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Abstract

This paper uses birth cohorts spanning several hundred locations over 40 years to examine the long-term consequences of in utero exposure to abnormal rainfall events in Colombia. The identification strategy exploits exogenous variation in extreme droughts or floods experienced by individuals while in utero in their birth location. The results indicate that individuals prenatally exposed to adverse rainfall shocks are more likely to report serious mental illness, have fewer years of schooling, display increased rates of illiteracy, and are less likely to work. These results are larger in magnitude for individuals born in areas with higher risk of malaria, consistent with the notion that exposure to infectious and parasitic diseases may play an important role.

Keywords: Drought; Heavy precipitation; Early life health; Later-life outcomes
JEL Codes: I15, O13, O15

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1 Introduction

It is now widely recognized that emissions of greenhouse gas will alter global climate, causing extreme weather events, such as droughts and floods, to become more frequent. One prominent body of work highlights that these extreme weather events may have persistent effects on human capital acquisition and welfare, especially for children in the developing world. The prenatal programming theory indicates that individuals exposed to an unhealthy environment during a sensitive period of fetal development are likely to suffer from a number of health and developmental difficulties that persist throughout life (Barker, 1997; Seckl, 1998). Since health is both a type of human capital and a contributor to other forms of human capital (Becker, 2007), increasing attention is being paid to the long-run impacts of a variety of early life shocks, including epidemics (Almond, 2006; Venkataramani, 2012; Barreca, 2010), maternal stress (Aizer et al., 2016) and even food availability (Lindeboom et al., 2010; Almond and Mazumder, 2011).[1] Recent notable work has documented that exposure to higher rainfall in early life has positive effects on health, educational and socioeconomic outcomes (Dinkelman, 2017; Maccini and Yang, 2009). This paper provides new evidence on this important question in a context where droughts and excessive precipitation are likely to have adverse consequences. Differently from previous studies, I exploit information on exact date of birth to accurately measure prenatal exposure to rainfall conditions and shed light on the timing of shocks during pregnancy.

There are several channels through which rainfall shocks can have intergenerational consequences on human capital and welfare. It is well established that changes in precipitation may affect the optimal conditions for infectious and parasitic diseases, which could adversely affect the health of pregnant mothers and thus increase the risk of poor health in early life. At the same time, lower yields of subsistence crops and reduced income from cash crops due to water scarcity or excessive precipitation may result in reduced nutrition intake during pregnancy, especially in countries with imperfect credit markets and fewer formal social safety net programs. As a result, poor health among school age children is likely to result in school absenteeism and higher probabilities of dropping out, most notably causing fewer completed schooling (Miguel and Kremer, 2004; Baird et al., 2016).

This paper uses birth cohorts spanning several hundred locations over 40 years (1942-1981) to conduct a systematic evaluation of the relationship between early rainfall shocks and long-run socioeconomic outcomes. There are a few features about Colombia, the focus of this paper, that make it an interesting case in which to study this question. Because Colombia is exposed to both *El Niño* and *La Niña* phenomena, precipitation records vary

[1]See Almond and Currie (2011) for a comprehensive review of literature.

widely over time and space, with some periods characterized by heavy rainfall and others by pervasive droughts. Indeed, Colombia has been considered one of the countries with the highest incidence of extreme events. In 2010, the Global Climate Risk Index placed Colombia in the top 3 countries most affected by loss related to floods and storms (Andalon et al., 2016; Germanwatch, 2011). Moreover, the cohorts this paper analyses were born in a context where a considerable fraction of population was living in rural areas and depended on farming for a living either directly or indirectly. Thus, this paper investigates a context where the aforementioned mechanisms are likely to be relevant.

My identification strategy exploits variation in rainfall records over time within municipalities. I construct a municipality-by-month weather dataset, which then is combined with microdata by using date and place of birth to identify the prevailing rainfall conditions during pregnancy. The empirical approach then compares later-life outcomes of individuals who were prenatally exposed to extreme droughts or heavy rainfall relative to those who experienced less severe rainfall conditions in utero. I control by a full set of municipality-of-birth and month-of-birth $\times$ year-of-birth fixed-effects to account for time invariant characteristics, aggregate shocks, and seasonal factors that might be correlated with the incidence of extreme rainfall events. Hence, this approach exploits arguably random fluctuations in rainfall from municipality-specific deviations in long-term rainfall after controlling for all seasonal factors and common shocks to all municipalities.

I find evidence that in utero exposure to adverse rainfall shocks leads to poorer adult outcomes. In particular, I find that a standard deviation increase in prenatal floods is associated with a 3.2-percent increase in mental disability rates, a 0.21-percent decline in years of schooling, a 1.7-percent increase in illiteracy rates and a 0.36-percent reduction in the likelihood of working. I find also negative effects of prenatal droughts on adult outcomes, although the effects tend to be smaller. These results are larger in magnitude for men than for women when considering health and educational outcomes. For instance, I find a treatment effect on mental disability that is approximately 20 times larger for males than for females. These gender heterogeneities are consistent with a literature pointing out that male fetuses are more vulnerable to in utero shocks than female fetuses (Almond and Mazumder, 2011; Eriksson et al., 2010a; Kraemer, 2000). Conversely, I also find that women experience larger changes in employment as a result of exposure to adverse rainfall shocks, which likely reflects the larger scope for improvements among females who have much lower employment rates relative to men.

When I consider separate exposure measures for each trimester, I find that the long-run effects on educational and health outcomes occur from exposure during the first trimester. This finding is consistent with medical literature emphasizing that the gestational environment during early stages of pregnancy can impact fetal brain structure and produce long-lasting or permanent consequences on cognition and health (Altshuler et al., 2003; Glynn et al., 2001; Lee et al., 2003; Mulder et al., 2002). In contrast, I find that the employment effects are concentrated in the third trimester. I take this finding as suggestive evidence that early rainfall shocks affect employment predominantly through a mechanism other than education. In particular, Santos (2016) finds that the effects of excessive precipitation on socio-emotional outcomes among children are concentrated in the third trimester, and this timing is consistent with medical literature documenting that assaults in the third trimester can have persistent effects on future emotional and personality outcomes (O'connor et al., 2002; Brown et al., 2000, 1995). Thus, a potential interpretation of my findings is that the employment effects of early rainfall shocks work primarily through noncognitive skills, which is supported by a growing literature documenting the importance of these abilities in the labor market (Heckman et al., 2006; Borghans et al., 2008). That said, I am circumspect regarding this interpretation given the lack of available data on noncognitive skills among the cohorts this paper studies. In any case, my findings highlight the importance of considering the timing of the shocks when assessing the effects of exposure to adverse rainfall shocks or other detrimental influences.

I then explore a set of additional heterogeneities that may provide insights on the mechanisms at play. I find that the effects of prenatal rainfall conditions are substantially larger among individuals born in areas with higher risk of malaria. In particular, I find that a standard deviation increase in normal rainfall conditions in utero causes a 4.3-percent reduction in mental disability rates, a 3.4-percent decrease in speech/hearing disability rates, a 0.42 percent increase in years of education, and a 2.3-percent decline in illiteracy rates. These findings are consistent with the idea that exposure to infectious and parasitic diseases may be an important channel of impact. In contrast, I do not find a consistent pattern when I explore heterogeneities with respect to other factors, including income and population size. In addition, I am not able to detect a statistically significant interaction between rural population rate and in utero rainfall shocks. To the extent that rural population rate adequately captures the fraction of population depending on farming and related agricultural activities, this result suggests that agricultural income is not the primary mechanism driving the long-run effects of rainfall shocks.

This paper is related to a growing literature linking early rainfall conditions and later-life outcomes. One of the first studies in this area is that of Maccini and Yang (2009), who find evidence that exposure to early droughts is associated with poorer self-reported health and less grades of schooling in Indonesia. Dinkelman (2017) shows that early drought raises later-life disability rates in South Africa, with the effects concentrated in physical and mental disabilities. Shah and Steinberg (2017) examine mid-term outcomes and find that droughts in early life reduce test scores and lead to fewer years of schooling among children in India. Adhvaryu et al. (2016) also examine mid-term outcomes and document that the effects of adverse rainfall shocks are smaller for children from families who receive conditional cash transfers in Mexico. To the best of my knowledge, no study has examine the effects of prenatal exposure to rainfall shocks on long-run outcomes in Latin American. My work is also related to a set of promising studies that focuses on the short-run effects of rainfall shocks, including Santos (2016), Aguilar and Vicarelli (2011), and Hoddinott and Kinsey (2001). Particularly relevant to the context of the present study is the contribution of Santos (2016) who finds that in utero exposure to floods translates into an increase in the incidence of low birth weight and an increased risk of poor socio-emotional problems in early childhood in Colombia.

