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

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

We are grateful to seminar and conference participants at Duke, IFPRI, Kent, NBER Children's meetings, Northwestern, NYU, UCSD, and PacDev for insightful comments and suggestions. 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.

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NBER Working Paper No. 27241
May 2020
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, especially for girls and lower castes. These effects persist and are intergenerational, affecting fertility, per capita household consumption, and other measures of household poverty, and lead to a divergence in the next generations' educational outcomes. Our results are robust to the inclusion of rich controls for district-level characteristics and an IV strategy. We provide evidence that reductions in educational investment in response to positive early life shocks are inefficient.

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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 overall 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). Beyond the high returns of these early interventions, there may be "dynamic complementarities" in the human capital production function, where early skills raise the returns to later human capital investments (Cunha and Heckman, 2007), endogenously leading to increases in those investments. A growing empirical literature tests for the presence of these dynamic complementarities.[1]

However, the success of these interventions in low-income countries may depend critically on the economic environment 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 around 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. Thus, 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. Including controls for district- and household-level socioeconomic characteristics and school quality has little effect on these results.

[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.

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.[2] 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 spend a higher share of their earnings on food. 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. Additionally, while positive early life shocks improve long-run per capita consumption and reduce female fertility in low child labor districts, they have no positive effects in high child labor districts. Altogether, these results suggest that the presence of child labor can eliminate the positive long-term impacts of early life investments and even ultimately cause these investments to make children worse off.

Decreased educational attainment and diminished long-run outcomes do not necessarily imply that reduced schooling is welfare-reducing. 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 education 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. The latter case 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, 2019). 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, thereby 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 a good early life shock are significantly higher than their assessments of those who do not, regardless of the prevalence

[2]A notable exception is Maccini and Yang (2009) in Indonesia, although our range of adult outcomes is considerably broader.

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.

One important concern about the validity of our results is that high child labor districts differ from low child labor districts on a variety of dimensions. In particular, high child labor districts might also have lower school quality, which could in turn attenuate the effects of positive early life shocks. We address this concern in several ways. First, we note that while it is plausible that alternative negative attributes of high child labor districts would attenuate the positive effects of early life income shocks, it is difficult to explain why they would lead these shocks to have a net negative effect on education and cognitive skills. Second, we 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 patterns that we observe. Third, we exploit an alternative, technological source of variation in the demand for child labor: crop variation. Children have a comparative advantage at growing sugar and cotton (Levy, 1985), and indeed, in our data, these are the two crops that have the highest share of children as laborers. When we compare the effects of positive early life rainfall in these districts to the effects in non-sugar/cotton producing districts or use this variation to instrument for child labor, we find similar results. Fourth, we show that the Mincerian returns to education are not lower in high child labor areas, suggesting that lower educational investment is not simply driven by lower returns to schooling.

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 (see footnote 1 for citations).[3] 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

[3]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.

later on. Akresh et al. (2017) find similar, positive effects of better early life rainfall on later educational investments in Burkino Faso. 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. Reduced educational investment in response to positive early life shocks does not rule 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 RossinSlater, 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 this paper's analyses. Section 4 describes our empirical strategy. Section 5 reports our primary results on children's schooling and work using a variety of specifications. Section 6 reports the heterogeneous long-run effects of early life investment on adult outcomes. 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 when children engage in child labor (e.g. Basu and Pham, 1998) and the trade-off between child labor and human capital formation (Basu, 1999; Dessy, 2000; Hazan and Berdugo, 2002) with 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 the positive effect of early life investment in schooling. In extreme cases, 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 long-run wages 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. We now formally derive these intuitions.

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, $d \in \{low, high\}$. d denotes whether a parent is in a high or low child labor district. 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. 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.[4] 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

[4] 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.

period utility that the parent captures as old age support. Suppressing the indices α and d, a parent's preferences in period 1 are represented by

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

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, $\delta = \beta\gamma$ is the product of the parent's discount factor β 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.[5] 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.[6] 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

[5] 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.

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

productive.

Before solving the model, we make several 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 the data in the next section.

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 predict 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 the fact that early life investments increase the returns to later educational investments.

The remaining propositions introduce the novel predictions of this paper. Propositions 3a and b show that standard dynamic complementarity results can be reversed or attenuated by child labor. In high child labor markets, positive early life investments can have zero or negative effects, despite their potential positive effect on the returns to education due to dynamic complementarities.

To introduce Proposition 3a, 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 1 illustrates this by plotting the ability distribution and showing that $e = 1$ if $\alpha > \alpha_d^*(h)$.

Proposition 3a. *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 Appendix A.

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 assumpton is satisfied that increased returns to child labor dominate two other, second order effects with ambiguous directions. 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.[7]

Figure 1 illustrates the intuition for Proposition 3a. 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, more children will be affected in low child labor districts, where the integral is taken over a larger set of values of α. While Proposition 3a shows that the effects of early investment on the

[7] 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.

returns to child labor can attenuate the positive effects of early life investment on schooling, the next proposition shows that, in extreme cases, early life investment can negatively affect schooling.

Proposition 3b. *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 3b 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). Rather, we make the following two new contributions. First, we show 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. Second, as we will discuss in subsequent sections, 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 3b) 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 degrees of incomplete contracting problems.

3 Background and Data

3.1 Data Sources and Measurement

This paper utilizes a variety of datasets from India, which we describe below. These data sources are summarized in Table 1. The summary statistics for our main outcome and explanatory variables, as well as the industrial composition of districts are reported in Table 2.

3.1.1 Child Labor, School Attendance, Consumption, and District Level Controls

National Sample Survey, Schedule 10. We use the National Sample Survey (NSS) to measure the main outcomes of interest: school attendance, work, and adult consumption.

The National Sample Survey 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 likely to be less affected by rainfall shocks. We assume that children are born in their district of residence, since cross-district migration in India is low, 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. These two 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. In addition to using this variable to measure schooling and working, we also use it to generate a district-level measure of child labor. To create that measure for a round t, we calculate the average of the fraction of children (aged 5-17) who report their primary activity as working in each district/round, leaving out survey round t. We also measure the degree of child labor by taking the share of survey years a district is above median and in the top quintile for child labor (leaving out own-year), focusing on the former. The NSS additionally asks respondents for their "principal industry," which we aggregate to get measures of sector activity. For another source of variation in the prevalence of child labor, for a survey in round t, we calculate the share of adult employment in cotton and sugar production in a district (since those two crops have the highest proportion of child workers), leaving out round 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 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. 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.

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 our analyses, but to look at the effects of lagged test scores, we augment it with the IHDS 2005.

Annual Status of Education Report. 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. It 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. In each section, we calculate the sum of correct questions.[8]

National Family Health Survey. We also investigate fertility outcomes and use the number of total children ever born from the National Family Health Survey (NFHS), which is a

[8] 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.

large-scale, multi-round survey conducted in a representative sample of households throughout India. We use a sample of adult women (aged 24-54) from Rounds 2 (1998-1999) and 4 (2015-2016) and restrict the sample to rural households. We focus on the sample that is aged 24-54, because women under 24 may not have completed their education, and we do not have rainfall data for women older than 54 in the earlier sample. We exclude round 3, since it does not record district information, which we need to match the NFHS to the rainfall data and focus on rounds 2 and 4 since the timing of these rounds matches the timing of our other data sets.

3.1.2 Educational Quality: Unified District Information System for Education

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.

3.1.3 Variation in Human Capital: Yearly Gridded Rainfall

Our 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.[9] We form an aggregate rainfall shock measure by summing over the shocks when the child is in utero (-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 various datasets using their birth year and district.

3.1.4 Variation in the Importance of Rainfall: Irrigation

For a robustness test, we examine how the effects we estimate vary with the share of a district's area that is irrigated. Since irrigation reduces the importance of rainfall, we expect, if our effects are truly driven by rainfall, that they will be attenuated in districts with more irrigation. For this analysis, we create district-level measures from the Global Irrigated

[9] In India, though flooding does 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 outcomes.

and Area Mapping data on irrigation in South Asia at the beginning of the 21st century assembled by Thenkabail et al. (2009).

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.[10] Moreover, agriculture and family-run businesses, the main employers of child labor, 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.[11] Figure 2 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 Human Capital and Child Wages

We test whether children with greater human capital have a greater opportunity cost of schooling, consistent with the key mechanisms in the theoretical framework. Table 3 reports results from a regression of child wages (conditional on working for pay) in the IHDS 2012 on height and lagged test scores. The sample that controls 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, and 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

[10] 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).

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

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 children who work without wages (e.g. on family farms). While the econometrician cannot observe opportunity costs in these cases since wages are no longer equal to the opportunity cost of schooling, it is still likely that the opportunity cost grows with human capital.

3.4 Early Life Rainfall and Human Capital

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 we showed in 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. This investment 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 3 provides evidence for the 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.[12] This 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. So, 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 shows the relationship between math and reading test scores in the ASER data, respectively, and early life rain. Here, we restrict to children aged 5-7 in order to minimize the impact of endogenous 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 A1 reports analogous regressions of height and math and reading scores on the aggregate early life rainfall measure by high and low child labor districts.

[12] 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.

Aggregate rainfall shocks are associated with improved early life human capital, and this effect is not differential across high and low child labor districts. Altogether, the figures and table suggest that early life rain is a significant shock to the early human capital stock.

4 Empirical Strategy

The theoretical framework predicts that the extent to which early human capital investments will be reinforced by additional schooling investment will depend upon the opportunity cost of children's time. In subsection 3.4, 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 the following sections, we estimate the different effects of early life rainfall shocks on subsequent schooling investment by the prevalence of child labor.

In our primary specifications, we estimate the following regression

$$y_{idmtag} = \alpha_a + \beta_1 ELR_{dta} + \beta_2 ELR_{dta} \times CL_{dt} + \gamma_{dmt} + \gamma_a + \gamma_g + \epsilon_{idmtag} \tag{1}$$

where y_{idmtag} is an outcome measure (i.e. working, being in school, test scores, consumption) for individual i of gender g in district d in month m and year t at age a, ELR_{dta} is individual i's early life aggregate rainfall shock, CL_{dt} is a measure of child labor in district d, γ_{dmt} is a district-month-year fixed effect, γ_a is an age fixed effect, and γ_g is a gender fixed effect. In our primary tables, we report β_1 and β_2 using three separate measures of CL_{dt}: a continuous measure of the share of children working, a variable measuring the share of rounds the district is above the median in child labor, and a variable for the share of rounds the district is in the top quintile. 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 high child labor districts.

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, 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.

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 3a) and may even be reversed (Proposition 3b), so that early life shocks decrease human capital investment.

Schooling and Work. Panel A of Table 4 reports the results from estimating equation (1) where the outcome is an indicator variable for whether a child lists her primary activity as working (columns 1-4), attending school (columns 5-8), or dropping out (columns 9-12), and CL is either the continuous measure of child labor prevalence, the share variable for above median district, or the share variable for living in a top-quintile district. The "total effect" row at the bottom of the panel reports the aggregate effects of rainfall shocks in districts whose share measure is equal to 1 (as opposed to the interaction effects). For all the columns, consistent with Proposition 2, an increase in ELR_{dta} reduces the likelihood of working on average in low child labor districts. In contrast, as the prevalence of child labor grows (column 2), this effect is attenuated, and the net effect on working becomes positive in above median and top quintile districts (columns 3-4). The remaining columns report symmetric results for attending school and dropping out. ELR_{dta} has positive effects on education on average and in low child labor districts, but these effects are reversed in the above median child labor districts. Notably, the total effect's magnitude grows monotonically in the prevalence of child labor. The first panel of Appendix Figure A1, which plots the total effect of aggregate shocks by child labor quintile, provides further evidence of this monotonic relationship.

Figure 4 explores these effects by graphing the relationship between the residual variation in our child labor and rainfall interaction variable and our outcomes of interest, after accounting for the control variables in equation (1). These graphs provide further evidence that the residual interactions between these two variables have strong, monotonic effects on working, schooling, and dropping out. They also provide evidence that the relationship between the early life rainfall shock aggregate and outcomes is roughly linear, consistent with our choice of specification.

The estimates in Panel A of Table 4 imply that one additional positive early life rainfall shock in an always below top quintile child labor district decreases the likelihood a child aged 5–17 works by 0.7 percentage points (8%), increases the probability she attends school by 0.8 percentage points (1%) and reduces dropout by 0.2 percentage points (6%). Adding up the enrollment effects over a child's life implies that total years of schooling increases by 0.1 years. In contrast, in an always top quintile child labor district, a positive rainfall shock *increases* work by 0.8 percentage points (8%), decreases schooling by 0.6 percentage points (0.7%) and increases dropout by 0.8 percentage points (23%). Adding up the enrollment effects implies a reduction in years of education of 0.07. 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 top-quintile district is on the order of one-half 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 (1.5 percentage points) is roughly 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 extremely 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 4 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 (albeit not statistically significant) 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.

