well as soil requirements (Krishna, 2014). Thus, agroclimatic conditions create variation in the prevalence of child labor. To exploit this variation, we use information on the share of adult agricultural labor employed in each disaggregated agricultural industry to predict CL_{dt}. To identify instruments in a principled way and maximize statistical power, we follow the IV-lasso methodology proposed by Belloni et al. (2012) and use lasso to choose the set of instruments that best predict CL_{dt} from a pool of measures of the share of adult agricultural employment in a district in each agricultural industry. The second stage regression is then the same as equation (1) above except we use predicted child labor.

Appendix Table A5 shows the set of instruments selected and their first stage coefficients when the second stage outcome variable is whether a child works (one of our main outcome variables). The estimates (listed in descending order of magnitude) align with our intuitions about which activities should be highly associated with child labor. For example, the largest coefficient is on the adult share of agricultural labor in "non-perennial crops" (e.g. flowers), which are typically light and close to the ground. Similarly, the crop with the third highest coefficient is cotton, which as described above, is a well-known child labor crop. To further illustrate the variation driving the first stage in the IV strategy, Figure 3 graphs the coefficients in a regression of the share of periods with above median agriculture on the crops that make up a non-negligible share of adult agricultural employment (>1%), including those that are not chosen by the IV lasso.[12] The size of the markers denotes the share of adult agricultural labor in a given crop. The results in Figure 3 echo those in Appendix Table A5. Cotton is the strongest predictor of child labor among the larger crops. Extremely prevalent crops like wheat and rice have relatively little predictive power, while crops that require a great deal of brawn, such as tree crops (coconuts, rubber, bamboo) are negatively associated with child labor.

Figure 3: Relationship Between Crop Shares and Child Labor



Notes: This figure plots the relationship between the share of adult agricultural employment in a "large crop" (> 1 percent share of agricultural employment) and the main measure of child labor used in the paper (share of periods a district has above median child labor). All the coefficients are from a single regression. Point estimates in the figure are weighted by the share of adult employment (in agriculture) for the crop.

Given our potentially large number of instruments, we minimize any potential bias from many weak instruments in three ways. First, we estimate our coefficients using LIML, which is less vulnerable to bias from weak instruments (Stock et al., 2005).[13] Second, following Lee et al. (2020), who argue for using weak instrument robust inference rather than using F-statistics to diagnose weak-iv problems, we report p-values from Anderson-Rubin weak instrument robust confidence intervals for our key coefficient of interest, β_2 (Anderson et al., 1949).[14] Third, we also report a reduced-form analog of the IV to check that the same patterns appear in reduced-form regressions as in the IV regressions.

[12]The measure of non-perennial crops is no longer included since it accounts for a very small share of adult labor.

[13]In practice, our results with LIML are almost identical to those with 2SLS, indicating that bias from weak instruments is small.

[14]However, even if we calculate a F-statistic for our first stage by including the selected instruments in a standard instrumental variables regression following Gilchrist and Sands (2016), the F-statistics for these instruments range from 20-21 (child-level NSS and ASER data) to 40 (NSS adult consumption sample).

5 Schooling and Child Labor Results

In this section, we test the key propositions of the model. Based on Proposition 2, we expect that if there are dynamic complementarities, in districts with low child labor, early life shocks will increase educational investment. In districts with high child labor, this effect will be attenuated (Proposition 3b) and may even be reversed (Proposition 3a), so that early life shocks decrease human capital investment.

Schooling and Work. Panel A of Table 3 reports the results from estimating equation (1) where the outcome is either an indicator variable for whether a child lists her primary activity as working (columns 1-3) or attending school (columns 4-6) or an indicator variable for dropout (columns 7-9). Columns 1, 4, and 7 report the average effects of early life rainfall, while columns 2, 5, and 8 report the differential effects using OLS, and columns 3, 6, and 9 report the differential effects using the IV specification. The "total effect" row at the bottom of the table reports the aggregate effects of rainfall shocks in districts whose CL_{dt} measure is equal to 1. Consistent with Proposition 2, an increase in ELR_{dta} reduces the likelihood of working in low child labor districts. In contrast, in high child labor districts, the net effect on working is positive. The results with the OLS and IV regressions are nearly identical, suggesting that the results in column 2 are not driven by omitted variable bias. ELR_{dta} has positive effects on education on average and in low child labor districts, but these effects are reversed in the high child labor districts.[15] We also re-estimate Panel A of Table 3 using an alternative measure of child labor, the leave-out average of child labor in Appendix Table A6. The results are robust to this alternative measure of child labor.

The estimates in Panel A of Table 3 imply that one additional positive early life rainfall shock in a low child labor district decreases the likelihood a child aged 5–17 works by 1.3 percentage points (14%), increases the probability she attends school by 1.5 percentage points (2%) and reduces dropout by 0.5 percentage points (13%). Adding the enrollment effects over a child's life implies that total years of schooling increases by 0.17 years. In contrast, a positive rainfall shock increases work by 0.4 percentage points (4%), decreases schooling by 0.3 percentage points (0.4%) and increases dropout by 0.4 percentage points (11%) in high child labor districts.[16]

[15] In Appendix Figure A1, we use a similar specification to estimate the effect of ELR_{dta} by quintile of the share of children working and plot the total effect of aggregate shocks for each quintile. This figure provides evidence that there is a monotonic relationship between the prevalence of child labor and ELR_{dta}.

[16] Appendix Table A7 further confirms that the IV results are not due to bias from many weak instruments. The table shows that the adult share of crops that are predictive of child labor also has significant effects with the same sign as the IV regressions.

Table 3: Effect of Early Life Shocks on Schooling

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Primary Activity									
	OLS	OLS	lasso IV	OLS	OLS	lasso IV	OLS	OLS	lasso IV
	Primary Activity Works (NSS)			Attends School (NSS)			Dropped Out (ASER)		
Early Life Rain	-0.0045***	-0.013***	-0.012***	0.0060***	0.015***	0.015***	-0.000030	-0.0046***	-0.0051***
	(0.00096)	(0.0017)	(0.0035)	(0.0011)	(0.0021)	(0.0041)	(0.00038)	(0.00069)	(0.0013)
Early Life Rain									
... × (Above Median) Child Labor		0.016***	0.015**		-0.018***	-0.019**		0.0085***	0.0094***
		(0.0030)	(0.0067)		(0.0033)	(0.0074)		(0.0012)	(0.0022)
Mean Outcome	.095	.095	.095	.817	.817	.817	.035	.035	.035
Anderson-Rubin p-value			0.009			0.012			<0.001
Total Effect		0.0038**	0.0032		-0.0033*	-0.0035		0.0039***	0.0043***
SE of Total Effect		(0.0018)	(0.0034)		(0.0018)	(0.0037)		(0.0007)	(0.0011)
Number Districts	568	568	568	568	568	568	559	559	559
Number Observations	486,190	486,190	486,190	486,190	486,190	486,190	7,608,681	7,608,681	7,608,681
B. Meals									
	OLS	OLS	lasso IV	OLS	OLS	lasso IV	OLS	OLS	lasso IV
	Any Meals at School			≥10 Meals at School			≥20 Meals at School		
Early Life Rain	0.0086***	0.018***	0.034***	0.0074***	0.016***	0.030***	0.0025**	0.0082***	0.019***
	(0.0018)	(0.0033)	(0.0049)	(0.0017)	(0.0032)	(0.0046)	(0.0012)	(0.0022)	(0.0037)
Early Life Rain									
... × (Above Median) Child Labor		-0.019***	-0.051***		-0.018***	-0.045***		-0.012***	-0.033***
		(0.0049)	(0.0093)		(0.0048)	(0.0087)		(0.0035)	(0.0076)
Mean Outcome	.244	.244	.244	.228	.228	.228	.153	.153	.153
Anderson-Rubin p-value			<0.001			0.001			0.012
Total Effect		-0.0012	-0.017***		-0.0017	-0.015***		-0.0033	-0.014***
SE of Total Effect		(0.0026)	(0.005)		(0.0025)	(0.005)		(0.0021)	(0.004)
Number Districts	568	568	568	568	568	568	568	568	568
Number Observations	540,074	540,074	540,074	540,074	540,074	540,074	540,074	540,074	540,074

Notes: This table reports the effect on schooling and working of early life shocks (β_1 and β_2 from equation 1), with fixed effects for age, gender, and month/year/district using the NSS data (columns 1-6 in Panel A and all columns in Panel B). The ASER regressions (columns 7-9) contain only year/district fixed effects, as the survey is entirely collected in November. District child labor classifications use the leave-out share of rounds a district has above median child labor, as described in the text. In columns 3, 6 and 9, child labor prevalence is instrumented using a lasso-selected set of crops, as described in section 4. Standard errors are clustered by district. The analysis includes all children between the ages of 5 and 17 in the NSS and 5-16 in ASER (ASER does not collect data from 17 year-olds).
Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012) and ASER (2005-2014).

Adding up the enrollment effects implies a reduction in years of education of 0.04. To put these effect sizes into perspective, Duflo (2001) finds that receiving one more school per 1,000 children in a district in Indonesia increased male education by 0.12 years, and Muralidharan and Prakash (2017) find that a bicycle program increased female secondary school enrollment by 5 percentage points in India. Thus, the reduction in education caused by an additional positive early life rainfall shock in a high child labor district is on the order of one-third the effect of receiving another school in Indonesia. The increase in inequality in enrollment outcomes between high and low child labor districts due to this shock (1.8 percentage points) is more than one-third the effect of the bicycle program. Altogether, these effects are economically meaningful but not enormous. Indeed, we would not expect a single positive rainfall shock in early childhood to have very dramatic effects on a child's outcomes. However, by studying these shocks, we hope to not only identify an economically important shifter of human capital investment but also improve our understanding of households' human capital investment decision-making.

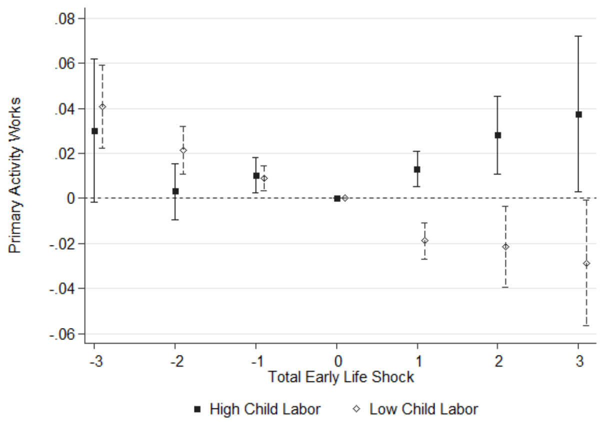
Panel B of Table 3 uses the number of meals a child ate in school in the last 30 days as an intensive margin measure of education. The results with this measure are similar. In low child labor districts, rainfall shocks in early childhood increase the number of meals eaten at school, while this effect becomes negative in high child labor districts. Interestingly, the effect sizes are similar whether the outcome is any meals at school, greater than 10 meals at school, or greater than 20 meals, suggesting that most of the effect on education is on the extensive rather than the intensive margin.

Figure 4 reports the non-parametrically estimated effects for each value of ELR_{dta} (from -3 to +3) for high and low child labor districts separately for the three main outcomes of interest. Intriguingly, the estimates suggest that the divergence in the effects of ELR_{dta} in high and low child labor districts is mainly driven by positive rather than negative shocks. More positive shocks seem to move the outcomes of healthier/larger children who are on the margin of attending school in low child labor districts and on the margin of working in high child labor districts. In contrast, more negative shocks have little differential effect on children's outcomes across districts, suggesting that making an already low human capital child smaller/less healthy reduces the incentive to invest in that child's education in both district types.

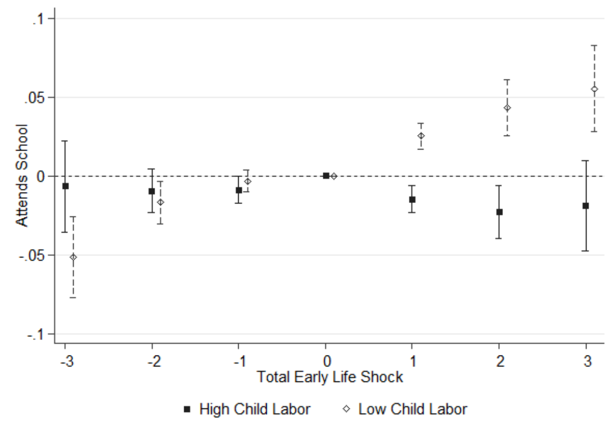
Complementing the educational results, in Table 4, we examine the effects of children's early life rainfall shocks on current household consumption. Per capita consumption is calculated by dividing the household's total monthly consumption by the total number of adults and children. Following Deaton (1997), children are treated as either one-third (columns 1-3) or one-half an adult (columns 4-6). Households with children who received more posi-

Figure 4: Outcomes by Value of the Shock

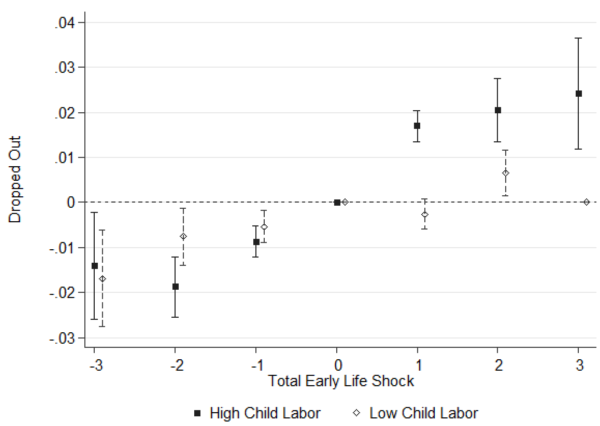
(a) Works



(b) Attends School



(c) Dropped Out



Notes: This figure plots the total effects of the possible values of the early life shock (ranging from three negative shocks to three positive shocks) for both below and above median child labor districts. All point estimates come from a joint regression.

tive early life rainfall shocks have lower consumption per capita in low child labor districts (consistent with children attending school) and relatively higher consumption per capita in high child labor districts (consistent with children being more likely to work).

In Appendix Table A8, we also examine how early life shocks affect which sector children work in using the OLS strategy. Panel A suggests that the net increase in work in high child labor districts is concentrated in agriculture and retail. These are sectors that are known for high child labor. In contrast, children do not appear to be leaving school to pursue jobs in relatively high wage sectors like manufacturing. Panel B further confirms that early life rainfall increases schooling in low child labor districts (and reduces it in high child labor districts) at the expense of both paid and unpaid work. Early life rainfall has a positive and marginally significant total effect on household work in high child labor districts (column 4).