The present study extends the existing literature in at least three important ways. First, this paper uses data on local conditions to distinguish between heterogeneous effects across subgroups, which may help understand the mechanisms linking rainfall conditions and long-run outcomes. As plausible as the hypotheses of changes in agricultural income and diseases in utero may be, previous studies tend to assume rather than test them. Second, I exploit precise information on date and place of birth to accurately measure prenatal exposure to abnormal rainfall events. The existing literature has made use of the individual's year of birth (rather than year and month of birth) to identify early rainfall exposure. Thus, it is unclear whether the effects are driven by prenatal or postnatal exposure to rainfall conditions. As discussed in detail by Doyle et al. (2009), interventions aimed at investing during the prenatal period can have costs radically different from those focused on the postnatal period. Hence, identifying the timing of the effects is crucial for guiding the design of policies intended to mitigate the adverse consequences of rainfall shocks. In addition, relying only on individual's year of birth for identifying exposure may be empirically problematic because it may include exposure at the time of conception. If different quality parents are more likely to postpone fertility when exposed to extreme rainfall shocks around time of conception, then it may lead to overestimates of the true effects of rainfall shocks on later-life outcomes. Using precise information on date of birth, the present study examines cohorts conceived before a shock occurred and contributes to the literature by exploring the extent to which this issue may be important in practice.

Third, I focus on a country with no known son preferences, and estimate effects separately for males and females.[2] This distinction is particularly important to understand the

[2]While Mexico is also a country with no known son preferences, Adhvaryu et al. (2016) do not estimate the effects separately by gender. In addition, they examine only medium-term outcomes and do not have any

mechanisms underlying the gender heterogeneities in the treatment effects. The evidence on gender differences in the effect of early rainfall on adult outcomes has been mixed. While Maccini and Yang (2009) find that the effects that are larger for women than for men, Dinkelman (2017) shows exactly the opposite. This should come as no surprise, since it is unclear whether gender bias in household resource allocation is contributing to exacerbate the repercussions of poor early health. A large literature suggests that males are biologically more vulnerable to poor conditions in utero (Almond and Mazumder, 2011; Eriksson et al., 2010a; Kraemer, 2000). So, the gender heterogeneities observed in Indonesia and South Africa may be the combination of gender discrimination and biological effects. An investigation of gender differences in treatment effects in countries with both non-gender and gender bias would allow to understand the importance of these mechanisms and some possible policy prescriptions that may mitigate the detrimental influences of rainfall fluctuations.

The rest of paper is organized as follows. Section 2 provides information on the data, while Section 3 introduces the empirical strategy. Section 4 presents the main results and robustness tests. Section 5 explore potential mechanisms of impact. Section 6 concludes.

2 Data

2.1 Weather data

This paper builds a series for temperature and precipitation using data from the Terrestrial Air Temperature and Terrestrial Precipitation: 1900-2010 Gridded Monthly Time Series, version 3.02, respectively (Matsuura and Willmott, 2012). This dataset provides worldwide estimates for weather conditions at the 0.5 $\times$ 0.5 degree latitude/longitude grid. Using an interpolation algorithm, Matsuura and Willmott (2012) compute values for each grid node from several nearby weather stations around the world. The number of stations in Colombia is 291, which were established from 1940 and onwards.[3] For the period prior to 1940, Matsuura and Willmott's (2012) interpolation method relies on nearby countries where stations did exist to generate weather data for every grid-months cell in Colombia. Since the data prior to 1940 are likely to be more noisy as a result of this imputation procedure, I focus on cohorts born after this date. To construct a municipality-by-month of weather panel, I employ the same approach as Rocha and Soares (2015). I begin by computing the centroid for each of the 1060 municipalities and then locate the four closest nodes to build

information on health outcomes, where the gender heterogeneity effects have be shown to be the strongest in other settings.

[3] See https://www.datos.gov.co/Ambiente-y-Desarrollo-Sostenible/Cat-logo-Nacional-de-Estaciones-del-IDEAM/hp9r-jxuu/data

a monthly series as the weighted average of estimates related to these four nodes. I use the inverse of the distance to each node as weight. The average number of municipalities per grid is 2.6 (with standard deviation of 2.9), and the number of grids involved in my sample is 405.

Following Adhvaryu et al. (2016), I define "normal rainfall" for a given month if rainfall fell within one standard deviation of historical mean for that calendar month within municipality. Since I am not comparing municipalities, the "normal" rainfall measure should not be taken in an absolute sense. These are simply normal rainfall months for each municipality within the given period. Both the historical mean and standard deviation are calculated for each municipality and calendar month over the 1900-2010 period. The results are extremely similar when I instead consider the 1942-2010 period. I then measure prenatal exposure as the fraction of normal rainfall months occurring in the 9 months before birth. For example, if an individual was born in December, then prenatal exposure to normal rainfall is computed as the share of normal rainfall months between April and December. While the focus is on normal rainfall conditions in utero, I also use specifications that separate floods and droughts. Flood and drought shocks are defined as $\pm$ 1 standard deviations with respect to the historical monthly mean of each municipality.

2.2 Census data

This paper uses microdata from the 2005 Colombia Census, the most recent full population census available. I use a randomly drawn sample available through the Integrated Public Use Microdata Series (IPUMS), a project to harmonize the coding census from several countries (Ruggles and Sobek, 1997; Sobek et al., 2012). Importantly for my analysis, the Census asks for municipality and exact date of birth.[4] This information allows me to match individuals with weather conditions of the municipality where they were born to identify the prevailing rainfall conditions in utero. My analysis sample consists of adults aged 25-65 at the time of the interview in 2005 (cohorts born between 1942 and 1981).

The Census provides information on basic socio-economic and demographic characteristics. I consider several adult outcomes. First, I explore years of schooling and an indicator for illiteracy. Since young individuals are excluded from the analysis, these measures are likely to capture completed schooling. Second, I examine an indicator for having any serious disability and the number of disabilities as measures of health human capital. Individuals who reported having any disability are asked to provide information on the type of disability, so I also construct indicators for individual disability types. These include vision, hearing

[4]The 2005 Census is the only Colombian Census with information on month and year of birth.

or speech, mental and physical disabilities. These disability measures have been widely used in the literature linking early shocks and later life outcomes (Almond, 2006; Almond and Mazumder, 2011; Lin and Liu, 2014). Unfortunately, the 2005 Census does not provide information on income. Hence, I use an indicator for employment status as a proxy for labor market success.

The expanded sample consists of 18,240,846 individuals. I drop observations with missing data for any of the outcomes of interest. This restriction results in dropping about 0.004 percent of the sample. I also exclude individuals born in the five main capitals of the country because these are very large urban centers where the mechanisms of impact should not be so relevant.[5] The resulting expanded sample consists of 15,049,738 individuals. Since the analysis exploits the municipality-by-month-by-year variation in rainfall conditions, I collapse the data into municipality-of-birth $\times$ month-of-birth $\times$ year-of-birth - cells and use the conditional means as dependent variables. In the regressions, I weight the observations by the cell size to adjust for precision with which the cell means are estimated. To explore potential heterogeneity in the treatment effects, I collapse these data separately for male and females. Estimates based on this type of group-means data are asymptotically equivalent to the ones derived from the micro-data counterpart (Donald and Lang, 2007), but the use of group-means data eases the computational burden.

In the original IPUMS sample, municipalities with population less than 20,000 are "regionalized" (combined) with neighboring municipalities within the same department to create geographical units with populations greater than 20,000. Under this definition, the total number of geographical units or simply municipalities in my sample is 524. To match individuals with weather data, I first collapse the weather dataset, which was constructed as described in previous section, into the broader definition of municipality used in the IPUMS sample. Some municipalities with population less than 20,000 within a same geographical unit may have different grid points. To cluster the standard errors in the empirical analysis, I create new "grid" codes by grouping municipalities with common grid points together into a new group.[6] For ease of exposition, I refer to these new groups simply as grids. The number of grids in the census-rainfall analysis is 216.

[5]Specifically, I exclude individuals born in either Bogotá, Medellín, Cali, Barranquilla or Cartagena. These municipalities have been historically the five main cities of Colombia in terms of population. While this sample restriction does not have large impacts on point estimates, it does improve the precision with which the parameters of interest are estimated.

[6]As an illustration, consider four municipalities, A, B, C, and D. A, B and D consist of one single municipality with population greater than 20,000. C consists of two neighboring municipalities, each of which has a population less than 20,000. Now suppose that A, B, and one of the municipalities in C have the same grid point code. In addition, suppose that D and the other municipality in C have the same grid point. In this case, I define A, B, C and D as being part of a same cluster.

Table 1 shows descriptive statistics for the outcomes of interest. About 6 percent of individuals have at least a disability and the average number of disabilities is 0.08. The most common disability in the data is that related to vision. The fraction of individuals suffering from this condition is 3 percent. By contrast, 1 percent of individuals report a serious mental disability. While the prevalence of some disability types is relatively low, I show below that there is sufficient variation across cohorts and birthplace for identification. The mean schooling level is 7.95. About 7 percent of individuals do not know how to read or write, and 58 percent of people have a job. On average, individuals spend 69 percent of their prenatal period in normal rainfall conditions, with a standard deviation of 0.18. This figure is similar across different trimesters, but the standard deviation increases by 60 percent.

