# Could Droughts Improve Human Capital? Evidence from India*

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**Abstract**

*Weather shocks are an important source of productivity variation in much of the developing world. Standard theory predicts that this should have an effect both on total income, and also on the relative price of time. While much attention has been paid to the potential impacts of the income effects of these shocks on health and human capital investment, relatively little emphasis has been placed on the potential substitution effects. If wages are affected by rainfall shocks, and child and parental time are important inputs into human capital, droughts could potentially increase human capital investment. We find that while children exposed to drought during critical periods (particularly in utero) score lower on cognitive tests, current-year droughts increase test scores. We examine potential mechanisms for this effect, and find that both children and parents work less and have lower wages in drought years. The converse holds true for positive rainfall shocks. We also find that early-life exposure to droughts has deleterious effects on health, schooling, and later-life wages. We conclude that both the income and substitution effects of rainfall shocks are important in this context.*

JEL Codes: I2, I1, J1

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## 1 Introduction

Weather shocks play a huge role in income variability in the developing world (Wolpin, 1982; Paxson, 1992). This income variability has potentially negative effects on human capital accumulation (Jacoby and Skoufias, 1997; Glewwe et al., 2001; Maccini and Yang, 2009; Alderman et al., 2006; Jensen, 2000). These shocks are also especially deleterious during critical periods such as the in utero phase and early childhood (Almond and Currie, 2011; Cunha and Heckman, 2008).[1] However, to date, much of the early life literature has focused on health outcomes (Currie et al., 2010; Almond and Mazumder, forthcoming; Hoddinott and Kinsey, 2001; Lawlor et al., 2006; Kudamatsu et al., 2010) and less on educational outcomes.[2]

In this paper we investigate how droughts (and positive weather shocks) affect human capital, exploiting fluctuations in monsoon rainfall over time and across districts. In rural areas in India, droughts constitute significant productivity shocks, as agriculture is the main source of income and employment and approximately 70 percent of the cultivated area is rainfed (Droogers et al., 2001). We examine the effect of in utero drought exposure as well as the effect of contemporaneous rainfall shocks on human capital attainment among children.

Rainfall shocks could affect human capital through wages in two important ways: by changing total income and by changing the price of time. To the extent that time and income are both important inputs into human capital, the expected effect of droughts on human capital attainment is ambiguous. Given the importance of maternal nutrition (Barker, 1994), the income effect is likely to be particularly important during the in utero phase. However as children age, time inputs become relatively more important as children start

[1] Most studies that show long term impacts of the in utero environment attribute findings to Barker's (1994) famous fetal origins hypothesis. The fetal origins hypothesis helps explain why economic and environmental conditions during pregnancy may have long-term impacts on health and socioeconomic status (See e.g., Almond (2006); Black et al. (2007); Dêschenes et al. (2009); Royer (2009)).

[2] The research which has focused on education generally uses educational attainment and enrollment as outcomes of interest (e.g. Fung (2010); Maccini and Yang (2009); Alderman et al. (2009)); however there are some recent working papers which look at cognitive test outcomes in children (e.g. Aguilar and Vicarelli (2011); Akresh et al. (2010)).

attending school, spending time on homework, etc. Agricultural productivity increases could cause both children and parents to substitute away from human capital investment toward productive activities (either in or out of the home). Under certain conditions the substitution effect could dominate, and negative rainfall shocks could increase human capital attainment.[3]

We examine this possibility using ASER data from 2005-2009; we observe nearly 3 million rural children from almost every state in India. We have four distinct measures of literacy and numeracy for each child regardless of whether he is currently enrolled or not. This is rare since tests are primarily conducted at school, and thus scores are usually only available for currently enrolled kids. In addition, our data allow us to look at more standard educational measures such as school enrollment, drop-out behavior, and being on track in school (age for grade). Since the survey was conducted every year over five years, we can control for age, year of survey, and district, identifying off within-district variation in drought exposure.

Using this data, we are able to focus directly on cognitive ability as an output variable, as well as more common outcomes such as schooling attainment. We find that children who are in utero during droughts score significantly worse on math and reading tests, are less likely to attend school, and less likely to be "on track" (age for grade). For example, experiencing a drought in utero is associated with being 2.6 percentage points less likely to recognize numbers from 1-10 and 1.2 percentage points less likely to be able to do a simple subtraction problem (from a baseline percent of 53.9% and 61.6%, respectively). We show these results are not likely due to selective mortality, selective fertility or migration. In addition, we show that young adults who experienced a drought in utero earn 5 percent less on average than their peers.

In looking at contemporaneous droughts, we find that current year droughts increase test scores and attendance rates for children ages 5-16. By contrast, positive rainfall shocks result in lower test scores and higher dropout rates. Using a labor survey on wages and employment, we show that individuals are less (more) likely to be currently employed and

[3]In the case of child mortality in Colombia time effects dominate income (Miller and Urdinola (2010)); however, as far as we know there is no empirical evidence of this in the human capital literature.

have lower (higher) wages in drought (positive rainfall) years. This suggests that time use is changing in response to rainfall shocks. However, since both young adults and parents are more likely to be working, we cannot distinguish between the relative importance of child and adult time. In reality, both most likely play an important role. We investigate alternative mechanisms that might explain these results such as teacher absences or school lunches, and find little evidence they are driving the results. As far as we know, this is the first paper to focus on the important role that the substitution effect may have on human capital development. Most human capital papers in development focus on changing income or the relative price of schooling via cash transfers (Schultz, 2004; Schady and Araujo, 2006), school uniforms (Duflo et al., 2006), etc. but none have focused on the relative price of the outside option to schooling. However, Atkin (2010) does show that school drop out increases with the arrival of new export jobs in Mexico.

The paper proceeds as follows. Section 2 provides a conceptual framework and Section 3 provides some background on droughts in India and describes our data sources and the sample used. Section 4 describes our empirical strategy, and Section 5 outlines our results for in utero drought exposure. Section 6 shows our results for contemporaneous drought exposure, and Section 7 concludes.

## 2 Conceptual Framework

In a drought year, agricultural productivity decreases. This can have a direct effect for landholders, or an indirect effect through wages for agricultural laborers. We consider a simplified model of human capital production in which human capital is a function of income and time (abstracting away from fixed endowments such as ability). In a drought year, the relative value of time spent farming decreases. The income effect of this change is straightforward. Families have fewer resources to spend on human capital production, whether this is school fees, books, or proper nutrition.

The effect of a drought on time inputs into the human capital production is ambiguous.

As with the standard labor/leisure tradeoff, the income effect of the wage change would cause families to devote less to human capital production, as long as human capital is a normal good. However, the relative price of schooling to the outside option for children has become cheaper. This could cause substitution toward human capital production since it has become relatively less expensive. We will show empirically that in drought years the substitution effect dominates the income effect in that children attend school more and work less, and parents work less (potentially spending more time at home in human capital production).

The overall effect of drought on human capital attainment is not obvious. The income effect will push human capital investment downward unambiguously, but if the substitution effect causes households to substitute time toward human capital production, this could increase human capital production. Thus, it is possible in some contexts that the substitution effect could dominate the income effect, and droughts would lead to an increase in human capital production.

Which of these effects will dominate is an empirical question (and the subject of this paper), but there are certain circumstances in which the substitution effect will likely be stronger. First, if non-time inputs are particularly important at certain ages (such as nutrition in utero, or college tuition), then the income effect will likely be stronger. Second, if agents are credit constrained, and school fees or other costs to human capital production are large, income effects again will likely be large. However, the substitution effect may be relatively stronger if children and their caretakers (often women) are able to easily take advantage of productive opportunities, either in home production or agricultural labor markets, as these opportunities dry up in drought years. Therefore, they might allocate this newly freed up time to human capital production instead.

In rural India the income effect is likely to dominate during the in-utero period, especially because the time input of parents (and children) is limited during this stage, and nutrition and other prenatal inputs are especially crucial for development (see Almond and Currie (2011). By contrast, the substitution effect or time as an input becomes relatively more important as children age. Primary school is free and compulsory,[4] and the Indian government has built many schools to keep the costs of attendance low. In 1971, 53 percent of villages had a public primary school, in 1991, 73 percent did (Banerjee and Somanathan, 2007), and today almost 100 percent of Indian villages have a primary school (Government of India, 2011). Though households in rural India are likely to experience credit constraints, to the extent that school fees are free (and there are few required purchases such as uniforms and books), credit constraints are unlikely to inhibit households from sending their children to primary school. In addition, child labor is still relatively common in rural India, particularly on household farms. Agricultural labor is often traded in spot markets, allowing women and older children to work for wages without long-term contracts or other extensive margin frictions (Kaur, 2011). While younger children are likely not working as day laborers, they could easily be substituting time in school for labor in the home (cooking, cleaning, childcare) for older relatives who leave the home to work if wages are high.

## 3 Background and Data

### 3.1 Droughts in India

Drought has affected millions of people over the past two centuries in India. The rain that comes during the monsoon season (June–October) is essential to raising crops, and if rainfall is below normal levels, many regions experience deleterious living conditions since most of the rural poor depend on rainfed agriculture. In fact, almost 70 percent of the total net area sown in India is rainfed; and 66.2 percent of rural males and 81.6 percent of rural females report agriculture (as cultivators or laborers) as their principal economic activity (Mahajan and Gupta, 2011).

While there is plenty of evidence showing droughts adversely affect agricultural output in India (see for example Rao et al. (1988)), we also explore this question empirically using

[4]Though primary schooling is compulsory this is clearly not a binding constraint for parents in rural areas.

the World Bank India Agriculture and Climate Data set. In Table A1 we test if crop yields react to drought by regressing agricultural yield (revenues per acre) data from 1957-1987 on drought. Our dependent variables are rice, wheat and jowar yields. Control variables include various inputs such as fertilizer, machinery, etc. All specifications include year and district fixed effects and standard errors are clustered at the district level. Table A1 indicates that drought strongly and significantly predicts crop yield. In drought years, rice, wheat and jowar yields are significantly lower. Pathania (2007) also shows that the annual yields of wheat and rice, the two major cereal crops consumed, are affected adversely by droughts; a second consecutive year of drought leads to a 9.4% decrease in wheat and a 20% decrease in rice. In addition, Kaur (2011) shows that crop yields and agricultural wages in India are significantly lower in years in which rainfall is in the lowest quintile. Given the majority of rural residents are engaged in agricultural activities in India, and that the majority of this land is rainfed, we are confident that droughts represent a significant shock to pre- and post-natal women, children, and other household members in this context.

### 3.2 Cognitive Testing and Schooling Data

Every year since 2005, the NGO Pratham has facilitated an innovative exercise for India: that of implementing the Annual Status of Education Report (ASER). The core of the survey is that simple tools are used to assess children's ability to read simple text and do basic arithmetic. Within the span of a hundred days, the survey reaches *every* rural district in the country: over 570 districts, 15,000 villages, 300,000 households and 700,000 children. ASER is the largest annual data collection effort with children in India. It is also the only annual source of information on educational achievement of primary school children in India. We have data on children for 2005-2009, giving us a sample size of close to 3 million rural children. The sample is a representative repeated cross-section at the district level, for every rural district in India.[5]

[5] For more information on ASER, see `http://www.asercentre.org/ngo-education-india.php?p=ASER+survey`

What differentiates ASER data from other educational datasets is that its sample is extremely large, and includes both in- and out-of-school children. Since cognitive tests are usually administered in schools, data on test scores is necessarily limited to the sample of children who are enrolled in school (and present when the test is given). However, ASER includes children ages 5-16, who are currently enrolled, dropped out, or have never enrolled in school.