In Appendix Table A2, we examine how early life shocks affect which sector children work in. Panel A suggests 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, there is no aggregate increase in employment in manufacturing. 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 other types of work. On net, early life rainfall has a positive and marginally significant total effect on household work in high child labor districts (col 4).

Heterogeneity. We next examine several potentially important sources of heterogeneity in the effects.

Age. 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 A3. 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, who are more likely to be on the margin of working, the effect of the shock on schooling is negative, large, and significant. In the top quintile districts, for older children, a positive shock increases work by 2.2 percentage points (8.5%) and decreases school by 2.4 percentage points (3%), roughly half 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 upper quintile districts is generally close to zero or even positive, in line with the effects in low child labor districts.

Oldest Sons. Next, motivated by Propositions 4a and 4b, we examine heterogeneity by eldest sons. From Proposition 4, 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 4 shows, if parents are imperfectly altruistic, we would 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 γ in the model 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, 2019) 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. 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.

Table 5 allows early life rainfall shocks to have different effects on oldest sons versus other children in high and low child labor districts, while Figure 5 reports the total effects of early life rainfall shocks on children in above median districts by gender, age, and birth order. The estimates reveal that while early life rainfall shocks 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. In other words, oldest sons in high child labor districts have a more similar response to early life rainfall to sons in low child labor districts. 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, and Table 5 Panel B shows that the interacted effect of early life shocks for older boys are robust to (grouped) fixed effects for district, time, age, gender, and early life shocks.[13] These results provide evidence that reductions in education due to early life rainfall may be inefficient as a result of incomplete contracting between parents and non-oldest sons and daughters.

Gender. Girls tend to receive less educational investment than boys in India. Appendix Table A4 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. In Appendix Table A5, we allow for heterogeneity by

[13] The identification for Table 5 Panel B compares the difference between high and low child labor districts in the relative effect of early life shocks for boys of the same age who are and are not oldest sons.

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.

6 Medium, Long-Term, and Intergenerational Effects

While ELR_{dta} may decrease educational attainment in high child labor districts, it's not clear that this reduction in education has negative long-term effects. In this section, we examine whether this is the case using four sets of outcomes. 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 the second subsection, we use longer-term measures from the NSS to examine the heterogeneous effects of early life rainfall on adult outcomes. We first consider the effect of rainfall on per capita adult consumption, focusing on consumption rather than wages since, in rural India, few adults work for a wage. We then 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 third subsection, we estimate the differential effects of the early life rainfall shocks on adult fertility using data from the NFHS. Finally, in the fourth section, we estimate the intergenerational effects of early life rainfall shocks.

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 4 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, using the share of rounds the district has above median child labor as our measure of CL_{dt}. 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.

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 reading scores by 0.5 (one-third a standard deviation) and of math scores of 0.4 (approximately one-half a standard deviation).

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 significant reduction in cognitive skills in districts where child labor is more prevalent on the order of the effect of 1 less year of schooling. While this effect seems large, recalling that it is the cumulative effect of the shock across all of childhood, the average yearly loss in test scores is on the order of 0.04 standard deviations.

6.2 Long-Term Effects on Consumption

We next consider whether these negative, medium-term effects on cognitive skills translate into lower consumption in the long-term. To do so, we re-estimate the specifications in Table 4 on a sample of adult male household heads (aged 25-54) with a measure of adult per capita consumption as the outcome variable.[14] Furthermore, since consumption data is available from both the NSS Schedule 1 and 10 surveys, we pool these surveys to maximize statistical power.

Panel A of Table 6 reports the results. 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 1-4) or one-half an adult (columns 5-8). 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 (though not significantly different from 0). In low child labor districts, one additional positive rainfall shock increases per capita consumption by 0.1-0.3%. In top quintile districts, it results in a reduction of roughly 0.2% (though the standard errors get large).

In Panel B, we turn to alternative measures of adult household socioeconomic status. Columns 1–4 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 5–8 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, with significant positive total effects on spending on food (+0.15%) in always highest quintile child labor districts. Thus, we conclude that at best early life rainfall did not improve adult outcomes

[14] 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.

and at worse, it may have reduced them in high child labor districts.

6.3 Long-Term Effects on Fertility

Using the NFHS data, we investigate the relationship between ELR_{dta} and fertility outcomes as another long-run outcome. We re-estimate equation (1) but with the number of total children ever born as the dependent variable. We first test how early life rainfall impacts the total number of children women aged 24-54 bear. Table 7 reports the results from this exercise. We find that ELR_{dta} reduces the total number of children born per woman by approximately .02 children. Turning to the heterogeneous effects of positive early life shocks in high and low child labor districts, we find that in below median districts, a one unit increase in ELR_{dta} decreases children born by 0.03 children. In high child labor districts, this positive effect completely disappears. While these effects may appear small, it is striking that—at least in low child labor districts—a single early rainfall shock can have such a meaningful effect on fertility.

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 8, 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 always above median districts, children of a household head who received an additional year of good rainfall in early life in a high child labor district are 0.2 percentage points more likely to work (2%) and 0.3 percentage points (0.2%) 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 0.036 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-third that magnitude.

7 Robustness to Alternative Explanations

Having presented our key results, we now explore potential sources of omitted variable bias, which may bias or complicate our interpretation of β_2. We evaluate the robustness of the results in several ways. In the first subsection, we explore which characteristics predict child labor. We then 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. Next, 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. In the fourth subsection, we use an alternative, technological source of variation in child labor – the production of sugar and cotton – as an instrument for child labor and replicate the same patterns as in Table 4. Finally, in the last subsection, we mention a series of additional robustness tests and extensions.

7.1 What Predicts Child Labor?

District-level school quality and socioeconomic status are candidate omitted variables that may be related to the prevalence of child labor and may attenuate the positive effects of ELR_{dta}. Intuitively, if poverty drives the relationship between ability and schooling (for instance, if parents cannot afford to send their higher ability children to more school) and poverty is correlated with levels of child labor, then our estimates could be biased. Similarly, less educated places and places with worse school quality may also lead high ability children to attend relatively less school (for example, if they've already learned the skills that are available to them). To explore the extent to which such biases may exist, in Table 9, we measure the association of district-level education, socioeconomic status, school quality, and crop variation measures with child labor. Column 1 regresses the child labor measure on average adult wages, column 2 on an education index,[15] column 3 on a school quality index,[16] column 4 on the percent of adults who work in agriculture, and column 5 on the percent who work in sugar and cotton. We include the final column not as a test for bias – we expect sugar and cotton to predict child labor – but rather to establish whether this variation in the crops grown by adults across districts, which is predominantly driven by differences in climate and soil, may be a relevant instrument for child labor. The panels of the table repeat

[15] To form this index, we standardize and average over the following measures from the NSS Schedule 10: average adult literacy, share of adults who graduated primary school, and share of adults who graduated secondary school.

[16] To form this index, we standardize and average the following measures from the DISE 2005: share of classrooms with more than 60 students (sign reversed to be negative), share of schools with girls' toilets, share of schools with blackboards, share of schools with no building (sign reversed), average number of textbooks at a school, and the number of schools per capita.

these regressions with our three main outcomes: the continuous measure of child labor, the share of rounds the district is above the median, and the share of rounds in which it is in the top quintile.

Table 9 shows that adult wages and the education index are negatively correlated with child labor. This should be mechanically true since child labor reduces schooling, and if schooling has positive returns, this will reduce adult wages. While negatively correlated, the relationship with adult wages is not large: a one standard deviation increase in the average adult wage decreases the share of rounds a district is in the top quintile of child labor by 4.4 percentage points. Interestingly, across all three measures of child labor, there is no relationship between the school quality index and child labor. This shows that high child labor districts are not necessarily low school quality districts. The share of adults working in agriculture (the most frequent sector for child labor) positively predicts child labor. For all the significant predictors in columns 1-4, the relationship between the predictors and the degree of child labor appears to be non-monotonic. Adult wages, the education index, and share in agriculture are more strongly related to being an above median than a top quintile district. This relationship already provides preliminary evidence that the correlation between these variables and child labor cannot be driving the effects we have observed, which are typically monotonic in the degree of child labor.

Finally, column 5 establishes that adult employment in sugar and cotton is a strong predictor of child labor. The coefficients imply that moving from 0 to 100% adults employed in sugar or cotton production increases the share of rounds that a district is a top quintile child labor district by 79 percentage points. We will exploit this alternative source of variation in child labor to further evaluate the robustness of the main results.

7.2 Controlling for Differences Across Districts

Given the potentially strong relationship between child labor and socioeconomic status/adult education observed in Table 9, we examine whether our main estimates in Table 4 are robust to the inclusion of controls for education, wages, socioeconomic status, and school quality at both the district and household-level in Appendix Table A6. All these controls are interacted with ELR_{dta}. Columns 1 and 7 include both the average wage of adults (as in column 1 of Table 9) but also include the share of adults who work for a wage. The education controls in columns 2 and 8 consist of the same variables that formed the education index in Table 9. The school quality controls in columns 3 and 9 include the same variables that formed the school quality index, and in addition include controls from DISE for share of single teacher schools and share of single classroom schools.[17] In columns 4 and 10, we include 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 the controls together and columns 6 and 12 use lasso to select the most important subset of the controls from the full set (Belloni et al., 2012). Thus, the controls included in Appendix Table A6 are a superset of the variables used to predict child labor in the first 3 columns of Table 9.

Appendix Table A6 shows that the regressions results for β_2 are nearly identical when we include all of these additional household and district control variables to the main results in Table 4.

7.3 Estimating Models 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. Panel A of Appendix Table A7 shows that the main results are robust to conditioning on household fixed effects.

Panel B of Appendix Table A7 tests directly for within-household spillovers of rainfall shocks across siblings. Using the sum of all shocks to siblings (with fixed effects for sibling age and gender composition), there is no evidence that children go to school more if their siblings are drawn out of school. There is weak evidence that siblings' positive early life shocks on average lead children to go to less school, although the point estimates are an order of magnitude smaller than the direct effects.

7.4 IV Strategy: Sugar and Cotton

Certain regions in India are particularly ideal for growing cotton or sugar cane due to agro-climatic conditions such as average temperatures and rainfall, as well as soil requirements (Krishna, 2014). Children are well-suited to help grow cotton and sugarcane, given the nature of the tasks associated with planting, weeding, and harvesting. Cotton is notorious as a child labor crop because it is low to the ground and very lightweight (Levy, 1985). Consistent with this, in the NSS, cotton and sugar are the two crops that have the highest proportion

[17] We did not include these variables in the school quality index because it is less clear, relative to variables denoting whether a school has more or less infrastructure/resources, whether these features of schools negatively or positively impact school quality.

of workers under 18 (around one-fifth of workers in each crop are children at the start of our sample). Therefore, we use variation in the prevalence of sugar and cotton – which in turn relies on agroclimatic conditions – as an instrument for child labor.

We use both the presence of any adult employment in sugar or cotton and the percent of adult employment in sugar or cotton in the district as an instrument for child labor. Recall Table 9 shows that the share of adults employed in sugar or cotton is a strong predictor of child labor. In Panel A of Appendix Table A8, we report the reduced-form results, re-estimating the results from Table 4 but replacing child labor with the cotton and sugar measures. The results tell a similar story. On average, children who experience better early life rainfall shocks are less likely to be working and more likely to be attending school. However, in places with cotton and sugar, these effects are either attenuated or fully reversed. In Panel B of Appendix Table A8, we instrument for the prevalence of child labor using adult employment in sugar and cotton and again find very similar results. The pattern of estimates is both quantitatively and qualitatively similar to the main OLS results.

7.5 Other Robustness Tests and Extensions

Appendix Tables A9-A12 report the results of additional robustness tests and extensions. In Appendix Table A9, we show that the results are robust to the inclusion of early life shock-age-gender and district-time fixed effects (Panel A) and the inclusion of separate fixed effects for district, survey round, age, and gender (Panel B). Appendix Table A10 shows that the negative effects of early life rainfall on schooling in high child labor places are concentrated in districts with less irrigation, consistent with the intuition that rainfall shocks should matter less for yields when areas are irrigated. Appendix Table A11 shows that the results are robust to using the first round of data from the NSS (round 60) to classify which districts are high child labor. Finally, Appendix Table A12 uses child labor measures at the district-caste and district-gender level instead of the district level and replicates the same patterns as before. If anything, the results are stronger, perhaps reflecting reduced measurement error.