2.3 Other data

To explore potential mechanisms, I use a rich set of municipality-specific historical data. In particular, I use data on number of residents, rural population rate, and per capita income from the 1973 Census. I use the percentage of the population residing in rural areas as a proxy for the fraction of population depending on farming and related agricultural activities for their livelihoods. I also obtain data on malaria ecology from Bleakley (2010). I use these variables to evaluate the degree to which these aggregate factors magnify or dampen the baseline effects of prenatal rainfall shocks.

I also use data from the Colombia Demographic Health Survey (DHS) to study whether prenatal rainfall shocks affect postnatal investments. The DHS is a nationally representative survey of women ages 15-49 and contains detailed information on early investments for all children under five. I use all the waves (1986, 1990, 1995, 2000, 2004-05, 2009-10) of the DHS and pool them into one dataset. The inputs I examine are vaccination and breastfeeding. Vaccinations have been shown to be effective in preventing ill health and mortality. Given the limited access to medical treatment in developing countries, vaccinations become an important health inputs. Likewise, breastfeeding plays a central role in nutrition, especially in environments characterized by unsafe drinking water and limited supply of food.[7] I also exploit information about date of birth and date of death (if deceased) to explore the role of selective mortality. Unlike vaccination and breastfeeding, death histories are recorded for all births linked to a woman. However, since these death histories are based on retrospective information, I focus on births that occurred less than ten years prior to the date of the survey, closely following Baird et al. (2011). Figure 1 shows the birth years where the DHS and Census samples cover roughly the same cohorts.

[7] A large body of work has documented that breastfeeding is predictive of later cognitive outcomes (see, for example, Del Bono et al. (2012)).

The DHS provides information on the exact date of birth, but not on the municipality of birth. Therefore, I match individuals with rainfall conditions using the municipality of residence and date of birth. One concern with using the municipality of residence rather than that of birth is measurement error, which is likely to induce a bias towards zero in estimates of the effects of prenatal rainfall shocks. To address this issue, I restrict the analysis sample to children in families that have been living in the current municipality for a greater time than child's age. These children represent about 83 and 78 percent of the full investment and mortality samples, respectively. As in the census sample, I exclude children in the five main capitals of the country. After these restrictions, the investment and mortality samples consist of about 30,000 and 78,000 children, respectively. Descriptive statistics for these samples are presented in Appendix Table A.1

2.4 Variation in rainfall shocks and outcomes

Because the statistical approach of this study relies on within-municipality variation, I confirm that there is in fact substantial within-municipality variability in the data for identification. Figure 2 plots the frequency with which normal rainfall months occur over time and space. It reveals that the incidence of normal rainfall conditions varies sharply across municipalities within a given month. Episodes of normal rainfall months occur, on average, in 67 percent of the Colombian municipalities. Yet, there are periods with pervasive rainfall shocks, with less than 30 percent of municipalities experiencing normal rainfall conditions.

To evaluate the within-municipality variability in the data more formally, I regress the exposure measure for a cell on a full set of municipality fixed effects and month-by-year fixed effects. The residual variation in this regression is a direct measure of within-municipality variability. An R-squared far from 1 is counted as evidence of substantial within-municipality variation. I find that about 80 percent of the total variation in prenatal rainfall exposure cannot be explained by this set of fixed effects. When I account for municipality-specific linear time trends in addition, I find still substantial within-municipality variation, with 68 percent of the variation due to within-municipality differences.

I also evaluate the within-municipality variability in the main outcomes of interest. Municipality and time fixed effects cannot explain 70 percent of the variation in years in schooling, and this hardly changes when municipality-specific time trends are accounted for. I also find that a substantial portion of the total variation in employment status is due to within-municipality differences, about 80 percent. After controlling for municipality-specific linear time trends, and fixed effects for municipality-of-birth and month-of-birth $\times$ year-of-birth, between 88 and 95 percent of the variation in disability outcomes remains unexplained. In summary, this analysis reveals that there is meaningful variation in the data for identification.

3 Empirical Strategy

To measure the relationship between prenatal rainfall conditions and later-life outcomes, I use the following specification:

$$\begin{aligned} Outcome_{jmt} = \alpha + \beta R_{jmt} + \gamma Z_{jmt} + \theta Trend_{tm} \times M_j \\ + \eta_j + \mu_{mt} + \xi_{jmt} \end{aligned} \tag{1}$$

for cohorts born in municipality j, month m and year t. *Outcome* is the dependent variable of interest, either a health, educational or employment outcome. R is the fraction of normal rainfall months during the 9 months prior to birth. The covariates Z include a set of predetermined individual characteristics, such as sex and race. In all specifications, I control for municipality-specific linear time trends ($Trend_{tm} \times M_j$) to account for factors changing over time that might affect the outcomes of interest.

The models include municipality-of-birth fixed effects (η_j), which absorb any unobservable time-invariant determinants of adult outcomes, including initial conditions, geography, and area-specific risks of diseases. The set of month-of-birth $\times$ year-of-birth fixed effects (μ_{mt}) controls for common time trends such as seasonal fluctuation in later outcomes, macroeconomic conditions and common national policies. Finally, ξ_{jmt} is the error term. All the models use robust standard errors clustered at the grid level. These standard errors therefore allow for arbitrary correlation in residuals across municipalities within a grid and for serial correlation at the grid or municipality level.

The coefficient β measures the effects of prenatal exposure to rainfall on the adult outcomes of interest. My quasi-experimental design rests on the assumption that the occurrence of extreme rainfall events is uncorrelated with omitted determinants of later-life outcomes. This assumption is plausible insofar as parents are unlikely to anticipate precisely a rainfall shock at a given moment in time and place. By conditioning on the full set of municipality and time fixed effects and local-specific time trends, the analysis uses arguably random fluctuations in rainfall from municipality-specific deviations in long-term rainfall after accounting for all seasonal factors and common shocks to all municipalities.

Yet, I address several identification issues that may arise when following this statistical framework. First, one may be concerned if more-educated and higher quality parents are more likely to postpone fertility when exposed to extreme rainfall shocks around time of conception. My focus on shocks occurring in the 9 months prior to birth should mitigate concerns related to selective fertility. However, I also explore specifications that include exposure to rainfall conditions in the 10-12, 13-15 and 16-18 months before birth to evaluate the degree to which selective fertility may be important in practice. The results from these models suggest little evidence that it is a major problem in my setting.

Second, a bias may arise if different types of women are likely to migrate away from areas affected by adverse rainfall conditions. It seems implausible that this is the case given that I focus on temporary variations in rainfall and by the low migration rates of pregnant women. To evaluate this issue empirically, I limit the census sample to children and construct an outcome that takes the value of 1 if the child was born in the municipality where he/she was interviewed in 2005. I then test whether rainfall shocks affect this outcome. If women are likely to migrate to different municipalities in response to rainfall shocks, one would expect to see statistically significant estimates in these regressions. I perform this analysis at the individual level and the regressions include controls for month-of-birth $\times$ year-of-birth fixed effects, municipality-of-birth fixed effects, and child's sex and race. To examine the possibility of heterogeneous responses across regions, I run the regressions separately for urban and rural areas. Consistent with the view that migration is unlikely to be related to temporary variations in rainfall, I find statistically insignificant estimates in these regressions (see Appendix Table A.14). This reduces concerns on migration.

Third, as the sample is based on surviving (and presumably higher quality) individuals, a potential issue is selective mortality, either during pregnancy or in early infancy. While most miscarriage happens in the first trimester, there is possibility of late miscarriage and stillbirth. If rainfall exposure during pregnancy affects this culling process, any estimated impacts after birth would need to be a combination of selection and a direct treatment effect. However, any bias from using this selected sample most likely will bias the estimates towards zero. If so, my estimates should be taken to be lower bound of the true effect, and large impacts would even become more telling. Therefore, I am less concerned about bias from selective mortality. I return to this discussion below.

4 Results

4.1 Main findings

I begin by examining graphically the relationship between prenatal rainfall and the outcomes of interest. I estimate local linear regressions of adult outcomes on fraction prenatal in normal rainfall, conditional on municipality-specific time trends, municipality-of-birth and year-of-birth $\times$ month-of-birth fixed effects. Figures 3-4 plot the respective estimates and 95 percent confidence intervals. Figure 3, panels (a)-(b) suggest that prenatal normal rainfall is negatively associated with the probability of reporting any serious disability and the number of disabilities, but the estimates tend to be imprecisely estimated. The same pattern is observed for mental and physical disabilities. There not clear patterns for vision and hearing/speech disabilities. Figure 4 indicates that individuals who spent more time of their prenatal period in normal rainfall conditions have more years of schooling, display lower likelihood of being illiterate, and are more likely to work in the labor market.

I formally present the regression results in Table 2. All estimates are based on the full specification that adjusts for municipality-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific time trends and the set of predetermined individual characteristics. Panel (a) uses the fraction of normal rainfall during the 9 months prior to birth as the key independent variable. Panel (b) shows the results from a specification that uses separate exposure measures for each trimester.