In Table 1 we describe the characteristics of the children in our sample as well as their test scores. The average age is about nine and a half, and the average grade is 4.6. The sex ratio is somewhat skewed, with 54.4% boys. This skewed sex ratio is expected given this is Indian data where sex selected abortion and female infanticide are common and girls have become relatively more scarce over time, especially in rural India (Sen, 1992; Jha et al., 2006).

The ASER surveyors ask each child four questions each in math, reading (in their native language), and English reading. The four math questions are whether the child can recognize numbers 1-9, recognize numbers 10-99, subtract, and divide. The scores are coded as 1 if the child correctly answers the question, and 0 otherwise. In addition, there are four literacy questions: whether the child can recognize letters, recognize words, read a paragraph, and read a story. We use the scores for numeracy and native-language literacy in our analysis, and those scores are reported in Table 1. In addition, we calculate a "math score" variable, which is the sum of the scores of the four numeracy questions. For example, if a child correctly recognizes numbers between 1-9 and 10-99, and correctly answers the subtraction question, but cannot correctly answer the division question, then that child's math score would be coded as 3. The "reading score" variable is calculated in exactly the same way. Approximately 65% of the children tested can recognize numbers between 1 and 9, and about 38% can correctly do a division problem. The reading scores are slightly higher: nearly 90% of children tested can recognize letters and 45% can read a story. The ASER data also contains scores on English reading, but given most of the variation in these scores will be caused by whether the child learns English at school, rather than cognitive ability, we exclude it from our analysis.

### 3.3 Rainfall Data

To determine rainfall shock years and districts, we use monthly rainfall data which is collected by the University of Delaware.[6] The data covers all of India in the period between 1900-2008. The data is gridded by longitude and latitude lines, so to match these to districts, we simply use the closest point on the grid to the center of the district, and assign that level of rainfall to the district for each year. In our main specification, we define drought using the cutoff of the Indian Meteorological Department, which is monsoon rainfall that is 75% of the 10-year average of rainfall for that district. Figure 1 shows the prevalence of drought by district over time (for the years we have cohort variation in in utero drought exposure) and indicates there is both a lot of variation over time and across districts in terms of drought exposure. Almost 6 percent of districts experienced a drought in 1998 to a high of 47.4% of districts in 2002. In fact, 80 percent of the districts experience at least one drought in the 16 year period that we have child cohort variation.

We also estimate our effects using linear rainfall and rainfall quintiles. In later specifications, we define a positive shock as yearly rainfall above the 80th percentile and negative shock (drought) as rainfall below 20th percentile.[7] It is important to note that in general positive rainfall shocks will be good for agricultural output, especially in the case of India where rice cultivation is very important. However, there might also be some cases where positive rainfall shock is capturing extreme rainfall which could have negative consequences for agricultural output. Therefore while negative shocks (i.e. droughts) are always bad for agricultural output, positive rainfall shocks could have positive or negative impacts on productivity.

[6] The data is available at: `http://climate.geog.udel.edu/~climate/html_pages/download.html#P2009`

[7] This is similar to the definitions employed in Jayachandran (2006) and Kaur (2011). Our results are not sensitive to the definition of drought we employ.

Unfortunately we do not have the exact date of birth for the children in our sample, only the current age, so we need to calculate which drought years will affect the in utero environment.[8] Since we observe child age and year of survey, we assume that each child has already had his birthday that year, that is, that his year of birth is the year of the survey minus age (e.g. a child who reports being 10 in the 2008 survey is coded as being born in 2008-10=1998). Since the Indian Monsoon (and, thus, droughts) typically takes place in the summer, and harvests happen the following fall, it is likely that children born in a given year will be more affected by a drought in the year before their birth than one in the year they are born. We assume a child born in 2008, on average, is born June 30. This child will be in utero roughly from October 2007 to June 2008. Thus, the harvest in 2007 (which is in turn affected by the 2007 rains) is likely to be the biggest determinant of his nutritional environment in utero (as opposed to harvest 2006 or 2008).[9] Of course, there will be children in the sample whose age is recorded incorrectly, and those who are born at the very end or beginning of the year for whom other drought years might be more relevant. However, unless these errors are systematic, they will simply add noise to our estimation and attenuate the results.

In Figure 2 we show the distribution of math scores separately for children who were and were not exposed to drought in utero. Figure 3 shows the same distribution for reading scores. The figures allude to the relationship we will find in the empirical results: children who were exposed to drought in utero are more likely to score lower on the math and reading tests. In section 5.1 we test explicitly for this relationship using a fixed effects model.

### 3.4 Health Outcomes Data

Though we cannot investigate the relationship between in utero drought exposure and health outcomes for the ASER sample children (since they do not ask about health outcomes), we

[8]This is not an important issue for the contemporaneous drought regressions since we are not identifying off birth age.

[9]This also implies that we will not be able to determine which trimester of drought exposure is relatively more important.

turn to another dataset, the National Family Health Survey-2 (NFHS-2)[10] to investigate this relationship. This will allow us to estimate the effect of in utero drought exposure on health outcomes, which could effect schooling. Though there is already evidence of this from other countries (see for example, Maccini and Yang (2009)), we would like to test explicitly for this channel using Indian children and our rainfall data. We use the 1998-99 NFHS-2 India survey because this is the latest year that district identifiers are publicly available. We merge the rainfall data used above (which is at the district level) to the NFHS-2.[11]

### 3.5 Work and Wages Data

We use the NSS (National Sample Survey) Round 62 which was collected between July 2005 and June 2006 by the Government of India's Ministry of Statistics. This is a national labor and employment survey collected at the household level all over India. This dataset gives us measures of employment status (currently works and hours worked) as well as wages at the individual level. We use data from all rural households in this survey and merge with our district level rainfall data to explore the relationship between weather shocks, labor force participation and wages.[12]

[10] This is also known as the Demographic and Health Survey (DHS) for India.

[11] The NFHS-2 survey covers a nationally representative sample of more than 90,000 eligible women age 15-49 from 26 states that comprise more than 99 percent of India's population. The survey provides information on fertility, mortality, family planning, and important aspects of nutrition, health, and health care. The NFHS-2 measured children's (ages 0-3) height and weight. Height and weight are a widely used proxy for overall health status and correlate positively with economic outcomes. For example, Case and Paxson (2008) show that height is positively correlated with earnings in the developed world. Similar patterns between height and wages for individuals in Brazil (Strauss and Thomas, 1998) and other developing countries have been shown (Behrman and Deolalikar, 1989). Similarly, being underweight is correlated with future health problems and worse schooling outcomes.

[12] Given the potential measurement error in the valuation of in-kind wages, we define wages paid in money terms.

## 4 Empirical Strategy

### 4.1 In Utero Regressions

Our main analysis is very straightforward. We take advantage of the quasi-random nature of droughts within district (and across districts within a year) in order to measure the effect of drought on test scores. We estimate the following regression:

$$S_{ijty} = \alpha + \beta\delta_{j,t-1} + \gamma_j + \phi_t + \psi_y + \epsilon_{ijt} \tag{1}$$

where $S_{ijty}$ is the test score of student $i$ in district $j$ born in year $t$ and surveyed in year $y$. $\delta_{j,t-1}$ is an indicator for whether there was a drought in district $j$ in year $t-1$, $\gamma_j$ is a vector of district fixed effects, $\phi_t$ is a vector of year-of-birth fixed effects, and $\psi_y$ is a vector of year-of-survey fixed effects. This way, any unobservables that vary with district or year will be absorbed by the fixed effects, and thus the effect we pick up will be that of being born in utero during a drought. $\beta$ is our coefficient of interest and it is the causal impact of in utero drought on the various test scores (or cognitive ability). Standard errors are clustered at the district level. We discuss some potential selection issues in Section 5.4 below.

In an alternative specification, we replace the district fixed effects with household fixed effects. This strategy identifies off the differences within household of in utero drought exposure.[13] If drought exposure is indeed IID, and there are no intervening mechanisms which could affect outcomes, this specification should yield exactly the same results. However, it is possible that parents could react to one child's drought exposure by reallocating resources within the household, either by shifting them toward or away from the affected child. Thus, other children in the household (effectively our "control group" in this specification) could be directly affected by their sibling's drought exposure. If this were the case, we would expect to see the coefficient of interest in this specification to be either larger than the main spec-

[13] In general, these are differences between siblings, however the data does not distinguish between relationships between children in the same household, who could be cousins, step-siblings, etc.

ification (if parents are shifting resources to higher-ability children) or lower (if parents are compensating for the drought exposure by shifting resources toward lower-ability children).

For this reason, our preferred specification uses district, rather than household, fixed effects. However, this specification is useful in that it can help rule out some possible selection biases. For example, if "good families" are delaying fertility during drought years, this might bias our main coefficient upward; however it would not bias the coefficient estimated with household fixed effects, since identification comes from within-household variation. We discuss this more specifically in Section 5.4.

We also examine the effect of in utero droughts on other outcome variables. In these regressions, we estimate equation (1) above, but we replace $S_{ijt}$ with several schooling and health outcomes. Specifically, we estimate the impact of drought exposure on whether a child reports having dropped out, never having enrolled in school, and being "on track" (age for grade).[14] In addition, we use height and weight from the NFHS data as alternative outcome variables. We also vary the independent variable, $\delta_{jt}$ in equation (1), in some alternate specifications. Since rainfall has a surprisingly monotonic effect on crop yields in India (Jayachandran, 2006), we also use a continuous measure of rainfall and quintiles of rainfall instead of a binary drought variable.

One possible issue with using droughts as quasi-random shocks, is that they may be correlated over time. There are certainly districts in which droughts are more common in all years, but this should not affect our empirical results, since the district fixed effects imply we are using within-district variation in timing of droughts to identify causal effects. However, if it is the case that droughts this year are correlated with droughts next year, then this undermines the effectiveness of our cohort identification as it will be difficult to separate the effects of droughts in the year before birth with those in early life (or before pregnancy). This would limit our scope for identification, since both "control" and "treatment" children

[14] We define "on track" as a binary variable which indicates if a child is in the "correct" grade for his/her age. The variable is coded 1 if age minus grade is at most six. That is, if an eight year old is in second or third grade, he is coded as on track, but if he is in first grade, he is not.

would have experienced several years of drought in early life. We test for serial correlation directly in Appendix Table A2.

While recent papers using Indian rainfall data have not found evidence of serial correlation in rainfall data (Kaur, 2011; Pathania, 2007), Table A2 tests for autocorrelation in rainfall. An observation is a district-year. The dependent variable in both regressions is the deviation from mean rainfall in the current year (in inches), where deviation is simply defined as current year rainfall minus the mean rainfall in sample period. The independent variable is deviation from mean rainfall last year, constructed in the same way. The specification in column 2 contains year fixed effects, while column 1 does not. In column 1 we find no significant evidence of serial correlation. In column 2 once we include year fixed effects, the coefficient becomes negative and statistically significant, however, the magnitude of the effect is very small. It is unlikely that such a small amount of negative rainfall correlation will affect our results, particularly because it would mean that children in utero during droughts would be *less* likely to be exposed in early childhood, which, if anything, sharpens the timing interpretation of our in utero results.