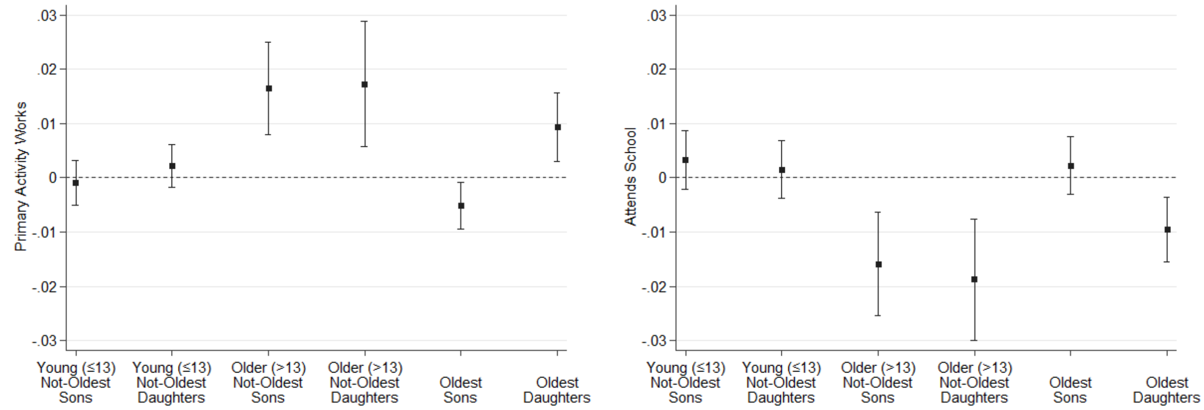
Table 4: Effect of Early Life Shocks on Family Consumption During Childhood

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	lasso IV	OLS	OLS	lasso IV
	ln(Consumption Per Adult + 1/3 * kids)			ln(Consumption Per Adult + 1/2 * kids)		
Total HH Early Life Rain	-0.0027***	-0.0048***	-0.0098***	-0.0025***	-0.0046***	-0.0094***
	(0.00064)	(0.0011)	(0.0022)	(0.00064)	(0.0011)	(0.0022)
Total HH Early Life Rain						
... × (Above Median) Child Labor		0.0046**	0.015***		0.0045**	0.015***
		(0.0018)	(0.0042)		(0.0018)	(0.0042)
Mean Outcome	7.04	7.04	7.04	6.92	6.92	6.92
Anderson-Rubin Wald p-value			0.040			0.078
Total Effect		-0.00024	0.0053**		-0.00011	0.0052**
SE of Total Effect		(0.0011)	(0.0022)		(0.0011)	(0.0022)
Number Districts	571	571	571	571	571	571
Number Observations	200,105	200,105	200,105	200,105	200,105	200,105

Notes: This table reports the effect of children's rainfall shocks on their households' consumption, allowing the effects to vary by the prevalence of child labor. Each household is in the data once, and its total early life rain is calculated as the sum of early life shocks of children in the household between the ages of 5 and 17. We include fixed effects for the gender and age composition of the household. The total effect rows report the total effect of a 1 unit increase in the aggregate rainfall shock in a district for whom the share rounds above median is equal to 1. District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. In columns 3 and 6, child labor prevalence is instrumented using a lasso-selected set of crops, as described in Section 4. Standard errors are clustered at the district level.
Source: NSS Schedule 10 (2004-2012).

Heterogeneity. We next examine several potentially important sources of heterogeneity. *Age at Time of Shock.* We first explore whether in-utero, year 0, and year 1 shocks have different effects. Appendix Table A9, which reports the heterogeneous effects by the timing of the shocks, indicates that there is little evidence that the timing of the three shocks matters. While this may be surprising given the importance of the in-utero period, it is consistent with fact that infancy is also a critical period for both stunting (Prendergast and Humphrey, 2014) and brain development (Prado and Dewey, 2014).
Age of Observation. Guided by the fact that older children are also more likely to be on the work margin, we estimate the effects of ELR_{dta} separately for children older and younger than age 13 in Appendix Table A10. In addition, Panels B and C of Appendix Figure A1 plot the total effects of the aggregate rainfall shocks by child labor quintile for older and younger children. For older children, the effect of the shock on schooling in high child labor districts is negative, large, and significant. In high child labor districts, for older children, a positive shock increases work by 1.2 percentage points (13%) and decreases school by 1.4

Figure 5: Total Effect of Early Life Shocks by Gender and Birth Order



Notes: This figure plots the total effect of early life shocks in above-median child labor districts. The coefficients are all estimated from one regression, which has the standard fixed effects in the tables (gender, age, and district/month/year), as well as additional indicators for age group by gender (each child falls into exactly one of the six categories). Standard errors are clustered at the district level.
Source: NSS Schedule 10 (2004-2012).

percentage points (2%), roughly one-third the magnitude of the effects of the bicycle program studied by Muralidharan and Prakash (2017). The results are consistent with the idea that the effect of early life human capital on the opportunity cost of schooling is most important when children are old enough to engage productively in child labor. For younger children, the net effect of a shock on schooling in high child labor districts is generally close to zero or even positive, in line with the effects in low child labor districts.

Gender. Girls tend to receive less educational investment than boys in India (Lancaster et al., 2008; Himaz, 2009; Azam and Kingdon, 2013). Appendix Table A11 reports the effects of the rainfall shocks by gender. The total effects of early life rainfall shocks in high child labor districts are indeed more pronounced for girls, who respond to early life rainfall shocks by significantly increasing their likelihood of working and significantly decreasing their likelihood of attending school. This result is consistent with the findings in Figure 5, which shows that both oldest daughters and non-oldest daughters over 13 are strongly affected by the rainfall shocks, while, among sons, only younger sons above 13 exhibit the same pattern.

Caste. Lower caste individuals are also likely to have poorer educational outcomes and to be more vulnerable to shocks in India (Deshpande, 2000; Drèze and Kingdon, 2001). In Appendix Table A12, we allow for heterogeneity by whether the individual belongs to a scheduled caste or tribe. We again find that early life rainfall shocks have more pronounced negative effects on lower caste individuals. Along with the gender results, this points to the possibility that programs that increase early life human capital can be inequality increasing—improving educational outcomes among the better-off but reducing them in the long-run for the most vulnerable.

Oldest Sons. Finally, motivated by Propositions 4a and 4b, we examine heterogeneity by eldest sons. From these propositions, we know that if parents are imperfectly altruistic, increased early life investment may inefficiently reduce educational investment. This is because parents will value the earnings from a child working, which they can expropriate today, more than the gains to a child's future income, from which they may not benefit. If a child could contract to share his future earnings with parents, parents would make efficient educational decisions.

As Proposition 4b shows, if parents are imperfectly altruistic, we expect the effects of early life shocks to be more positive in high child labor places for children for whom intergenerational incomplete contracting problems are likely to be small (e.g. when the altruism/contracting parameter γ is sufficiently high). Cultural traditions where specific children provide parents with old age support are one informal mechanism to solve this incomplete contracting problem (Bau, forthcoming) and can generate variation in incomplete contracting problems across children in the same household. In India, oldest sons are expected to care for parents in their old age (Dyson and Moore, 1983; Gupta, 1987). Jayachandran and Pande (2017) provide evidence that this is associated with son preference and greater investment in oldest sons. Since incomplete contracting problems with oldest sons are likely to be smaller, we can test whether the impact of early rainfall shocks depends on the strength of incomplete contracting problems by examining how these shocks interact with birth order among boys.[17]

Table 5 allows early life rainfall shocks to have different effects on oldest sons versus other children in high and low child labor districts.[18] In both OLS and IV specifications, oldest sons behave more like children in low child labor districts than children in high child labor districts. For oldest sons in high child labor districts, the net effect of an increase in ELR_{dt} is *negative* for working and *positive* for schooling. In contrast, for their siblings, both signs are reversed. Further corroborating these results, Figure 5 uses the OLS specification to report the total effects of early life rainfall shocks on children in above median districts by gender, age, and birth order. As in Table 5, the estimates reveal that, while early life rainfall shocks

[17]In terms of the model, γ will be higher for oldest sons than other children, and as the model shows, when γ is sufficiently high, parents will not inefficiently reduce educational investment in response to greater early life human capital investment. Thus, if we observe that early life rainfall shocks have different effects on oldest sons versus other children in high child labor districts, this provides evidence that parents are inefficiently reducing educational investment in response to rainfall shocks for the other children.

[18]We do not directly observe oldest sons in the NSS, so we impute "oldest son" for the oldest male who reports being the son of the household head. We verify this method using the NFHS-4 data, which identifies birth order. In the NFHS sample, the probability that the person who we would impute as oldest son is the actual oldest living son is .94.

Table 5: Effect of Early Life Shocks on Schooling for Oldest Sons

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	lasso IV	OLS	OLS	lasso IV
	Primary Activity Works (NSS)			Attends School (NSS)		
Early Life Rain	-0.0029***	-0.012***	-0.012***	0.0048***	0.014***	0.016***
	(0.0010)	(0.0018)	(0.0036)	(0.0012)	(0.0022)	(0.0040)
Early Life Rain						
... × Oldest son	-0.0072***	-0.0033**	-0.00087	0.0051***	0.0032*	-0.0033
	(0.0011)	(0.0014)	(0.0033)	(0.0013)	(0.0017)	(0.0035)
... × (Above Median) Child Labor		0.018***	0.018**		-0.019***	-0.023***
		(0.0031)	(0.0069)		(0.0034)	(0.0074)
... × (Above Median) Child Labor × Oldest son		-0.0079***	-0.013*		0.0039	0.017**
		(0.0029)	(0.0068)		(0.0035)	(0.0072)
Mean Outcome	.095	.095	.095	.817	.817	.817
Mean Outcome, Oldest Sons	.077	.077	.077	.848	.848	.848
Total Effect, Not-Oldest Sons		0.0062***	0.0062*		-0.0048**	-0.0066*
SE of Total Effect, Not-Oldest Sons		(0.002)	(0.0037)		(0.0019)	(0.0037)
Total Effect, Oldest Sons		-0.005**	-0.0078*		0.0023	0.0075
SE of Total Effect, Oldest Sons		(0.0022)	(0.0041)		(0.0027)	(0.0052)
Number Districts	568	568	568	568	568	568
Number Observations	486,190	486,190	486,190	486,190	486,190	486,190

Notes: This table reports the effect of early life shocks on schooling and working (β_1 and β_2 from equation 1), controlling for fixed effects for age, gender, and month/year/district. District child labor classifications use the leave-out share of rounds a district has above median child labor, as described in the text. Oldest sons are assumed to be the oldest male in the household who reports being a child (or grandchild) of the household head. In columns 3 and 6, child labor prevalence is instrumented using a lasso-selected set of crops, as described in section 4. Standard errors are clustered by district. The analysis includes all children between the ages of 5 and 17.
Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012).

reduce schooling and increase work for non-oldest sons and all daughters older than 13 in high child labor districts, this effect disappears or is reversed for oldest sons. Furthermore, Figure 5 also shows that this result is not simply driven by some other characteristic of being the oldest child. Oldest girls do not experience the same benefits as oldest boys. Altogether, these results provide preliminary evidence that reductions in educational investment in high child labor districts are inefficient. We further explore whether this is the case in Section 8.

6 Medium, Long-Term, and Intergenerational Effects

While ELR_{dta} may decrease educational attainment in high child labor districts, it is not clear that this reduction in education has negative long-term effects. In the first subsection, we use the ASER data, which tests children aged 5–16, including dropouts, to estimate the net effects of ELR_{dta} in above median child labor districts on cognitive achievement by the end of childhood. In subsection 6.2, before examining longer-term measures from the NSS, we estimate the effects of ELR_{dta} on migration to evaluate the scope for differential bias from assigning adults rainfall shocks from their current districts. After finding that the scope for bias is small, we measure the effect of rainfall on longer-term outcomes in the NSS, such as per capita adult consumption, focusing on consumption rather than wages since, in rural India, few adults work for a wage. We also consider the effects of early life shocks on other measures of household socioeconomic status, such as use of a fuel source associated with poverty and percent of consumption spent on food. In the last subsection, we estimate the intergenerational effects of early life rainfall shocks in the NSS.

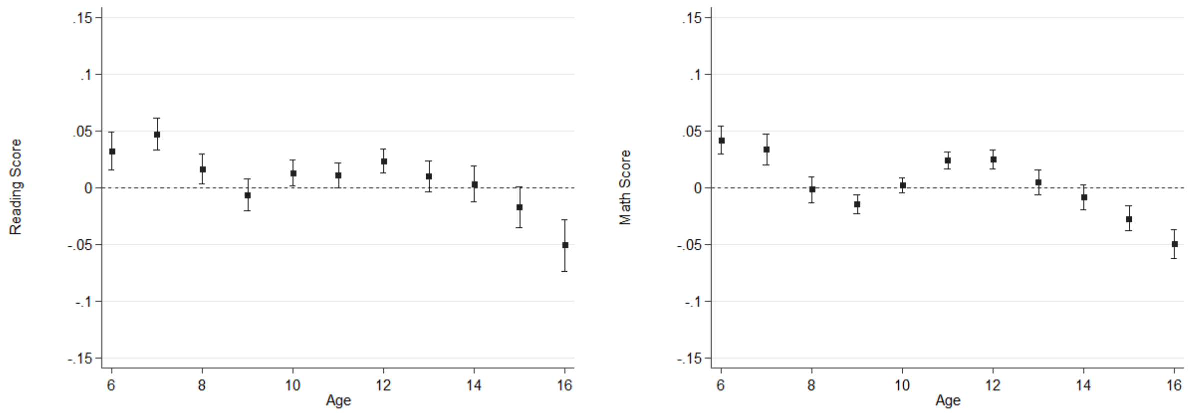
6.1 Medium-Term Effects on Cognitive Outcomes

To obtain age-specific estimates of the net effect of ELR_{dta} on cognitive achievement and dropout in the ASER data, we estimate analogous regressions to those in Table 3 except with ASER test scores as the outcome. We fully interact the key variables of interest, CL_{dt}, ELR_{dta}, and their interaction, with indicator variables for age groups. For a given age a, we calculate the net effect of ELR_{dta} in high child labor districts by adding together the coefficients on $ELR_{dta} \times I_i^{age=a}$ and $CL_{dt} \times ELR_{dta} \times I_i^{age=a}$, where $I_i^{age=a}$ is an indicator variable equal to 1 if a respondent is a years old. Figure 6 reports these net effects using the OLS specification (panel (a)) and the IV specification (panel (b)) (regression table available upon request).