Columns (1)-(2) look at an indicator for any serious disability and the number of disabilities, respectively. The coefficients are negative as one would expect, but very imprecisely estimated. Still, these aggregate measures of disabilities may mask important form of heterogeneities across disability types. Columns (3)-(6) explore the effects of early rainfall shocks on disability types. I find evidence that greater exposure to normal rainfall in utero is significantly associated with lower incidence of mental and physical disabilities. Column (3) shows that a standard deviation increase in normal rainfall during the first trimester decreases mental disability rates by 0.029 percentage points. Relative to a mean of 1 percent, this represents a 2.9-percent reduction in the prevalence of mental disability. Column (4) suggests that for one standard deviation of normal rainfall in the 9 months before birth, the rate of physician disability declines by 0.046 percentage points, or 2.34-percent relative to the mean. When I look at separate exposure measures for each trimester, I find that the effect of in utero exposure to normal rainfall on physical disability occurs from exposure during the second trimester - although this effect is imprecisely estimated and thus is only statistically significant at the 16 percent level.

To better place the disability results in perspective, I compare these estimated effects to the differences in the disability outcomes between less- and more-educated individuals. This seems to be a relevant comparison given the well-established striking correlation between health and education.[8] In my sample, a standard deviation increase in years of education is associated with a decrease of 0.48 percentage points in the probability of reporting any serious mental disability.[9] Relative to this difference, the estimated effect of exposure to

[8] See Adams et al. (2003) for a good summary of this literature

[9] This estimate is obtained by regressing mental disability on years of schooling, and controls for age,

normal rainfall during the first trimester on mental disability is about 6 percent.

Column (7) investigates the relationship between prenatal rainfall conditions and years of schooling. When I use separate exposure measures for each trimester, I find evidence that greater normal rainfall during the first trimester leads to more years of schooling, with an estimate of 0.077 (standard error=0.026). This implies that for one standard deviation of normal rainfall during the first trimester, years of schooling increases by 0.023, or 0.28-percent relative to the mean. For comparison, Duflo (2001) finds that a large school construction program led to an increase of 0.15 years of education in Indonesia. Column (8) reveals that exposure to normal rainfall in utero has significant effects on illiteracy. This effect also occurs from exposure during the first trimester. A standard deviation increase in normal rainfall during the first trimester is associated with a decline in the likelihood of being illiterate of about 0.10 percentage points. This represents a decline of 1.6 percent relative to the mean illiteracy rate. Finally, column (9) shows robust evidence that greater exposure to normal rainfall in utero leads to an increase in the probability of working. This relationship is entirely driven by exposure to normal rainfall in the third trimester. The estimated coefficient implies that a standard deviation increase in normal rainfall during the third period increases employment probability by 0.30 percentage points.

Table 3, panel (a) considers separately the effects of prenatal floods and droughts on later life outcomes. I find that a standard deviation increase in prenatal floods is associated with a 3.2-percent increase in mental disability rates, a 0.21-percent decline in years of schooling, a 1.7-percent increase in illiteracy rates and a 0.36-percent reduction in the likelihood of working. The corresponding effects of droughts tend to be smaller and statistically insignificant. For example, the effect of exposure to floods in the 9 months before birth on mental disability is about 11 times larger than that of exposure to droughts. Panel (b) examines the effects of droughts and floods separately in each trimester. The results suggest that greater exposure to floods during the first trimester leads to higher mental disability rates, fewer years of schooling and increased illiteracy probabilities. The effects of floods on employment are driven by exposure during the second and third trimesters. I also find evidence that higher exposure to droughts during the first trimester leads to an increase in the probability of reporting any serious hearing or speech disability, and to poorer educational outcomes.

Overall, the results of this section suggest that prenatal rainfall has significant effects on mental disability, years of schooling, illiteracy and employment. In general, these results are largely driven by excessive precipitation. In addition, my findings suggest that the effects of adverse rainfall shocks on health and educational outcomes are stronger in the first trimester. This is consistent with a vast medical literature indicating that the first

sex, and race.

trimester of pregnancy is a key period where the fetus develops most of its organs and thus is most vulnerable to poor environmental conditions (Glynn et al., 2001; Lee et al., 2003; Mulder et al., 2002). Some of these studies also emphasize that the gestational environment during early stages of pregnancy can impact fetal brain structure and produce long-lasting or permanent consequences on cognition (Altshuler et al., 2003). This suggests that the negative effects of exposure to adverse rainfall shocks on education may be not only a result of health channels, but also a direct consequence of cognition deficits.

Differently from mental disability and educational outcomes, I find that the timing of the employment effects is the third trimester. A possible interpretation of this finding is that this relationship works through a different mechanism. Previous studies in epidemiology have shown that exposure to stressful events in the third trimester of pregnancy leads to poorer emotional and personality outcomes later in life (O'connor et al., 2002; Brown et al., 2000, 1995), and a growing literature in economics documents the importance of non-cognitive skills in the labor market (Heckman et al., 2006; Borghans et al., 2008). Thus, a possible interpretation of my findings may be that the employment effects of prenatal rainfall work primarily through non-cognitive skills. This interpretation is supported by the evidence in Santos (2016) that the negative short-term effects of excessive precipitation in Colombia on socio-emotional outcomes are concentrated in the third trimester of gestation. Of course, this is a very suggestive interpretation, since I do not have any measure of non-cognitive skills among adults to draw strong conclusions.

4.2 Selective mortality

Since my analysis is based on surviving individuals, a concern with my findings is selective mortality. If there is a meaningful mortality effect of prenatal rainfall shocks, then who survives is subject to selection, and this selection varies exactly with the exposure measure. If there is a hypothetical underlying distribution of health, and those in the left tail die (with the threshold shifting with the exposure measure), it would mean that those who survive under worse circumstances actually look better later in life (for mechanical reasons due to this selection process). As discussed above, this would bias the estimates of the effect of rainfall on later life outcomes towards zero.

Appendix Table A.2 explores the degree to which selective mortality may be important in practice. Panel (a) examines the effects of prenatal rainfall on cohort size (in logarithms). The results show evidence that greater exposure to floods decreases cohort size. With cohort size interpreted as cumulative survival, this result indicates some evidence that exposure to adverse rainfall shocks in utero is associated with increases in mortality. While significant, the estimated effect is not substantially large in magnitude. The point estimate of -0.031 implies that cohort size decreases by 0.56 percent for a standard deviation increase in floods in the 9 months before birth. When I examine the effects separately for younger and older cohorts, I find larger point estimates for the former group, but the coefficients are now imprecisely estimated and thus statistically insignificant.

Panels (b)-(c) examine the effects of prenatal rainfall on cohort size separately for males and females. One may observe stronger effects for men if male fetuses are more vulnerable to detrimental influences in utero than female fetuses Eriksson et al. (2010b); Kraemer (2000). While the results in the table provide evidence highly consistent with this hypothesis, they do not indicate substantial cohort size effects. A standard deviation increase in prenatal floods reduces the number of live males only by 0.7 percent. The corresponding decline in the number of live females is about 0.3 percent and statistically indistinguishable from zero. I continue to find limited evidence of a meaningful cohort size effect when I simultaneously stratify the sample according to sex and age.

To further check for selective mortality, I explore the effects of prenatal shocks on mortality using individual mortality records in the DHS. Specifically, I estimate linear probability models where the dependent variable is either an indicator for neonatal mortality or infant mortality using data at the child level. The results are presented Appendix Table A.3. I find evidence that is highly consistent with the cohort size results discussed above. The results in panel (a), which consider the full sample, reveal that greater exposure to floods in utero is associated with increased rates of neonatal mortality. The estimated coefficient suggests that a standard deviation increase in prenatal floods causes an increase of 0.16 percentage points in the likelihood of dying in the first month of life. Panels (b)-(c) document that this effect is larger for males than for females. The corresponding increase in the probability of dying in the first month is 0.25 percentage points for males, while it is 0.09 percentage points and statistically insignificant for females. Finally, Appendix Table A.4 shows limited evidence of an interaction between rainfall shocks and mother's observable characteristics.

While these results provide suggestive evidence that adverse rainfall shocks in utero are associated with increases in mortality rates, the estimated effects are not substantial. Therefore, even if extreme rainfall conditions killed off those who would have had better later-life outcomes, this channel is unlikely to be of the right order of magnitude to explain the results of the present study. For example, suppose that one standard deviation of prenatal floods killed off 0.5 percent of children (as suggested by the cohort results in Appendix Table A.2), and this non-surviving group would have completed 10 percent years of schooling more than the rest of population. Then, prenatal exposure to floods would generate a 0.05 percent increase in years of schooling. Even if floods killed 1 percent of children, and these non-survivors would have completed 10 percent years of schooling more than the rest of population, this would still only generate an effect of 0.1 percent on years of schooling.