### 4.2 School Aged Children Regressions

We then examine the effects of negative and positive weather shocks at all ages, which means we can no longer exploit the cohort level variation used in the in utero regressions. Instead, we use variation in the year the survey was administered, and estimate the impact of rainfall shock in the current year on test scores of children. Specifically, we estimate the regression:

$$S_{ijty} = \alpha + \beta_1 \delta_{jy} + \beta_2 \delta_{j,y-1} + \beta_3 \delta_{j,t-1} + \gamma_j + \phi_t + \psi_y + \epsilon_{ijt} \tag{2}$$

where $S_{ijty}$ is the test score of student $i$ in district $j$ born in year $t$ and surveyed in year $y$, $\delta_{jy}$ is an indicator for whether there was a negative (or positive) rainfall shock in district $j$ in year $y$, $\delta_{j,y-1}$ is a lagged indicator of the rainfall shock, $\delta_{j,t-1}$ is an indicator for in utero drought exposure, $\gamma_j$ is a vector of district fixed effects, $\phi_t$ is a vector of year-of-birth fixed effects, and $\psi_y$ is a vector of year-of-survey fixed effects. Rather than above, where our identification comes from comparing children of different cohorts in the same district, in this regression we compare children who are surveyed in different years from the same district. Since our regressions still contain district-level fixed effects, we should not be biased by systematic differences across districts. $\beta_1$ is the coefficient of interest and it is the causal impact of current-year drought on the various test scores (or cognitive ability). Standard errors are clustered at the district level.

# 5 Results

## 5.1 Results: In Utero Drought and Cognitive Test Scores

Table 2 presents our main estimates of the effect of in utero droughts on cognitive test scores. Panel A shows the effect of in utero drought exposure on the four math questions as well as the math score variable, which is the sum of the binary indicators for each of the four math questions. The effect on the overall math score is negative and significant, and represents about .05 points.[15] The negative effects are quite a bit higher for the number recognition questions relative to the subtraction and division questions. Experiencing a drought in utero is associated with being 2.6 percentage points less likely to recognize numbers from 1-10 and 1.2 percentage points less likely to be able to do a simple subtraction problem (from a baseline percent of 53.9% and 61.6%, respectively). The magnitude of the effect for recognizing numbers 1-9 is smaller, and while the coefficient on division is negative, it is not statistically significant.

Panel B of Table 2 shows the effect of in utero drought exposure on the reading questions. The two more difficult questions—reading a paragraph and reading a story—are statistically significant at the .01 level. Children in utero during a drought are 0.7 percentage points less

[15] Since the test is out of five points, this is equivalent to about 1 point on a test that was scored out of 100.

likely to be able to read a paragraph and 0.7 percentage points less likely to be able to read a story (from a baseline percent of 60.8% and 44.6%, respectively). The effect on the overall reading score is negative (-0.9 points), but not statistically significant. While the magnitude of the effects on reading tend to be smaller than the effects on math, they are still negative, except for being able to read a letter which is positive.

Panels C and D of Table 2 show the results using household, rather than district, fixed effects. Overall, the results are remarkably similar. This is a little surprising, given we are now identifying off within-household differences in exposure to in utero drought, and one might expect the differences to be larger between rather than within households. However, the results lend credence to our assumption that droughts are quasi-random, and that there are no systematic ex-ante differences between those who are exposed to drought in utero and those who are not, once we control for cohort and district effects. The fact that these estimates are so similar to the main estimates also suggests that there is not large, systematic shifts in resource allocation between siblings by parents, either toward or away from drought affected children. Of course, we cannot test for this phenomenon specifically, so the evidence is not conclusive. In addition, the reading results become slightly stronger and the standard errors decrease; the positive coefficient on being able to read becomes negative in this specification.

The main results are fairly similar when broken down by gender. Panels A and B of Table A3 show reading and math scores for boys, and Panels C and D show the same effects for girls. The effects on reading are slightly higher for girls, though not significantly so; while the impacts on math are higher for boys, and this is a statistically significant difference.

In Table 4, we regress the test score on experiencing drought two years before the in utero period up to age two. The results indicate that the magnitude of the effects of drought decrease after the in utero period and stop mattering at age two. Interestingly, the magnitudes of the in utero effects are similar to the main results even after we add these additional years of drought in the regressions. Our results are very similar to Akresh et al. (2010) who also find that negative education impacts are largest for in utero shocks, diminished for shocks before age two, and have no impact for shocks after age two in rural Burkina Faso. Unlike Aguilar and Vicarelli (2011) who find that negative impacts on child development persist four to five years after rainfall shocks in rural Mexico, we do not find any evidence that the negative impacts persist after age two.

In Figure 4 we graph the coefficients from the math score regression at each age. The largest negative impact comes from in utero exposure to drought. As stated above, the negative effect disappears by age 2. However, interestingly it appears that drought has a positive effect on test scores from ages 3 up. We investigate these results further in Section 6.

### 5.1.1 Alternative Rainfall Measures

So far we have investigated the impact of drought on test scores since drought is the most serious weather shock that rural households in rainfed agricultural areas face. However, it is possible that other measures of rainfall might also be relevant weather shocks. For example, Maccini and Yang (2009) find that *higher* early-life rainfall leads to improved health, schooling, and socioeconomic status for women. In Table 3 we measure the effect of two different measures of rainfall on test scores. The dependent variable is math score (columns 1-2) and reading score (columns 3-4), defined previously as the sum of questions answered correctly out of the four questions given in the ASER survey. The independent variable in column 1 and 3 is rainfall (in inches). The independent variables in column 2 and 4 are quintiles of rainfall (the third quintile, the middle, is omitted). Both specifications include district, age, and year of survey fixed effects and standard errors are reported in parentheses.

Column 1 indicates that a one standard deviation increase in rainfall (788.5 inches) increases the math score by .078 points. This translates to a 4 percent increase off of a mean of 2.19. The quintile analysis suggests that the effects are surprisingly monotonic.

The bottom two quintiles of rainfall are negatively associated with math test scores (relative to the middle quintile), and the top two quintiles are positively associated with test scores (relative to the middle). These results are significant at the .01 level. While these results make sense in the context of India, where the main weather hazard is drought, they may not hold up in contexts in which floods cause significant damage. Still, they illustrate the importance of rainfall in Indian agriculture, and its effects on nutrition and cognitive development. The reading score results in columns 3 and 4 are noisier with larger standard errors, however, the coefficients show similar patterns.

### 5.2 Results: Schooling Outcomes

It is reasonable to hypothesize that exposure to in utero drought could affect schooling outcomes. There are two main channels through which this could occur. First, children exposed to drought in utero could be less healthy, which directly affects their ability to attend school. Second, droughts could directly affect cognitive ability, and parents could react by altering the amount of schooling given to these lower-ability children. If ability and schooling are substitutes—this would be reasonable in a setting in which, say, there are high returns to basic literacy and numeracy and little else—then parents might be more likely to enrol their lower-ability children, and less likely to pull them out of school at a young age. By contrast, if ability and schooling are complements—if high ability children get more out of each year, or have a higher probability of leaving the village for high-paying work—then parents would, on average, provide less schooling for their lower-ability children.[16]

Of course, we will not be able to directly test for the difference between these two possibilities. However, if the effect of in utero drought on schooling is positive, we could speculate that schooling and ability are substitutes, but if the effect is negative we cannot distinguish between the "sickly child" mechanism and the ability-schooling complementarity mechanism,

[16]For empirical evidence of parents reinforcing initial cognitive ability differences across children by investing in higher ability children, see Ayalew (2005); Akresh et al. (2010); Frijters et al. (2010). In fact, the majority of recent empirical evidence suggests that parents reinforce initial cognitive differences and invest more in high ability children.

since we do not have random assignment of schooling. We can, however, test for the reduced-form effect of droughts on schooling, and we do find that the effect is negative suggesting that ability and schooling are complements.

Table 5 shows the effect of drought in utero on schooling achievement measured by dropping out, never enrolling in school, and being on track. Panel A shows the results for all children, Panel B for boys only, Panel C for girls only, and Panel D includes household fixed effects.

Panel A indicates that children born the year after a drought are 2 percentage points more likely to never enrol in school. This is quite a large effect given the mean of never enrolled is 2.8%. This is some evidence toward the hypothesis that schooling and ability are complements. If a child has some cognitive deficiency because of being malnourished in utero, his parents are less likely to send him to school, presumably because schooling is less valuable for him/the family. In addition, children born in the year following a drought are 2.2 percentage points less likely to be on track, from a baseline of 81.3%. Panels B and C of Table 5 show the results separately for boys and girls. The results look fairly similar for boys and girls in terms of being on track in schools. However, one big difference is that girls are significantly more likely to have never enrolled in school and the effect is quite large. This is probably the result of differential parental investment decisions in boys and girls as parents are significantly more likely to never enrol a daughter. Like the table of main results, the household fixed effects results in Panel D are very similar to the results in Panel A for all children.

### 5.3 Results: Child Health and In Utero Drought Exposure

In Table 6 we regress the dependent variables height and weight on in utero drought exposure. We include birth year fixed effects and state fixed effects; all specifications are clustered at the district level. By including birth year fixed effects, in some sense we are investigating the relationship between height-for-age and weight-for-age and drought exposure of these young children. In addition, the NFHS-2 asks mothers for exact birth year, so we do not have to make any assumptions about age at the time of the survey when we merge the rainfall data.

The results are quite striking: in utero drought exposure significantly decreases current height. For example, a child exposed to in utero drought is approximately 6.9 centimeters shorter on average than a similar child born during a non-drought year. This is about a 10 percent decrease and it is statistically significant at .05 level. In addition, the coefficient on weight is also negative, though not statistically significant.[17]

These results allude to the fact that the ASER children who were exposed to in utero drought are not only cognitively disadvantaged but most likely have worse health outcomes as well. As discussed earlier, there are two likely channels through which drought effects later-life cognitive ability. First, maternal nutrition and the in utero environment could have a direct effect on cognitive development, lowering IQ. Second, children exposed to drought could be less healthy overall, and this could impact school attendance. Children who attend less school will most likely have lower test scores. For children in this rural India, it appears that both mechanisms are likely at play.

### 5.4 Potential Selection Bias Issues

#### 5.4.1 Migration

One weakness with our data is that we do not have information on location of birth. Therefore we assume that the current district is the same as the birth district when we assign each child the district level drought measure which corresponds to his/her year of birth. Because these children are relatively young, ages 3-15 with a mean age of 9, we do not think this is a strong assumption. Nevertheless, we explore this issue below. If it is the case that higher ability families are more likely to migrate out of rural areas (say to urban areas), this could affect our empirical results. However in this case, our result will be a lower bound.

[17] Height is a lagged, long-term indicator of nutrition/health whereas weight is a current measure, so in some sense this is not a surprising result.

Various pieces of evidence suggest that out-migration rates are low for rural Indian families. For example, Topalova (2005) using data from the National Sample Surveys finds that only 3.6 percent of the rural population in 1999-2000 reported changing districts in the previous 10 years. Munshi and Rosenzweig (2009) using the Rural Economic Development Survey also conclude that rural emigration rates are low and actually declined between 1982 and 1999 in India. Pathania (2007) using Indian Census data also finds that only a small fraction of rural women reside in districts different from their district of birth. Using data from the 2001 census on internal migration, he shows that 82.65 percent of rural women aged 15-59 are residing in the district of birth and 96 percent of this demographic group is residing in the state of birth. He writes that marriage and the subsequent move to the house of the husband's family is the major reason for female migration, and most marriages are local. Given women have children after marriage, it is unlikely that many of our rural sample of women is moving after they have had children. While temporary migration of rural men in search of employment is more common, this should not affect our results.