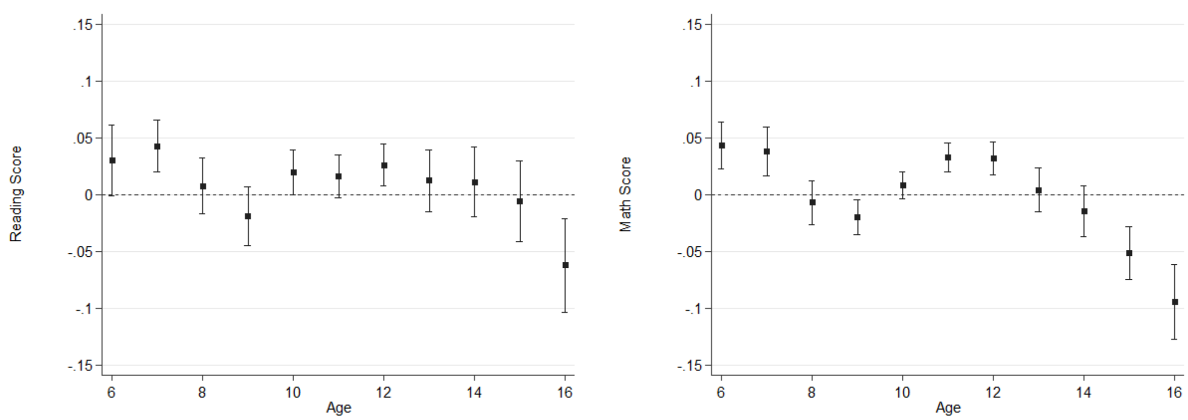
Earlier in childhood, ELR_{dta} increases cognitive skills in high child labor districts. But by the end of childhood, a one-unit increase of ELR_{dta} (equivalent to one extra good rainfall year during the critical period) leads to a reduction in both reading and math scores of 0.05

Figure 6: Total Effect of Early Life Rainfall in High Child Labor Districts By Age

(a) OLS Specification



(b) IV Specification



Notes: This figure plots the total effect of early life shocks in above-median child labor districts by each age. The coefficients are from one regression, and the regressions have the standard fixed effects in the tables (gender, age, and district/month/year). Panel (a) reports results from the OLS specification, while Panel (b) reports results from the lasso-IV specification. Standard errors are clustered at the district level.
Source: ASER (2005-2014).

(equivalent to 0.03 test score sd in reading and 0.07 sd in math). Reading is on a scale of 0-4, and math is on a scale of 0-3. For comparison, with the caveat that the small number of items on the ASER exams make comparisons in standard deviations difficult, Singh (2016) shows that a year of learning in India is associated with a 0.4 standard deviation increase in test scores. Thus, by the end of childhood, an increase in ELR_{dta} leads to a reduction in cognitive skills in districts where child labor is more prevalent on the order of the effect of 0.08 fewer years of schooling in reading and 0.18 fewer years in math. Interestingly, these are very close to the total effects on years of schooling implied by the enrollment effects documented above.

6.2 Effects on Migration

Migration is both an interesting outcome in its own right and important for understanding the scope of bias in our long-term estimates, since in the NSS Schedules 1 and 10, we can only observe an individual's contemporaneous district, rather than his birth district. If rainfall shocks lead to migration and this has differential effects in high and low child labor districts, we may differentially misclassify individuals' rainfall shocks. We estimate the differential effects of ELR_{dta} on migration, exploiting a 2007 round of the NSS that asked households detailed questions about out-migrants.

Appendix Table A13 reports the average and differential effects of ELR_{dta} on an indicator variable coded as 1 for an individual that the household reports has out-migrated. Since men and women typically migrate for different reasons in India (men for employment and women for marriage), we separately report the estimates for men (panel A) and women (panel B). On average, ELR_{dta} has a very small negative and marginally significant effect on migration for men and no effect on migration for women. We find no evidence that ELR_{dta} has differential effects on migration in high child labor districts. As the results suggest that the scope for bias is small, we now turn to our longer-run outcomes.

6.3 Long-Term Effects on Consumption

To measure the differential effects of ELR_{dta} on consumption, we re-estimate the specifications in Table 3 on a sample of adult male household heads (aged 25-54) with a measure of adult per capita consumption as the outcome variable.[19] Furthermore, since consumption data is available from both the NSS Schedule 1 and 10 surveys, we pool these surveys to maximize statistical power.

[19]The NSS directly asks if someone is the household head (or married to the household head). For households with no reported head, we impute the oldest male as the head.

Panel A of Table 6 reports the results. For both calculations, ELR_{dta} increases long-run per capita adult consumption in low child labor places, but this effect is completely eliminated in high child labor districts, and the net effect is negative. In low child labor districts, one additional positive rainfall shock increases per capita consumption by 0.3 (OLS) to 0.7% (IV). In high child labor districts, it results in a reduction of 0.6% (OLS and IV).

In Panel B, we turn to alternative measures of adult household socioeconomic status. Columns 1–3 use data from the NSS Schedule 1 to examine whether the household uses a fuel source associated with poverty (non-LPG, gobar gas, charcoal, kerosene, and electricity sources, which consist of coke, coal, firewood and chips, dung cake, and others). Columns 4–6 use the share of a household's budget that is spent on food as the outcome variable (also from the NSS Schedule 1). In both cases, ELR_{dta} is again associated with better outcomes in low child labor districts and worse outcomes in the high child labor districts. We conclude that at best early life rainfall did not improve adult outcomes and at worse, it may have reduced them in high child labor districts.

6.4 Intergenerational Effects

Finally, the NSS allows us to examine whether early life rainfall shocks have intergenerational effects and whether these effects differ by the prevalence of child labor in a district. In Table 7, we estimate the effect of the household head's rainfall shock on a child (aged 5-17), allowing for that shock's effects to depend on the prevalence of child labor. We control directly for the early life shock (by district) for the child. We find evidence that the negative effects of the early life shock in high child labor areas persist across generations. In high child labor districts, children of a household head who received an additional year of good rainfall in early life are 0.4-0.6 percentage points more likely to work (5-7%) and 0.4 percentage points (0.5%) less likely to attend school. Adding up the effects over the years, a one unit increase in the household head's ELR_{dta} leads to a 0.024 reduction in the child's years of education. While there is limited evidence on the intergenerational effects of health and education interventions in low-income countries, one point of comparison is an estimate of the intergenerational effect of the Indonesian INPRES school construction program. Akresh et al. (2018) estimate that having a father who was young enough to be exposed to the program and was born in a district that received 1 more school per 1,000 children increases years of schooling by 0.1. Our estimate of the reduction in a child's education due to her father's positive rainfall shock (in a high child labor district) is about one-fifth that magnitude.

Table 6: Effect of Early Life Shocks on Adult Household Consumption

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Household Consumption					
	OLS	OLS	lasso IV	OLS	OLS	lasso IV
	ln(Consumption Per Adult + 1/3 * kids)			ln(Consumption Per Adult + 1/2 * kids)		
Early Life Rain	0.0015**	0.0033***	0.0073***	0.0012*	0.0029***	0.0070***
	(0.00065)	(0.0011)	(0.0020)	(0.00064)	(0.0011)	(0.0020)
Early Life Rain						
... × (Above Median) Child Labor		-0.0039**	-0.012***		-0.0037**	-0.012***
		(0.0018)	(0.0039)		(0.0018)	(0.0038)
Mean Outcome	7.07	7.07	7.07	6.98	6.98	6.98
Anderson-Rubin p-value			0.221			0.117
Total Effect		-0.00059	-0.0051**		-0.00076	-0.0054***
SE of Total Effect		(0.0011)	(0.0021)		(0.0011)	(0.002)
Number Districts	568	568	568	568	568	568
Number Observations	421,214	421,214	421,214	421,214	421,214	421,214
	B. Indirect Measures of Consumption					
	OLS	OLS	lasso IV	OLS	OLS	lasso IV
	Poor Fuel Source			Food Share		
Early Life Rain	-0.0021***	-0.0036***	-0.00087	0.0000071	-0.00078**	-0.00056
	(0.00079)	(0.0014)	(0.0027)	(0.00024)	(0.00037)	(0.00062)
Early Life Rain						
... × (Above Median) Child Labor		0.0033	-0.0026		0.0017***	0.0012
		(0.0022)	(0.0053)		(0.00063)	(0.0012)
Mean Outcome	.792	.792	.792	.574	.574	.574
Total Effect		-0.00034	-0.0035		0.0009**	0.00065
SE of Total Effect		(0.0013)	(0.0028)		(0.00042)	(0.00071)
Number Districts	568	568	568	568	568	568
Number Observations	225,657	225,657	225,657	225,657	225,657	225,657

Notes: This table reports estimates of the effect of early life shocks on consumption (β_1 and β_2 from equation 1), controlling for fixed effects for age of the household head, and month/year/district. Each household is in the data once, and a household's shock is coded as the male household head's shock. Household heads are either self-reported household heads, married to the reported household head (if the head is female), or if no member is coded as the household head, the oldest male under the age of 55. District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. Households whose heads are under 25 are not included in the sample. In columns 3 and 6, child labor prevalence is instrumented using a lasso-selected set of crops, as described in section 4. Standard errors are clustered by district.
Source: NSS Schedules 1 and 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012).

Table 7: Intergenerational Effect of Early Life Shocks

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	lasso IV	OLS	OLS	lasso IV
	Primary Activity Works (NSS)			Attends School (NSS)		
Early Life Rain (Self)	-0.0032***	-0.011***	-0.013***	0.0048***	0.014***	0.014***
	(0.00096)	(0.0018)	(0.0037)	(0.0011)	(0.0022)	(0.0044)
Early Life Rain (Self)						
... × (Above Median) Child Labor		0.015***	0.020***		-0.018***	-0.018**
		(0.0031)	(0.0067)		(0.0034)	(0.0078)
Early Life Rain (Head)	0.00071	-0.0019*	-0.0054**	-0.0011	0.00075	0.0026
	(0.00068)	(0.0011)	(0.0022)	(0.00091)	(0.0014)	(0.0030)
Early Life Rain (Head)						
... × (Above Median) Child Labor		0.0056***	0.013***		-0.0039	-0.0078
		(0.0020)	(0.0045)		(0.0025)	(0.0059)
Mean Outcome	.085	.085	.085	.826	.826	.826
Total Effect (Self)			0.0067**			-0.0039
SE of Total Effect (Self)			(0.0034)			(0.0038)
Total Effect (Head)		0.0037***	0.0075***		-0.0032*	-0.0052
SE of Total Effect (Head)		(0.0013)	(0.0025)		(0.0017)	(0.0032)
Number Districts	571	568	568	571	568	568
Number Observations	317,851	317,664	317,664	317,851	317,664	317,664

Notes: This table reports the intergenerational effect of early life shocks on working and schooling (β_1 and β_2 from equation 1), controlling for fixed effects for age of the household head, month/year/district, age, gender, and the direct effect of early life shocks for the children. District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. Each household is in the data once, and a household's shock is coded as the male household head's shock. Household heads are either self-reported household heads, married to the reported household head (if the head is female), or if no member is coded as the household head, the oldest male under the age of 55. The sample is restricted to households with heads between the ages of 25 and 54 (the oldest age for which rainfall shock information is available). In columns 3 and 6, child labor prevalence is instrumented using a lasso-selected set of crops, as described in section 4. Standard errors are clustered by district.
Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012).

7 Robustness to Alternative Explanations

We now explore potential sources of omitted variable bias, which may bias or complicate our interpretation of β_2. In the first subsection, we control for a variety of district-level covariates that may be associated with the prevalence of child labor and may also lead ELR_{dta} to have heterogeneous effects. In the second subsection, to account for the possibility that positive values of ELR_{dta} affect work and schooling through savings, we control for household fixed effects and compare the outcomes of two children in the same household at the same time.

7.1 Controlling for Differences Across Districts

In Appendix Table A14, we examine whether our OLS estimates in Table 3 are robust to the inclusion of controls for education, wages, socioeconomic status, and school quality at both the district and household-level. All these additional controls are included separately and interacted with ELR_{dta}. One primary concern with the results is that child labor is correlated with income and that we are picking up the effects of differences in income across districts. In columns 1 and 7, we control for the average wage of adults as well as the share of adults who work for a wage, yet the main result is robust to this inclusion. Another concern is that our child labor results are simply a proxy for more (or less) educated places. To test for this, in columns 2 and 8, we include additional controls for average adult literacy and the share of adults who graduated primary school, and secondary school. Again, the main results are robust to these additional controls. Differential school quality is another major concern and potential source of bias. Regions with more child labor might also have lower school quality. In columns 3 and 9, we include school quality controls (constructed from DISE 2005) such as the share of classrooms with more than 60 students, the share of schools with girls' toilets, the share of schools with blackboards, the share of schools with no building, the average number of textbooks at a school, the number of schools per capita, and the share of single classroom schools. Again, the main results are robust to the various controls for quality.

In columns 4 and 10, we include various additional household-level controls from the NSS schedule 10: whether the male head of the household is literate, whether he has completed primary and high school, whether he works for a wage, household size, and number of children in the household. Finally, columns 5 and 11 include all of the controls together, and columns 6 and 12 use lasso to select the most important subset of the controls from the full set (Urminsky et al., 2016). The main OLS results are remarkably robust to the inclusion of all these controls (see column 5 and 11). In fact, Appendix Table A14 shows that the regression results for β_2 are nearly identical to the main results in Table 3 when we include all of the additional household and district control variables.

7.2 Specifications with Household Fixed Effects

In our second robustness test, we include household fixed effects in equation (1) to account for alternative channels through which shocks may have affected household's outcomes, such as saving. Including fixed effects means that the estimates are identified by the gap in the outcomes between two siblings who received different shocks in the same household and are observed at the same time. Appendix Table A15 shows that the main results are robust to this more stringent specification.

8 Discussion: Are Parents Making Efficient Decisions?

We now consider whether households are inefficiently reducing children's human capital in high child labor districts in response to positive rainfall shocks. If this is the case, since ELR_{dta} strictly increases a child's early human capital, it may be that children and/or their parents are not making efficient decisions about the trade-off between schooling and work. Recalling that early life shocks do not reduce educational investments for oldest sons in high child labor districts, there is already evidence that parents underinvest in education in response to positive early life shocks due to incomplete intergenerational contracting. In this section, we explore other reasons parents may fail to invest in education in response to the positive rainfall shock. We first test whether parents in high child labor districts are really forgoing the benefits of early life shocks we see in low child labor districts or whether the returns to education are simply lower in these districts. We find similar returns to education in high and low child labor districts. Next we consider whether parents are unaware of the increased ability of children who receive positive early life shocks, perhaps leading them to inefficiently fail to respond to these shocks by investing in schooling (as discussed in the theoretical framework). Finally, we use a back-of-the-envelope calculation to estimate what discount factor would rationalize parents' educational investment decisions in high child labor districts and discuss whether this value is consistent with efficient investment.