4.3 Additional analyses and robustness checks

I conduct a number of other specification checks to test the robustness of the main results. As mentioned above, one could be concerned if different parents change fertility decisions when exposed to adverse rainfall shocks around conception time. My focus on the 9 months before birth should reduce concerns regarding selective fertility. However, I can extend the baseline specification and include exposure measures several trimesters before pregnancy to explore the extent to which selective fertility may be important in practice. If it is a major issue, one would expect to see statistically significant coefficients on these additional exposure measures. As can be seen from Table 4, including these variables leaves the estimates of interest unchanged. Moreover, there is no evidence that exposure to rainfall shocks prior to pregnancy is correlated with adult outcomes. This finding suggests that selective fertility is unlikely to be important in my setting.

In the main results, I use standard errors clustered at the grid level to account for correlation across municipalities within a grid and for serial correlation at the grid or municipality level. A possible disadvantage of these standard errors is that they do not allow for correlation across municipalities in different grid points, which may be important if rainfall shocks are spatially correlated. To check the robustness of my main findings to this issue, I compute standard errors that are adjusted for arbitrary spatial and serial correlation in the data using Conley's (1999) method. This method creates a spatial weighted covariance matrix where the weights start at 1 and decline linearly to 0 when a prespecified cutoff is reached (Conley, 1999). I compute Conley standard errors at the cutoff distances of 100 and 500 kilometers (Appendix Table A.5). Comparing the Conley (1999) standard errors to the robust errors clustered by grid level suggests that spatial correlation is not a major issue and the findings discussed above remain essentially the same with the Conley standard errors. In fact, the Conley standard errors are generally smaller than the baseline in the case of years of schooling. Now, the estimated effect of exposure to normal rainfall in the 9 months before birth on years of education is statistically significant at the 5 percent level.

Colombia has experienced a war between governments, paramilitary groups, crime syndicates and left-wing guerrillas that began in the mid-1960s. A work by Dube and Vargas (2013) shows that income shocks significantly influence the intensity of this conflict, so a natural question is to what extent the estimates of rainfall shocks could be affected by prenatal exposure to violence. This is a relevant point given that my sample includes some individuals who were potentially exposed to the conflict. To explore this question, I create conflict intensity measures using data from Dube and Vargas (2013) and include interactions between such measures and linear time trends in the main regressions.[10] As can be seen from Appendix Tables A.6-A.7, including these additional controls has no effect on the estimated coefficients of interest.

Appendix Table A.8 considers alternative measures of rainfall shocks. Panel (a) defines early rainfall shocks by the deviation of rainfall 9 months before birth from the average historical rainfall in each municipality. More specifically, the variable is the natural log of prenatal rainfall minus the natural log of mean rainfall in the given municipality. This is the measure used by Maccini and Yang (2009). When I follow this specification, I find estimates that are statistically significant only in some cases. Indeed, I find that higher rainfall relative to the normal local rainfall is associated with increased rates of mental disability and fewer years of schooling, but there is no evidence of significant effects for the rest of outcomes. This weaker evidence is perhaps unsurprising given the evidence that exposure to both extreme positive and negative rainfall shocks lead to poorer adult outcomes.

Panel (b) defines a positive (negative) rainfall shock for a given month if rainfall was above the 90th (below the 10th) percentile of the distribution for that calendar month within the municipality. The fractions of prenatal drought and excess rainfall are computed using these definitions of extreme drought and wet months. I find coefficients that are generally very imprecisely estimated under these measures.

Panel (c) defines extreme droughts and floods based on the Spatial Precipitation Index (SPI). The SPI relaxes the assumption of normality and fits a gamma distribution to rainfall data before constructing measures of the deviation of rainfall from average historical rainfall in a given municipality. Having computed drought and flood months based on the SPI score, the fraction of early exposure to either extreme droughts or floods is calculated using the same logic as in the baseline measure. Using exposure measures based on the SPI leads to results that are in general consistent with the baseline findings, although the coefficients of interest are also very imprecisely estimated.

The main regressions have focused on the effects of prenatal rainfall on adult outcomes. A natural extension of my is analysis is to evaluate whether postnatal exposure also affects long-run outcomes. While the prenatal programming theory highlights that the nine months of gestation is a particularly important period of human development (Barker, 1997), some studies suggest that the first year of life may be also important (Hoddinott and Kinsey, 2001; Glewwe and King, 2001). Because I have information on the exact date of birth, I can

[10] This strategy is similar to that of Bleakley (2010), who tries to control for conflict intensity in his analysis of the long-term impacts of malaria eradication in Colombia.

examine whether and the extent to which exposure to normal rainfall during the first year of life affects adult outcomes. The results of this exercise are presented in Table 5. They show that greater exposure to normal rainfall during the first year of life is associated with poorer educational outcomes. A standard deviation increase in normal rainfall during the first year is associated with a 0.19-percent increase in years of education and a 1.4-percent decline in illiteracy. I find no evidence that postnatal rainfall affects the rest of outcomes.

Previous studies have documented seasonal fluctuations in adult outcomes according to the month of birth that may be driven by factors other than rainfall variations (Buckles and Hungerman, 2013). Although I control for month-of-birth $\times$ year-of-birth fixed effects in all regressions, one could be even concerned if there is regional-specific seasonal variation in adult outcomes spuriously correlated with variation in rainfall shocks. In Appendix Tables A.9-A.10, I examine this issue by estimating models that control for a full set of municipality-of-birth $\times$ month-of-birth fixed effects. Point estimates are virtually identical to the ones derived from the baseline specification, casting doubt on this additional source of bias.

4.4 Gender heterogeneities

I now investigate the gender specificity of the main results. Table 6 shows the results from running regressions separately for males and females. The results for mental disability indicate larger impacts for males than for females. Men who spent 29 percent of their first trimester exposed to normal rainfall conditions are 0.06 percentage points less likely to report any mental disability. Relative to mean rate of 0.1, this represents a 6-percent reduction. In addition, I now find a statistically significant (at the 5 percent level) effect of exposure to normal rainfall in the second trimester on mental disability among males, although the effect is smaller magnitude compared to that in the first trimester. The results also suggest that greater prenatal exposure to normal rainfall is associated with a decline in the likelihood of reporting any serious hearing or speech disabilities among males. Conversely, the corresponding treatment effects on these outcomes among females are smaller in magnitude and statistically indistinguishable from zero. The differences in point estimates are striking. For instance, the treatment effect of exposure to normal rainfall in the first trimester on mental disability is at least 20 times larger (in absolute value) for males than for females.

There are also striking differences in the the effects of prenatal rainfall on educational outcomes between males and females. The estimated effect of exposure during the fist trimester on years of schooling is about 3 times larger for men than for women. Now, a standard deviation increase in normal rainfall in the first trimester implies 0.035 more years of schooling among males. The corresponding increase in years of schooling for females is about

0.011 and statistically insignificant. In the case of literacy, the estimated effect of normal rainfall in the first trimester is about 2 times larger for males than for females. A standard deviation increase in normal rainfall during the first trimester generates a 2.2-percent decline in illiteracy rate among men, while the corresponding estimated effect among women is 1 percent and statistically significant. Finally, I find that the effect of prenatal rainfall in the third trimester on employment is larger for females than for males. The results suggest that females who spent 29 percent of the third trimester in normal rainfall conditions are 0.44 percentage points more likely to work in the labor market. The corresponding estimate for males implies an increase of 0.15 percentage points in the probability of working, which is about 3 times smaller than that for females.

Overall, the results suggest strong gender heterogeneities. Taken in their entirety, the results reveal larger treatment effects for males than for females when considering health and educational outcomes. One might think that differential selective mortality between women and men drives the observed differences. If the culling process is much more pronounced for women than for men, then this may drive the gender differences in the effects of prenatal rainfall on these outcomes. However, the evidence suggests that the exact opposite is true. As shown in section 4.2, greater prenatal exposure to adverse rainfall shocks results in a larger reduction in the number of live cohorts among males than among females. If anything, this evidence suggests that the differences in the effects prenatal rainfall shocks on long-run educational and health outcomes between men and women are likely to be larger than my estimates show.

These patterns are in general inconsistent with Maccini and Yang (2009), who show larger effects for females in Indonesia, but are in line with Dinkelman (2017), who find stronger impacts of droughts for males in South Africa. A major distinction between the setting that these authors study and mine is that Colombia is a country with not known gender bias at early ages. Indeed, the sex ratio at birth, which has emerged as an indicator of sex-discrimination at early ages, is in the normal range 104-107. Thus, it seems implausible that sex discrimination accounts for the gender differences in the effects I document here. Rather, my findings are consistent with the literature on fragile males, which suggests that male fetuses have less ability to produce nutrients in the placenta than female fetuses. This is supported by studies documenting gender-specific effects of different shocks during pregnancy (Ross and Desai, 2005).