Indeed, since we control for district fixed effects in all specifications, even if "better families" are leaving drought-prone districts, this alone would not be enough to drive our results (though it could attenuate the results). To think that migration might bias our coefficient upward, one would have to imagine not only that higher ability kids move out of drought-prone districts (which could be true), and that this effect is also large enough to affect the coefficient (unlikely, given the limited movement), but that the higher ability kids who are leaving are particularly likely to be certain ages, namely those which correspond to being in utero during a drought year, which seems extremely unlikely.

#### 5.4.2 Selective Mortality and/or Fertility

One potential concern with trying to understand the effect of drought on cognitive development is that we only observe children who survive and make it into the sample; if drought exposure increases infant and early childhood mortality, it could affect the composition of our sample in "control" and "treatment" years. This selection would most likely bias our results downward; since these are the children who survived, they are positively selected and probably do better on health and educational outcomes relative to the children who died off. Therefore, we are less concerned about bias from selective mortality.

However, another potential concern with our results could be if women are delaying and/or changing fertility patterns in response to droughts. For example, mothers may choose to wait out a drought year before having a child. Rural fathers could migrate during drought years in search of work and their absence would result in delayed fertility. If droughts are in fact impacting fertility decisions, the empirical results will most likely be biased upward, since the children being born in drought years would be negatively selected.

Since our dataset includes only children ages 3-16, both of these selection effects would show up as smaller cohort sizes observed for treatment cohorts (assuming that most of the selective mortality happens before age 3). Unfortunately, population by district is only available every 10 years from census data. Therefore we investigate the issue of selective fertility for children born in 1991 and 2001. We regress the ln number of children in each cohort by district on measures of drought and ln total population by district. Given we are not exactly sure when mothers and fathers make decisions about when to conceive, we investigate the period 5 years prior to birth.

Table A4 reports the results of these OLS regressions for 1991 and 2001. All regressions contain state and year of survey fixed effects and standard errors are clustered at the district level. Most of the coefficients are small, and only two are statistically significant. In column 1, drought in t-3 is significantly (and negatively) correlated with number of births. However, in column 2, drought in t-4 is significantly (and positively) correlated with number of births. These data do not suggest that there is a systematic difference in the size of "treated" cohorts, and thus selective fertility and mortality are unlikely to be driving our results.

Another piece of evidence which points against selective fertility (and selective migration) are the household fixed effects results in Panels C and D of Table 2. If either of these mechanisms is driving the results, then within-household variation in drought exposure should not affect cognitive test scores. This story relies on *between* household variation–i.e. that "good" households are acting differently with respect to droughts compared to "bad" households. That is, if "good households" are leaving the area after droughts, or delaying their fertility when there are droughts, then our sample of exposed children would be more heavily weighted toward "bad households" which could bias our results upward. However, the results with and without household fixed effects are extremely similar (if anything reading score coefficients are a bit higher), which leads us to conclude that selection of this type is not contributing significantly to our estimates.

## 6 Results for School Aged Children

### 6.1 Main Results

As shown in Figure 4, children who are in utero during droughts score worse on average on cognitive tests. However, it appears as though children who experience droughts when they are young (particularly ages 4-6) score better, on average, than their peers. Since we are using cohort identification, these coefficients represent the effect of droughts at each age relative to the other cohorts in the district. Thus, it is impossible to determine the *overall* effect of drought on cognitive test scores for all ages using this empirical strategy. We turn to variation in the year of survey to determine whether current-year droughts are affecting children's test scores.

Table 7 shows our results from Equation 2. Children who are tested during a drought year in their district score 0.1 points higher on math tests, and report 2 percentage points higher attendance rates in the previous week. This is about twice the size, in absolute terms, of the coefficient of in utero drought on math scores. Children who experience a positive shock (higher than the 80th percentile of rainfall for their district), score .05 points lower on math tests, and are more likely to report having dropped out of school. In Appendix Table A8, we run these regressions separately by gender, and find little difference. In Appendix

Table A6, we run regressions separately for rich and poor states, and find that the effects are stronger in poorer states, as well as for children whose mother never attended school. These results are consistent with poorer children being more likely to be on the margin between school attendance and work.

### 6.2 Mechanisms

As laid out in Section 2, the positive impacts of drought on test scores are consistent with a context in which the importance of time inputs into human capital production is relatively high. It is clear from the attendance and dropout results in Table 7 that children are indeed substituting toward schooling in droughts, and away during positive rainfall shocks. In the following sections we will show that children and parents are also less likely to be working, and work fewer hours conditional on working, which is consistent with this mechanism. In addition, we will examine (and ultimately reject) the potential explanations of teacher attendance and school lunch provision.

#### 6.2.1 Work and Wages

One possible mechanism that could be driving the results is the increased wages (due to increased agricultural productivity) which accompany higher rainfall years. Since wages affect not only income but the price of time, increased wages create a substitution effect, in which the outside option to schooling becomes relatively more expensive in high rainfall years. When wages are higher, older children might be more likely to work in the labor market, and even younger children could be engaged in home production.[18] In addition, higher wages and agricultural productivity could lead parents to be more likely to work, and thus less likely to be at home. This could imply less time spent with children, and particularly less time investing in children's human capital. To the extent that both parental and child

[18] This work could be agricultural, but need not be. For example, higher wages could drive older relatives out of the home and into the workforce, and young children could be substituting for their labor in chores like cleaning and caring for younger siblings.

time are important inputs into children's human capital, both of these channels could cause lower cognitive test scores in years with higher rainfall.

Table 8 shows the effect of rainfall shocks on work and hours worked (conditional on working) and Table 9 does the same for log wages (conditional on non-zero wages). In drought years, children ages 6-16 (same age as ASER sample children) are less likely to report working by one percentage point, or approximately 17 percent. The coefficients on wages are negative and large. Since the sample size becomes quite small for the age group 6-16, we also look at young adults (ages 6-25). Young adults earn approximately fourteen percent less in drought years. Likewise, in positive shock years young adults are more likely to report working, and though we do not see a similar increase in wages for the current year, we do observe it for the lagged year. This is consistent with the hypothesis that there is more agricultural and related work in years with more rainfall, and students who are on the margin between continuing with school and dropping out to work are more likely to choose to work in good years.

For the sample of adults (these are men and women who have children living in their households so are likely to be parents), both men and women are less likely to report working in negative shock years and more likely to report working in positive shock years. Women are more 20 percent less likely to work during drought years and 6 percent more likely to work in positive shock years. Men are only 4 percent less likely to work during drought years and 12 percent more likely to work in positive rainfall shock years. As we might expect, mothers respond more to rainfall shocks in terms of choosing to work or not (relative to fathers). In terms of wages, negative shocks imply lower wages though the standard errors are large. The wage results for the positive shock years are somewhat mixed. In general, these findings are consistent with a changing price of parental time, which could also affect human capital attainment, particularly of young children.

Figures 5 and 6 show the main results for older children broken down by age. Rainfall shocks change the price of time for both children and parents, and these inputs might be more or less important at different ages. It seems likely that the effect of parental time would be stronger for younger children, while the relative price of market work or home production might have more of an effect for older children. As is clear in both Figure 5 and 6, the coefficients for various ages look very similar, and we cannot reject that they are all the same size. It is likely that both parental and child time are important inputs into human capital, and since both relative prices are moving, neither dominates our results.

While we do not have long-term outcomes of the children in the ASER survey, we can look at whether young adults exposed to drought in utero have worse labor market outcomes than their peers. In Table 8, using NSS data we show that children ages 6-16 who experienced a drought in utero are less likely to work, and they have significantly lower wages. The in-utero effect on working disappears once we look at young adults (ages 6-25); however, the wage effect holds. The magnitude of the wage effect is fairly large, approximately 6 percent, and it is roughly equivalent to an additional year of schooling.

#### 6.2.2 Alternative Explanations

**Teacher Absences** From Table 8, it is clear that employment and wages are affected by rainfall shocks. Thus, as the outside option for students and parents increases in value, so does the outside option for teachers. It is possible that the effects of rainfall shocks on test scores, and even on student absence and dropout rates, could be affected by teacher absences. We think this is unlikely in the context of India, because while absence rates for teachers are high overall, teachers are well-educated and fairly highly paid workers, and generally the wages that are most affected by rainfall shocks are those for agricultural laborers, who earn very little. The additional wage income available during good years for day labor such as weeding and harvesting is still very little relative to teacher's salaries.[19]

In Table 10 we show the effects of negative and positive shocks on teacher absence rates recorded by surveyors in the ASER School Survey. While the coefficients on positive shocks

[19]Indeed, wages in the educational sector can be as much as 10 times higher than wages in the agricultural sector (NSS 2005).

are positive and those on negative shocks are positive, the magnitudes are small and the coefficients are not statistically significant. Therefore we do not think that teacher absences are the main driver of our results for older children.

**School Lunches** In 2005, the Indian Supreme Court determined that all Indian schools should provide lunches to their students. Since that time, many schools have begun lunch programs, but compliance is still well under 100 percent. It is possible that during drought years, families value these lunches more, and thus send their children to school more during these years. The ASER School survey asks about lunch provision, and we use the variation in response by district to try and disentangle this effect. In Table 11 we show results from our main specification (in Table 7) for districts which are in the top quartile of lunch provision (at least 90 percent of schools report serving lunch).

The coefficients on negative shock this year for both the math and reading scores indicate that it cannot be the case that school lunches are driving the the main test score results. In fact, the effect of drought on math scores is smaller when the school provides lunches, which is the opposite of what we would expect. The reading coefficient becomes negative. In addition, though it appears that attendance might be slightly higher at these schools which provide lunches, we cannot reject that the attendance results are the same in Tables 11 and 7 for negative shock years. It is important to note the caveat that the school lunch and teacher absence results presented in Tables 10 and 11 are suggestive because the schools sampled in the ASER School Survey (unlike the households) are not a representative, random sample of schools in the district.

## 7 Discussion and Conclusion

We have shown that droughts affect cognitive test scores, and that these effects differ by age. Children who are exposed to drought in utero score significantly worse on literacy and numeracy tests than their peers. The magnitude of the effects are strongest for exposure during the in utero period and stop being negative after the age of two. Further, we show evidence that these children are less likely to be on track in school and less likely to ever enrol. We argue that the results are causal and not due to differences in the sample of children exposed to these shocks. For the in utero and infancy periods, it is likely that nutrition is the most important input into human capital acquisition, and that droughts and other income shocks are detrimental to later cognitive ability.

While the same pathways are still likely relevant for older children, our analysis shows that time inputs become relatively more important. Children who are tested during a drought year score significantly better on math tests, and report higher attendance rates. In positive rainfall years, the opposite holds true. At least for cognitive test scores, it appears that nutrition is not the most important factor for human capital development for school aged children. Even though droughts are associated with negative income shocks, children are scoring higher on cognitive tests in these bad years. We argue that the likely explanation lies in the relative paucity of outside options during bad rainfall years, both in the home and in the labor market, leading to increased school attendance. Children on the margin of missing school or dropping out might stay in school if wages are low and outside opportunities are scarce.

While we do not have direct time use data for the children in the ASER data set, we show that for similar aged samples in rural areas, both the probability of working and hours worked decrease during droughts, which is consistent with this theory. Parents also work less during droughts, and are likely able to spend more time investing in their children. We cannot distinguish between the relative importance of child vs. parent time, but speculate that both are important inputs into human capital production.