8.1 Different Returns to Education

Parents in high child labor areas may respond less positively to early life shocks in high child labor areas because the returns to education are differentially low in these areas. To evaluate if this is the case, we use the IHDS 2012 data to measure the effect of an additional year of schooling on consumption in high and low child labor districts. With the caveat that these Mincerian returns estimates may not be causal, the results reported in Appendix

Table A16 indicate that education has positive returns with each additional year increasing consumption by approximately 3%. Moreover, the interaction between education and the prevalence of child labor is small, insignificant, and positive. Thus, the Mincerian returns to education appear to be similar in high and low child labor areas.

8.2 Parental Perceptions

We next consider the possibility that parents do not realize that a positive early life shock increases a child's cognitive skills, leading them to inefficiently underinvest in education. This may be a larger problem in higher child labor districts, where parents themselves may be less educated and less able to assess a child's human capital. In Appendix Table A17, using IHDS data, we regress a parent's report of a child's ability on the early life rainfall shock and its interaction with child labor to test whether parents' perceptions of children are affected by early life shocks, and whether this is differential across high and low child labor districts. We find that parents report that children with positive early life rainfall shocks are higher ability, but this is not differential by high and low child labor areas. Thus, we conclude that parents are not failing to invest in education because they are unaware of the child's increased early life human capital.

8.3 Discount Factor Calibration

Finally, we calculate the minimum discount factor for which an altruistic parent would rationally forgo the value of relative long-run consumption gains due to positive early life rainfall shocks in low child labor places to obtain relative short run consumption gains from positive early life rainfall shocks in high child labor places. If this discount factor is low, it implies that parents are not both rational/fully-informed and perfectly altruistic and that educational investment is inefficient. The long-run gains in low child labor places come from increased education, which increases adult consumption. The short-run consumption gains come from increased child labor and reduced educational investment, which lead to more household consumption. This exercise requires strong assumptions. In particular, by comparing present discounted Rupee pay-offs, we are implicitly assuming linear utility. We are also likely to underestimate the gains to education by only focusing on long-run per capita consumption. Appendix B provides the details of how we calibrate the discount factor.

When we make the conservative assumption that there is no economic growth in the future, we calculate that the discount factor (ρ) is 0.91 (based on the IV estimates) to 0.93 (OLS). This assumption leads to a higher value for the discount factor since growth increases the pay-off to education in levels (and thus increases the consumption children forgo in the future by working). We can compare this estimate of ρ to two benchmarks. First, 0.91 is below estimates of the social discount factor, which is thought to be between 0.95 (in low-income countries) and 0.97 (in high-income countries) (Haacker et al., 2020). Second, India's interest rate of 5-7% during the sample period implies a discount factor on the order of 0.93-0.95. Thus, the implied discount factor if parents are perfectly altruistic and fully informed appears to be somewhat low. Moreover, even if this discount factor is correct for the household, given the social discount rate, it suggests that, from the perspective of policymakers, parents fail to undertake socially efficient educational investments.

In the more realistic world, where we allow for moderate growth, the calibrated discount factor falls further. A 1% growth rate leads to a discount factor of 0.90 (IV) to 0.92 (OLS), and allowing for a 5% growth rate (which is closer to India's current growth rates) leads to a discount factor of 0.87-0.88 (IV and OLS). These estimates are even farther from the benchmarks discussed above and further suggest that reductions in educational investment are inefficient. They also highlight an additional potentially important cause of these inefficiencies: parents may not be able to anticipate economic growth and its effects on the returns to education. If parents "estimate" the returns to education by observing the level difference in income between uneducated and educated individuals when they are making their decision, they implicitly assume growth rates of 0 and will systematically underestimate the returns to education.

9 Conclusion

Interventions that increase early childhood investment may be a powerful tool for increasing educational attainment and ultimately setting children on a better life trajectory. However, such policies can have counterintuitive effects in low-income countries, where child labor is common. We provide new evidence that early life investments increase child wages, increasing the attractiveness of child labor. Furthermore, we document the fact that while early life investments positively affect educational outcomes in places where child labor is low, consistent with the existence of dynamic complementarities, this effect is attenuated and even reversed in places where child labor is high.

Our results speak to the need for targeting policies to local conditions. Using our estimates from the ASER data and scaling them by the population of children 5-16 in India in 2014 shows that a policy that simulates the effect of a positive early childhood rainfall shock can have very different and economically meaningful effects across districts. Among districts with more child labor, such a policy would lead to 351,064 additional dropouts. Yet, among districts with little child labor, such a policy would have net positive effects, reducing total dropouts by 406,401. While a one size fits all policy would reduce total dropouts, it would increase inequality in dropout across districts. Targeted policymaking could harness the educational benefits of early life investment across both types of districts.

This is particularly important since, in places where child labor is the highest, early life interventions not only reduce long-term educational attainment and cognitive skills, but also negatively affect adult socioeconomic status. The positive and negative effects of early life investment are even transmitted across generations. Moreover, the fact that early life investment increases the opportunity cost of schooling appears to be particularly important for the most vulnerable, such as women and members of scheduled castes. In the absence of targeting or complementary policies, policies that increase early life investment can increase inequality in the presence of child labor.

These results have important implications both for policymakers interested in increasing educational outcomes and for researchers interested in identifying the parameters of the human capital production function. For the former, early life investment coupled with other complementary policies such as conditional cash transfers may be the best way to increase education if early life investments also affect the opportunity cost of schooling. For the latter, the results suggest that researchers, particularly those working on low-income countries, must take into account how the child human capital stock affects the opportunity cost of schooling, as well as the benefits of schooling.

References

Adhvaryu, Achyuta, Anant Nyshadham, Teresa Molina, and Jorge Tamayo, "Helping Children Catch Up: Early Life Shocks and the PROGRESA Experiment," Working Paper 24848, National Bureau of Economic Research July 2018.

— **and** — , "Endowments at Birth and Parents' Investments in Children," *Economic Journal*, June 2016, *126* (593), 781–820.

Agostinelli, Francesco and Matthew Wiswall, "Estimating the Technology of Children's Skill Formation," Working Paper 22442, National Bureau of Economic Research July 2016.

Aizer, Anna and Flavio Cunha, "The Production of Human Capital: Endowments, Investments and Fertility," 2012. NBER Working Paper No. 18429.

Akresh, Richard, Daniel Halim, and Marieke Kleemans, "Long-term and intergenerational effects of education: Evidence from school construction in Indonesia," 2018.

— **, Emilie Bagby, Daien de Walque, and Harounan Kazianga**, "Child Labor, Schooling, and Child Ability," Working Paper, Mathematica Policy Research Reports, Mathematica Policy Research 2017.

Almond, Douglas and Janet Currie, "Killing Me Softly: The Fetal Origins Hypothesis," *Journal of Economic Perspectives*, 2011, *25* (3), 153–72.

Anderson, Theodore W, Herman Rubin et al., "Estimation of the parameters of a single equation in a complete system of stochastic equations," *The Annals of Mathematical Statistics*, 1949, *20* (1), 46–63.

Ashraf, Nava, Natalie Bau, Nathan Nunn, and Alessandra Voena, "Bride Price and Female Education," *Journal of Political Economy*, 2020, *128* (2), 591–641.

Atkin, David, "Endogenous Skill Acquisition and Export Manufacturing in Mexico," *American Economic Review*, August 2016, *106* (8), 2046–2085.

Attanasio, Orazio, Costas Meghir, and Emily Nix, "Human Capital Development and Parental Investment in India," *Review of Economic Studies*, forthcoming.

Azam, Mehtabul and Geeta Gandhi Kingdon, "Are Girls the Fairer Sex in India? Revisiting Intra-Household Allocation of Education Expenditure," *World development*, 2013, *42* (C), 143–164.

Baland, Jean-Marie and James A. Robinson, "Is Child Labor Inefficient?," *Journal of Political Economy*, 2000, *108* (4), 663–679.

Banerjee, Abhijit V, "Educational Policy and the Economics of the Family," *Journal of Development Economics*, 2004, *74* (1), 3–32.

Basu, Kaushik, "Child Labor: Cause, Consequence, and Cure, with Remarks on International Labor Standards," *Journal of Economic literature*, 1999, *37* (3), 1083–1119.

— **and Van Pham**, "The Economics of Child Labor," *American Economic Review*, 1998, *88* (3), 412–27.

Bau, Natalie, "Can Policy Change Culture? Government Pension Plans and Traditional Kinship Practicess," *American Economic Review*, forthcoming.

Beegle, Kathleen, Rajeev Dehejia, and Roberta Gatti, "Why Should We Care About Child Labor? The Education, Labor Market, and Health Consequences of Child Labor," *Journal of Human Resources*, 2009, *44* (4), 871–889.

Behrman, Jere R, Mark R Rosenzweig, and Paul Taubman, "Endowments and the Allocation of Schooling in the Family and in the Marriage Market: The Twins Experiment," *Journal of Political Economy*, 1994, *102* (6), 1131–1174.

Belloni, Alexandre, Daniel Chen, Victor Chernozhukov, and Christian Hansen, "Sparse Models and Methods for Optimal Instruments With an Application to Eminent Domain," *Econometrica*, 2012, *80* (6), 2369–2429.

— **, Victor Chernozhukov, Christian Hansen, and Damian Kozbur**, "Inference in High-Dimensional Panel Models With an Application to Gun Control," *Journal of Business & Economic Statistics*, 2016, *34* (4), 590–605.

Bharadwaj, Prashant, Juan Pedro Eberhard, and Christopher A. Neilson, "Health at Birth, Parental Investments, and Academic Outcomes," *Journal of Labor Economics*, 2018, *36* (2), 349–394.

— **, Leah K Lakdawala, and Nicholas Li**, "Perverse Consequences of Well Intentioned Regulation: Evidence from India's Child Labor Ban," *Journal of the European Economic Association*, 2013.

Cascio, Elizabeth U and Ayushi Narayan, "Who Needs a Fracking Education? The Educational Response to Low-Skill Biased Technological Change," Working Paper 21359, National Bureau of Economic Research, Revised February 2019.

Case, Anne and Christina Paxson, "Stature and Status: Height, Ability, and Labor Market Outcomes," *Journal of Political Economy*, June 2008, *116* (3), 499–532.

Charles, Kerwin Kofi, Erik Hurst, and Matthew J. Notowidigdo, "Housing Booms and Busts, Labor Market Opportunities, and College Attendance," *American Economic Review*, 2018, *108* (10), 2947–2994.

Cunha, Flavio and James J. Heckman, "The Technology of Skill Formation," *American Economic Review*, 2007, *97* (2), 31–47.

— **and** — , "Formulating, Identifying and Estimating the Technology of Cognitive and Noncognitive Skill Formation," *Journal of Human Resources*, 2008, *43* (4), 738–82.

Currie, Janet and Maya Rossin-Slater, "Early-Life Origins of Life-Cycle Well-Being: Research and Policy Implications," *Journal of Policy Analysis and Management*, 2015, *34* (1), 208–242.

— **and Tom Vogl**, "Early-Life Health and Adult Circumstance in Developing Countries," *Annual Review of Economics*, 2013, *5* (1), 1–36.

Deaton, Angus, *The Analysis of Household Surveys: a Microeconometric Approach to Development Policy*, The World Bank, 1997.

Deshpande, Ashwini, "Does Caste Still Define Disparity? A Look at Inequality in Kerala, India," *American Economic Review*, 2000, *90* (2), 322–325.

Dessy, Sylvain E., "A Defense of Compulsive Measures Against Child Labor," *Journal of Development Economics*, 2000, *62* (1), 261–275.

Dizon-Ross, Rebecca, "Parents' Beliefs About Their Children's Academic Ability: Implications for Educational Investments," *American Economic Review*, 2019, *109* (8), 2728–65.

Drèze, Jean and Geeta Gandhi Kingdon, "School Participation in Rural India," *Review of Development Economics*, 2001, *5* (1), 1–24.

Duflo, Esther, "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment," *The American Economic Review*, 2001, *91* (4), 795.

Duque, Valentina, Maria Rosales-Rueda, and Fabio Sanchez, "How Do Early-Life Shocks Interact with Subsequent Human Capital Investments? Evidence from Administrative Data," 2020. Working Paper.

Dyson, Tim and Mick Moore, "On Kinship Structure, Female Autonomy, and Demographic Behavior in India," *Population and Development Review*, 1983, pp. 35–60.

Edmonds, Eric V, "Child labor," *Handbook of Development Economics*, 2007, *4*, 3607–3709.

Edmonds, Eric V. and Nina Pavcnik, "Child Labor in the Global Economy," *Journal of Economic Perspectives*, 2005, *19* (1), 199–220.

García, Jorge Luis, James J. Heckman, Duncan Ermini Leaf, and Prados, "Quantifying the Life-Cycle Benefits of a Prototypical Early Childhood Program," IZA Discussion Papers, Institute of Labor Economics (IZA) May 2017.

Gilchrist, Duncan Sheppard and Emily Glassberg Sands, "Something to talk about: Social spillovers in movie consumption," *Journal of Political Economy*, 2016, *124* (5), 1339–1382.

Gilraine, Mike, "School Accountability and the Dynamics of Human Capital Formation," 2017.

Gupta, Monica Das, "Selective Discrimination Against Female Children in Rural Punjab, India," *Population and Development Review*, 1987, *13* (1), 77–100.

Haacker, Markus, Timothy B Hallett, and Rifat Atun, "On Discount Rates for Economic Evaluations in Global Health," *Health Policy and Planning*, 2020, *35* (1), 107–114.

Hazan, Moshe and Binyamin Berdugo, "Child Labor, Fertility, and Economic Growth," *The Economic Journal*, 2002, *112* (482), 810–828.

Heckman, James J., "The Economics, Technology, and Neuroscience of Human Capability Formation," *Proceedings of the National Academy of Sciences*, 2007, *104* (33), 13250–13255.

Himaz, Rozana, *Is There a Boy Bias in Household Education Expenditure? The Case of Andhra Pradesh in India Based on Young Lives Data*, Young Lives, 2009.

Jayachandran, Seema, "Selling Labor Low: Wage Responses to Productivity Shocks in Developing Countries," *Journal of Political Economy*, 2006, *114* (3), 538–575.

_ **and Rohini Pande**, "Why Are Indian Children So Short? The Role of Birth Order and Son Preference," *American Economic Review*, September 2017, *107* (9), 2600–2629.

Jensen, Robert, "The (Perceived) Returns to Education and the Demand for Schooling," *The Quarterly Journal of Economics*, 2010, *125* (2), 515–548.

Johnson, Rucker C. and C. Kirabo Jackson, "Reducing Inequality through Dynamic Complementarity: Evidence from Head Start and Public School Spending," *American Economic Journal: Economic Policy*, 2019, *11* (4), 310–349.