8 Discussion: Are Parents Making Efficient Decisions?

We now consider whether children in high child labor districts are being made worse off by experiencing a higher value of ELR_{dta}. 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 A13 indicate that education has positive returns with each additional year increasing consumption by 4 percent. 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 returns to schooling, leading them to inefficiently underinvest in education. This may be a larger problem in higher child labor districts, where parents themselves are less educated and less able to assess a child's human capital. In Appendix Table A14, 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 (using IHDS data). We find that parents report that children with positive early life rainfall shocks are higher ability, and 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 (β) is 0.91. 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 β 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, and allowing for a 5% growth rate (which is closer to India's current growth rates) leads to a discount factor of 0.87. 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 high child labor districts, such a policy would lead to 334,770 additional dropouts. Yet, among low child labor districts, such a policy would have net positive effects, reducing total dropouts by 381,036. While a one size fits all policy would have relatively little effect on 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 and increase adult fertility. The positive and negative effects of early life investment are also 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.

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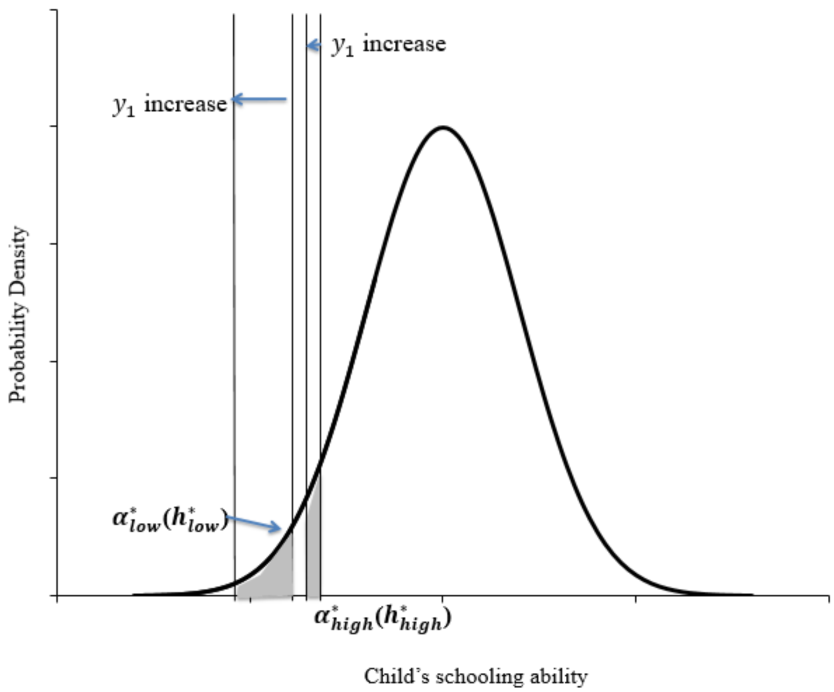
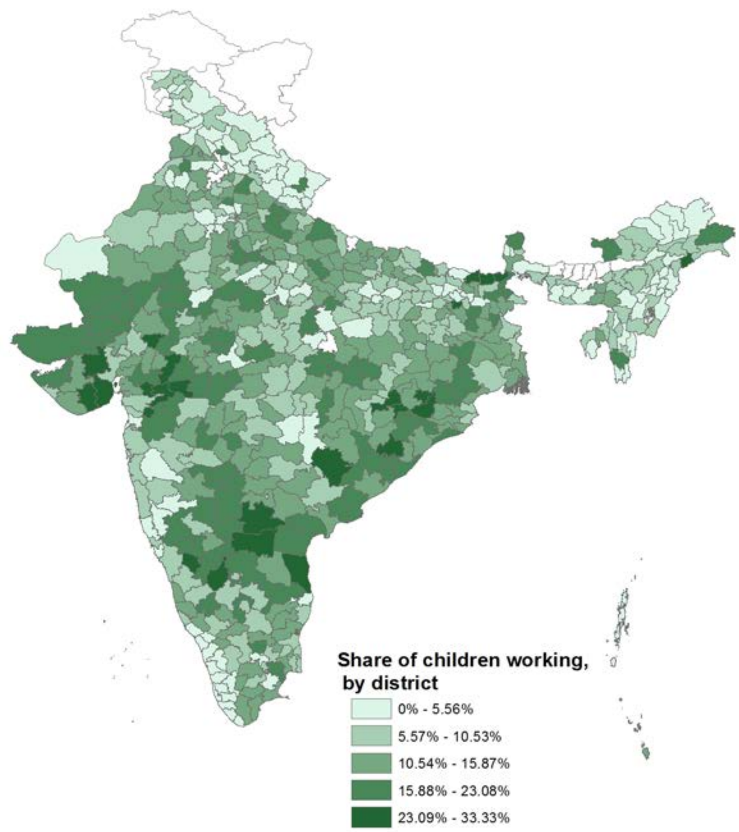
Figures

Figure 1: Illustration of Proposition 3a



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.

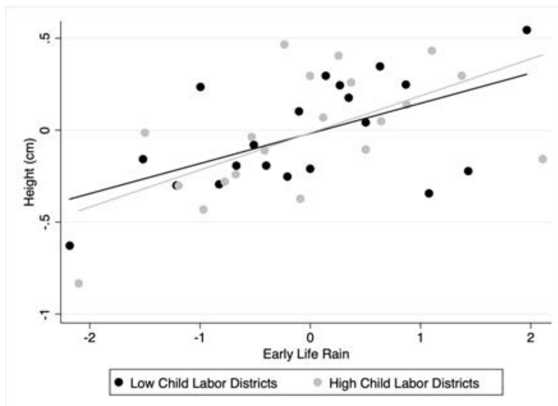
Figure 2: Child Labor Prevalence by District, 2004



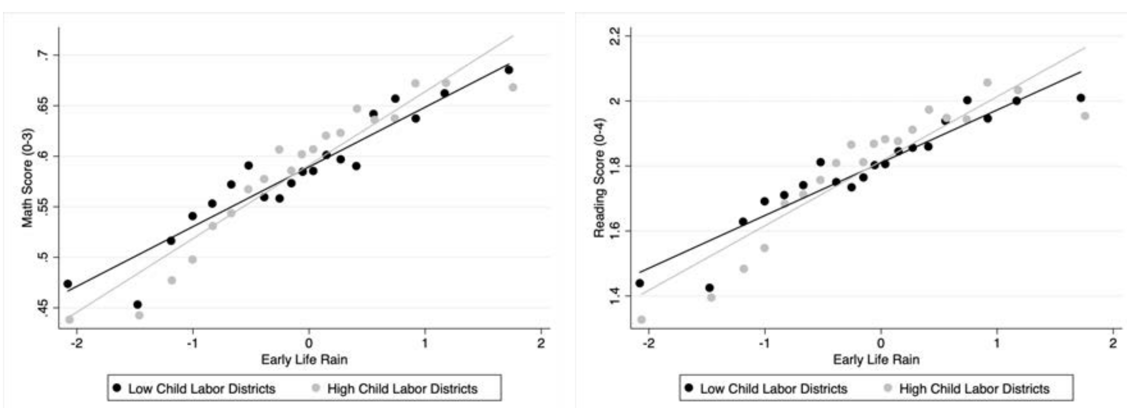
Notes: This figure shows a map of the districts of India, shaded by the prevalence of child labor, which is defined as the proportion of children aged 5-17 who report working in the market, in domestic work, or for a home enterprise as their primary activity.
Source: NSS Schedule 10 (2004).

Figure 3: Effect of Early Life Shocks on Height and Early Test Scores

(a) Height Age 5-17 (IHDS)



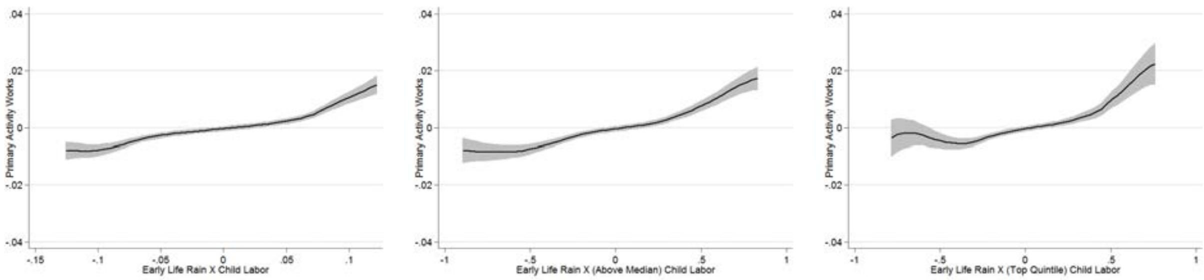
(b) Test Scores Age 5-7 (ASER)



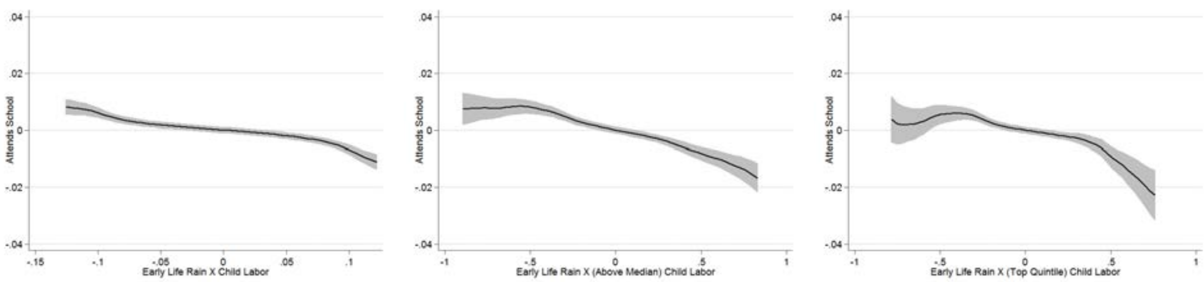
Notes: Each panel plots the relationship between residual outcomes (y-axis) and residual early life shocks × child labor. Residuals are calculated after controlling for fixed effects for district and age. A district is defined as above median if it had above the median level of child labor in the NSS Round 68 (the most proximate NSS round to the IHDS 2012).
Source: IHDS (2012) and ASER (2005-2014).

Figure 4: Local Polynomial Relationships Between Schooling and Early Life Shocks × Child Labor

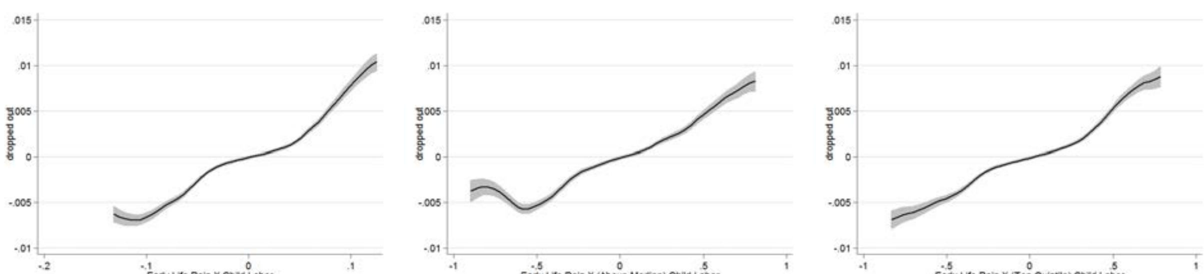
(a) Works (NSS)



(b) Attends School (NSS)

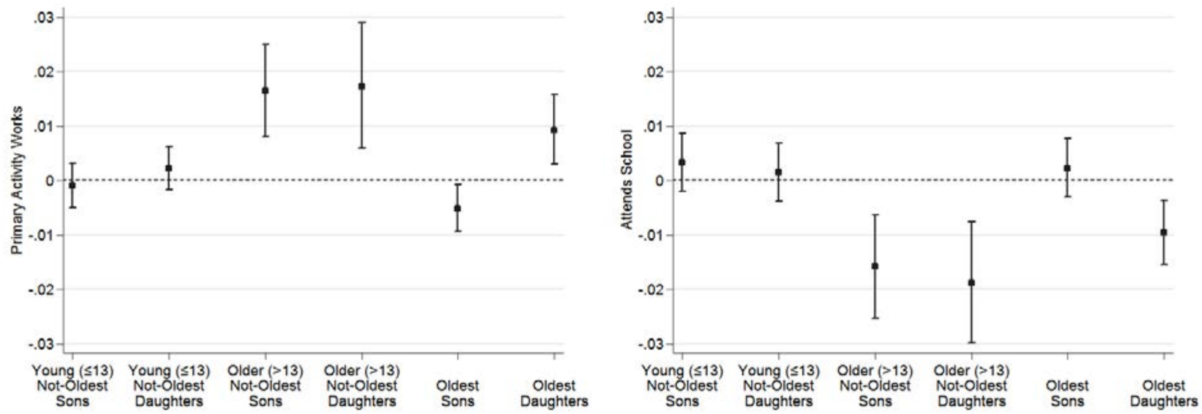


(c) Dropped Out (ASER)



Notes: Each panel plots the relationship between residual outcomes (y-axis) and residual early life shocks × child labor. Residuals are calculated after controlling for fixed effects for month × year × district as well as fixed effects for age × gender × early life shocks. The local polynomial is based on an Epanechnikov kernel function with Stata's default bandwidth. The shaded region reflects the 95% confidence interval.
Source: NSS Schedule 10 (2004-2012) and ASER (2005-2014).