It is important to note that these results are likely to hold only in a context in which there is sufficient scope to substitute from labor market to human capital time allocation. In particular, a child labor market (or significant home production capacity) is necessary. In addition, low (or no) school fees are important factors for the substitution effect to dominate.

In Appendix Table A7, we show that our effects are stronger for children in government (free) schools, relative to those in private schools, which lends credence to this assertion. However, as with the heterogeneous treatment effects shown in Table A6, this is also consistent with poorer children being more likely to be on the margin of school attendance.

The consequence of these results for the overall picture of primary schooling in India is somewhat mixed. On the one hand, while nominal enrollment rates are high, attendance rates remain low, and are clearly sensitive to labor market opportunities. On the other hand, income does not appear to be a significant barrier to primary education, even for likely credit constrained households, since enrollment and attendance increase during periods of bad income shocks. India's stated goal is free and compulsory schooling for all children under 14; our evidence suggests that while schooling is likely close to free, it is in fact still not a compulsory practice for rural children.

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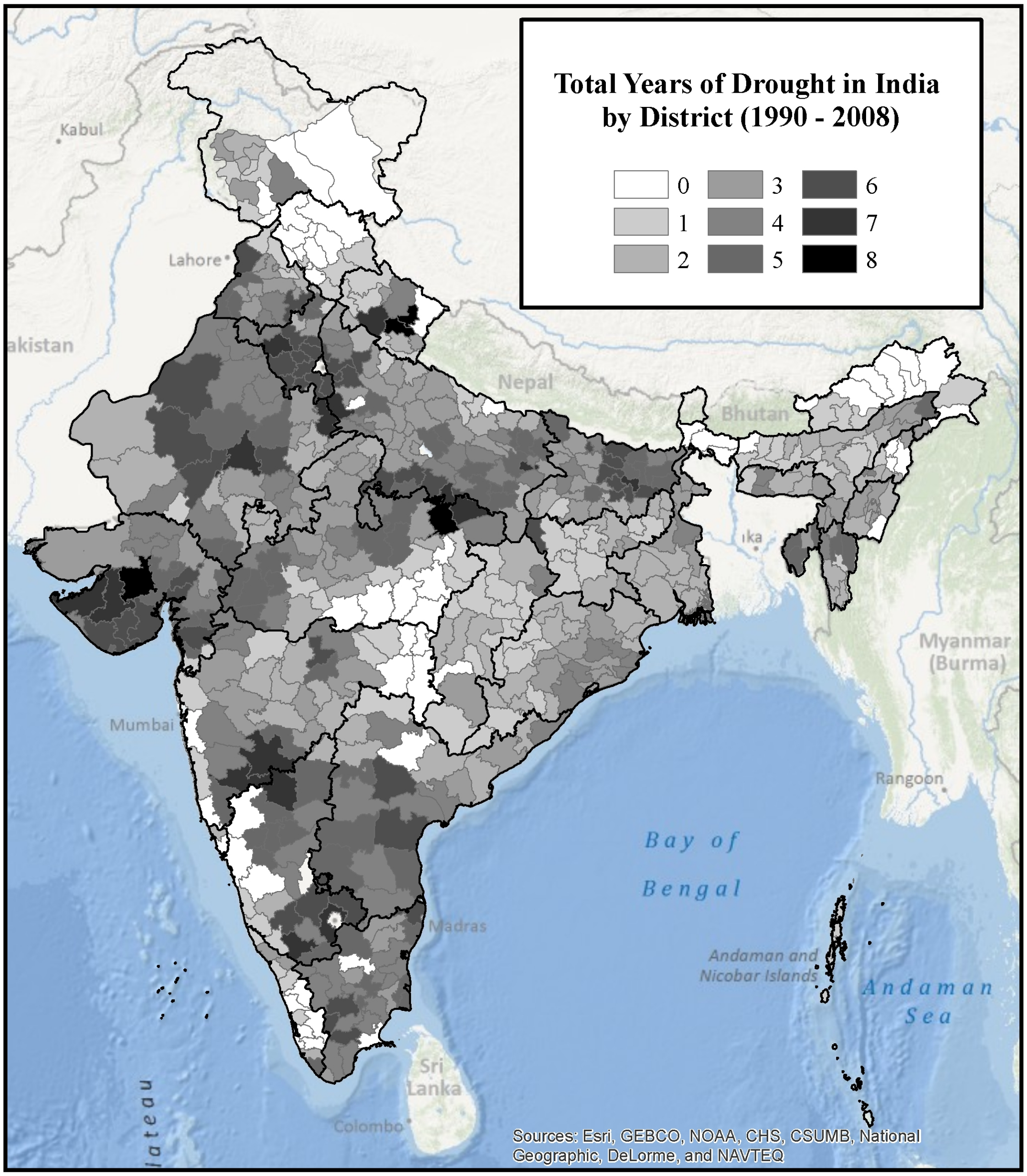