Kaur, Supreet, "Nominal Wage Rigidity in Village Labor Markets," *American Economic Review*, October 2019, *109* (10), 3585–3616.

Knudsen, Eric I., James J. Heckman, Judy L. Cameron, and Jack P. Shonkoff, "Economic, Neurobiological and Behavioral Perspectives on Building America's Future Workforce," *Proceedings of the National Academy of Sciences*, 2006, *103* (27), 10155–10162.

Kone, Zovanga L, Maggie Y Liu, Aaditya Mattoo, Caglar Ozden, and Siddharth Sharma, "Internal Borders and Migration in India," *Journal of Economic Geography*, 2018, *18* (4), 729–759.

Krishna, Kowligi R, *Agroecosystems: Soils, Climate, Crops, Nutrient Dynamics and Productivity*, Apple Academic Press, 2014.

Lancaster, Geoffrey, Pushkar Maitra, and Ranjan Ray, "Household Expenditure Patterns and Gender Bias: Evidence from Selected Indian States," *Oxford Development Studies*, 2008, *36* (2), 133–157.

Lee, David L, Justin McCrary, Marcelo J Moreira, and Jack Porter, "Valid t-ratio Inference for IV," *arXiv preprint arXiv:2010.05058*, 2020.

Levy, Victor, "Cropping Pattern, Mechanization, Child Labor, and Fertility Behavior in a Farming Economy: Rural Egypt," *Economic Development and Cultural Change*, 1985, *33* (4), 777–791.

Maccini, Sharon and Dean Yang, "Under the Weather: Health, Schooling, and Economic Consequences of Early-Life Rainfall," *American Economic Review*, June 2009, *99* (3), 1006–26.

Malamud, Ofer, Cristian Pop-Eleches, and Miguel Urquiola, "Interactions Between Family and School Environments: Evidence on Dynamic Complementarities?," NBER Working Papers 22112 March 2016.

Munshi, Kaivan and Mark Rosenzweig, "Networks and Misallocation: Insurance, Migration, and the Rural-urban Wage Gap," *American Economic Review*, 2016, *106* (1), 46–98.

Muralidharan, Karthik and Nishith Prakash, "Cycling to school: Increasing secondary school enrollment for girls in India," *American Economic Journal: Applied Economics*, 2017, *9* (3), 321–50.

Ortiz-Ospina, Esteban and Max Roser, "Child Labor," *Our World in Data*, 2020. https://ourworldindata.org/child-labor.

Prado, Elizabeth L and Kathryn G Dewey, "Nutrition and brain development in early life," *Nutrition reviews*, 2014, *72* (4), 267–284.

Prendergast, Andrew J and Jean H Humphrey, "The stunting syndrome in developing countries," *Paediatrics and international child health*, 2014, *34* (4), 250–265.

Ravallion, Martin and Quentin Wodon, "Does Child Labour Displace Schooling? Evidence on Behavioural Responses to an Enrollment Subsidy," *Economic Journal*, 2000, *110* (462), 158–175.

Rossin-Slater, Maya and Miriam Wüst, "What Is the Added Value of Preschool for Poor Children? Long-Term and Intergenerational Impacts and Interactions with an Infant Health Intervention," *American Economic Journal: Applied Economics*, forthcoming.

Santangelo, Gabriella, "Firms and Farms: The Local Effects of Farm Income on Firms' Demand," Cambridge Working Papers in Economics 1924, Faculty of Economics, University of Cambridge January 2019.

Sayre, Rebecca K, Amanda E Devercelli, Michelle J Neuman, and Quentin Wodon, *Investing in early childhood development: review of the World Bank's recent experience*, The World Bank, 2015.

Shah, Manisha and Bryce Millett Steinberg, "Drought of Opportunities: Contemporaneous and Long Term Impacts of Rainfall Shocks on Human Capital," *Journal of Political Economy*, April 2017, *125* (2), 527–561.

_ and _ , "Workfare and Human Capital Investment: Evidence from India," *Journal of Human Resources*, forthcoming, pp. 1117–9201R2.

Singh, Abhijeet, "Learning more with every year: School year productivity and international learning divergence," *Journal of the European Economic Association*, 2016.

Stock, James H, Motohiro Yogo et al., "Testing for weak instruments in linear IV regression," *Identification and inference for econometric models: Essays in honor of Thomas Rothenberg*, 2005, *80* (4.2), 1.

Topalova, Petia, "Trade Liberalization, Poverty and Inequality: Evidence from Indian Districts," in "Globalization and Poverty," University of Chicago Press, 2007, pp. 291–336.

UNICEF, "The Situation of Children in India: A Profile," *New Delhi: UNICEF*, 2011.

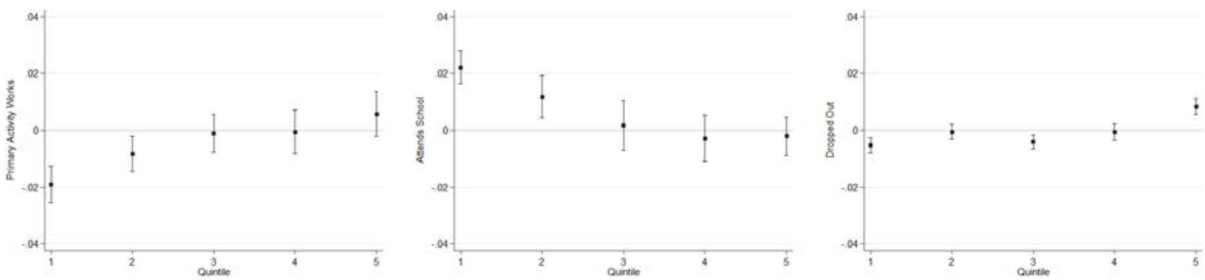
Urminsky, Oleg, Christian Hansen, and Victor Chernozhukov, "Using double-lasso regression for principled variable selection," *Working Paper*, 2016.

Online Appendix

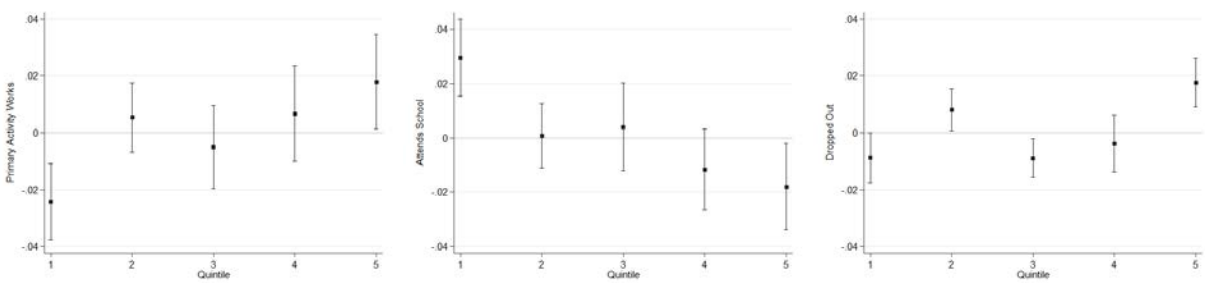
Appendix Figures

Figure A1: Effect of Early Life Rainfall on Schooling for Each Quintile of Child Labor Prevalence

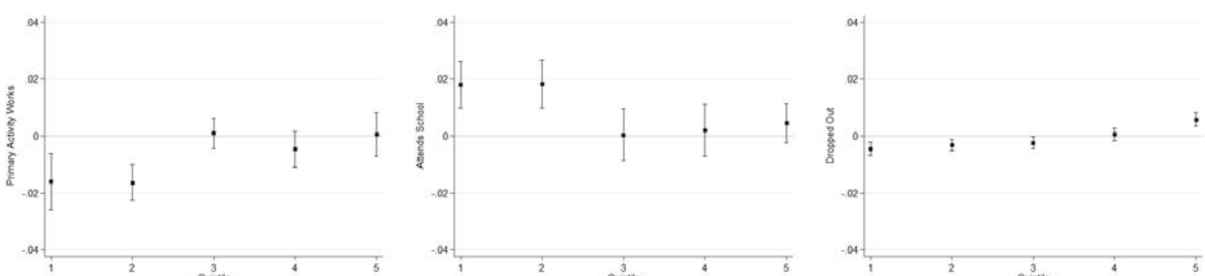
(a) All Children (Ages 5-17)



(b) Older Children (> 13)

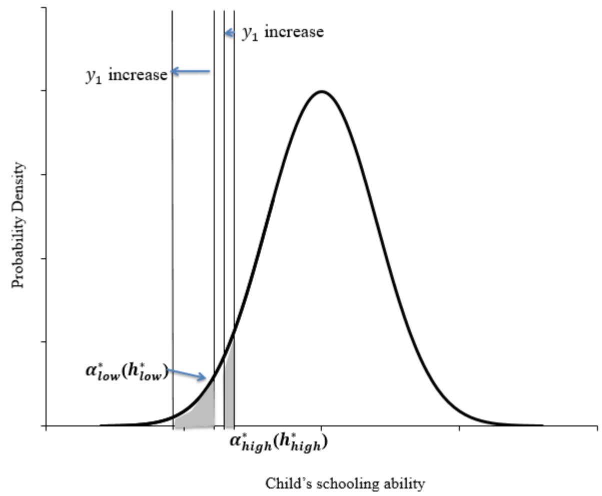


(c) Younger Children (≤ 13)



Notes: Districts are categorized by their leave-out mean quintile of child labor intensity. Each figure plots the total effect of early life shocks for each quintile for primary activity working, attends school, and dropout. Panel (a) includes all children, Panel (b) focuses on older children (>13), and Panel (c) on younger children ($\leq$ 13). The coefficients in Panel (a) are calculated from a single regression, and Panels (b) and (c) are calculated from the same regression. The regressions control for the standard fixed effects in the tables (gender, age, and district/month/year). Standard errors are clustered by district.
Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012) and ASER (2005-2014).

Figure A2: Illustration of Proposition 3b



Notes: This figure illustrates the intuition for Proposition 3a. $a^*_{low}(h^*_{low})$ denotes the cutoff innate ability above which a child is educated in a low child labor district for a given first period human capital investment h^*_{low}, and $a^*_{high}(h^*_{high})$ denotes the cutoff for high child labor districts. The graph illustrates how these cutoffs change as a function of shocks to first period income y_1. The gray shaded areas represent the children who were not educated before and become educated as a result of the change in y_1.

Appendix Tables

Table A1: Summary Statistics

	Mean Value (Std. Dev.)
NSS Schedule 10 (Employment) for Children 5–17	
Primary Activity Works	0.09 (0.294)
Attends School	0.81 (0.386)
NSS Schedule 1 (Consumption) for Children 5–17	
Ate At Least 1 Meal in School	0.24 (0.429)
Ate At Least 10 Meals in School	0.22 (0.420)
Ate At Least 20 Meals in School	0.15 (0.360)
NSS Schedule 1 and 10, Household-level	
ln(Consumption per adult + 1/3 kids)	7.07 (0.590)
ln(Consumption per adult + 1/2 kids)	6.98 (0.595)
Basic Cooking Fuel	0.79 (0.407)
Share of Expenditure on Food	0.57 (0.116)
ASER for Children 5–16	
Dropped Out	0.03 (0.184)
Math Score	0.92 (0.733)
Read Score	2.48 (1.549)
IHDS	
ln(wage)	2.55 (0.532)
Parental Assessment of Ability (0-3)	1.96 (0.544)
Height (cm)	135 (19.511)
District Characteristics	
NSS Schedule 10	
Share Adults in Agriculture	0.46 (0.202)
Share Adults in Manufacturing	0.04 (0.055)

Notes: This table reports summary statistics for our main outcomes, explanatory variables, and key district characteristics.
Source: NSS Schedules 1 and 10 (2004-2012); ASER (2005-2014); IHDS (2012).

Table A2: Effect of Early Life Shocks on Height and Early Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	OLS	OLS	lasso IV	OLS	OLS	lasso IV	OLS	OLS	lasso IV
	Height			Math Score			Reading Score		
Early Life Rain	0.26***	0.30***	0.093	0.015***	0.010**	0.020***	0.012***	0.014*	0.028**
	(0.072)	(0.10)	(0.20)	(0.0020)	(0.0041)	(0.0076)	(0.0039)	(0.0075)	(0.014)
Early Life Rain									
... × (Above Median) Child Labor		-0.078	0.29		0.0093	-0.0089		-0.0044	-0.030
		(0.13)	(0.36)		(0.0060)	(0.013)		(0.012)	(0.025)
Mean Outcome	128.	128.	128.	.493	.493	.493	1.11	1.11	1.11
Anderson-Rubin Wald p-value			0.801			<0.001			0.033
Total Effect		0.22**	0.38**		0.02***	0.011*		0.0096	-0.0019
SE of Total Effect		(0.09)	(0.18)		(0.003)	(0.006)		(0.0065)	(0.012)
Number Districts	260	260	260	559	559	559	559	559	559
Number Observations	26,834	26,834	26,834	1,763,023	1,763,023	1,763,023	1,763,023	1,763,023	1,763,023

Notes: This table reports the effect of early life shocks on height (β_1 and β_2 from equation 1) for children age 5-17 in the IHDS and test scores of children aged 5-7 in the ASER, controlling for fixed effects for age, gender, and year/district. District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. In columns 3, 6 and 9, child labor prevalence is instrumented using a lasso-selected set of crops, as described in section 4. Standard errors are clustered by district.
Source: IHDS I and II (2005 and 2012) and ASER (2005-2014).

Table A3: Effect of Early Life Shocks on Child Wages

	(1)	(2)	(3)
	OLS	OLS	lasso IV
	ln(wage)		
Early Life Rain	0.022	-0.025	0.0082
	(0.017)	(0.028)	(0.071)
Early Life Rain			
... × (Above Median) Child Labor		0.064*	0.019
		(0.035)	(0.098)
Mean Outcome		2.55	2.55
Anderson-Rubin Wald p-value			0.993
Total Effect		0.039*	0.027
SE of Total Effect		(0.021)	(0.033)
Number Districts	181	181	181
Number Observations	1,275	1,275	1,275

Notes: This table reports the impact of early life rainfall shocks on children's reported wages in the IHDS (β_1 and β_2 from equation 1). Regressions control fixed effects for age, gender, and month/year/district. District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. In column 3, child labor prevalence is instrumented using a lasso-selected set of crops, as described in section 4. Standard errors are clustered at the district level.
Source: IHDS I and II (2005 and 2012).