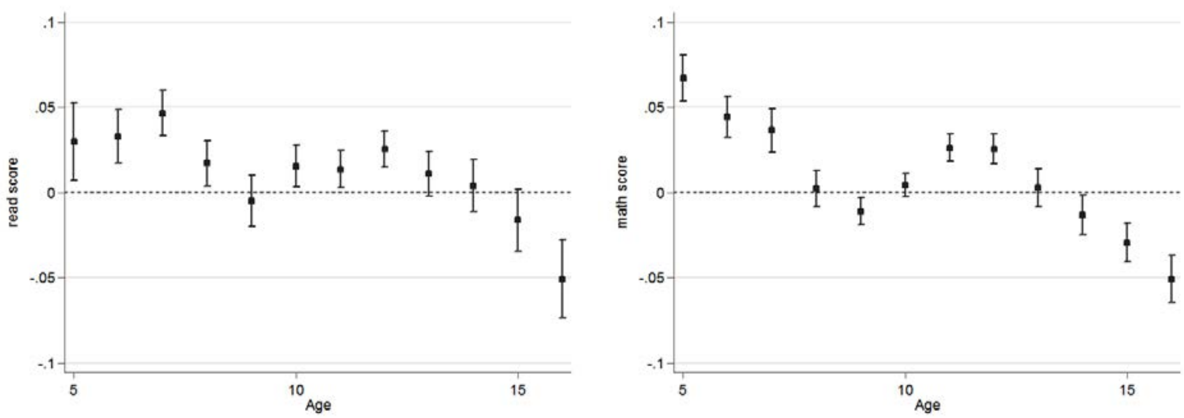
Figure 5: Total Effect of Early Life Shocks by Gender and Birth Order



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

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

(a) Cognitive Skill



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

Tables

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	Average child labor Primary activity Consumption Socioeconomic status Education Family size/number of children
National Family Health Survey (NFHS)	Repeated Cross-Section	1998 and 2015	Fertility
Annual Status of Education Report (ASER)	Repeated Cross-Section	2005-2014	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
Global Irrigated Area Mapping	Cross-Section	2001	Irrigation
Unified District Information System (DISE)	Cross-Section	2005	Education quality measures

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

Table 2: 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 Sugar/Cotton	0.01 (0.051)
Share Adults in Retail & Hotels	0.04 (0.033)
Share Adults in Manufacturing	0.04 (0.055)
Global Irrigated Area Mapping	
Share Irrigated	0.56 (0.348)

Notes: This table contains 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); Global Irrigated Mapping data (2001).

Table 3: Hedonic Wage Regression for Children

	(1)	(2)	(3)
		ln(wage)	
Height (cm)	0.0069***		0.0064**
	(0.002)		(0.003)
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	1307	766	604

Notes: This table reports the association between size, cognitive skill, and ln(earnings) conditional on working in the IHDS. Column 1 includes all children with wages, while columns 2 and 3 are restricted to children 15-17. All columns include district/round, age, and gender fixed effects. The outcome data are drawn from the IHDS 2012. The IHDS 2005 is used to provide information on lagged math scores from when an individual was 8-11.
Source: IHDS (2005, 2012).

Table 4: Effect of Early Life Shocks on Schooling

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. Primary Activity												
	Primary Activity Works (NSS)				Attends School (NSS)				Dropped Out (ASER)			
Early Life Rain	-0.0045***	-0.017***	-0.013***	-0.007***	0.005***	0.019***	0.015***	0.008***	-0.00003	-0.006***	-0.004***	-0.002***
	(0.0009)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.0003)	(0.0009)	(0.0006)	(0.0004)
Early Life Rain												
... × Child Labor		0.130***				-0.130***				0.064***		
		(0.024)				(0.024)				(0.009)		
... × (Above Median) Child Labor			0.016***				-0.018***				0.008***	
			(0.003)				(0.003)				(0.0012)	
... × (Top Quintile) Child Labor				0.016***				-0.015***				0.012***
				(0.004)				(0.004)				(0.001)
Mean Outcome	0.095	0.095	0.095	0.095	0.817	0.817	0.817	0.817	0.035	0.035	0.035	0.035
Total Effect			0.003**	0.008**			-0.003*	-0.006*			0.003***	0.008***
SE of Total Effect			(0.001)	(0.003)			(0.001)	(0.003)			(0.000)	(0.001)
Number Districts	574	574	574	574	574	574	574	574	559	559	559	559
Number Observations	486,699	486,699	486,699	486,699	486,699	486,699	486,699	486,699	7,608,681	7,608,681	7,608,681	7,608,681
B. Meals												
	Any Meals at School (NSS)				≥10 Meals at School (NSS)				≥20 Meals at School (NSS)			
Early Life Rain	0.008***	0.020***	0.018***	0.012***	0.0074***	0.018***	0.016***	0.011***	0.002**	0.010***	0.008***	0.004***
	(0.001)	(0.004)	(0.003)	(0.002)	(0.001)	(0.003)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)
Early Life Rain												
... × Child Labor		-0.120***				-0.110***				-0.077***		
		(0.033)				(0.032)				(0.026)		
... × (Above Median) Child Labor			-0.019***				-0.018***				-0.012***	
			(0.004)				(0.004)				(0.003)	
... × (Top Quintile) Child Labor				-0.019***				-0.017***				-0.008*
				(0.005)				(0.005)				(0.004)
Mean Outcome	0.243	0.243	0.243	0.243	0.228	0.228	0.228	0.228	0.153	0.153	0.153	0.153
Total Effect			-0.001	-0.006			-0.001	-0.0061			-0.003	-0.004
SE of Total Effect			(0.002)	(0.004)			(0.002)	(0.0045)			(0.002)	(0.003)
Number Districts	574	574	574	574	574	574	574	574	574	574	574	574
Number Observations	540,554	540,554	540,554	540,554	540,554	540,554	540,554	540,554	540,554	540,554	540,554	540,554

Notes: This table shows estimates of β_1 and β_2 from equation 1, with fixed effects for age, gender, and month/year/district (there is no month reported in the ASER data). The analysis includes all children between the ages of 5 and 17. The total effect row reports the total effect of a 1 unit increase in the aggregate rainfall shock in a district for whom the share rounds above median or the share rounds in the top quintile variable is equal to 1 (depending on the specification). Standard errors are clustered by district. Source: NSS Schedule 1 and 10 (2004-2012) and ASER (2005-2014).

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Primary Activity								
	Primary Activity Works (NSS)				Attends School (NSS)			
Early Life Rain	-0.002***	-0.016***	-0.012***	-0.006***	0.004***	0.018***	0.014***	0.007***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
Early Life Rain								
... × Oldest son	-0.007***	-0.001	-0.003**	-0.005***	0.005***	0.003	0.003*	0.005***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
... × Child Labor		0.140***				-0.140***		
		(0.025)				(0.025)		
... × Child Labor × Oldest son		-0.056**				0.023		
		(0.022)				(0.029)		
... × (Above Median) Child Labor			0.018***				-0.019***	
			(0.003)				(0.003)	
... × (Above Median) Child Labor × Oldest son			-0.007***				0.003	
			(0.002)				(0.003)	
... × (Top Quintile) Child Labor				0.018***				-0.015***
				(0.004)				(0.004)
... × (Top Quintile) Child Labor × Oldest son				-0.007*				0.0002
				(0.004)				(0.005)
Mean Outcome	0.095	0.095	0.095	0.095	0.817	0.817	0.817	0.817
Mean Outcome, Oldest Sons	0.077	0.077	0.077	0.077	0.848	0.848	0.848	0.848
Total Effect, Not-Oldest Sons			0.006***	0.012***			-0.004**	-0.007**
SE of Total Effect, Not-Oldest Sons			(0.002)	(0.004)			(0.001)	(0.003)
Total Effect, Oldest Sons			-0.005**	-0.001			0.002	-0.002
SE of Total Effect, Oldest Sons			(0.002)	(0.004)			(0.002)	(0.005)
Number Districts	574	574	574	574	574	574	574	574
Number Observations	486,699	486,699	486,699	486,699	486,699	486,699	486,699	486,699
B. With Additional Fixed Effects for District × Round × Age × Sex × Early Life Rain								
	Primary Activity Works (NSS)				Attends School (NSS)			
Early Life Rain								
... × Oldest son	-0.003***	0.002	0.0006	-0.001	0.002**	-0.003	-0.001	0.000
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
... × Child Labor × Oldest son		-0.060***				0.072**		
		(0.021)				(0.030)		
... × (Above Median) Child Labor × Oldest son			-0.008***				0.009**	
			(0.002)				(0.003)	
... × (Top Quintile) Child Labor × Oldest son				-0.008**				0.011*
				(0.003)				(0.006)
Mean Outcome	0.095	0.095	0.095	0.095	0.817	0.817	0.817	0.817
Mean Outcome, Oldest Sons	0.077	0.077	0.077	0.077	0.848	0.848	0.848	0.848
Number Districts	574	574	574	574	574	574	574	574
Number Observations	465,076	465,076	465,076	465,076	465,076	465,076	465,076	465,076

Notes: This table shows estimates of β_1 and β_2 from equation 1, with fixed effects for age, gender, and month/year/district. Oldest sons are imputed as the oldest male in the household who reports being a child (or grandchild) of the household head. Oldest sons are demographically different than other children, since they are older (and male). Panel B includes fixed effects for District/Month/Year/Age/Sex/Early Life Shocks, so the remaining variation compares oldest sons to not-oldest boys of the same age (as a result, we do not report the effect of early life shocks or the total effect of early life shocks for oldest sons, since the former is absorbed by the fixed effects). 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 or the share rounds in the top quintile variable is equal to 1 (depending on the specification) for oldest and non-oldest sons. Standard errors are clustered by district.
Source: NSS Schedule 10 (2004-2012).

Table 6: Effect of Early Life Shocks on Household Consumption

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Household Consumption								
	ln(Consumption Per Adult + 1/3 * kids) (NSS)				ln(Consumption Per Adult + 1/2 * kids) (NSS)			
Early Life Rain	0.001**	0.004***	0.003***	0.002***	0.001*	0.004***	0.002***	0.001**
	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)
Early Life Rain								
... × Child Labor		-0.034**				-0.034**		
		(0.014)				(0.014)		
... × (Above Median) Child Labor			-0.004**				-0.003**	
			(0.001)				(0.001)	
... × (Top Quintile) Child Labor				-0.004*				-0.004*
				(0.002)				(0.002)
Mean Outcome	7.070	7.070	7.070	7.070	6.980	6.980	6.980	6.980
Total Effect			-0.00058	-0.0017			-0.00076	-0.0021
SE of Total Effect			(0.0011)	(0.002)			(0.0011)	(0.002)
Number Districts	574	574	574	574	574	574	574	574
Number Observations	421,770	421,770	421,770	421,770	421,770	421,770	421,770	421,770
B. Indirect Measures of Consumption								
	Poor Fuel Source (NSS)				Food Share (NSS)			
Early Life Rain	-0.002***	-0.004**	-0.003***	-0.003***	0.000	-0.0009*	-0.0007**	-0.0003
	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Early Life Rain								
... × Child Labor		0.026				0.009*		
		(0.018)				(0.005)		
... × (Above Median) Child Labor			0.003				0.001***	
			(0.002)				(0.000)	
... × (Top Quintile) Child Labor				0.006**				0.001*
				(0.003)				(0.000)
Mean Outcome	0.791	0.791	0.791	0.791	0.573	0.573	0.573	0.573
Total Effect			-0.0003	0.0029			0.0009**	0.0015*
SE of Total Effect			(0.001)	(0.002)			(0.000)	(0.000)
Number Districts	574	574	574	574	574	574	574	574
Number Observations	225,916	225,916	225,916	225,916	225,916	225,916	225,916	225,916

Notes: This table shows estimates of β_1 and β_2 from equation 1, with fixed effects for age of the household head, and month/year/district. Each household is in the data once, and their exposure is coded as the male household head's exposure. Household heads are either reported household heads, married to the reported household head (if the head is female), or if neither exist, the oldest male under the age of 55. Poor Fuel Source is the primary source of energy for cooking, and it includes: coal, firewood and chips, dung cake, and others. Households whose heads are under 24 (and therefore, may not have completed their education) are not included. The total effect row reports the total effect of a 1 unit increase in the aggregate rainfall shock in a district for whom the share rounds above median or the share rounds in the top quintile variable is equal to 1 (depending on the specification). Standard errors are clustered by district.
Source: NSS Schedule 1 and 10 (2004-2012).