The results also reveal stronger employment effects among females, which contrasts with the pattern observed for health and educational outcomes. Comparing employment rates for men and women in my sample, I find that females have much lower labor supply than males: 37 percent of women are employed in the labor market, while that figure is about

78 percent for men. Therefore, the gender differences in employment effects may simply reflect the much larger scope for improvements in this outcome among women. On the other hand, this result is also consistent with the notion that the primary mechanisms driving the employment and education effects are different. As discussed in section 4.1, a potential candidate behind the relationship between prenatal rainfall shocks and employment is non-cognitive skill formation. If this is indeed the case, the gender specificity of my results would imply that this mechanism is more important for females than for males. Consistent with this hypothesis, a series of studies has found that a number of non-cognitive skills are more rewarded for females than for males (Semykina and Linz, 2007; Heckman et al., 2006; Nyhus and Pons, 2005). Heckman et al. (2006) find a much stronger relationship between non-cognitive skills and employment for women compared to that of men in the United States. In the same line, Nyhus and Pons (2005) show that the labor market returns to personality dimensions such as emotional stability and agreeableness are larger for females in Netherlands. Semykina and Linz (2007) reach a similar conclusion using data from Russia. These potential explanations need not be mutually exclusive.

5 Mechanisms

5.1 Heterogeneous treatment effects

In this section, I explore heterogeneity in treatment effects to help understand the possible mechanisms at play. The literature generally attributes the effects of prenatal rainfall to agricultural income and diseases. Precipitation is crucial for agricultural productivity, so extreme fluctuations in rainfall can adversely affect the income of rural families that depend on agricultural activities. Reduced income in turn may adversely affect living conditions of pregnant women and thus the quality of the prenatal period. At the same time, infections diseases such as malaria are affected by rainfall shocks. Previous studies have documented that both excessive precipitation and droughts are associated with malaria epidemics (Gagnon et al., 2002).[11] While plausible, previous studies tend to assume rather than test these mechanisms. Understanding the importance of these mechanisms is important for the design of policy.

To investigate the relative role of agricultural income and disease environment, I regress the outcomes of interest on interactions of prenatal rainfall with a set of municipality-specific

[11] A comprehensive discussion of the relationship between rainfall shocks and malaria incidence can be found in Sta (2013)

variables. This regression is specified as follows:

$$Outcome_{jmt} = \alpha + \beta R_{jmt} + \sum_k \delta^k R_{jmt} \times I_j^k + \gamma Z_{jmt} + \theta Trend_{tm} \times M_j + \eta_j + \mu_{mt} + \xi_{jmt} \tag{2}$$

A significance of coefficients on interactions (δ^k) would point to the presence of differences in the effect of prenatal rainfall on later-life outcomes. If for example one expects agricultural income to be an important factor underlying the results above, then one should observe larger impacts among cohorts born in areas with a high fraction of population depending on agriculture for their living. I use rural population rate in 1973 as a proxy for the proportion of individuals depending on agricultural income. To examine the importance of the disease channel, I examine heterogeneities with respect to a measure of malaria risk. Many cohorts analyzed by this paper were born in a period where malaria had not been eradicated and was a major cause of morbidity in Colombia. Furthermore, the risk of malaria varies widely across areas of Colombia, with some regions with very high risk of malaria and others with low or no incidence. Naturally, there could be other important diseases driving the long-run effects of prenatal rainfall on adult outcomes and they are likely to be correlated with malaria incidence. Since limitations of the available data do not allow me to disentangle all possible infectious disease mechanisms, I interpret any significant interaction with respect to malaria simply as evidence supporting the existence of a disease channel.

I also examine heterogeneities with respect to income and population size. Because all these variables are correlated, examining interactions with income and population size is useful to help understand the relative importance of agricultural income and malaria from economic development. To better compare the importance of each factor, I standardize each factor to have mean 0 and standard deviation 1.

As shown in Table 7, the effects of prenatal rainfall shocks tend to be larger among cohorts born in municipalities with higher risk of malaria. This is especially true when considering disability outcomes. Differently from the baseline results in Table 2, I now observe statistically significant effects on the number of disabilities, and the probability of reporting a serious speech or hearing disability among cohorts born in malarious areas. In addition, the effects on educational outcomes are stronger that the baseline ones. For individuals in endemic areas (one standard deviation above the mean of malaria incidence), a standard deviation increase in normal rainfall causes a 1.2-percent decline in the number of disabilities, a 4.3-percent reduction in mental disability rates, a 3.4-percent decrease in speech/hearing disability rates, a 0.42 percent increase in years of education, and a 2.3-

percent decline in illiteracy rates.[12] By contrast, I find no evidence of an interaction between prenatal rainfall shocks and rural population, suggesting that agricultural income is not the main channel driving the results. Moreover, when heterogeneities with respect income and population size are examined, I do not find systematic evidence of a significant interaction. This suggests that is disease risk, and not only economic development, behind the malaria results.

To uncover more details on these interactions, I estimate a specification that separates floods and droughts. This separation may be particularly useful for malaria. The mosquito that transmits the malaria parasite depends on standing water to survive to adulthood, so the effects of excessive precipitation may be more important in endemic areas than that of droughts (Barreca, 2010). Nevertheless, there is also evidence that droughts can favor the development of malaria epidemics in South America (Gagnon et al., 2002). As shown in Table 8, there are statistically significant interactions between droughts, floods and malaria incidence. I find that a standard deviation increase in prenatal droughts in endemic areas leads to a 2.4-percent increase in mental disability rates, a 3-percent increase in the likelihood of reporting any physical disability, a 5.8-percent increase in hearing/speech disability incidence, a 0.32-percent decline in years of schooling, and a 1.57-percent increase in illiteracy rates. Analogously, one standard deviation increase in floods among individuals in endemic areas causes a 5-percent increase in mental disability rates and a 0.41-percent decline in years of schooling. Although the interaction between floods and malaria tends to be less precisely estimated for other outcomes, I find point estimates that are similar or somewhat larger in magnitude compared to that of the interaction between droughts and malaria. In particular, I find that greater prenatal floods in endemic areas increases illiteracy rates by 2.4 percent. The results in the table also reveal little evidence of a systematic interaction between rainfall shocks and other factors across outcomes.

Between 1940 and 1980, Colombia experienced substantial changes. In particular, malaria was eradicated and the economy shifted from an agricultural and mainly rural economy to a predominantly urban economy. So it is natural to expect that there will have been changes in the relationship between early rainfall shocks and adult outcomes. For example, more individuals living in areas with low malaria risk would imply smaller effects of rainfall shocks among more recent cohorts if malaria risk is an important mechanism. With this in mind, I run the regressions separately for cohorts born before and after 1960 (Appendix Table A.11-A.12). A shortcoming of this exercise is that the effects of prenatal rainfall shocks for a given individual may be increasing throughout life, so different effects across cohorts may be the result of this mechanism rather than changes in agricultural income or disease risk. But

[12]These effects are relative to the mean of the outcome of interest.

since I focus on individuals aged 25-65, which are likely to have completed their schooling, the "age" effect should be a minor issue for educational outcomes.

Although the coefficients are estimated very imprecisely likely due to reduced sample sizes, I find a consistent pattern showing smaller effects in magnitude for more recent cohorts when examining educational outcomes. The treatment effect of normal rainfall in the 9 months prior to birth on years of schooling is about 3.8 times larger for individuals born before 1960 than for those born after 1960. In the same vein, one standard deviation increase in normal rainfall reduces the probability of being illiterate by 0.15 percentage points among individuals born before 1960, while it implies a reduction of only 0.03 percentage points among individuals born after 1960. The results in Appendix Table A.12, which consider the effects of prenatal rainfall in each trimester, consistently show larger effects on long-run educational outcomes among older cohorts.

5.2 Postnatal investments

The long-run effects shown above represent the impacts of rainfall throughout an individual's life-cycle, which include parental investments after birth. Existing literature suggests several pathways through which rainfall shocks may affect parental investments. The income effects of rainfall shocks may have direct repercussions on the ability of parents to allocate important resources to their children early after-birth, especially in contexts characterized by credit constraints and other market imperfections. In this case, postnatal investments would reinforce the baseline impacts of prenatal rainfall shocks. Alternatively, income shocks induced by fluctuations in precipitation may affect the opportunity cost of time-intensive investments. For example, if parents anticipate that returns to agricultural activities are low due to unfavorable rainfall conditions, they may be more likely to be at home and devote more time to crucial time-intensive investments, such as traveling to distant facilities for free health services (Miller and Urdinola, 2010). As a result, investments would contribute to compensating the initial adverse effects of rainfall shocks on infant health.

Shifts in child endowments at birth due to prenatal rainfall shocks may also affect household behavior independently of changes in income. A prominent literature both theoretical and empirical suggests that parents' investments respond to variations in birth endowments. An early study by Becker and Tomes (1976) suggests that complementarities in the production function of child quality create an incentive for families to devote more resources in highly endowed children. This implies that parental investments reinforce the long-term consequences of poor infant health. Conversely, Behrman et al. (1982) argue that parents are likely to undertake compensatory investments in weaker children because of altruism and aversion to inequality. In addition, one could see significant effects of prenatal rainfall on postnatal investments if sick children get more doctors' visits (and thus gets vaccinations) and get more maternal care (perhaps breastfeeding longer). Empirically the evidence on the relationship between birth endowments and parental investments has been mixed, with some studies finding evidence for reinforcing investments (Adhvaryu and Nyshadham, 2016; Datar et al., 2010) and others showing the opposite (Del Bono et al., 2012; Bharadwaj et al., 2013). Hence, it is difficult to infer how prenatal rainfall shocks affect investments through the child endowments channel.