Table A4: Effect of Early Life Shocks in Months of High Demand

	(1)	(2)	(3)	(4)	(5)	(6)
	Defined	Imputing High	Imputing Low	Defined	Imputing High	Imputing Low
	Primary Activity Works (NSS)			Attends School (NSS)		
Early Life Rain	-0.0064***	-0.0066***	-0.0061***	0.0067***	0.0064***	0.0070***
	(0.0012)	(0.0014)	(0.0012)	(0.0016)	(0.0017)	(0.0015)
Early Life Rain						
... $\times$ High Monthly Demand	0.0038***	0.0036**	0.0038**	-0.0013	-0.00065	-0.0024
	(0.0014)	(0.0016)	(0.0017)	(0.0017)	(0.0019)	(0.0021)
Mean Outcome in Low Demand Months	.093	.095	.09	.812	.805	.821
Mean Outcome in High Demand Months	.097	.096	.102	.822	.825	.812
Total Effect	-0.0026**	-0.003***	-0.0022	0.0053***	0.0057***	0.0046***
SE of Total Effect	(0.0012)	(0.0011)	(0.0014)	(0.0013)	(0.0013)	(0.0015)
Number Districts	571	571	571	571	571	571
Number Observations	486,479	486,479	486,479	486,479	486,479	486,479

Notes: This table reports the effect of early life shocks on schooling and working decisions, controlling for fixed effects for age, gender, month/year/district. We use crop calendars to determine the months for sowing and harvesting for the main crops of India. Columns 1 and 4 multiply early life shocks by the (weighted) share of crops in one of those periods, where the weights are the share of adult employment in agriculture and we only consider crops in the crop calendar. Columns 2 and 5 impute high demand for the missing crops, columns 3 and 6 impute low demand. Standard errors are clustered by district. A district is coded as "Low demand" if the weighted share of crops in either sowing or harvesting is below half (and "high demand" if it is above half). The analysis includes all children between the ages of 5 and 17.

Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012).

Table A5: Lasso-Selected Predictors of Above Median Child Labor

	(1)	(2)
	Share of Adult Agricultural Labor	**Coefficient**
Non-perennial crops (such as flowers)	0.003	2.51***
		(0.94)
Sheep and goats	0.004	2.50*
		(1.32)
Cotton and fiber crops	0.03	0.94***
		(0.10)
Jowar, bajra, and millet	0.02	0.86***
		(0.14)
Pulses, dals	0.05	0.85***
		(0.15)
Sugar cane	0.03	0.55***
		(0.15)
Wheat	0.24	0.48***
		(0.08)
Rice	0.32	0.39***
		(0.09)
Cattle and buffalo	0.07	0.27*
		(0.16)
Other cereals	0.09	0.08
		(0.12)
F-statistic		19.868
Number Districts		568
Number Observations		486190

Notes: This table reports coefficients from the first stage regression for above median child labor using the lasso-selected instruments from the pool of variables for the adult share of agricultural employment in each disaggregated industry in the NSS. This is the first stage selected when the outcome variable is whether a child works, and the F-statistic reported is the F-statistic for this two-stage least squares regression. Column 1 reports the mean for each explanatory variable, while column 2 reports its coefficient in the first stage regression of the above median child labor measure on the selected instruments.
Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012).

Table A6: Alternative (Continuous) Measure of Child Labor

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	lasso IV	OLS	lasso IV	OLS	lasso IV
	Primary Activity Works (NSS)		Attends School (NSS)		Dropped Out (ASER)	
Early Life Rain	-0.017***	-0.014***	0.019***	0.018***	-0.0067***	-0.0073***
	(0.0024)	(0.0049)	(0.0027)	(0.0056)	(0.00096)	(0.0018)
Early Life Rain						
... × Mean Child Labor	0.13***	0.10**	-0.13***	-0.12**	0.064***	0.070***
	(0.024)	(0.049)	(0.024)	(0.055)	(0.0091)	(0.017)
Mean Outcome	.095	.095	.817	.817	.035	.035
Anderson-Rubin p-value		0.010		0.008		<0.001
Number Districts	568	568	568	568	559	559
Number Observations	486,190	486,190	486,190	486,190	7,608,681	7,608,681

Notes: This table reports the effect of early life rainfall interacted with child labor on schooling and working decisions controlling for fixed effects for age, gender, and month/year/district (β_1 and β_2 from equation 1). District classifications use the leave-out district mean of child labor. In columns 2, 4, and 6, child labor prevalence is instrumented using a lasso-selected set of crops, as described in section 4. Standard errors are clustered by district. The analysis includes all children between the ages of 5 and 17.
Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012).

Table A7: Reduced Form Effect of Main Child Crops

	(1)	(2)	(3)
	OLS		
	Primary Activity Works (NSS)	Attends School (NSS)	Dropped Out (ASER)
Early Life Rain	-0.0067***	0.0086***	-0.00035
	(0.0015)	(0.0019)	(0.00056)
Early Life Rain			
... × (Above Median) Share Main Child Crops	0.0042**	-0.0050**	0.00056
	(0.0020)	(0.0024)	(0.00083)
Mean Outcome	.095	.817	.035
Number Districts	568	568	559
Number Observations	486,190	486,190	7,608,681

Notes: This table reports the effects of early life shocks interacted with a measure of adult participation in child labor predictive crops (β_1 and β_2 from equation 1), controlling for fixed effects for age, gender, month/year/district in the NSS and year-district in the ASER data (there is no month reported in the ASER data). The 'Main Child Crops' are the large (at least 1% of agricultural employment) crops that have a significant effect on child labor: cotton, millet, sugar, and pulses. We compute the sum of the share of adults (in agriculture) working in any of those crops. Standard errors are clustered by district. The analysis includes all children between the ages of 5 and 17 in the NSS and 5 and 16 in ASER (which does not include 17 year-olds).
Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012) and ASER (2005-2014).

Table A8: Effect of Early Life Shocks on Activity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Sector									
	Agriculture		Retail/Hotels		Other Services		Manufacturing		
Early Life Rain	-0.00060	-0.0043***	-0.000096	-0.00054***	-0.00031***	-0.00049***	-0.00066***	-0.0013***	
	(0.00067)	(0.0011)	(0.00011)	(0.00017)	(0.000100)	(0.00018)	(0.00023)	(0.00044)	
Early Life Rain									
... × (Above Median) Child Labor		0.0075***		0.00090***		0.00037		0.0013*	
		(0.0021)		(0.00031)		(0.00028)		(0.00070)	
Mean Outcome	.033	.033	.003	.003	.003	.003	.007	.007	
Total Effect		0.0032**		0.00036*		-0.00013		0.000015	
SE of Total Effect		(0.0013)		(0.00021)		(0.00017)		(0.00040)	
Number Districts	568	568	568	568	568	568	568	568	
Number Observations	486,190	486,190	486,190	486,190	486,190	486,190	486,190	486,190	
B. Location									
	Goes to School		Household Work		Salaried Work		Casual Wage Labor		
Early Life Rain	0.0060***	0.015***	-0.0017**	-0.0061***	-0.00051***	-0.00077***	-0.0023***	-0.0057***	
	(0.0011)	(0.0021)	(0.00080)	(0.0014)	(0.00012)	(0.00021)	(0.00042)	(0.00061)	
Early Life Rain									
... × (Above Median) Child Labor		-0.018***		0.0089***		0.00052*		0.0069***	
		(0.0033)		(0.0024)		(0.00030)		(0.0012)	
Mean Outcome	.817	.817	.072	.072	.003	.003	.02	.02	
Total Effect		-0.0033*		0.0028*		-0.00025		0.0012	
SE of Total Effect		(0.0018)		(0.0015)		(0.00017)		(0.0008)	
Number Districts	568	568	568	568	568	568	568	568	
Number Observations	486,190	486,190	486,190	486,190	486,190	486,190	486,190	486,190	

Notes: This table reports the effect on sector and location of early life shocks, controlling for fixed effects for age, gender, and month/year/district (β_1 and β_2 from equation 1). District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. Standard errors are clustered by district. The analysis includes all children between the ages of 5 and 17.

Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012).

Table A9: Timing of Early Life Shock

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS					
	Primary Activity Works (NSS)		Attends School (NSS)		Dropped Out (ASER)	
Shock Year Before Birth	-0.0044***	-0.0097***	0.0056***	0.013***	0.00027	-0.0040***
	(0.0013)	(0.0022)	(0.0014)	(0.0026)	(0.00042)	(0.00072)
Shock Year Of Birth	-0.0055***	-0.014***	0.0059***	0.017***	-0.00048	-0.0050***
	(0.0012)	(0.0022)	(0.0014)	(0.0024)	(0.00044)	(0.00082)
Shock Year After Birth	-0.0035***	-0.014***	0.0064***	0.015***	0.00012	-0.0048***
	(0.0011)	(0.0020)	(0.0013)	(0.0023)	(0.00041)	(0.00072)
Shock Year Before Birth		0.011***		-0.015***		0.0079***
... × (Above Median) Child Labor		(0.0038)		(0.0041)		(0.0013)
Shock Year Of Birth		0.018***		-0.022***		0.0084***
... × (Above Median) Child Labor		(0.0036)		(0.0040)		(0.0014)
Shock Year After Birth		0.020***		-0.018***		0.0091***
... × (Above Median) Child Labor		(0.0033)		(0.0038)		(0.0012)
Mean Outcome	.095	.095	.817	.817	.035	.035
Total Effect, Year Before Birth		0.0014		-0.0024		0.0039***
SE of Total Effect, Year Before Birth		(0.0024)		(0.0024)		(0.0008)
Total Effect, Year Of Birth		0.0034		-0.0051**		0.0034***
SE of Total Effect, Year Of Birth		(0.0022)		(0.0024)		(0.0008)
Total Effect, Year After Birth		0.0067***		-0.0024		0.0043***
SE of Total Effect, Year After Birth		(0.002)		(0.0023)		(0.0007)
Number Districts	568	568	568	568	559	559
Number Observations	486,190	486,190	486,190	486,190	7,608,681	7,608,681

Notes: The table splits up our aggregate measure of Early Life Rainfall into its three components: year before birth, year of birth, and year after birth. We control fixed effects for age, gender, and month/year/district in the NSS and year/district in ASER (there is no month reported in the ASER data). District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. Standard errors are clustered by district. The analysis includes all children between the ages of 5 and 17.
Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012) and ASER (2005-2014).

Table A10: Effect of Early Life Shocks on Schooling by Age

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	lasso IV	OLS	OLS	lasso IV
	Primary Activity Works (NSS)			Attends School (NSS)		
Early Life Rain	-0.0068***	-0.014***	-0.015***	0.0085***	0.015***	0.018***
	(0.0010)	(0.0021)	(0.0039)	(0.0013)	(0.0024)	(0.0043)
Early Life Rain						
... $\times > 13$	0.0068***	0.0026	0.0066	-0.0073***	0.00048	-0.0011
	(0.0022)	(0.0040)	(0.0089)	(0.0023)	(0.0040)	(0.0080)
... $\times$ (Above Median) Child Labor		0.013***	0.017**		-0.013***	-0.018**
		(0.0033)	(0.0068)		(0.0036)	(0.0076)
... $\times$ (Above Median) Child Labor $\times > 13$		0.0095	0.0013		-0.017**	-0.014
		(0.0071)	(0.017)		(0.0072)	(0.015)
Mean Outcome	.095	.095	.095	.817	.817	.817
Mean Outcome, >13	.258	.258	.258	.703	.703	.703
Total Effect, $\leq$13		-0.00018	0.0017		0.0021	-0.00055
SE of Total Effect, $\leq$13		(0.0017)	(0.0033)		(0.002)	(0.0037)
Total Effect, > 13		0.012***	0.0096		-0.014***	-0.016**
SE of Total Effect, > 13		(0.004)	(0.0082)		(0.004)	(0.008)
Number Districts	568	568	568	568	568	568
Number Observations	486,190	486,190	486,190	486,190	486,190	486,190

Notes: This table reports the effect of early life shocks on working and schooling by age group, controlling for fixed effects for age, gender, and month/year/district. District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. In columns 3 and 6, child labor prevalence is instrumented using a lasso-selected set of crops, as described in section 4. Standard errors are clustered by district. The analysis includes all children between the ages of 5 and 17.
Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012).

Table A11: Effect of Early Life Shocks on Schooling by Gender

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	lasso IV	OLS	OLS	lasso IV
	Primary Activity Works (NSS)			Attends School (NSS)		
Early Life Rain	-0.0082***	-0.016***	-0.014***	0.0090***	0.018***	0.017***
	(0.0011)	(0.0021)	(0.0043)	(0.0013)	(0.0024)	(0.0047)
Early Life Rain						
... × Girl	0.0080***	0.0067***	0.0049	-0.0066***	-0.0058***	-0.0033
	(0.0013)	(0.0019)	(0.0041)	(0.0015)	(0.0020)	(0.0040)
... × (Above Median) Child Labor		0.015***	0.013		-0.018***	-0.016*
		(0.0033)	(0.0078)		(0.0038)	(0.0086)
... × (Above Median) Child Labor × Girl		0.0025	0.0062		-0.0015	-0.0066
		(0.0036)	(0.0079)		(0.0041)	(0.0079)
Mean Outcome	.095	.095	.095	.817	.817	.817
Mean Outcome, Girls	.123	.123	.123	.79	.79	.79
Total Effect, Boys		-0.00057	-0.0019		0.00015	0.001
SE of Total Effect, Boys		(0.0018)	(0.0038)		(0.0022)	(0.0042)
Total Effect, Girls		0.0086***	0.0093**		-0.0071***	-0.0089**
SE of Total Effect, Girls		(0.0026)	(0.0043)		(0.0025)	(0.0043)
Number Districts	568	568	568	568	568	568
Number Observations	486,190	486,190	486,190	486,190	486,190	486.190

Notes: This table reports the effect of early life rainfall on schooling and working decisions by gender, controlling for fixed effects for age, gender, and month/year/district. District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. In columns 3 and 6, child labor prevalence is instrumented using a lasso-selected set of crops, as described in section 4. Standard errors are clustered by district. The analysis includes all children between the ages of 5 and 17.
Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012).