Table 7: Effect of Early Life Shocks on Fertility

	(1)	(2)	(3)	(4)
	Number of Children (NFHS)			
Early Life Rain	-0.017***	-0.026***	-0.027***	-0.019***
	(0.004)	(0.006)	(0.005)	(0.004)
Early Life Rain				
... × Child Labor		0.146**		
		(0.071)		
... × (Above Median) Child Labor			0.023***	
			(0.007)	
... × (Top Quintile) Child Labor				0.010
				(0.009)
Mean Outcome	2.840	2.840	2.840	2.840
Total Effect			-0.005	-0.009
SE of Total Effect			(0.006)	(0.008)
Number Districts	559	554	554	554
Number Observations	432,061	427,710	427,710	427,710

Notes: This table shows estimates of β_1 and β_2 from equation 1, with fixed effects for age, and year/district. Number of children is calculated as total children ever born. The total effect row reports the total effect of a 1 unit increase in the aggregate rainfall shock in a district for whom the share rounds above median or the share rounds in the top quintile variable is equal to 1 (depending on the specification). Standard errors are clustered by district.
Source: NFHS Round 2 (1998-1999) and Round 4 (2015-2016).

Table 8: Intergenerational Effect of Early Life Shocks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Primary Activity Works (NSS)				Attends School (NSS)			
Early Life Rain (Head)	0.0009*	-0.0018*	-0.0010	-0.0000	-0.0009	0.0024	0.0017	0.0004
	(0.0005)	(0.0010)	(0.0007)	(0.0005)	(0.0007)	(0.0016)	(0.0011)	(0.0008)
Early Life Rain (Head)								
... × Child Labor		0.026**				-0.030*		
		(0.011)				(0.017)		
... × (Above Median) Child Labor			0.003**				-0.004*	
			(0.001)				(0.002)	
... × (Top Quintile) Child Labor				0.003				-0.004
				(0.002)				(0.003)
Mean Outcome	0.095	0.095	0.095	0.095	0.817	0.817	0.817	0.817
Total Effect			0.002**	0.003*			-0.002*	-0.004
SE of Total Effect			(0.001)	(0.002)			(0.0016)	(0.003)
Number Districts	574	577	577	577	574	577	577	577
Number Observations	465,076	465,305	465,305	465,305	465,076	465,305	465,305	465,305

Notes: This table shows estimates of β_1 and β_2 from equation 1, with fixed effects for age of the household head, month/year/district, age, gender, and the direct effect of early life shocks for the children. Exposure is imputed with that of the male household head. Household heads are either reported household heads, married to the reported household head, or if neither exist, the oldest male under the age of 55. The total effect row reports the total effect of a 1 unit increase in the aggregate rainfall shock in a district for whom the share rounds above median or the share rounds in the top quintile variable is equal to 1 (depending on the specification). Standard errors are clustered by district.
Source: NSS Schedule 10 (2004-2012).

Table 9: What Predicts Child Labor?

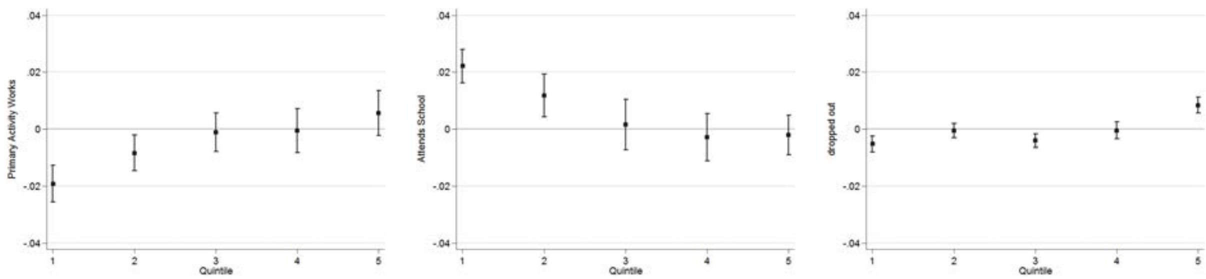
	(1)	(2)	(3)	(4)	(5)
A. Mean Child Labor					
Average Adult Wage	-0.011***				
	(0.0011)				
Education Index		-0.033***			
		(0.0016)			
School Quality Index			0.0021		
			(0.0042)		
Percent Adults Work in Agriculture				0.048***	
				(0.0096)	
Percent Adults Work in Sugar/Cotton					0.150***
					(0.036)
Mean Outcome	0.096	0.096	0.096	0.096	0.096
Number Observations	556	556	556	556	556
B. Above Median Labor					
Average Adult Wage	-0.086***				
	(0.007)				
Education Index		-0.230***			
		(0.0097)			
School Quality Index			0.025		
			(0.032)		
Percent Adults Work in Agriculture				0.350***	
				(0.072)	
Percent Adults Work in Sugar/Cotton					1.090***
					(0.230)
Mean Outcome	0.500	0.500	0.500	0.500	0.500
Number Observations	556	556	556	556	556
C. Top Quintile Child Labor					
Average Adult Wage	-0.044***				
	(0.0048)				
Education Index		-0.140***			
		(0.011)			
School Quality Index			0.029		
			(0.024)		
Percent Adults Work in Agriculture				0.180***	
				(0.049)	
Percent Adults Work in Sugar/Cotton					0.790***
					(0.240)
Mean Outcome	0.201	0.201	0.201	0.201	0.201
Number Observations	556	556	556	556	556

Notes: This table reports coefficients from regressions of measures of child labor on district characteristics. An observation is a district. "Mean wages" is the average adult (age 25-60) wage reported in the NSS, "Percent of Adults in Agriculture" is the percent of adults (age 25-60) who report their primary industry as agriculture in the NSS, and "Percent of Adults in Sugar/Cotton" is the percent of adults (age 25-60) who report cultivating either sugar or cotton in the NSS. The "Education Index" includes average literacy, average female literacy, and average primary school completion rate from the NSS. The "School Quality Index" includes the percent of schools with a girls' toilet, the percent of school with a blackboard, the average number of textbooks per school, the (negative) percent of schools with more than 60 students per classroom, the (negative) percent of schools with no building from the DISE, and the number of schools per capita. Both indices are calculated by standardizing the components and taking their average. Robust standard errors are reported in parentheses.
Source: NSS Schedule 10 (2004-2012) and DISE (2005).

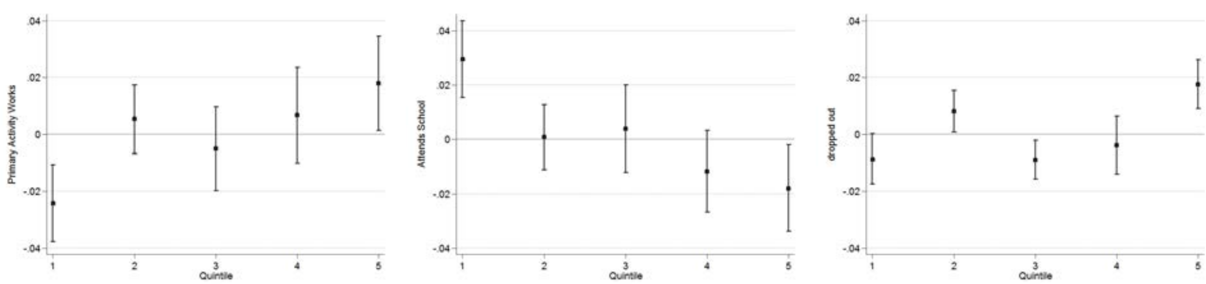
Appendix Figures

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

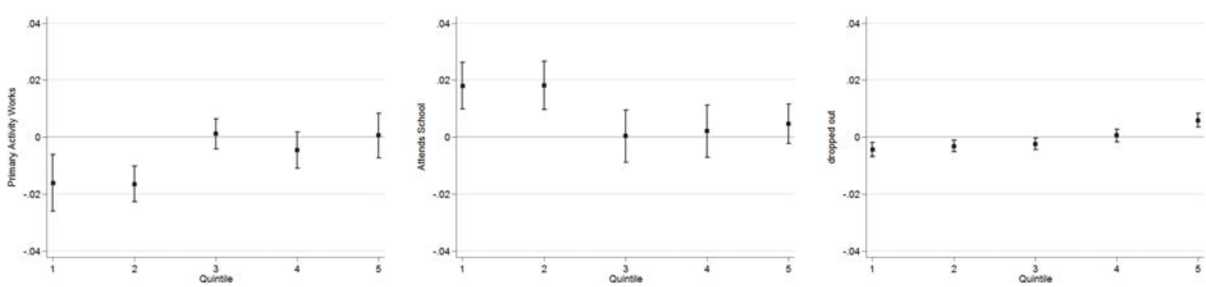
(a) All Children (Ages 5-17)



(b) Older Children (> 13)



(c) Younger Children (≤ 13)



Notes: Districts are categorized by their leave-out mean quintile of child labor intensity. This figure then plots the total effect of early life shocks within each quintile. Panel (a) includes all children jointly, Panel (b) focuses on older children (>13), and Panel (c) on younger children ($\leq$ 13). The coefficients in Panel (a) are all calculated in one regression, and Panels (b) and (c) are calculated from the same regression. The regressions have the standard fixed effects in the tables (gender, age, and district/month/year). Standard errors are clustered by district.
Source: NSS Schedule 10 (2004-2012).

Appendix Tables

Table A1: Effect of Early Life Shocks on Height and Early Test Scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Height (IHDS)				Math Score (ASER)				Reading Score (ASER)			
Early Life Rain	0.260***	0.110	0.240**	0.200**	0.016***	0.011**	0.010**	0.013***	0.011***	0.013	0.014*	0.011**
	(0.075)	(0.110)	(0.099)	(0.086)	(0.002)	(0.005)	(0.004)	(0.002)	(0.003)	(0.010)	(0.007)	(0.005)
Early Life Rain												
... × Child Labor		2.490**				0.037				-0.012		
		(1.250)				(0.046)				(0.088)		
... × (Above Median) Child Labor			0.029				0.009				-0.004	
			(0.130)				(0.006)				(0.012)	
... × (Top Quintile) Child Labor				0.290**				0.009				0.001
				(0.130)				(0.007)				(0.014)
Mean Outcome	128	128	128	128	0.493	0.493	0.493	0.493	1.110	1.110	1.110	1.110
Total Effect			0.270***	0.490***			0.020***	0.022***			0.009	0.013
SE of Total Effect			(0.100)	(0.100)			(0.003)	(0.005)			(0.006)	(0.012)
Number Districts	342	342	342	342	562	559	559	559	562	559	559	559
Number Observations	27,219	27,219	27,219	27,219	1,770,010	1,763,023	1,763,023	1,763,023	1,770,010	1,763,023	1,763,023	1763023

Notes: This table shows estimates of β_1 and β_2 from equation 1 for children aged 5-17, with fixed effects for age, gender, and year/district. The total effect row reports the total effect of a 1 unit increase in the aggregate rainfall shock in a district for whom the share rounds above median or the share rounds in the top quintile variable is equal to 1 (depending on the specification). Standard errors are clustered by district.
Source: IHDS (2012) and ASER (2005-2014).

Table A2: Effect of Early Life Shocks on Activity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Sector (NSS)										
	Agriculture		Retail/Hotels		Other Services		Manufacturing		Other	
Early Life Rain	-0.0006	-0.0043***	0.000035	-0.000051	-0.000022	-0.00012***	-0.00066***	-0.0013***	-0.0040	0.0087
	(0.00067)	(0.0011)	(0.000030)	(0.000051)	(0.000024)	(0.000040)	(0.00023)	(0.00044)	(0.0031)	(0.0061)
Early Life Rain										
... × (Above Median) Child Labor		0.0076***		0.00017**		0.00021***		0.0013*		-0.019**
		(0.0021)		(0.000089)		(0.000062)		(0.00070)		(0.0092)
Mean Outcome	0.033	0.033	0.0003	0.0003	0.0002	0.0002	0.007	0.007	0.579	0.579
Total Effect		0.0032**		0.00012**		0.000082**		0.000016		-0.011**
SE of Total Effect		(0.0013)		(0.00006)		(0.000038)		(0.00040)		(0.005)
Number Districts	574	574	574	574	574	574	574	574	567	567
Number Observations	486,699	486,699	486,699	486,699	486,699	486,699	486,699	486,699	41,019	41,019
B. Location (NSS)										
	Goes to School		Household Work		Salaried Work		Casual Wage Labor		Other	
Early Life Rain	0.0059***	0.015***	-0.0017**	-0.0061***	-0.00051***	-0.00077***	-0.0023***	-0.0057***	-0.0015*	-0.0025
	(0.0011)	(0.0021)	(0.00080)	(0.0014)	(0.00012)	(0.00021)	(0.00042)	(0.00061)	(0.00090)	(0.0017)
Early Life Rain										
... × (Above Median) Child Labor		-0.018***		0.0089***		0.00053*		0.0069***		0.0020
		(0.0033)		(0.0024)		(0.00030)		(0.0012)		(0.0027)
Mean Outcome	0.817	0.817	0.072	0.072	0.003	0.003	0.020	0.020	0.087	0.087
Total Effect		-0.0033*		0.0028*		-0.00025		0.0012		-0.00048
SE of Total Effect		(0.0018)		(0.0015)		(0.00017)		(0.0008)		(0.0015)
Number Districts	574	574	574	574	574	574	574	574	574	574
Number Observations	486,699	486,699	486,699	486,699	486,699	486,699	486,699	486,699	486,699	486,699

Notes: This table shows estimates of β_1 and β_2 from equation 1, with fixed effects for age, gender, and month/year/district. The total effect row reports the total effect of a 1 unit increase in the aggregate rainfall shock in a district for whom the share rounds above median or the share rounds in the top quintile variable is equal to 1 (depending on the specification). Standard errors are clustered by district.
Source: NSS Schedule 10 (2004-2012).