I then use data from the DHS to understand the relationship between prenatal rainfall shocks and postnatal investments, driven either by an income effect, parents investing more or less in weaker children, or any other channel. Since the cohorts in the DHS are not the same as in the main results, this analysis should be view as an exploratory exercise. Using data at the child-level, I estimate the following specification:

$$Investment_{ijmt} = \alpha + \beta R_{ijmt} + \gamma Z_{jmt} + \theta Trend_{tm} \times M_j + \eta_j + \mu_{mt} + \xi_{ijmt} \tag{3}$$

for the child i born in municipality j, month m, and year t. R is the fraction of normal rainfall months during the 9 months prior to birth. The vector Z contains basic demographic characteristics, including mother's age at first birth, indicators for mother's education level, an indicator for marital status (married), an indicator for child's sex, and birth order. The regressions control for municipality-specific linear time trends ($Trend_{tm} \times M_j$), municipality-of-birth fixed effects (η_j), and month-of-birth × year-of-birth fixed effects (μ_{mt}). In all regressions, I also control for average temperature during the 9 months before birth. Standard errors are clustered at the grid level in all regressions. I also present results from a specification that separates the effects of rainfall in each trimester.

The results are reported in Table 9. Column (1) shows that children with greater exposure to normal rainfall conditions in utero are less likely to have ever been breastfed. Columns (2)-(3) look at the duration of breastfeeding by using linear and log-linear regressions.[13] I find evidence that greater exposure to normal rainfall in utero is associated with a decline in the duration of breastfeeding. Column (4) uses a dummy indicating whether the child was breastfed for more than six months, the minimum length recommended by the World Health Organization (WHO). Using this breastfeeding measure, I find that exposure to normal rainfall during the first and second trimester reduces the likelihood of being breastfed.

In the next set of columns, I look at BCG (*Bacillus Calmette-Guerin*), polio, DPT (diphtheria, pertussis and tetanus combination) and measles vaccinations. I construct dummies

[13] In the log-linear regressions, I use log(duration of breastfeeding + 1) as dependent variable.

indicating whether the child has all the recommended vaccination doses for specific diseases.[14] The results indicate that greater exposure to normal rainfall conditions in the first trimester reduces the probability of being vaccinated for these diseases. Appendix Table A.13 shows estimates of the effects of prenatal rainfall for each of the three DPT and polio vaccines separately. I find stronger effects on receiving the third doses compared to that on receiving the first and second doses.

Overall, I find evidence that prenatal exposure to normal rainfall leads to less health investments in early-life. This indicates that postnatal investments may be contributing in mitigating the long-run adverse consequences of poor neonatal endowments. The fact that the effects on earlier investments such as first doses of polio and DPT vaccinations are less strong suggests that parents engage in compensatory investments once child quality is better known. In addition, the timing of the impacts on vaccination contrast with the income or opportunity cost mechanisms in driving the investments effects, since they would imply significant impacts on early postnatal investments. While it is beyond the scope of this paper understanding the nature of these investment effects and it is impossible to rule out alternative histories, the evidence suggests that parental responses to birth endowments may be an important source of these findings. Independently of the specific mechanism generating these investment effects, the results of this section suggest that the reduced-form impacts of early rainfall shocks on adult outcomes are likely to represent lower bounds of biological effects.

6 Conclusion

The health and other consequences of extreme weather events are an increasingly salient issue in the public debate about the costs and benefits of climate change mitigation policies. Several scholars highlight that more heavy rainfall and droughts will have serious repercussions for children's development in poorer and more fragile states. This paper uses Colombian data to gain new insights into the effects of early rainfall shocks on later-life welfare. The findings suggest that prenatal exposure to adverse rainfall conditions results in poorer long-run health, educational, and employment outcomes. The effects of rainfall shocks on health and educational outcomes tend be larger when exposure occurs during the first trimester, which is consistent with medical literature emphasizing that the gestational environment during early stages of pregnancy is particularly important for future cognition and health. Con-

[14] In Colombia, the recommended vaccination schedule is: BCG within weeks after birth, polio at two months, four months, and six months; DPT at two months, four months, and six months; measles at 11 months.

versely, the results indicate that the impact of prenatal rainfall shocks on employment are concentrated in the third trimester. This finding indicates that rainfall affects this outcome primarily through a mechanism other than education. Previous evidence from the medical literature and recent work in economics suggest that non-cognitive skills may be a candidate channel of impact, but more work is required to draw strong conclusions.

The results of this study also reveal substantial heterogeneities between men and women. Remarkably, I find that the effects on health and educational outcomes are larger for males than for females. This finding contrasts with the seminal work of Maccini and Yang (2009), but it is in line with the recent contribution of Dinkelman (2017). A distinctive feature of the present study is its focus on a country with no known son preferences, so gender bias in household resource allocation is unlikely to drive the differences observed in the treatment effects. Rather, my findings are consistent with the literature on "fragile males" that postulates a more central role of in utero conditions for male. In contrast, I find larger effects on employment for females than for males, consistent with Maccini and Yang (2009). This finding is consistent with the notion of a different mechanism affecting employment, but it may also reflect the much larger scope for improvements in this outcome for females given the relatively low employment rates among this group.

To gain insights into the mechanisms underlying these results, I explore heterogeneities in treatment effects across different groups. The results are substantially larger among individuals born in malarious areas, consistent with the notion that exposure to infectious and parasitic diseases may be an important mechanism. Contrary to what many observers have argued, I find limited evidence supporting the agricultural income channel. Indeed, the effects of rainfall shocks are the same between individuals born in areas with low and high fraction of population depending on agricultural and farming activities. While perhaps surprising, this finding is line with suggestive evidence in Rocha and Soares (2015) that agricultural income is not the major driver of the relationship between in utero rainfall shocks and infant health in Brazil. Future studies performing more subgroup analysis in different settings would provide a more definitive understanding of the importance of the agricultural income channel.

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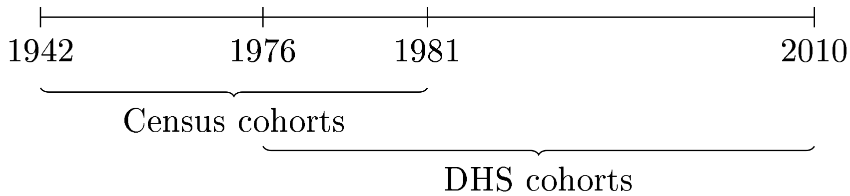
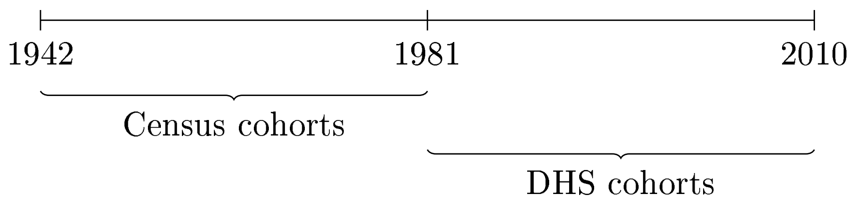
Figure 1: Distribution of census and DHS birth years