Table A12: Effect of Early Life Shocks on Schooling by Caste

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	lasso IV	OLS	OLS	lasso IV
	Primary Activity Works (NSS)			Attends School (NSS)		
Early Life Rain	-0.0052***	-0.011***	-0.014***	0.0060***	0.014***	0.019***
	(0.0010)	(0.0020)	(0.0036)	(0.0014)	(0.0026)	(0.0049)
Early Life Rain						
... × SC/ST	0.0022	-0.0050*	0.0045	0.000094	0.0039	-0.010
	(0.0016)	(0.0028)	(0.0057)	(0.0020)	(0.0037)	(0.0070)
... × (Above Median) Child Labor		0.012***	0.017**		-0.016***	-0.026***
		(0.0033)	(0.0068)		(0.0040)	(0.0088)
... × (Above Median) Child Labor × SC/ST		0.014***	-0.0051		-0.0069	0.021*
		(0.0045)	(0.010)		(0.0055)	(0.012)
Mean Outcome	.095	.095	.095	.817	.817	.817
Mean Outcome, SC/ST	.109	.109	.109	.79	.79	.79
Total Effect, not SC/ST		0.00067	0.0037		-0.0022	-0.0072*
SE of Total Effect, not SC/ST		(0.0019)	(0.0035)		(0.0021)	(0.0043)
Total Effect, SC/ST		0.0096***	0.0031		-0.0053**	0.0039
SE of Total Effect, SC/ST		(0.0027)	(0.0051)		(0.0027)	(0.0051)
Number Districts	568	568	568	568	568	568
Number Observations	486,190	486,190	486,190	486,190	486,190	486,190

Notes: This table reports the effect of early life rainfall on schooling and working decisions by caste, controlling for fixed effects for age, gender, month/year/district, and caste. District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. In columns 3 and 6, child labor prevalence is instrumented using a lasso-selected set of crops, as described in section 4. Standard errors are clustered by district. The analysis includes all children between the ages of 5 and 17.
Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012).

Table A13: Effect of Early Life Shocks on Migration

	(1)	(2)	(3)
	A. Adult Men		
	OLS	OLS	lasso IV
	Out-migrants		
Early Life Rain	-0.003*	-0.001	-0.001
	(0.002)	(0.002)	(0.005)
Early Life Rain			
... × (Above Median) Child Labor		-0.004	-0.002
		(0.004)	(0.010)
Mean Outcome	.27	.27	.27
Total Effect		-.005	-0.0039
SE of Total Effect		(.003)	(0.0056)
Number Districts	570	570	570
Number Observations	86,626	86,626	86,626
	B. Adult Women		
	OLS	OLS	lasso IV
	Out-migrants		
Early Life Rain	0.000	0.003	0.003
	(0.002)	(0.002)	(0.005)
Early Life Rain			
... × (Above Median) Child Labor		-0.005	-0.005
		(0.004)	(0.012)
Mean Outcome	.243	.243	.243
Total Effect		-.003	-0.0025
SE of Total Effect		(.003)	(0.0067)
Number Districts	570	570	570
Number Observations	90,459	90,459	90,459

Notes: This table reports the effects of early life rainfall on migration separately for men and women (β_1 and β_2 from equation 1), controlling for fixed effects for age, gender, and month/year/district. District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. Standard errors are clustered by district. The analysis includes all adults 25-54. Source: NSS 2007 migration supplement.

Table A14: Effect of Early Life Shocks Including Additional Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Primary Activity Works (NSS)						Attends School (NSS)					
Early Life Rain	0.0050	0.0040	-0.0096	-0.0081***	0.011	-0.033***	-0.0063	0.0054	-0.0011	0.011***	-0.014	0.024
	(0.0043)	(0.015)	(0.0087)	(0.0025)	(0.015)	(0.012)	(0.0049)	(0.025)	(0.0098)	(0.0031)	(0.023)	(0.016)
Early Life Rain												
... × (Above Median) Child Labor	0.0076**	0.013***	0.018***	0.014***	0.012***	0.016***	-0.0068**	-0.016***	-0.021***	-0.018***	-0.015***	-0.019***
	(0.0031)	(0.0037)	(0.0031)	(0.0029)	(0.0034)	(0.0033)	(0.0031)	(0.0048)	(0.0032)	(0.0033)	(0.0041)	(0.0040)
Mean Outcome	.095	.095	.095	.095	.095	.095	.817	.817	.817	.817	.817	.817
Additional Controls	Income	Education	School Quality	HH Info	All	Lasso Controls	Income	Education	School Quality	HH Info	All	Lasso Controls
Number Districts	568	560	560	568	560	560	568	560	560	568	560	560
Number Observations	485,933	475,614	475,614	486,190	475,357	475,521	485,933	475,614	475,614	486,190	475,357	475,521

Notes: This table reports the effect of early life shocks on schooling and working decisions (β_1 and β_2 from equation 1), controlling for fixed effects for age, gender, and month/year/district. District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. Additional district or household characteristics are interacted with early life shocks and included as additional controls, as described in the text. In columns 6 and 12, "lasso" selects the controls following the post-double-selection approach proposed by Belloni et al. (2012, 2016) (with the main fixed effects selected to always be included). Standard errors are clustered by district. The analysis includes all children between the ages of 5 and 17.
Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012).

Table A15: Effect of Early Life Shocks Within Households

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	lasso IV	OLS	OLS	lasso IV
	Primary Activity Works (NSS)			Attends School (NSS)		
Early Life Rain	-0.0041***	-0.011***	-0.012***	0.0050***	0.011***	0.013***
	(0.00094)	(0.0017)	(0.0039)	(0.0011)	(0.0022)	(0.0047)
Early Life Rain						
... × (Above Median) Child Labor		0.013***	0.015**		-0.013***	-0.015*
		(0.0030)	(0.0075)		(0.0035)	(0.0087)
Mean Outcome	.095	.095	.095	.817	.817	.817
Anderson-Rubin Wald p-value			0.036			0.177
Total Effect		0.0025	0.0033		-0.0013	-0.0025
SE of Total Effect		(0.0019)	(0.0038)		(0.0019)	(0.0043)
Number Districts	568	568	568	568	568	568
Number Observations	409,699	409,699	409,699	409,699	409,699	409,699

Notes: This table reports the effect of early life shocks on schooling (β_1 and β_2 from equation 1), controlling for fixed effects for age, gender, and month/year/district. All regressions include household fixed effects. District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. Standard errors are clustered by district. In columns 3 and 6, child labor prevalence is instrumented using a lasso-selected set of crops, as described in section 4. The analysis includes all children between the ages of 5 and 17.

Source: NSS Schedule 10 rounds 60, 61, 62, 64, 66 and 68 (2004-2012).

Table A16: Minncerian Regressions of Consumption on Years of Schooling

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	lasso IV	OLS	OLS	lasso IV
	ln(Consumption Per Adult + 1/3*kids)			ln(Consumption Per Adult + 1/2*kids)		
Years of School	0.031***	0.030***	0.034***	0.033***	0.032***	0.035***
	(0.0011)	(0.0016)	(0.0028)	(0.0012)	(0.0016)	(0.0029)
Years of School						
... × (Above Median) Child Labor		0.0019	-0.0050		0.0025	-0.0041
		(0.0019)	(0.0052)		(0.0019)	(0.0053)
Mean Outcome	10.1	10.1	10.1	10.0	10.0	10.0
Anderson-Rubin Wald p-value			<0.001			<0.001
Total Effect		0.032***	0.029***		0.034***	0.031***
SE of Total Effect		(0.001)	(0.003)		(0.001)	(0.003)
Number Districts	260	260	260	260	260	260
Number Observations	51,677	51,677	51,677	51,677	51,677	51,677

Notes: This table reports the descriptive Mincerian relationship between years of schooling and log household consumption, testing for heterogeneity by child labor prevalence. Consumption is measured per capita, with children counting as 1/3 (columns 1-3) or 1/2 an adult (columns 4-6). The key explanatory variable is the education of the household head. District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. In columns 3 and 6, child labor prevalence is instrumented using a lasso-selected set of crops, as described in Section 4. Standard errors are clustered by district.
Source: IHDS I and II (2005 and 2012).

Table A17: Effect of Early Life Shocks on Parental Perceptions

	(1)	(2)	(3)
	OLS	OLS	lasso IV
	Parental Assessment (0-3)		
Early Life Rain	0.015**	0.0089	0.031
	(0.0068)	(0.0100)	(0.027)
Early Life Rain			
... × (Above Median) Child Labor		0.0097	-0.027
		(0.013)	(0.044)
Mean Outcome	1.96	1.96	1.96
Anderson-Rubin Wald p-value			0.581
Total Effect		0.019**	0.0036
SE of Total Effect		(0.009)	(0.019)
Number Districts	341	341	341
Number Observations	12,202	12,202	12,202

Notes: This table reports the effect of early life shocks on parental perception of children's ability (on a scale from 1-3 where 1 is "below average", 2 is "average" and 3 is "above average"), controlling for fixed effects for age, gender, and year/district. District classifications use the leave-out share of rounds a district has above median child labor, as described in the text. In column 3, child labor prevalence is instrumented using a lasso-selected set of crops, as described in Section 4. Standard errors are clustered by district. The analysis includes all children between the ages of 5 and 17.
Source: IHDS I and II (2005 and 2012).

Appendix A: Mathematical Appendix

A1 Additional Propositions

Proposition 3b shows that even if opportunity cost effects are not large enough to fully reverse the positive effects of early life human capital investment on education, they can still dampen those positive effects. To introduce Proposition 3b, we first note that for a given value of h, the parent will educate a child if $U_2^p(1,h) \geq U_2^p(0,h)$. Since $\frac{\partial U^p(h,1)}{\partial \alpha} > 0$ and $\frac{\partial U^p(h,0)}{\partial \alpha} = 0$, this relationship exhibits single-crossing. Thus, for any combination of h and d, there exists a cutoff value $\alpha_d^*(h)$ for α where $e = 1$ for all children with $\alpha \geq \alpha_d^*(h)$. Figure A2 illustrates this by plotting the ability distribution and showing that $e = 1$ if $\alpha > \alpha_d^*(h)$.

Proposition 3b. *If* $\frac{f(\alpha^*_{high}(h^*_{high}(y_1)))}{f(\alpha^*_{low}(h^*_{low}(y_1)))} < \Phi$, *then* $\frac{\partial \lambda_{high}(y_1)}{\partial y_1} < \frac{\partial \lambda_{low}(y_1)}{\partial y_1}$.

Proof. See subsection A2.

This proposition indicates that a positive income shock increases education (and adult wages) more in low child labor districts than high child labor districts, as long as an assumption is satisfied that the increased returns to child labor dominate two other, second order effects with ambiguous directions (the value of Φ is given below in section A2). The effect we expect to dominate is that an increase in h increases the relative returns to education more in low child labor areas because, in high child labor areas, increasing h also increases the outside option, $w^c_{2,high}$. The additional ambiguous effects come from the fact that (1) the density of children on the margin of being educated is different in high and low child labor regions since enrollment rates are different, and (2) the derivative of adult wages with respect to early childhood investment may be different in high and low child labor regions if underlying investment in h is different in these regions. If underlying early life human capital investment rates are similar and the densities of the distribution at $\alpha_d^*(h_d(y_1))$ are similar across these regions, these additional, second order effects will be small.[20]

Figure A2 illustrates the intuition for Proposition 3b. In both high and low child labor districts, the increase in y_1 increases the relative returns to schooling, causing $\alpha_d^*(h_d^*))$ to fall. But α^*_{low} falls more than α^*_{high} because the relative returns to schooling increase more in low child labor districts. The share of children whose educational outcomes are changed is captured by the gray areas, which integrate over the ability distribution from the old to the new values of α^*_{low} and α^*_{high}. Even though the density at the cutoff is different in high and low child labor districts, as long as it is not too much greater in high child labor districts,

[20]The assumption that $\frac{f(\alpha^*_{high}(h(y_1)))}{f(\alpha^*_{low}(h(y_1)))} < \Phi$ bounds how much greater the density at α^*_{low} can be relatively to the density at α^*_{high}. That is, if the density at α^*_{high} is sufficiently high, it can lead the response to shocks to be greater in high child labor places even though the change in the ability cutoff is smaller.

more children will be affected in low child labor districts, where the integral is taken over a larger set of values of α.

A2 Proofs

Proof of Proposition 1.
Define $V = E\left[\max_e u(y_2 - c_e e + w^c_{2,d}(h)(1-e)) + \delta(U^c(w^c_3(e,h)) + \alpha e)\right]$, where the expectation is taken over realizations of α. Then, in period 1, the parent solves

$$\max_h u(y_1 - c_h h) + \rho V(h),$$

where ρ is the discount rate. From the first order condition, h^* must satisfy

$$-c_h u'(y_1 - c_h h^*) + \rho \frac{\partial V(h^*)}{\partial h} = 0,$$

To sign $\frac{\partial h^*}{\partial y_1}$, differentiate this expression with respect to y_1 and re-arrange to get

$$\frac{\partial h^*}{\partial y_1} = \frac{c_h u''(y_1 - c_h h^*)}{c_h^2 u''(y_1 - c_h h^*) + \rho \frac{\partial^2 V(h^*)}{\partial h}}.$$

To sign $\frac{\partial h^*}{\partial y_1}$, note that $c_h u''(y_1 - c_h h^*) < 0$ and $c_h^2 u''(y_1 - c_h h^*) < 0$ since $c_h > 0$ and $u'' < 0$. Then, the only term that remains to sign is $\frac{\partial^2 V(h^*)}{\partial h}$. To sign $\frac{\partial^2 V(h^*)}{\partial h^2}$, observe that

$$\begin{aligned}
\frac{\partial^2 V(h^*)}{\partial h^2} =& E\big[u''(y_2 - c_e e^* + w^c_{2,d}(h^*)(1-e^*))\Big(\frac{w^c_{2,d}(h^*)}{\partial h}\Big)^2(1-e^*) \\
&+ u'(y_2 - c_e e^* + w^c_{2,d}(h^*)(1-e^*))\frac{\partial^2 w^c_{2,d}(h^*)}{\partial h^2}(1-e^*) \\
&+ \delta\Big(U^{c\prime\prime}(w^c_3(h^*,e^*) + \alpha e^*)\Big(\frac{\partial w^c_3(e^*,h^*)}{\partial h}\Big)^2 + (U^{c\prime}(w^c_3(e^*,h^*) + \alpha e^*)\frac{\partial^2 w^c_3(e^*,h^*)}{\partial h^2}\Big)\big],
\end{aligned}$$

where e^* is the equilibrium choice of e. This expression is < 0 if $\frac{\partial^2 w^c_3(h)}{\partial h^2} \le 0$ and $\frac{\partial^2 w^c_2(h)}{\partial h^2} \le 0$. Therefore, $\frac{\partial h^*}{\partial y_1} > 0$.