Table A3: Effect of Early Life Shocks on Schooling by Age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Primary Activity Works (NSS)				Attends School (NSS)			
Early Life Rain	-0.0068***	-0.017***	-0.014***	-0.0093***	0.0085***	0.017***	0.015***	0.010***
	(0.0010)	(0.0031)	(0.0021)	(0.0013)	(0.0013)	(0.0032)	(0.0024)	(0.0016)
Early Life Rain								
... × > 13	0.0068***	-0.0010	0.0026	0.0047*	-0.0073***	0.0066	0.00057	-0.0038
	(0.0022)	(0.0059)	(0.0040)	(0.0026)	(0.0023)	(0.0058)	(0.0040)	(0.0027)
... × Child Labor		0.10***				-0.086***		
		(0.029)				(0.027)		
... × Child Labor × > 13		0.089				-0.15***		
		(0.058)				(0.057)		
... × (Above Median) Child Labor			0.013***				-0.013***	
			(0.0033)				(0.0036)	
... × (Above Median) Child Labor × > 13			0.0096				-0.017**	
			(0.0070)				(0.0072)	
... × (Top Quintile) Child Labor				0.012***				-0.0090**
				(0.0044)				(0.0043)
... × (Top Quintile) Child Labor × > 13				0.014				-0.021**
				(0.0098)				(0.0100)
Mean Outcome	0.095	0.095	0.095	0.095	0.817	0.817	0.817	0.817
Mean Outcome, >13	0.258	0.258	0.258	0.258	0.703	0.703	0.703	0.703
Total Effect, ≤13			-0.00018	0.003			0.0021	0.0013
SE of Total Effect, ≤13			(0.0017)	(0.0038)			(0.002)	(0.0035)
Total Effect, > 13			0.012***	0.022***			-0.014***	-0.024***
SE of Total Effect, > 13			(0.004)	(0.008)			(0.004)	(0.008)
Number Districts	574	574	574	574	574	574	574	574
Number Observations	486,699	486,699	486,699	486,699	486,699	486,699	486,699	486,699

Notes: This table shows estimates of β_1 and β_2 from equation 1 by age, with fixed effects for age, gender, and month/year/district. 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 or the share rounds in the top quintile variable is equal to 1 (depending on the specification). Standard errors are clustered by district.
Source: NSS Schedule 10 (2004-2012).

Table A4: Effect of Early Life Shocks on Schooling by Gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Primary Activity Works (NSS)				Attends School (NSS)			
Early Life Rain	-0.0082***	-0.019***	-0.016***	-0.011***	0.0090***	0.021***	0.018***	0.012***
	(0.0011)	(0.0029)	(0.0021)	(0.0014)	(0.0013)	(0.0032)	(0.0024)	(0.0016)
Early Life Rain								
... × Girl	0.0080***	0.0049*	0.0068***	0.0072***	-0.0066***	-0.0052	-0.0058***	-0.0065***
	(0.0013)	(0.0027)	(0.0019)	(0.0015)	(0.0015)	(0.0031)	(0.0020)	(0.0016)
... × Child Labor		0.110***				-0.130***		
		(0.027)				(0.030)		
... × Child Labor × Girl		0.032				-0.014		
		(0.028)				(0.035)		
... × (Above Median) Child Labor			0.015***				-0.018***	
			(0.0033)				(0.0038)	
... × (Above Median) Child Labor × Girl			0.0024				-0.0015	
			(0.0036)				(0.0041)	
... × (Top Quintile) Child Labor				0.015***				-0.015***
				(0.0044)				(0.0049)
... × (Top Quintile) Child Labor × Girl				0.0036				-0.00019
				(0.0054)				(0.0065)
Mean Outcome	0.095	0.095	0.095	0.095	0.817	0.817	0.817	0.817
Mean Outcome, Girls	0.123	0.123	0.123	0.123	0.790	0.790	0.790	0.790
Total Effect, Boys			-0.00057	0.0035			0.00015	-0.0032
SE of Total Effect, Boys			(0.0018)	(0.0037)			(0.0022)	(0.0042)
Total Effect, Girls			0.0086***	0.014***			-0.0071***	-0.0098**
SE of Total Effect, Girls			(0.0026)	(0.005)			(0.0025)	(0.0049)
Number Districts	574	574	574	574	574	574	574	574
Number Observations	486,699	486,699	486,699	486,699	486,699	486,699	486,699	486,699

Notes: This table shows estimates of β_1 and β_2 from equation 1 by gender, with fixed effects for age, gender, and month/year/district. 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 or the share rounds in the top quintile variable is equal to 1 (depending on the specification). Standard errors are clustered by district.
Source: NSS Schedule 10 (2004-2012).

Table A5: Effect of Early Life Shocks on Schooling by Caste

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Primary Activity Works (NSS)				Attends School (NSS)			
Early Life Rain	-0.0053***	-0.013***	-0.011***	-0.0074***	0.0060***	0.016***	0.014***	0.0082***
	(0.0010)	(0.0027)	(0.0020)	(0.0013)	(0.0014)	(0.0034)	(0.0026)	(0.0018)
Early Life Rain								
... × SC/ST	0.0023	-0.0097**	-0.0050*	-0.00078	0.000053	0.0073	0.0038	0.0022
	(0.0016)	(0.0038)	(0.0028)	(0.0020)	(0.0020)	(0.0047)	(0.0037)	(0.0025)
... × Child Labor		0.084***				-0.11***		
		(0.026)				(0.029)		
... × Child Labor × SC/ST		0.120***				-0.072*		
		(0.035)				(0.041)		
... × (Above Median) Child Labor			0.012***				-0.016***	
			(0.0032)				(0.0040)	
... × (Above Median) Child Labor × SC/ST			0.014***				-0.0068	
			(0.0045)				(0.0055)	
... × (Top Quintile) Child Labor				0.011**				-0.012**
				(0.0045)				(0.0048)
... × (Top Quintile) Child Labor × SC/ST				0.014**				-0.0094
				(0.0058)				(0.0066)
Mean Outcome	0.095	0.095	0.095	0.095	0.817	0.817	0.817	0.817
Mean Outcome, SC/ST	0.109	0.109	0.109	0.109	0.790	0.790	0.790	0.790
Total Effect, not SC/ST			0.00067	0.0039			-0.0022	-0.0037
SE of Total Effect, not SC/ST			(0.0019)	(0.0039)			(0.0021)	(0.0039)
Total Effect, SC/ST			0.0096***	0.017***			-0.0053**	-0.011**
SE of Total Effect, SC/ST			(0.0027)	(0.005)			(0.0027)	(0.005)
Number Districts	574	574	574	574	574	574	574	574
Number Observations	486,699	486,699	486,699	486,699	486,699	486,699	486,699	486,699

Notes: This table shows estimates of β_1 and β_2 from equation 1, with fixed effects for age, gender, month/year/district, and caste. 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 or the share rounds in the top quintile variable is equal to 1 (depending on the specification). Standard errors are clustered by district.
Source: NSS Schedule 10 (2004-2012).

Table A6: 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	0.0051	0.0042	-0.0097	-0.0081***	0.011	-0.033***	-0.0061	0.0053	-0.0011	0.011***	-0.014	0.024
	(0.0043)	(0.015)	(0.0087)	(0.0025)	(0.015)	(0.012)	(0.0048)	(0.025)	(0.0098)	(0.0031)	(0.022)	(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)	(0.0030)	(0.0029)	(0.0034)	(0.0033)	(0.0031)	(0.0048)	(0.0032)	(0.0033)	(0.0041)	(0.0040)
Mean Outcome	0.924	0.924	0.924	0.924	0.924	0.924	0.043	0.043	0.043	0.043	0.043	0.043
Additional Controls	Income	Education	School Quality	HH Info	All	LASSO	Income	Education	School Quality	HH Info	All	LASSO
Number Districts	574	562	562	574	562	562	574	562	562	574	562	562
Number Observations	486,442	475,769	475,769	486,699	475,512	475,677	486,442	475,769	475,769	486,699	475,512	475,677

Notes: This table shows estimates of β_1 and β_2 from equation 1, with fixed effects for age, gender, month/year/district, including rich controls. Additional district or household characteristics are interacted with early life shocks and included as additional controls, as described in the text. "Income controls" consist of average adult wages and share of the population that works for a wage. "Education controls" consist of average adult literacy, share of adults that graduated primary school, and share of adults who graduated secondary school. "School quality controls" consist of share of classrooms with more than 60 students, share of schools with girls' toilets, share of schools with blackboards, share of schools with no building, average number of textbooks at a school, number of schools per capita, share of single teacher schools, and share of single classroom schools. "Household controls" consist of a control for if the male head is literate, whether he completed primary and secondary school, whether he works for a wage, household size, and number of child. The full controls include all the controls above. "LASSO" selects the controls from the full set following the post-double-selection approach proposed by Belloni et al. (2012, 2016) (the main fixed effects are always included). Standard errors are clustered by district. Standard errors are clustered by district.
Source: NSS Schedule 10 (2004-2012) and DISE (2005).

Table A7: Effect of Early Life Shocks Within Households

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. Household Fixed Effects				
	Primary Activity Works (NSS)				Attends School (NSS)			
Early Life Rain	-0.0041***	-0.014***	-0.011***	-0.0066***	0.0050***	0.013***	0.011***	0.0067***
	(0.00094)	(0.0024)	(0.0017)	(0.0012)	(0.0011)	(0.0028)	(0.0022)	(0.0014)
Early Life Rain								
... × Child Labor		0.10***				-0.087***		
		(0.024)				(0.026)		
... × (Above Median) Child Labor			0.013***				-0.013***	
			(0.0030)				(0.0035)	
... × (Top Quintile) Child Labor				0.013***				-0.0087**
				(0.0044)				(0.0043)
Mean Outcome	0.095	0.095	0.095	0.095	0.817	0.817	0.817	0.817
Total Effect			0.0025	0.0063			-0.0013	-0.002
SE of Total Effect			(0.0019)	(0.0039)			(0.0019)	(0.0036)
Number Districts	574	574	574	574	574	574	574	574
Number Observations	410,097	410,097	410,097	410,097	410,097	410,097	410,097	410,097
				B. Number of Early Life Shocks of Siblings				
	Primary Activity Works (NSS)				Attends School (NSS)			
Early Life Rain	-0.0039***	-0.015***	-0.011***	-0.0069***	0.0049***	0.016***	0.013***	0.0075***
	(0.00092)	(0.0023)	(0.0017)	(0.0011)	(0.0012)	(0.0029)	(0.0023)	(0.0015)
Early Life Rain								
... × Child Labor		0.120***				-0.12***		
		(0.023)				(0.025)		
... × (Above Median) Child Labor			0.015***				-0.017***	
			(0.0029)				(0.0035)	
... × (Top Quintile) Child Labor				0.015***				-0.013***
				(0.0042)				(0.0043)
Sibling Early Life Rain	-0.00066**	-0.00090	-0.00092**	-0.00077**	0.00090**	0.0013	0.00094	0.0012***
	(0.00027)	(0.00057)	(0.00042)	(0.00030)	(0.00036)	(0.00080)	(0.00058)	(0.00042)
Sibling Early Life Rain								
... × Child Labor		0.0030				-0.0048		
		(0.0059)				(0.0083)		
... × (Above Median) Child Labor			0.00065				-0.00019	
			(0.00079)				(0.0011)	
... × (Top Quintile) Child Labor				0.00069				-0.0015
				(0.0011)				(0.0015)
Mean Outcome	0.089	0.089	0.089	0.089	0.821	0.821	0.821	0.821
Total Effect			0.0036**	0.0085**			-0.0036*	-0.0056
SE of Total Effect			(0.0018)	(0.0037)			(0.0019)	(0.0035)
Total Spillover Effect			-0.00027	-0.000075			0.00074	-0.00031
SE of Total Spillover Effect			(0.00054)	(0.00096)			(0.00073)	(0.0013)
Number Districts	574	574	574	574	574	574	574	574
Number Observations	440,835	440,835	440,835	440,835	440,835	440,835	440,835	440,835

Notes: This table shows estimates of β_1 and β_2 from equation 1, with fixed effects for age, gender, month/year/district, comparing children within the same household. Panel A also includes fixed effects for households. Panel B instead calculates the total household shock of the siblings (as in Table A15), with additional fixed effects for the household composition of the siblings and estimates the effects of siblings' shocks on own outcomes. The sample is restricted to children aged 5-17. 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 or the share rounds in the top quintile variable is equal to 1 (depending on the specification). Standard errors are clustered by district.
Source: NSS Schedule 10 (2004-2012) and DISE (2005).