Figure 1: Variation in Drought Across District and Time

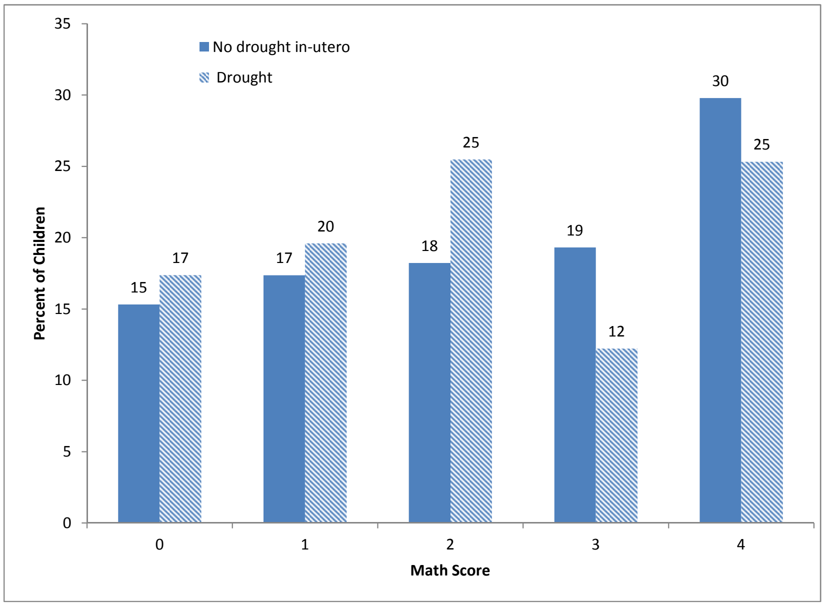


Figure 2: In Utero Drought Exposure and Math Scores

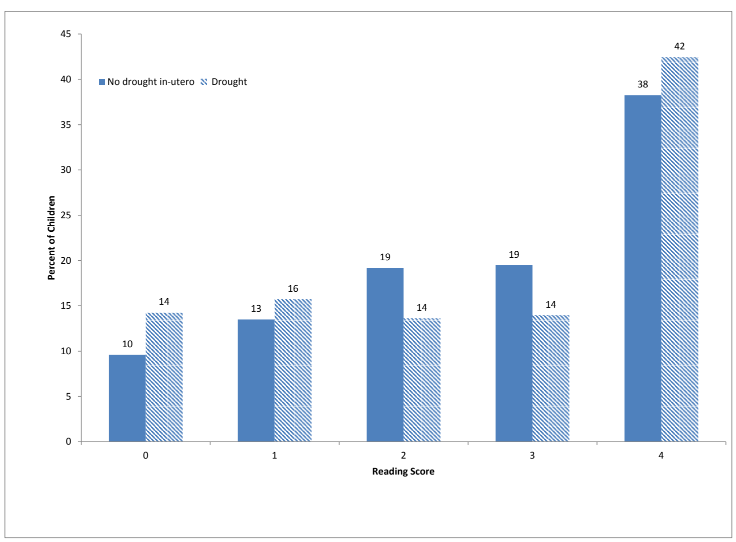


Figure 3: In Utero Drought Exposure and Reading Scores

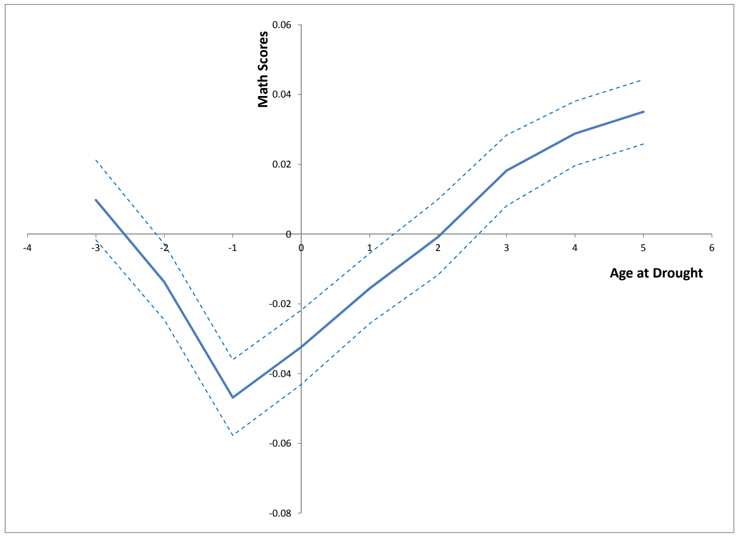


Figure 4: Effect of Early Drought Exposure at Each Age on Current Test Scores

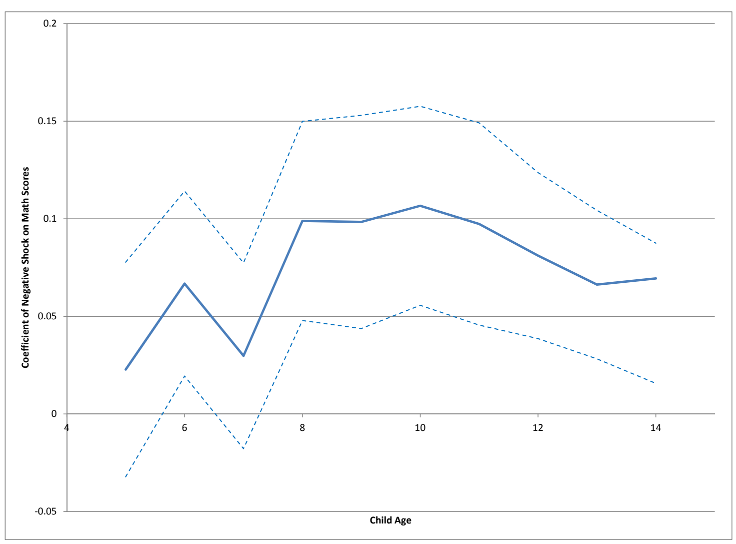


Figure 5: Effect of Negative Shocks on Current Test Scores

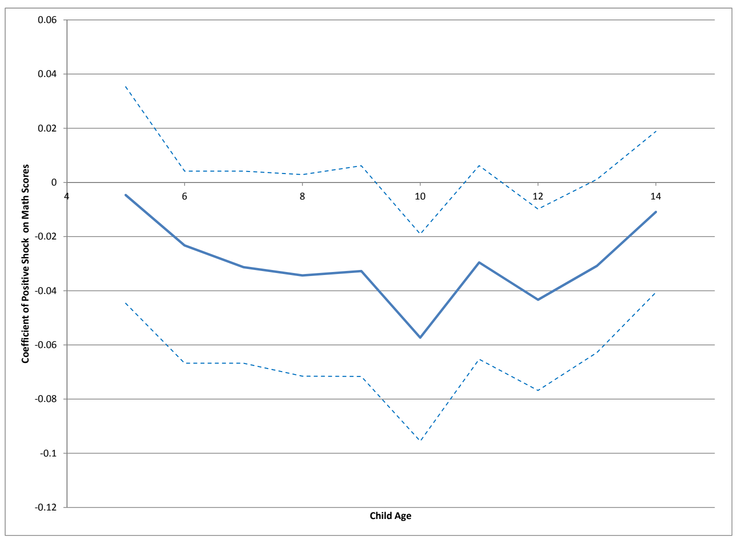


Figure 6: Effect of Positive Shocks on Current Test Scores

Table 1: ASER Summary Statistics

| Child Characteristics | | | |
|---|---|---|---|
| | Mean | Std. Dev. | Observations |
| Male | .54 | .498 | 2,729,313 |
| Age | 10.3 | 3.62 | 2,717,368 |
| Grade | 4.60 | 2.85 | 2,509,008 |
| **Math Scores** | | | |
| | Mean | Std. Dev. | Observations |
| Can Recognize Numbers 1-9 | .651 | .477 | 2,499,352 |
| Can Recognize Numbers 10-99 | .539 | .498 | 2,499,352 |
| Can Subtract | .616 | .486 | 2,499,352 |
| Can Divide | .384 | .487 | 2,499,352 |
| Math Score | 2.20 | 1.35 | 2,499,352 |
| **Reading Scores** | | | |
| | Mean | Std. Dev. | Observations |
| Can Read Letters | .897 | .304 | 2,729,313 |
| Can Read Words | .754 | .431 | 2,729,313 |
| Can Read Paragraph | .608 | .488 | 2,729,313 |
| Can Read Story | .446 | .497 | 2,729,313 |
| Reading Score | 2.71 | 1.4 | 2,729,313 |
| **Schooling Outcomes** | | | |
| | Mean | Std. Dev. | Observations |
| Never Enrolled | .028 | .165 | 2,811,160 |
| Dropped Out | .036 | .187 | 2,811,160 |
| On Track | .813 | .390 | 2,134,088 |
| **Drought Exposure** | | | |
| In Utero Drought | .177 | .382 | 2,876,063 |
| Drought in Year of Birth | .174 | .379 | 2,876,063 |

Notes: This table shows summary statistics for the ASER data set as well as exposure to drought from the rainfall data, which we use in subsequent analysis.

Table 2: Effect of In Utero Drought on Test Scores

**Panel A: Math Scores**

| | Recognizes 1 to 9 | Recognizes 10 to 99 | Can Subtract | Can Divide | Math Score |
|---|---|---|---|---|---|
| In Utero Drought | -.007 | -.03 | -.01 | -.002 | -.05 |
| | (.002)*** | (.003)*** | (.003)*** | (.003) | (.006)*** |
| Observations | 2,178,830 | 2,178,830 | 2,178,830 | 2,178,830 | 2,178,830 |
| Mean Dependent Variable | .65 | .54 | .62 | .39 | 2.20 |

**Panel B: Reading Scores**

| | Can Read Letter | Can Read Word | Can Read Paragraph | Can Read Story | Reading Score |
|---|---|---|---|---|---|
| In Utero Drought | .005 | -.0003 | -.007 | -.007 | -.009 |
| | (.002)** | (.002) | (.002)*** | (.002)*** | (.006) |
| Observations | 2,389,240 | 2,389,240 | 2,389,240 | 2,389,240 | 2,389,240 |
| Mean Dependent Variable | .90 | .76 | .61 | .45 | 2.71 |

**Panel C: Math Scores, Household Fixed Effects**

| | Recognizes 1 to 9 | Recognizes 10 to 99 | Can Subtract | Can Divide | Math Score |
|---|---|---|---|---|---|
| In Utero Drought | -.007 | -.018 | -.014 | -.002 | -.040 |
| | (.002)*** | (.002)*** | (.003)** | (.003) | (.007)*** |
| Observations | 2,003,320 | 2,003,320 | 2,003,320 | 2,003,320 | 2,003,320 |
| Mean Dependent Variable | .65 | .54 | .62 | .39 | 2.20 |

**Panel D: Reading Scores, Household Fixed Effects**

| | Can Read Letter | Can Read Word | Can Read Paragraph | Can Read Story | Reading Score |
|---|---|---|---|---|---|
| In Utero Drought | -.002 | -.004 | -.007 | -.007 | -.020 |
| | (.002) | (.003) | (.003)** | (.003)** | (.008)** |
| Observations | 2,138,351 | 2,138,351 | 2,138,351 | 2,138,351 | 2,138,351 |
| Mean Dependent Variable | .90 | .76 | .61 | .45 | 2.71 |

Notes: This table shows our estimates of the effect of drought in utero on test scores. All regressions contain fixed effects for age, year of survey, and are clustered at the district level. Panels A and B contain fixed effects for district, while Panels C and D contain fixed effects for household. Children are marked as having a drought occur while in utero if there was a drought during the monsoon season of the year prior to their birth. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 3: Alternative Measures of Rainfall

| | Math Score (1) | Math Score (2) | Reading Score (3) | Reading Score (4) |
|---|---|---|---|---|
| Rainfall (in) | .0001 | | .00001 | |
| | (.00001)*** | | (.00001) | |
| Bottom Quintile Rainfall | | -.030 | | .006 |
| | | (.006)*** | | (.006) |
| Second Quintile Rainfall | | -.014 | | -.012 |
| | | (.005)*** | | (.004)*** |
| Fourth Quintile Rainfall | | .015 | | .005 |
| | | (.005)*** | | (.004) |
| Highest Quintile Rainfall | | .029 | | .01 |
| | | (.005)*** | | (.005)*** |
| Observations | 2,186,446 | 2,186,446 | 2,389,240 | 2,389,240 |

Notes: This table shows our estimates of the effect of two different measures of rainfall on math and reading test scores. The mean of rainfall is 1,286 inches and the standard deviation is 788.5 inches. All specifications include district, age, and year of survey fixed effects and are clustered at the district level. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 4: Timing of Drought Effects

| Independent Variable: | Dependent Variable: | |
|---|---|---|
| Age at Drought | Math Score | Reading Score |
| -3 | .006 | .004 |
| | (.006) | (.006) |
| -2 | -.01 | .01 |
| | (.006)** | (.006)* |
| -1 (In Utero) | -.04 | -.008 |
| | (.006)*** | (.006) |
| 0 | -.03 | -.006 |
| | (.006)*** | (.006) |
| 1 | -.02 | -.01 |
| | (.006)*** | (.006)** |
| 2 | -.004 | .001 |
| | (.005) | (.006) |
| Observations | 2,178,830 | 2,389,240 |

Notes: This table shows our estimates of the effect of a drought from two years before to two years after birth. All regressions contain age, year of survey, and district fixed effects and are clustered at the district level. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 5: Effect of In Utero Drought on Schooling Attainment

| | | | |
|---|---|---|---|
| **Panel A: All Children** | | | |
| | *Dependent Variable* | | |
| | Dropped Out | Never Enrolled | On Track |
| In Utero Drought | .0001 | .002 | -.02 |
| | (.0007) | (.0005)*** | (.003)*** |
| Observations | 2,451,738 | 2,451,738 | 1,988,846 |
| Mean of Dependent Variable | .04 | .03 | .81 |
| **Panel B: Boys Only** | | | |
| In Utero Drought | .001 | .001 | -.02 |
| | (.0008) | (.0006) | (.003)*** |
| Observations | 1,319,974 | 1,319,974 | 1,069,615 |
| Mean of Dependent Variable | .03 | .02 | .81 |
| **Panel C: Girls Only** | | | |
| In Utero Drought | -.001 | .003 | -.02 |
| | (.001) | (.001)*** | (.003)*** |
| Observations | 1,104,267 | 1,104,267 | 898,577 |
| Mean of Dependent Variable | .04 | .03 | .81 |
| **Panel D: All Children, With Household Fixed Effects** | | | |
| In Utero Drought | .00003 | .002 | -.02 |
| | (.001) | (.001)*** | (.004)*** |
| Observations | 2,160,152 | 2,160,152 | 1,734,632 |
| Mean of Dependent Variable | .04 | .03 | .81 |

Notes: This table shows our estimates of the effect of drought in utero on schooling outcomes. Panel A includes all children, while Panels B and C restrict of boys and girls, respectively. All regressions contain age, year of survey, and district fixed effects and are clustered at the district level. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 6: Effect of In Utero Drought on Child Health Outcomes

| | | |
|---|---|---|
| | *Dependent Variable* | |
| | Height (cm) | Weight (kilos) |
| In Utero Drought | -6.89 | -.80 |
| | (3.39)** | (.62) |
| Observations | 18,283 | 18,283 |
| Mean of Dependent Variable | 71.7 | 8.17 |

Notes: This table shows our estimates of the effect of drought in utero on health outcomes. The sample is children under 3 in the NFHS data set. All regressions contain year of birth and state fixed effects. All specifications are clustered at the district level. Children are marked as having a drought occur while in utero if there was a drought during the monsoon season of the year prior to their birth. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 7: Effect of Contemporaneous Rainfall Shock on Cognitive Test Scores

| **Panel A: Negative Shocks** | | | | |
|---|---|---|---|---|
| | *Dependent Variable:* | | | |
| | Math Score | Reading Score | Dropped Out | Attendance |
| Negative Shock This Year | .1 | -.01 | .001 | .02 |
| | (.02)*** | (.02) | (.002) | (.01)** |
| Negative Shock Last Year | .03 | .007 | -.002 | |
| | (.02) | (.02) | (.001) | |
| Drought In-Utero | -.05 | -.01 | -.0003 | -.004 |
| | (.006)*** | (.007) | (.0007) | (.003) |
| Observations | 1,631,831 | 1,838,708 | 1,885,367 | 446,780 |
| **Panel B: Positive Shocks** | | | | |
| | *Dependent Variable:* | | | |
| | Math Score | Reading Score | Dropped Out | Attendance |
| Positive Shock This Year | -.05 | -.01 | .002 | -.004 |
| | (.01)*** | (.01) | (.001) | (.008) |
| Positive Shock Last Year | -.05 | -.05 | .003 | |
| | (.02)*** | (.02)*** | (.001)** | |
| Drought In-Utero | -.05 | -.01 | -.0002 | -.004 |
| | (.006)*** | (.007)* | (.0007) | (.003) |
| Observations | 1,631,831 | 1,838,708 | 1,885,367 | 446,780 |
| State FEs | N | N | N | Y |
| District FEs | Y | Y | Y | N |
| Year FEs | Y | Y | Y | N |
| Mean of DV | 2.20 | 2.71 | 0.04 | 0.86 |