Proof of Proposition 2. For a given h, a child drops out if $U^p_2(0,h) \ge U^p_2(1,h)$. Substituting in the values for consumption, this expression can be rewritten as

$$u(y_2 + w^c_{2,d}(h)) - u(y_2 - c_e) \ge \delta(U^c(w^c_3(h,1) + \alpha) - U^c(w^c_3(h,0))). \tag{2}$$

The derivative of the LHS with respect to y_1 is $\frac{\partial LHS}{\partial y_1} = u'(y_2 + w^c_2(h^*))\frac{\partial w^c_{2,d}(h^*)}{\partial h}\frac{\partial h^*}{\partial y_1}$, which is equal to 0 in low child labor places by assumption. The derivative of the RHS is $\frac{\partial RHS}{\partial y_1} =$

$\delta\Big(U^{c\prime}(w_3^c(h^*,1)+\alpha)\frac{\partial w_3^c(h^*,1)}{\partial h}\frac{\partial h^*}{\partial y_1} - U^{c\prime}(w_3^c(h^*,0))\frac{\partial w_3^c(h^*,0)}{\partial h}\frac{\partial h^*}{\partial y_1}\Big)$. From diminishing marginal returns, $U^{c\prime}(w_3^c(h,1)+\alpha) < U^{c\prime}(w_3^c(h,0))$, so for the RHS to be increasing, we need that $\frac{\partial w_3^c(h,1)}{\partial h} > \frac{\partial w_3^c(h,0)}{\partial h}$. This expression implies that, for an early life shock to increase education rates in low child labor areas, there are dynamic complementarities between e and h.

Proof of Proposition 3a. Observe that $\lambda_d(h_d^*(y_1)) = 1 - F(\alpha_d^*(h_d^*(y_1)))$. Therefore, $\frac{\partial \lambda_d(h_d^*(y_1))}{\partial y_1} = -f(\alpha_d^*(h_d^*(y_1))\frac{\partial \alpha_d^*(h_d^*(y_1))}{\partial y_1}$. Therefore, $\frac{\partial \lambda_d(h_d^*(y_1))}{\partial y_1} = -f(\alpha_d^*(h_d^*(y_1))\frac{\partial \alpha_d^*(h_d^*(y_1))}{\partial y_1} \Rightarrow \frac{\partial \lambda_{high}(h_{high}^*(y_1))}{\partial y_1} = -f(\alpha_{high}^*(h_{high}^*(y_1))\frac{\partial \alpha_{high}^*(h_{high}^*(y_1))}{\partial y_1}$, where $f(\alpha_{high}^*) > 0$. To solve for $\frac{\partial \alpha_d^*(h_d^*(y_1))}{\partial y_1}$, note that $\alpha_d^*(h_d^*(y_1))$ is characterized by $U_2^p(0, h_d^*(y_1)) = U_2^p(1, h_d^*(y_1))$, which can be rewritten as

$$u(y_2 + w_{2,d}^c(h_d^*)) - u(y_2 - c_e) - \delta U^c(w_3^c(1, h_d^*) + \alpha_d^*) + \delta U^c(w_3^c(0, h_d^*)) = 0$$

Applying the implicit function theorem to this expression, we arrive at an expression for $\frac{\partial \alpha_d^*}{\partial y_1}$:

$$\frac{\partial \alpha_d^*}{\partial y_1} = -\frac{\partial w_3^c(1, h_d^*)}{\partial y_1} + \frac{u'(y_2 + w_{2,d}^c(h_d^*))\frac{\partial w_{2,d}^c(h_d^*)}{\partial y_1} + \delta\frac{\partial w_3^c(0,h_d^*)}{\partial y_1}U^{c\prime}(w_3^c(0, h_d^*))}{\delta U^{c\prime}(w_3^c(1, h_d^*) + \alpha_d^*)}.$$

Then,

$$\frac{\partial \alpha_{high}^*}{\partial y_1} = -\frac{\partial w_3^c(1, h_{high}^*)}{\partial y_1} + \frac{u'(y_2 + w_{2,high}^c(h_{high}^*))\frac{\partial w_{2,high}^c}{\partial y_1} + \delta\frac{\partial w_3^c(0,h_{high}^*)}{\partial y_1}u'(w_3^c(0, h_{high}^*))}{\delta U^{c\prime}(w_3^c(1, h_{high}^*) + \alpha_{high}^*)}.$$

Then, $\frac{\partial \lambda_{high}(h_{high}^*(y_1))}{\partial y_1} < 0$ if $\frac{\partial \alpha_{high}^*}{\partial y_1} > 0$. Rearranging $\frac{\partial \alpha_{high}^*}{\partial y_1} > 0$ shows that this satisfied if

$$\delta\left(\frac{\frac{\partial w_3^c(1,h_{high}^*)}{\partial y_1}U^{c\prime}(w_3^c(1, h_{high}^*) + \alpha_{high}^*) - \frac{\partial w_3^c(0,h_{high}^*)}{\partial y_1}U^{c\prime}(w_3^c(0, h_{high}^*))}{u'(y_2 + w_2^c(h_{high}^*))}\right)\left(\frac{\partial h}{\partial y_1}\right)^{-1} < \frac{\partial w_2^c(h_{high}^*)}{\partial h}.$$

Before proving Proposition 3b, we define Assumption A1.

Assumption A1.

$$\Phi > \frac{f(\alpha_{high}^*(h_{high}^*(y_1)))}{f(\alpha_{low}^*(h_{low}^*(y_1)))},$$

where

$$\Phi = \frac{\frac{\partial w_3^c(1,h_{low}^*)}{\partial y_1} - \frac{\frac{\partial w_3^c(0,h_{low}^*)}{\partial y_1}U^{c\prime}(w_3^c(0,h_{low}^*))}{U^{c\prime}(w_3^c(1,h_{low}^*)+\alpha_{low}^*)}}{\frac{\partial w_3^c(1,h_{high}^*)}{\partial y_1} - \frac{u'(y_2+w_{2,high}^c(h_{high}^*))\frac{\partial w_{2,high}^c(h_{high}^*)}{\partial y_1}+\delta\frac{\partial w_3^c(0,h_{high}^*)}{\partial y_1}U^{c\prime}(w_3^c(h_{high}^*,0))}{\delta U^{c\prime}(w_3^c(1,h_{high}^*)+\alpha_{high}^*)}}$$

Proof of Proposition 3b.

Recall that $\lambda_d(h_d^*(y_1)) = 1 - F(\alpha_d^*(h_d^*(y_1)))$. Therefore, $\frac{\partial \lambda_d(h_d^*(y_1))}{\partial y_1} = -f(\alpha_d^*(h_d^*(y_1))\frac{\partial \alpha_d^*(h_d^*(y_1))}{\partial y_1}$. Using the expression for $\frac{\partial \alpha_d^*(h_d^*(y_1))}{\partial y_1}$ from the proof of Proposition 3a and substituting this expression into $\frac{\partial \lambda_d(h^*(y_1))}{\partial y_1} = -f(\alpha_d^*(h_d^*(y_1))\frac{\partial \alpha_d^*(h^*(y_1))}{\partial y_1}$, we find that

$$\frac{\partial \lambda_{low}(h_{low}^*(y_1))}{\partial y_1} = \left(\frac{\partial w_3^c(1, h_{low}^*)}{\partial y_1} - \frac{\frac{\partial w_3^c(0,h_{low}^*)}{\partial y_1} U^{c\prime}(w_3^c(h_{low}^*, 0))}{U^{c\prime}(w_3^c(1, h_{low}^*) + \alpha_{low}^*)} \right) f(\alpha_{low}^*)$$

$$\begin{aligned}\frac{\partial \lambda_{high}(h_{high}^*(y_1))}{\partial y_1} = &\left(\frac{\partial w_3^c(1, h_{high}^*)}{\partial y_1} - \frac{u'(y_2 + w_{2,high}^c(h_{high}^*))\frac{\partial w_{2,high}^c(h_{high}^*)}{\partial y_1} + \delta \frac{\partial w_3^c(0,h_{high}^*)}{\partial y_1} U^{c\prime}(w_3^c(h_{high}^*, 0))}{\delta U^{c\prime}(w_3^c(1, h_{high}^*) + \alpha_{high}^*)} \right) \\ &\times f(\alpha_{high}^*).\end{aligned}$$

Thus, $\frac{\partial \lambda_{low}(h^*(y_1))}{\partial y_1} > \frac{\partial \lambda_{high}(h^*(y_1))}{\partial y_1}$ under Assumption A1. To provide intuition for when Assumption A1 is satisfied, when h_d^* and α_d^* are sufficiently similar across the two types of districts, $\Phi > 1$. This is because the additional term in the denominator, $u'(y_2 + w_{2,high}^c(h_{high}^*))\frac{\partial w_{2,d}^c(h_{high}^*)}{\partial y_1} > 0$, indicating that the denominator is smaller than the numerator. If α_{low}^* and α_{high}^* are sufficiently similar, $\frac{f(\alpha_{high}^*(h(y_1)))}{f(\alpha_{low}^*(h(y_1)))} \approx 1$ and Assumption A1 will be satisfied.

Proof of Proposition 4a. Returning to the proof of Proposition 2, an increase in y_1 will cause child labor to increase if the derivative of the LHS of equation (2) is greater than the derivative of the RHS for the marginal child whose ability is $\alpha_d^*(h_d(y_1))$. This is true if

$$u'(y_2 + w_{2,d}^c(h))\frac{\partial w_{2,d}^c(h^*)}{\partial h}\frac{\partial h^*}{\partial y_1} \geq \delta\Big(U^{c\prime}(w_3^c(h^*, 1) + \alpha^*)\frac{\partial w_3^c(h, 1)}{\partial h}\frac{\partial h^*}{\partial y_1} - U^{c\prime}(w_3^c(h, 0))\frac{\partial w_3^c(h, 0)}{\partial h}\frac{\partial h^*}{\partial y_1}\Big). \tag{3}$$

Substituting ρ for δ and $\tilde{w}_3^c$ for w_3^c, this is efficient if

$$u'(y_2 + w_{2,d}^c(h))\frac{\partial w_{2,d}^c(h^*)}{\partial h}\frac{\partial h^*}{\partial y_1} \geq \rho\Big(U^{c\prime}(\tilde{w}_3^c(h^*, 1) + \alpha^*)\frac{\partial \tilde{w}_3^c(h, 1)}{\partial h}\frac{\partial h^*}{\partial y_1} - U^{c\prime}(\tilde{w}_3^c(h, 0))\frac{\partial \tilde{w}_3^c(h, 0)}{\partial h}\frac{\partial h^*}{\partial y_1}\Big). \tag{4}$$

Now consider each of our two cases. If $\gamma < 1$ and $\frac{\partial w_3^c(h,1)}{\partial h} = \frac{\partial \tilde{w}_3^c(h,1)}{\partial h}$, $\rho > \delta$ and the RHS of equation (4) is greater than that of equation (3). This implies that there is a range of values over which equation (3) is satisfied while equation (4) is not and therefore, changes in educational investment are inefficient. If $\rho = \delta$, inefficiency will occur for a given h^* if the left-side of equation (4) is greater than the left-side of equation (3) (since the right sides of the equations are the same). With some algebra, we can see this will be the case if

$$\frac{\partial \tilde{w}^c(h^*,1)/\partial h}{\partial w^c(h^*,1)/\partial h} > \frac{U^{c\prime}(w_3^c(h^*,1)+\alpha^*)}{U^{c\prime}(\tilde{w}_3^c(h^*,1)+\alpha^*)}.$$

Thus, as long as this condition is satisfied, inefficiency will occur. This condition is intuitive: a larger increase in wages due to an increase in h pushes parents toward educating their children (left-side), but this is offset by the fact that the higher wage decreases the marginal value of more income (right side). That is, it is satisfied as long as the substitution effect dominates the income effect. If there is no diminishing marginal utility of consumption (utility is linear), this expression is always satisfied.

Proof of Proposition 4b. Note that $\frac{\partial \alpha^*}{\partial h} < 0$ if

$$u'(y_2 + w_2^c(h))\frac{\partial w_{2,d}^c(h^*)}{\partial h}\frac{\partial h^*}{\partial y_1} < \delta\Big(U^{c\prime}(w_3^c(h^*,1)+\alpha^*)\frac{\partial w_3^c(h,1)}{\partial h}\frac{\partial h^*}{\partial y_1} - U^{c\prime}(w_3^c(h,0))\frac{\partial w_3^c(h,0)}{\partial h}\frac{\partial h^*}{\partial y_1}\Big). \tag{5}$$

By assumption,

$$u'(y_2 + w_{2,d}^c(h))\frac{\partial w_{2,d}^c(h^*)}{\partial h}\frac{\partial h^*}{\partial y_1} \leq \rho\Big(U^{c\prime}(\tilde{w}_3^c(h^*,1)+\alpha^*)\frac{\partial \tilde{w}_3^c(h,1)}{\partial h}\frac{\partial h^*}{\partial y_1} - U^{c\prime}(\tilde{w}_3^c(h,0))\frac{\partial \tilde{w}_3^c(h,0)}{\partial h}\frac{\partial h^*}{\partial y_1}\Big),$$

so equation (5) is satisfied if $\gamma = 1$. Additionally, the RHS of equation (5) is strictly increasing in γ, while the LHS does not depend on γ. Thus, there is single-crossing in γ, indicating there exists a $\bar{\gamma}$ above which $\frac{\partial \alpha^*}{\partial h} < 0$.

Appendix B: Details of Discount Factor Calibration

We model the parent as choosing between the high and low child labor stream of consumption when a child is 13, which is when education and working outcomes deviate between high and low child labor districts. The increase in consumption from an early life unit increase in aggregate rainfall in a high child labor place is given by

$$\sum_{t=0}^{6} \rho^t \Delta c^h,$$

where ρ is the discount factor, and Δc^h is the change in consumption per capita for a household when the child is between 13 and 18 in a high child labor district relative to a low child labor district. In a low child labor district, the relative payoff from the rainfall shock occurs due to increased consumption in adulthood, which is represented by

$$\sum_{t=7}^{T} \rho^t \Delta c^l,$$

where Δc^l is the relative change in per capita consumption of the child in his adulthood in a low child labor district and T is the last year that the adult experiences consumption gains.

The results in Table 6, which estimate the long-run effects of rainfall shock on adult male's consumption, can be used to estimate Δc^l. The level value of Δc^l is just given by converting the log per capita effect of a unit increase in rainfall in a low relative to a high child labor district into a level effect using average consumption. To be consistent with Table 6, we assume no long-run decline in consumption until the age of 25, and none after the age of 54.

To calibrate Δc^h, we use estimates of the effect of rainfall shocks on per capita consumption by high and low child labor districts during the affected individual's childhood. The results of these regressions are reported in Table 4. Specifically, we calculate the sum of early life shocks for all children in a household, and see its effect on household consumption, controlling for the demographic make-up of the household. In low child labor places, early life rainfall shocks reduce per capita household consumption, consistent with children investing in human capital instead of working. In high child labor places, this effect is significantly attenuated. Using these estimates, we calculate Δc^h the same way as we calculated Δc^l. With these estimates in hand, we can now solve for the maximum ρ for which $\sum_{t=0}^{6} \rho^t \Delta c^h \geq \sum_{t=7}^{T} \rho^t \Delta c^l$. Since geometric sums have a closed-form solution, setting the left and right side of this equation equal results in one equation with one unknown.