Table A8: Effect of Early Life Shocks in Sugar/Cotton Districts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Reduced form									
	Primary Activity Works (NSS)			Attends School (NSS)			Dropped Out (ASER)		
Early Life Rain	-0.0045***	-0.0052***	-0.0072***	0.0060***	0.0062***	0.0084***	-0.000030	-0.00012	-0.00075
	(0.00096)	(0.0010)	(0.0012)	(0.0011)	(0.0012)	(0.0014)	(0.00038)	(0.00040)	(0.00046)
Early Life Rain									
... × Sugar/Cotton		0.028**			-0.0085			0.0027	
		(0.011)			(0.013)			(0.0042)	
... × Has Sugar/Cotton			0.0071***			-0.0063***			0.0019**
			(0.0020)			(0.0023)			(0.00087)
Mean Outcome	0.095	0.095	0.095	0.817	0.817	0.817	0.035	0.035	0.035
Total Effect			-0.00015			0.0021			0.0012
SE of Total Effect			(0.0016)			(0.0018)			(0.0007)
Number Districts	574	574	574	574	574	574	559	559	559
Number Observations	486,788	486,788	486,788	486,788	486,788	486,788	7,609,826	7,609,826	7,609,826
B. IV									
	Primary Activity Works (NSS)			Attends School (NSS)			Dropped Out (ASER)		
Early Life Rain	-0.0045***	-0.025***	-0.021***	0.0060***	0.012	0.020***	-0.000030	-0.0021	-0.0061**
	(0.00096)	(0.0086)	(0.0049)	(0.0011)	(0.0094)	(0.0053)	(0.00038)	(0.0032)	(0.0026)
Early Life Rain									
... × Child Labor		0.21**			-0.065			0.020	
		(0.089)			(0.097)			(0.030)	
... × (Above Median) Child Labor			0.033***			-0.029***			0.011**
			(0.0098)			(0.010)			(0.0049)
Mean Outcome	0.095	0.095	0.095	0.817	0.817	0.817	0.035	0.035	0.035
First Stage F statistic		22	35		22	35		20.8	27.7
Total Effect			0.012**			-0.0089*			0.0052**
SE of Total Effect			(0.005)			(0.0054)			(0.0023)
Number Districts	574	574	574	574	574	574	559	559	559
Number Observations	486,788	486,699	486,699	486,788	486,699	486,699	7,609,826	7,608,681	7,608,681

Notes: This table shows estimates of β_1 and β_2 from equation 1, with fixed effects for age, gender, month/year/district, exploiting variation in sugar and cotton. Panel A shows the reduced form effect of sugar cotton production. Panel B instead uses sugar and cotton production as an IV for child labor, instrumenting for mean child labor with the share of adults, and above median & top quintile with an indicator for any sugar/cotton production. The analysis includes all children between the ages of 5 and 17. The total effect row reports the total effect of a 1 unit increase in the aggregate rainfall shock in a district for whom the share rounds above median or the share rounds in the top quintile variable is equal to 1 (depending on the specification). Standard errors are clustered by district.
Source: NSS Schedule 10 (2004-2012).

Table A9: Effect of Early Life Shocks with Inclusion of Additional Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. Fixed Effects for Early Life Shocks × Age × Gender & District ×Time												
	Primary Activity Works (NSS)				Attends School (NSS)				Dropped Out (ASER)			
Early Life Rain												
... × Child Labor		0.160***				-0.160***				0.070***		
		(0.026)				(0.023)				(0.0094)		
... × (Above Median) Child Labor			0.020***				-0.020***				0.0094***	
			(0.0032)				(0.0030)				(0.0012)	
... × (Top Quintile) Child Labor				0.021***				-0.019***				0.012***
				(0.0041)				(0.0039)				(0.0016)
Mean Outcome		0.095	0.095	0.095		0.817	0.817	0.817		0.035	0.035	0.035
Number Districts		574	574	574		574	574	574		559	559	559
Number Observations		486,693	486,693	486,693		486,693	486,693	486,693		7,608,681	7,608,681	7,608,681
B. Separate Fixed Effects for District, Survey Round, Age, and Gender												
	Primary Activity Works (NSS)				Attends School (NSS)				Dropped Out (ASER)			
Early Life Rain	-0.0044***	-0.018***	-0.013***	-0.0077***	0.0061***	0.020***	0.016***	0.0092***	-0.00021	-0.0062***	-0.0043***	-0.0025***
	(0.00097)	(0.0024)	(0.0017)	(0.0012)	(0.0011)	(0.0026)	(0.0019)	(0.0014)	(0.00034)	(0.00087)	(0.00064)	(0.00041)
Early Life Rain												
... × Child Labor		0.14***				-0.14***				0.058***		
		(0.024)				(0.024)				(0.0081)		
... × (Above Median) Child Labor			0.018***				-0.019***				0.0075***	
			(0.0030)				(0.0032)				(0.0011)	
... × (Top Quintile) Child Labor				0.017***				-0.016***				0.010***
				(0.0043)				(0.0042)				(0.0013)
Mean Outcome	0.095	0.095	0.095	0.095	0.817	0.817	0.817	0.817	0.035	0.035	0.035	0.035
Total Effect			0.0049***	0.0096**			-0.0036*	-0.0066*			0.0032***	0.0076***
SE of Total Effect			(0.0019)	(0.0038)			(0.0019)	(0.0036)			(0.0006)	(0.0011)
Number Districts	574	574	574	574	574	574	574	574	559	559	559	559
Number Observations	486,883	486,883	486,883	486,883	486,883	486,883	486,883	486,883	7,608,681	7,608,681	7,608,681	7,608,681

Notes: This table shows estimates of β_1 and β_2 from equation 1, with different fixed effects than in the main tables. Panel A includes finer fixed effects for the early life shock by age by gender, as well as district/month/year. As a result, the total effect is not reported, since the baseline rainfall effect (in low child labor districts) is absorbed by the former set of fixed effects. Panel B includes a coarser set of fixed effects, for District, Round, Age, and Gender separately. The analysis includes all children between the ages of 5 and 17. The total effect row reports the total effect of a 1 unit increase in the aggregate rainfall shock in a district for whom the share rounds above median or the share rounds in the top quintile variable is equal to 1 (depending on the specification). Standard errors are clustered by district.
Source: NSS Schedule 10 (2004-2012).

Table A10: Effect of Early Life Shocks in Irrigated Districts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Primary Activity Works (NSS)				Attends School (NSS)			
Early Life Rain	-0.010***	-0.018***	-0.0086***	-0.017***	0.017***	0.024***	0.014***	0.022***
	(0.0022)	(0.0031)	(0.0018)	(0.0025)	(0.0024)	(0.0035)	(0.0021)	(0.0031)
Early Life Rain								
... × Share Irrigated	0.0099***	0.013***			-0.020***	-0.022***		
	(0.0031)	(0.0047)			(0.0032)	(0.0049)		
... × High Irrigation			0.0067***	0.0095***			-0.013***	-0.015***
			(0.0021)	(0.0030)			(0.0025)	(0.0039)
... × (Above Median) Child Labor		0.024***		0.021***		-0.022***		-0.021***
		(0.0060)		(0.0046)		(0.0065)		(0.0052)
... × (Above Median) Child Labor × Share Irrigated		-0.018**				0.016*		
		(0.0090)				(0.0090)		
... × (Above Median) Child Labor × High Irrigation				-0.012**				0.011*
				(0.0058)				(0.0065)
Mean Outcome	0.095	0.095	0.095	0.095	0.817	0.817	0.817	0.817
Mean Outcome, Over Half Irrigated	0.105	0.105	0.105	0.105	0.796	0.796	0.796	0.796
Total Effect, Below Half Irrigated				0.0046				0.001
SE of Total Effect, Below Half Irrigated				(0.0034)				(0.0035)
Total Effect, Above Half Irrigated				0.002				-0.0029
SE of Total Effect, Above Half Irrigated				(0.0022)				(0.0021)
Number Districts	573	573	576	573	573	573	576	573
Number Observations	485,847	485,847	486,177	485,847	485,847	485,847	486,177	485,847

Notes: This table shows estimates of β_1 and β_2 from equation 1, with fixed effects for age, gender, month/year/district by district irrigation. Measures of irrigation are either the share of cropland irrigated, or an indicator variable for $> 50\%$ irrigated. The analysis includes all children between the ages of 5 and 17. 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. Standard errors are clustered by district.
Source: NSS Schedule 10 (2004-2012) and Global Irrigated Area Mapping (2001).

Table A11: Effect of Early Life Shocks Using the First Round of NSS to Categorize Districts

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Primary Activity Works (NSS)				Attends School (NSS)			
Early Life Rain	-0.0045***	-0.012***	-0.0081***	-0.0061***	0.0059***	0.013***	0.0099***	0.0073***
	(0.00096)	(0.0020)	(0.0013)	(0.0010)	(0.0011)	(0.0021)	(0.0017)	(0.0013)
Early Life Rain								
... × Child Labor		0.063***				-0.064***		
		(0.016)				(0.015)		
... × (Above Median) Child Labor			0.0080***				-0.0087***	
			(0.0020)				(0.0023)	
... × (Top Quintile) Child Labor				0.0087***				-0.0075***
				(0.0028)				(0.0027)
Mean Outcome	0.095	0.095	0.095	0.095	0.817	0.817	0.817	0.817
Total Effect			-0.000059	0.0026			0.0011	-0.00022
SE of Total Effect			(0.0015)	(0.0026)			(0.0015)	(0.0023)
Number Districts	574	562	562	562	574	562	562	562
Number Observations	486,699	482,138	482,138	482,138	486,699	482,138	482,138	482,138

Notes: This table shows estimates of β_1 and β_2 from equation 1, with fixed effects for age, gender, month/year/district. The analysis includes all children between the ages of 5 and 17. 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.
Source: NSS Schedule 10 (2004-2012).

Table A12: Effect of Early Life Shocks Using Finer Variation to Determine Child labor Prevalence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. By District & Gender												
	Primary Activity Works (NSS)				Attends School (NSS)				Dropped Out (ASER)			
Early Life Rain	-0.0045***	-0.016***	-0.014***	-0.0087***	0.0059***	0.017***	0.015***	0.0100***	-0.000030	-0.0054***	-0.0040***	-0.0023***
	(0.00096)	(0.0018)	(0.0016)	(0.0011)	(0.0011)	(0.0021)	(0.0019)	(0.0014)	(0.00038)	(0.00066)	(0.00057)	(0.00040)
Early Life Rain												
...× Child Labor		0.12***				-0.12***				0.053***		
		(0.017)				(0.017)				(0.0062)		
... × (Above Median) Child Labor			0.019***				-0.019***				0.0076***	
			(0.0028)				(0.0028)				(0.00096)	
... × (Top Quintile) Child Labor				0.023***				-0.022***				0.011***
				(0.0037)				(0.0036)				(0.0012)
Mean Outcome	0.095	0.095	0.095	0.095	0.817	0.817	0.817	0.817	0.035	0.035	0.035	0.035
Total Effect			0.0055***	0.014***			-0.0039**	-0.012***			0.0037***	0.0085***
SE of Total Effect			(0.0018)	(0.003)			(0.0016)	(0.003)			(0.0007)	(0.0011)
Number Districts	574	573	573	573	574	573	573	573	559	559	559	559
Number Observations	486,699	485,980	485,980	485,980	486,699	485,980	485,980	485,980	7,608,681	7,582,758	7,582,758	7,582,758
B. By District & SC/ST												
	Primary Activity Works (NSS)				Attends School (NSS)							
Early Life Rain	-0.0045***	-0.015***	-0.013***	-0.0075***	0.0059***	0.016***	0.016***	0.0092***				
	(0.00096)	(0.0019)	(0.0017)	(0.0011)	(0.0011)	(0.0022)	(0.0021)	(0.0014)				
Early Life Rain												
... × Child Labor		0.11***				-0.11***						
		(0.019)				(0.019)						
... × (Above Median) Child Labor			0.018***				-0.020***					
			(0.0030)				(0.0033)					
... × (Top Quintile) Child Labor				0.017***				-0.019***				
				(0.0042)				(0.0042)				
Mean Outcome	0.095	0.095	0.095	0.095	0.817	0.817	0.817	0.817				
Total Effect			0.0048**	0.0099***			-0.0042**	-0.0094***				
SE of Total Effect			(0.0019)	(0.0038)			(0.0019)	(0.0035)				
Number Districts	574	573	573	573	574	573	573	573				
Number Observations	486,699	473,276	473,276	473,276	486,699	473,276	473,276	473,276				

Notes: This table shows estimates of β_1 and β_2 from equation 1, with fixed effects for age, gender, month/year/district. District classifications use the leave-out mean by gender (panel A) or caste (panel B), as described in the text. The analysis includes all children between the ages of 5 and 17. We exclude ASER in Panel B, since it does not include caste data. 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. Standard errors are clustered by district.
Source: NSS Schedule 10 (2004-2012) and ASER (2005-2014).