Notes: This table shows our estimates of the effect of positive and negative rainfall shocks in utero on current scores. Positive and negative shocks are defined as rainfall above the 80th percentile and below the 20th percentile of district rainfall respectively. All regressions contain fixed effects for age, year of survey, and are clustered at the district level. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 8: Effect of Shocks on Labor Force Participation

| **Panel A: Effect of Negative Shocks on Work and Hours** | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| *Dependent Variable:* | *Works (Males)* | *Works (Females)* | *Works (Age 6-16)* | *Works (Age 6-25)* | *ln Hours (Males)* | *ln Hours (Females)* | *ln Hours (Age 6-16)* | *ln Hours (Age 6-25)* |
| Negative Shock This Year | -.03 (.009)*** | -.07 (.03)** | -.01 (.005)** | -.02 (.009)** | -.06 (.03)* | -.02 (.05) | -.08 (.05)* | -.08 (.04)** |
| Negative Shock Last Year | .007 (.005) | .01 (.03) | .007 (.005) | .006 (.008) | .03 (.02)* | .03 (.03) | .0002 (.03) | .008 (.02) |
| Household Size | -.003 (.0007)*** | -.01 (.002)*** | -.0004 (.0005) | -.001 (.0006)** | .005 (.002)*** | .01 (.002)*** | .007 (.006) | .005 (.002)*** |
| School | .001 (.0005)** | -.02 (.002)*** | .007 (.0005)*** | .02 (.0006)*** | .01 (.001)*** | .01 (.002)*** | -.03 (.006)*** | .005 (.002)*** |
| Experience | .01 (.0003)*** | .001 (.0003)*** | .04 (.002)*** | .05 (.0006)*** | .002 (.0003)*** | .0007 (.0005) | -.03 (.005)*** | -.002 (.001)** |
| Female | | | -.03 (.003)*** | -.23 (.005)*** | | | -.07 (.03)*** | -.02 (.01) |
| In Utero Drought | | | -.02 (.005)*** | -.006 (.005) | | | -.03 (.04) | -.01 (.01) |
| Observations | 48,908 | 48,322 | 45,345 | 85,388 | 41,658 | 17,216 | 2837 | 22,566 |
| **Panel B: Effect of Positive Shocks on Work and Hours** | | | | | | | | |
| *Dependent Variable:* | *Works (Males)* | *Works (Females)* | *Works (Age 6-16)* | *Works (Age 6-25)* | *ln Hours (Males)* | *ln Hours (Females)* | *ln Hours (Age 6-16)* | *ln Hours (Age 6-25)* |
| Positive Shock This Year | .02 (.007)** | .11 (.02)*** | .02 (.008)*** | .04 (.01)*** | .06 (.02)*** | .02 (.03) | .04 (.04) | .05 (.02)** |
| Positive Shock Last Year | .007 (.01) | .03 (.05) | -.01 (.008) | .006 (.02) | -.04 (.04) | -.05 (.06) | -.02 (.07) | -.01 (.05) |
| Household size | -.003 (.0007)*** | -.01 (.002)*** | -.0005 (.0005) | -.002 (.0006)** | .005 (.001)*** | .009 (.002)*** | .006 (.006) | .005 (.002)*** |
| School | .001 (.0005)** | -.02 (.002)*** | .007 (.0005)*** | .02 (.0006)*** | .01 (.001)*** | .01 (.002)*** | -.03 (.006)*** | .005 (.002)*** |
| Experience | .01 (.0003)*** | .001 (.0003)*** | .04 (.002)*** | .05 (.0006)*** | .002 (.0003)*** | .0007 (.0005) | -.03 (.006)*** | -.003 (.001)** |
| Female | | | -.03 (.003)*** | -.23 (.005)*** | | | -.07 (.03)** | -.02 (.01) |
| In Utero Drought | | | -.02 (.005)*** | -.006 (.005) | | | -.03 (.04) | -.01 (.01) |
| Observations | 48,908 | 48,322 | 45,345 | 85,388 | 41,658 | 17,216 | 2837 | 22,566 |
| Mean of Dep Var | .85 | .36 | .06 | .26 | 3.91 | 3.82 | 4.2 | 3.9 |

Notes: These are weighted OLS regressions using the 2005-06 NSS data where the dependent variable is currently works (adult male with children in household) in column 1, currently works (adult female with children in household) in column 2, currently works (ages 6-16) in column 3, and currently works (ages 6-25) in column 4. The dependent variable in columns 5-8 is ln hours (conditional on working) for the same groups. All regressions contain state region fixed effects. All specifications are clustered at the district level. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 9: Effect of Shocks on Wages

| Panel A: Effect of Negative Shocks on Wages | | | | |
|---|---|---|---|---|
| *Dependent Variable:* | *ln Wages (Males)* | *ln Wages (Females)* | *ln Wages (Ages 6-16)* | *ln Wages (Ages 6-25)* |
| Negative Shock This Year | -.11 | -.05 | -.24 | -.14 |
| | (.07) | (.09) | (.19) | (.09) |
| Negative Shock Last Year | -.08 | -.1 | -.11 | -.13 |
| | (.03)** | (.05)** | (.07) | (.04)*** |
| Household Size | -.004 | -.003 | .01 | -.007 |
| | (.004) | (.006) | (.01) | (.005) |
| School | .09 | .08 | .08 | .07 |
| | (.002)*** | (.006)*** | (.02)*** | (.004)*** |
| Experience | .02 | .008 | .09 | .04 |
| | (.0007)*** | (.001)*** | (.02)*** | (.003)*** |
| Female | | | -.28 | -.42 |
| | | | (.05)*** | (.03)*** |
| In Utero Drought | | | -.14 | -.06 |
| | | | (.08)* | (.02)** |
| Observations | 18953 | 7147 | 1457 | 11446 |

| Panel B: Effect of Positive Shocks on Wages | | | | |
|---|---|---|---|---|
| *Dependent Variable:* | *ln Wages (Males)* | *ln Wages (Females)* | *ln Wages (Ages 6-16)* | *ln Wages (Ages 6-25)* |
| Positive Shock This Year | -.16 | -.11 | -.02 | -.09 |
| | (.04)*** | (.06)* | (.08) | (.05)* |
| Positive Shock Last Year | .06 | .07 | .39 | .14 |
| | (.06) | (.06) | (.11)*** | (.06)** |
| Household Size | -.003 | -.002 | .01 | -.006 |
| | (.004) | (.006) | (.01) | (.005) |
| School | .09 | .08 | .08 | .07 |
| | (.002)*** | (.006)*** | (.02)*** | (.004)*** |
| Experience | .02 | .008 | .09 | .04 |
| | (.0007)*** | (.001)*** | (.02)*** | (.003)*** |
| Female | | | -.26 | -.42 |
| | | | (.05)*** | (.03)*** |
| In Utero Drought | | | -.12 | -.06 |
| | | | (.08) | (.02)** |
| Mean Dep Var | 5.87 | 5.18 | 5.17 | 5.55 |
| Observations | 18,953 | 7147 | 1457 | 11446 |

Notes: These are weighted OLS regressions using the 2005-06 NSS data where the dependent variable is ln wages for adult men with children in household in column 1, adult female with children in household in column 2, children ages 6-16 in column 3, and individuals ages 6-25 in column 4. All regressions contain state region fixed effects. All specifications are clustered at the district level. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 10: Teacher Absences

| Panel A: Negative Shocks | |
|---|---|
| | *Dependent Variable:* |
| | Teacher Attendance Rate |
| Negative Shock This Year | .011 |
| | (.022) |
| Negative Shock Last Year | .005 |
| | (.016) |
| Observations | 20,297 |
| **Panel B: Positive Shocks** | |
| | *Dependent Variable:* |
| | Teacher Attendance Rate |
| Positive Shock This Year | -.031 |
| | (.017) |
| Positive Shock Last Year | -.003 |
| | (.024) |
| Observations | 20,297 |
| Mean of Dependent Variable | .847 |

Notes: This table shows the effect of positive and negative rainfall shocks on teacher absence rates from the ASER School Survey. Independent variable is drought in the year of survey. All regressions have fixed effects for year of survey and district. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table 11: School Lunches

| Districts in Top Quartile of School Lunch Provision | | | | |
|---|---|---|---|---|
| | *Dependent Variable:* | | | |
| | Math Score | Read Score | Dropped Out | Attendance |
| Negative Shock This Year | .01 | -.08 | -.001 | .04 |
| | (.04) | (.05)* | (.003) | (.02)** |
| Negative Shock Last Year | .01 | -.02 | -.004 | |
| | (.04) | (.04) | (.002)** | |
| In Utero Drought | -.03 | -.008 | .001 | -.005 |
| | (.01)*** | (.01) | (.001) | (.003) |
| Observations | 529,127 | 591,489 | 607,889 | 149,260 |
| Mean of Dependent Variable | 2.37 | 2.85 | 0.03 | 0.88 |

Notes: This table shows the estimates from Table 7 broken down by districts whose schools were the most likely to report serving school lunches (top quartile) in the ASER school survey. The top quartile of districts had more than 90% of schools which reported serving lunch. All regressions contain fixed effects for district and year (except for attendance which has state fixed effects). Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table A1: Drought and Crop Yields: 1957-1987

| | *Dependent Variable:* | | | | | |
|---|---|---|---|---|---|---|
| | Rice | | Wheat | | Jowar | |
| Drought | -.41 | -.32 | -.14 | -.16 | -.09 | -.16 |
| | (.04)*** | (.04)*** | (.02)*** | (.02)*** | (.02)*** | (.03)*** |
| Year FE | Y | Y | Y | Y | Y | Y |
| District FE | Y | Y | Y | Y | Y | Y |
| Controls | Y | N | Y | N | Y | N |
| Observations | 7161 | 8401 | 6680 | 8401 | 6265 | 7409 |
| Mean of Dependent Variable | 1.51 | 1.51 | .856 | .856 | .589 | .589 |

Notes: This table tests if crop yields react to drought using the World Bank India Agriculture and Climate Data set which has agricultural yield (revenues per acre) data from 1957-1987. Controls include inputs such as fertilizer, machinery, etc. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table A2: Testing for Serial Correlation in Rainfall

| Dependent Variable: Deviation from district mean this year | | |
|---|---|---|
| | (1) | (2) |
| Deviation from district mean last year | .005 | -.031*** |
| | (.011) | (.010) |
| Year Fixed Effects | N | Y |
| Observations | 9,248 | 9,248 |

Notes: This table tests if there is serial correlation in rainfall in our data. An observation is a district-year. The dependent variable in both regressions is the deviation from mean rainfall in the current year (in inches), where deviation is simply defined as current year rainfall minus the mean rainfall in sample period. The independent variable is deviation from mean rainfall last year (in inches), constructed in the same way. The mean of the deviation is 0 (2.2e-06)and the standard deviation is 223 inches. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

## A Appendix

Table A3: Effect of In Utero Drought on Test Scores, by Gender

| *Independent Variable: Drought in Utero* | | | | |
|---|---|---|---|---|
| **Panel A: Math Scores, Boys Only** | | | | |
| | Coeff | Std. Error | Observations | R-Sq. |
| *Dependent Variable:* | | | | |
| Can Recognize 1 to 9 | -.008*** | .002 | 1,176,065 | 0.7549 |
| Can Recognize 10 to 99 | -.028*** | .003 | 1,176,065 | 0.5821 |
| Can Subtract | -.012** | .003 | 1,176,065 | 0.3905 |
| Can Divide | -.0001 | .003 | 1176065 | 0.3064 |
| Math Score | -.048*** | .006 | 1,176,065 | 0.5546 |
| **Panel B: Reading Scores, Boys Only** | | | | |
| | Coeff | Std. Error | Observations | R-Sq. |
| *Dependent Variable:* | | | | |
| Can Read Letter | .005** | .002 | 1,290,070 | 0.1730 |
| Can Read Word | -.001 | .002 | 1,290,070 | 0.3249 |
| Can Read Paragraph | -.007*** | .002 | 1,290,070 | 0.3768 |
| Can Read Story | -.005** | .002 | 1,290,070 | 0.3353 |
| Reading Score | -.008 | .006 | 1,290,070 | 0.4371 |
| **Panel C: Math Scores, Girls Only** | | | | |
| | Coeff | Std. Error | Observations | R-Sq. |
| *Dependent Variable:* | | | | |
| Can Recognize 1 to 9 | .004* | .002 | 983,569 | 0.7366 |
| Can Recognize 10 to 99 | -.001 | .003 | 983,569 | 0.5610 |
| Can Subtract | -.008*** | .003 | 983,569 | 0.3769 |
| Can Divide | -.011*** | .003 | 983,569 | 0.2922 |
| Math Score | -.015*** | .006 | 983,569 | 0.5429 |
| **Panel D: Reading Scores, Girls Only** | | | | |
| | Coeff | Std. Error | Observations | R-Sq. |
| *Dependent Variable:* | | | | |
| Can Read Letter | .002 | .002 | 1,079,822 | 0.1742 |
| Can Read Word | .0001 | .002 | 1,079,822 | 0.3218 |
| Can Read Paragraph | -.005*** | .003 | 1,079,822 | 0.3739 |
| Can Read Story | -.012*** | .003 | 1,079,822 | 0.3348 |
| Reading Score | -.015** | .006 | 1,079,822 | 0.4317 |

Notes: This table shows our estimates of the effect of drought in utero on test scores by gender. All regressions contain age, year of survey, and district fixed effects and are clustered at the district level. Children are marked as having a drought occur while in utero if there was a drought during the monsoon season of the year prior to their birth. Panels A and B restrict the sample to only male children, and Panels C and D restrict the sample to only female children. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table A4: Does Drought Impact Fertility Decisions?

| | ln cohort size (born 1991) (1) | ln cohort size (born 2001) (2) |
|---|---|---|
| Drought (t) | .02 | -.007 |
| | (.03) | (.02) |
| Drought In utero (t-1) | -.06 | .01 |
| | (.04) | (.03) |
| Drought (t-2) | -.03 | -.02 |
| | (.03) | (.02) |
| Drought (t-3) | -.12 | -.03 |
| | (.06)** | (.04) |
| Drought (t-4) | -.04 | .11 |
| | (.02) | (.02)*** |
| Drought (t-5) | -.04 | -.03 |
| | (.03) | (.03) |
| ln Population 1991 | .04 | |
| | (.02)** | |
| ln Population 2001 | | .02 |
| | | (.02) |
| Observations | 104,630 | 207,905 |
| Mean of dependent variable | 5.33 | 5.98 |

Notes: These are OLS regressions where the dependent variable is ln number of births in each district in 1991 and 2001. All regressions contain state and year of survey fixed effects. Standard errors are clustered at the district level and are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table A5: Effect of Contemporaneous Shocks on Test Scores, by Gender

| **Panel A: Negative Shocks, Boys Only** | | | | |
|---|---|---|---|---|
| | *Dependent Variable:* | | | |
| | Math Score | Reading Score | Dropped Out | Attendance |
| Negative Shock This Year | .09 | -.01 | .002 | .02 |
| | (.02)*** | (.02) | (.002) | (.01)** |
| Negative Shock Last Year | .02 | .004 | -.002 | |
| | (.02) | (.02) | (.001) | |
| Observations | 1,033,251 | 1,153,456 | 1,192,358 | 251,397 |
| **Panel B: Negative Shocks, Girls Only** | | | | |
| | *Dependent Variable:* | | | |
| | Math Score | Reading Score | Dropped Out | Attendance |
| Negative Shock This Year | .11 | -.01 | -.0003 | .02 |
| | (.02)*** | (.02) | (.002) | (.01)* |
| Negative Shock Last Year | .04 | .01 | -.002 | |
| | (.02)** | (.02) | (.002) | |
| Observations | 855,030 | 957,720 | 988,483 | 209,589 |
| **Panel C: Positive Shocks, Boys Only** | | | | |
| | *Dependent Variable:* | | | |
| | Math Score | Reading Score | Dropped Out | Attendance |
| Positive Shock This Year | -.04 | -.008 | .002 | -.003 |
| | (.01)*** | (.01) | (.001) | (.008) |
| Positive Shock Last Year | -.05 | -.05 | .003 | |
| | (.01)*** | (.02)*** | (.001)** | |
| Observations | 1,033,251 | 1,153,456 | 1,192,358 | 251,397 |
| **Panel D: Positive Shocks, Girls Only** | | | | |
| | *Dependent Variable:* | | | |
| | Math Score | Reading Score | Dropped Out | Attendance |
| Positive Shock This Year | -.05 | -.02 | .002 | -.006 |
| | (.02)*** | (.01) | (.001) | (.008) |
| Positive Shock Last Year | -.05 | -.05 | .002 | |
| | (.02)*** | (.02)*** | (.001) | |
| Observations | 855,030 | 957,720 | 988,483 | 209,589 |

Notes: This table shows the results of our current-year drought specification (see table 7) for several sub-populations. Panels A and B show effects of current year droughts separately for boys and girls, while Panels C and D show the effect of positive shocks separately for boys and girls. All specifications are clustered at the district level. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table A6: Heterogeneous Treatment Effects

**Panel A: Poorest 7 States**

| | *Dependent Variable:* | |
|---|---|---|
| | Math Score | Reading Score |
| Negative Shock This Year | .13 | .009 |
| | (.03)*** | (.03) |
| Negative Shock Last Year | .02 | .01 |
| | (.03) | (.03) |
| Drought In Utero | -.09 | -.02 |
| | (.01)*** | (.01)* |
| Observations | 759,674 | 857,560 |

**Panel B: Richest 7 States**

| | *Dependent Variable:* | |
|---|---|---|
| | Math Score | Reading Score |
| Negative Shock This Year | -.02 | .03 |
| | (.07) | (.05) |
| Negative Shock Last Year | -.08 | -.1 |
| | (.04)* | (.05)** |
| Drought In Utero | -.02 | -.003 |
| | (.01) | (.008) |
| Observations | 281,024 | 315,702 |

**Panel C: Mother No Schooling**

| | *Dependent Variable:* | |
|---|---|---|
| | Math Score | Reading Score |
| Negative Shock This Year | .11 | -.01 |
| | (.02)*** | (.03) |
| Negative Shock Last Year | .05 | .03 |
| | (.02)* | (.03) |
| Drought In Utero | -.04 | -.02 |
| | (.008)*** | (.008)* |
| Observations | 760,881 | 833,611 |

**Panel D: Mother Any Schooling**

| | *Dependent Variable:* | |
|---|---|---|
| | Math Score | Reading Score |
| Negative Shock This Year | .07 | -.03 |
| | (.02)*** | (.03) |
| Negative Shock Last Year | .04 | .03 |
| | (.02)* | (.02) |
| Drought In Utero | -.04 | -.01 |
| | (.007)*** | (.007)** |
| Observations | 634,687 | 688,996 |

Notes: This table shows the results of our current-year drought specification (see table 7) for several sub-populations. Panel A shows results for the 7 poorest states in India measured by 2011-2012 GDP per capita, which are Bihar, Uttar Pradesh, Jharkhand, Assam, Madhya Pradesh, Rajasthan, and Orissa. Panel B shows results for the 7 richest states, which are Goa, Sikkim, Himachal Pradesh, Maharashtra, Haryana, and Gujarat. Panel C shows results for children whose mother reported having never attended school, and Panel D shows results for children whose mother had ever attended school. All specifications are clustered at the district level. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table A7: Effects of Drought on Test Scores, by School Type

| **Panel A: Effect of Drought on School Type** | | |
|---|---|---|
| | Child Enrolled in: | |
| | Private School | Government School |
| Negative Shock This Year | .008 | -.01 |
| | (.006) | (.007)** |
| Negative Shock Last Year | .0002 | .002 |
| | (.005) | (.005) |
| Drought In Utero | .0007 | -.001 |
| | (.002) | (.003) |
| Observations | 1,885,367 | 1,885,367 |
| **Panel B: Effect of Drought on Math Scores, by School Type** | | |
| | Math Score | |
| | Private School | Government School |
| Negative Shock This Year | .07 | .12 |
| | (.02)*** | (.02)*** |
| Negative Shock Last Year | .02 | .01 |
| | (.02) | (.02) |
| Drought In Utero | -.05 | -.04 |
| | (.008)*** | (.007)*** |
| Observations | 340,437 | 1,172,332 |

Notes: This table shows our estimates of the effect of drought on enrollment in private and government school, and the effects of drought on math scores for children in each type of school. All regressions contain fixed effects for district and survey year. Standard errors in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table A8: Effect of Contemporaneous Drought on Test Scores in High Malaria States

| | *Dependent Variable:* | | | |
|---|---|---|---|---|
| | Math Score | Reading Score | Dropped Out | Attendance |
| Negative Shock This Year | .07 | .007 | -.00 | .02 |
| | (.03)** | (.04) | (.002) | (.01)* |
| Negative Shock Last Year | -.009 | -.008 | -.0001 | |
| | (.03) | (.03) | (.002) | |
| Drought In Utero | -.07 | -.03 | -.001 | -.007 |
| | (.009)*** | (.008)*** | (.001) | (.003)** |
| Observations | 827,316 | 935,231 | 961,366 | 212,451 |

Notes: This table shows the results of our current-year drought specification for high malaria states. All specifications are clustered at the district level. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.

Table A9: Effect of Drought on LFP in Rainfed Rice States

| *Dependent Variable:* | *Works* | *Works* | *Works* | *Works* | *Works* |
|---|---|---|---|---|---|
| *Sample:* | *(Males)* | *(Females)* | *(Age 6-25)* | *(Landed Females)* | *(Landless Females)* |
| Negative Shock This Year | -.03 | -.10 | -.02 | -.14 | -.17 |
| | (.01)** | (.05)** | (.01) | (.06)** | (.08)** |
| Negative Shock Last Year | .004 | -.0001 | -.005 | -.03 | -.05 |
| | (.01) | (.06) | (.02) | (.05) | (.09) |
| Household size | -.005 | -.01 | -.001 | -.01 | -.01 |
| | (.001)*** | (.003)*** | (.001) | (.004)*** | (.01) |
| School | .0006 | -.01 | .02 | -.01 | -.02 |
| | (.001) | (.002)*** | (.001)*** | (.004)*** | (.005)*** |
| Experience | .01 | .002 | .04 | .004 | .0002 |
| | (.0004)*** | (.001)*** | (.001)*** | (.001)*** | (.001) |
| Female | | | -.24 | | |
| | | | (.009)*** | | |
| In Utero Drought | | | -.02 | | |
| | | | (.008)** | | |
| Observations | 21,242 | 19,790 | 35,859 | 4612 | 1390 |

Notes: These are weighted OLS regressions using the 2005-06 NSS data where the dependent variable is currently works (adult male with children in household) in column 1, currently works (adult female with children in household) in column 2, and currently works (ages 6-25) in column 3. Column 4 is women in households with more than 1 hectare of land and column 5 is women in households that are landless or have less than 0.005 hectares of land. Rainfed only rice states include Assam, Bihar, Madhya Pradesh, Orissa, West Bengal, Kerala, Uttaranchal and the North-Eastern hill states (Assam, Meghalaya, Arunachal Pradesh, Nagaland, Manipur, Mizorum, Trupura). All regressions contain state region fixed effects. All specifications are clustered at the district level. Standard errors are reported in parentheses. ***indicates significance at 1% level, ** at 5% level, * at 10% level.