Table A13: Mincerian Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Consumption Per Adult + 1/3*kids) (IHDS)				ln(Consumption Per Adult + 1/2*kids) (IHDS)			
Years of School	0.038***	0.038***	0.037***	0.038***	0.041***	0.040***	0.039***	0.040***
	(0.0012)	(0.0023)	(0.0018)	(0.0014)	(0.0013)	(0.0023)	(0.0019)	(0.0014)
Years of School								
... × Child Labor		0.0013				0.0086		
		(0.028)				(0.028)		
... × (Above Median) Child Labor			0.0020				0.0026	
			(0.0022)				(0.0022)	
... × (Top Quintile) Child Labor				0.0030				0.0035
				(0.0025)				(0.0026)
Mean Outcome	10.1	10.1	10.1	10.1	10.0	10.0	10.0	10.0
Total Effect			0.039***	0.041***			0.042***	0.044***
SE of Total Effect			(0.001)	(0.002)			(0.001)	(0.002)
Number Districts	343	343	343	343	343	343	343	343
Number Observations	52,239	52,239	52,239	52,239	52,239	52,239	52,239	52,239

Notes: This table reports observational Mincerian relationship between years of schooling and consumption, testing for heterogeneity by district-level measures of child labor, and restricting the sample to household heads. Standard errors are clustered by district.
Source: IHDS (2012) and NSS Schedule 10 (2004-2012).

Table A14: Effect of Early Life Shocks on Parental Perceptions

	(1)	(2)	(3)	(4)
	Parental Assessment (0-3) (IHDS)			
Early Life Rain	0.022***	0.025*	0.016	0.022**
	(0.007)	(0.015)	(0.012)	(0.009)
Early Life Rain				
... × Child Labor		-0.047		
		(0.200)		
... × (Above Median) Child Labor			0.009	
			(0.015)	
... × (Top Quintile) Child Labor				-0.001
				(0.017)
Mean Outcome	1.950	1.950	1.950	1.950
Total Effect			0.025***	0.021
SE of Total Effect			(0.010)	(0.014)
Number Districts	341	341	341	341
Number Observations	10,194	10,194	10,194	10,194

Notes: This table reports the effect on parental perception of children's ability (on a scale from 0-3), with fixed effects for age, gender, and year/district. The analysis includes all children between the ages of 5 and 17. 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. Standard errors are clustered by district.
Source: IHDS (2012).

Table A15: Effect of Early Life Shocks on Household Consumption When Affected Individuals are Children

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	ln(Consumption Per Adult + 1/3 * kids) (NSS)				ln(Consumption Per Adult + 1/2 * kids) (NSS)			
Total HH Early Life Rain	-0.003***	-0.006***	-0.004***	-0.003***	-0.003***	-0.005***	-0.004***	-0.003***
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Total HH Early Life Rain								
... × Child Labor		0.033***				0.030***		
		(0.012)				(0.011)		
... × (Above Median) Child Labor			0.003**				0.0033**	
			(0.001)				(0.001)	
... × (Top Quintile) Child Labor				0.002				0.002
				(0.001)				(0.001)
Mean Outcome	7.040	7.040	7.040	7.040	6.930	6.930	6.930	6.930
Total Effect			-0.0014*	-0.0013			-0.0013	-0.0013
SE of Total Effect			(0.000)	(0.001)			(0.000)	(0.001)
Number Districts	577	577	577	577	577	577	577	577
Number Observations	1,028,042	452,382	452,382	452,382	1,028,042	452,382	452,382	452,382

Notes: This table reports the effect on consumption for households as a function of early life shocks and district characteristics. Each household is in the data once, and their exposure 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. Standard errors are clustered at the district level.
Source: NSS Schedule 10 (2004-2012).

Appendix A: Mathematical Appendix

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

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

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

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

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

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

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 $\frac{\partial^2 w^c_3(h)}{\partial h^2} \leq 0$ and $\frac{\partial^2 w^c_2(h)}{\partial h^2} \leq 0$. Therefore, $\frac{\partial h^*}{\partial y_1} > 0$.

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

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

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 $\frac{\partial RHS}{\partial y_1} = \delta\Big(U^{c\prime}(w^c_3(h^*,1) + \alpha)\frac{\partial w^c_3(h^*,1)}{\partial h}\frac{\partial h^*}{\partial y_1} - U^{c\prime}(w^c_3(h^*,0))\frac{\partial w^c_3(h^*,0)}{\partial h}\frac{\partial h^*}{\partial y_1}\Big)$. From diminishing marginal returns, $U^{c\prime}(w^c_3(h,1) + \alpha) < U^{c\prime}(w^c_3(h,0))$, so for the RHS to be increasing, we need that $\frac{\partial w^c_3(h,1)}{\partial h} > \frac{\partial w^c_3(h,0)}{\partial h}$. 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.

Before proof Proposition 3a, we define Assumption A1.

Assumption A1.

$$\Phi > \frac{f(\alpha^*_{high}(h^*_{high}(y_1)))}{f(\alpha^*_{low}(h^*_{low}(y_1)))},$$

where

$$\Phi = \frac{\frac{\partial w^c_3(1,h^*_{low})}{\partial y_1} - \frac{\frac{\partial w^c_3(0,h^*_{low})}{\partial y_1} U^{c\prime}(w^c_3(0,h^*_{low}))}{U^{c\prime}(w^c_3(1,h^*_{low})+\alpha^*_{low})}}{\frac{\partial w^c_3(1,h^*_{high})}{\partial y_1} - \frac{u'(y_2+w^c_{2,high}(h^*_{high}))\frac{\partial w^c_{2,high}(h^*_{high})}{\partial y_1}+\delta\frac{\partial w^c_3(0,h^*_{high})}{\partial y_1}U^{c\prime}(w^c_3(h^*_{high},0))}{\delta U^{c\prime}(w^c_3(1,h^*_{high})+\alpha^*_{high})}}$$

Proof of Proposition 3a.

Observe that $\lambda_d(h^*_d(y_1)) = 1-F(\alpha^*_d(h^*_d(y_1)))$. Therefore, $\frac{\partial \lambda_d(h^*_d(y_1))}{\partial y_1} = -f(\alpha^*_d(h^*_d(y_1))\frac{\partial \alpha^*_d(h^*_d(y_1))}{\partial y_1}$. 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 $U^p_2(0, h^*_d(y_1)) = U^p_2(1, h^*_d(y_1))$, which can be rewritten as

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

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

$$\frac{\partial \alpha^*_d}{\partial y_1} = -\frac{\partial w^c_3(1, h^*_d)}{\partial y_1} + \frac{u'(y_2 + w^c_{2,d}(h^*_d))\frac{\partial w^c_{2,d}(h^*_d)}{\partial y_1} + \delta\frac{\partial w^c_3(0,h^*_d)}{\partial y_1}U^{c\prime}(w^c_3(0, h^*_d))}{\delta U^{c\prime}(w^c_3(1, h^*_d) + \alpha^*_d)}$$

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

$$\frac{\partial \lambda_{low}(h^*_{low}(y_1))}{\partial y_1} = \left(\frac{\partial w^c_3(1, h^*_{low})}{\partial y_1} - \frac{\frac{\partial w^c_3(0,h^*_{low})}{\partial y_1}U^{c\prime}(w^c_3(h^*_{low}, 0))}{U^{c\prime}(w^c_3(1, h^*_{low}) + \alpha^*_{low})}\right) f(\alpha^*_{low})$$

$$\frac{\partial \lambda_{high}(h^*_{high}(y_1))}{\partial y_1} = \left(\frac{\partial w^c_3(1, h^*_{high})}{\partial y_1} - \frac{u'(y_2 + w^c_{2,high}(h^*_{high}))\frac{\partial w^c_{2,high}(h^*_{high})}{\partial y_1} + \delta\frac{\partial w^c_3(0,h^*_{high})}{\partial y_1}U^{c\prime}(w^c_3(h^*_{high}, 0))}{\delta U^{c\prime}(w^c_3(1, h^*_{high}) + \alpha^*_{high})}\right) \times f(\alpha^*_{high})$$

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, $u'(y_2 +$

$w^c_{2,high}(h^*_{high}))\frac{\partial w^c_{2,d}(h^*_{high})}{\partial y_1} > 0$, 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 3b. Recall that $\frac{\partial \lambda_{high}(h^*_{high}(y_1))}{\partial y_1} = -f(\alpha^*_{high}(h^*_{high}(y_1))\frac{\partial \alpha^*_{high}(h^*_{high}(y_1))}{\partial y_1}$, where $f(\alpha^*_{high}) > 0$ and

$$\frac{\partial \alpha^*_{high}}{\partial y_1} = -\frac{\partial w^c_3(1, h^*_{high})}{\partial y_1} + \frac{u'(y_2 + w^c_{2,high}(h^*_{high}))\frac{\partial w^c_{2,high}}{\partial y_1} + \delta\frac{\partial w^c_3(0,h^*_{high})}{\partial y_1}u'(w^c_3(0, h^*_{high}))}{\delta U^{c\prime}(w^c_3(1, h^*_{high}) + \alpha^*_{high})}.$$

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

$$\delta\left(\frac{\frac{\partial w^c_3(1,h^*_{high})}{\partial y_1}U^{c\prime}(w^c_3(1, h^*_{high}) + \alpha^*_{high}) - \frac{\partial w^c_3(0,h^*_{high})}{\partial y_1}U^{c\prime}(w^c_3(0, h^*_{high}))}{u'(y_2 + w^c_2(h^*_{high}))}\right)\left(\frac{\partial h}{\partial y_1}\right)^{-1} < \frac{\partial w^c_2(h^*_{high})}{\partial h}.$$

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

$$u'(y_2 + w^c_{2,d}(h))\frac{\partial w^c_{2,d}(h^*)}{\partial h}\frac{\partial h^*}{\partial y_1} \geq \delta\Big(U^{c\prime}(w^c_3(h^*, 1) + \alpha^*)\frac{\partial w^c_3(h, 1)}{\partial h}\frac{\partial h^*}{\partial y_1} - U^{c\prime}(w^c_3(h, 0))\frac{\partial w^c_3(h, 0)}{\partial h}\frac{\partial h^*}{\partial y_1}\Big). \tag{3}$$

Substituting β for δ and $\tilde{w}^c_3$ for w^c_3, this is efficient if

$$u'(y_2 + w^c_{2,d}(h))\frac{\partial w^c_{2,d}(h^*)}{\partial h}\frac{\partial h^*}{\partial y_1} \geq \beta\Big(U^{c\prime}(\tilde{w}^c_3(h^*, 1) + \alpha^*)\frac{\partial \tilde{w}^c_3(h, 1)}{\partial h}\frac{\partial h^*}{\partial y_1} - U^{c\prime}(\tilde{w}^c_3(h, 0))\frac{\partial \tilde{w}^c_3(h, 0)}{\partial h}\frac{\partial h^*}{\partial y_1}\Big). \tag{4}$$

Now consider each of our two cases. If $\gamma < 1$ and $\frac{\partial w^c_3(h,1)}{\partial h} = \frac{\partial \tilde{w}^c_3(h,1)}{\partial h}$, $\beta > \delta$ 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 $\beta = \delta$, 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

$$\frac{\partial \tilde{w}^c(h^*, 1)/\partial h}{\partial w^c(h^*, 1)/\partial h} > \frac{U^{c\prime}(w^c_3(h^*, 1) + \alpha^*)}{U^{c\prime}(\tilde{w}^c_3(h^*, 1) + \alpha^*)}.$$

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,

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

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

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

where β 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

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

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 can estimate the effects 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 Appendix Table A15 and accord with the results earlier in the paper. 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 β for which $\sum_{t=0}^{6} \beta^t \Delta c^h \geq \sum_{t=7}^{T} \beta^t \Delta c^l$. 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.