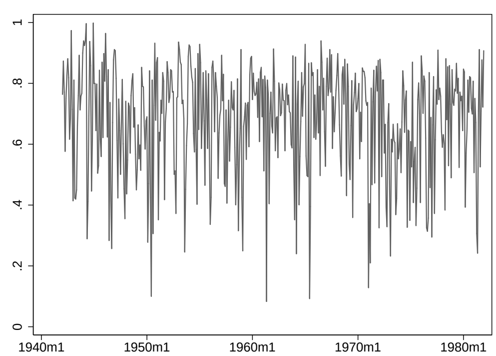
(a) Census and DHS mortality sample



(b) Census and DHS investment sample

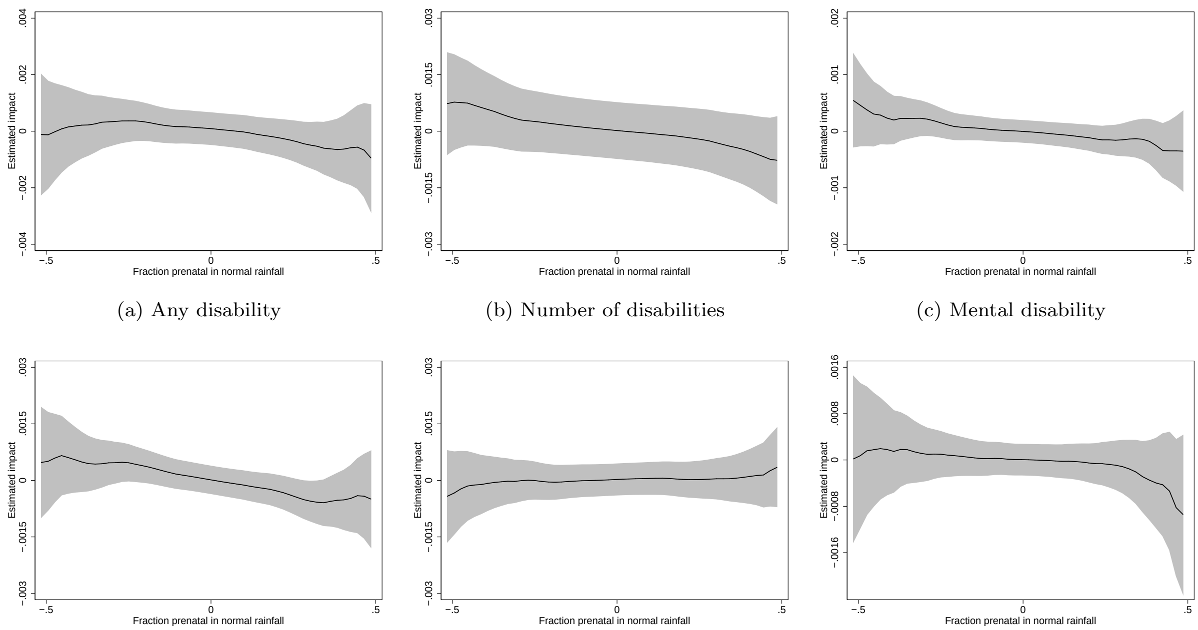
Notes. Figure 1 shows the birth years where the DHS and Census samples cover roughly the same cohorts.

Figure 2: Normal rainfall across time and place



Notes. Figure 2 presents the percentage of municipalities with normal rainfall conditions in each month. Author's calculation based on data from the Terrestrial Air Temperature and Terrestrial Precipitation: 1900-2010 Gridded Monthly Time Series, Version 3.02.

Figure 3: Effects of prenatal rainfall on disabilities

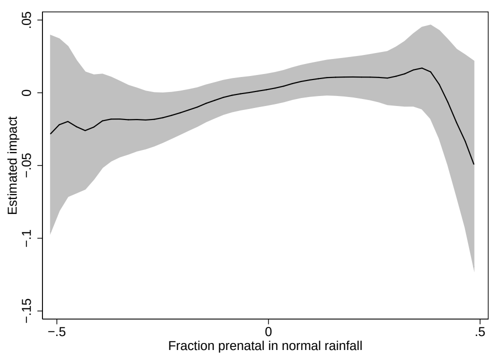


(a) Any disability (b) Number of disabilities (c) Mental disability

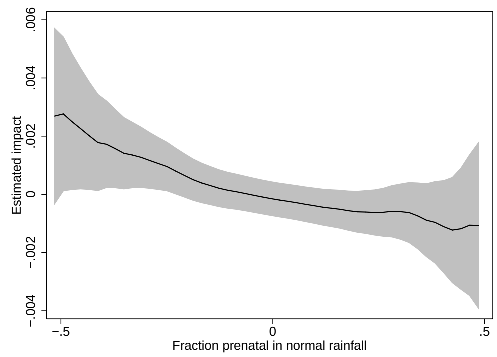
(d) Physical disability (e) Vision disability (f) Hearing/speech disability

Notes. Local regressions of outcomes on fraction prenatal in normal rainfall. To produce these plots, the outcomes and the prenatal rainfall exposure are both regressed on all other explanatory variables in equation (1). The residual terms from the outcome regression is then locally regressed on the residual from the prenatal rainfall regression using a locally weighted polynomial regression with Epanechnikov kernel functions. The degree of the polynomial is zero, meaning local-mean smoothing. The bandwidth is obtained using the Rule-of-Thumb method, which minimizes the conditional weighted mean integrated squared error. 95% confidence intervals are based on standard errors that block-bootstrap the local regressions at the grid level.

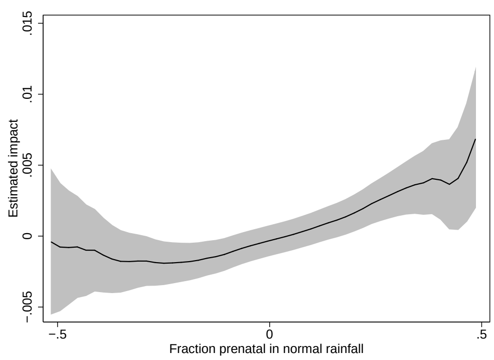
Figure 4: Effects of prenatal rainfall on socioeconomic outcomes



(a) Years of schooling



(b) Illiteracy



(c) Employment

Notes. Local regressions of outcomes on fraction prenatal in normal rainfall. To produce these plots, the outcomes and the prenatal rainfall exposure are both regressed on all other explanatory variables in equation (1). The residual terms from the outcome regression is then locally regressed on the residual from the prenatal rainfall regression using a locally weighted polynomial regression with Epanechnikov kernel functions. The degree of the polynomial is zero, meaning local-mean smoothing. The bandwidth is obtained using the Rule-of-Thumb method, which minimizes the conditional weighted mean integrated squared error. 95% confidence intervals are based on standard errors that block-bootstrap the local regressions at the grid level.

Table 1: Summary statistics

	Mean	Standard deviation	Min	Max
Any disability	0.06	0.13	0	1
Number of disabilities	0.08	0.17	0	4
Mental disability	0.01	0.04	0	1
Physical disability	0.02	0.08	0	1
Vision disability	0.03	0.09	0	1
Hearing/speech disability	0.01	0.06	0	1
Years of schooling	7.95	3.11	0	17
Illiteracy	0.07	0.14	0	1
Employment	0.58	0.26	0	1
Fraction normal rainfall (in utero)	0.69	0.18	0	1
Fraction normal rainfall - 3rd trimester	0.69	0.29	0	1
Fraction normal rainfall - 2nd trimester	0.69	0.29	0	1
Fraction normal rainfall - 1st trimester	0.69	0.29	0	1
Fraction floods (in utero)	0.14	0.15	0	1
Fraction droughts (in utero)	0.17	0.16	0	1

Notes. The data are collapsed to municipality-of-birth $\times$ month-of-birth $\times$ year-of-birth level. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The total number of municipality $\times$ month $\times$ year observations is 232962. Sample includes 524 municipalities.

Table 2: The effects of early rainfall shocks on later-life outcomes

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel (a): Full gestational period									
Normal rainfall	-0.002	-0.0039	-0.0012	-0.0026	0.0006	-0.0007	0.085	-0.0057	0.0127
	[0.0027]	[0.0038]	[0.0008]	[0.0014]*	[0.0021]	[0.0012]	[0.0524]	[0.0020]***	[0.0047]***
Panel (b): Effects of rainfall in each trimester									
3rd trimester	-0.0001	0.0004	0.0005	-0.0005	0.0003	0.0002	-0.0126	-0.0014	0.0105
	[0.0012]	[0.0018]	[0.0005]	[0.0009]	[0.0011]	[0.0007]	[0.0283]	[0.0011]	[0.0030]***
2nd trimester	-0.0012	-0.0018	-0.0006	-0.0014	0.0002	-0.0001	0.0207	-0.0007	0.0022
	[0.0015]	[0.0019]	[0.0005]	[0.0010]	[0.0010]	[0.0007]	[0.0268]	[0.0013]	[0.0028]
1st trimester	-0.0007	-0.0025	-0.001	-0.0006	0.0001	-0.0009	0.077	-0.0037	0.0001
	[0.0013]	[0.0019]	[0.0005]**	[0.0009]	[0.0011]	[0.0006]	[0.0266]***	[0.0011]***	[0.0029]

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. The total number of observations is 232962. Sample includes 524 municipalities. *Normal* rainfall is the fraction of months during the 9 months before birth that the *normal* rainfall indicator equals one. 1st trimester is the fraction of months during the 6-8 months before birth that the *normal* rainfall indicator equal one. 2nd trimester is the fraction of months during the 3-5 months before birth that the *normal* rainfall indicator equal one. 3rd trimester is the fraction of months during the 0-2 months before birth that the *normal* rainfall indicator equal one. All regressions control for municipality-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 3: The effects of prenatal floods and droughts on later-life outcomes

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel (a): Full gestational period				
Floods	0.002	0.0046	0.0023	0.0034	-0.0003	-0.0008	-0.1197	0.0085	-0.0153
	[0.0033]	[0.0048]	[0.0012]*	[0.0020]*	[0.0027]	[0.0013]	[0.0624]*	[0.0029]***	[0.0067]**
Droughts	0.0021	0.0033	0.0002	0.0019	-0.0008	0.002	-0.0568	0.0035	-0.0106
	[0.0033]	[0.0045]	[0.0010]	[0.0017]	[0.0025]	[0.0016]	[0.0637]	[0.0028]	[0.0054]**
					Panel (b): Effects of rainfall in each trimester				
3rd trimester - floods	0.0009	0.0022	0.0002	0.0015	0.0000	0.0005	-0.0114	0.0025	-0.0088
	[0.0017]	[0.0026]	[0.0007]	[0.0012]	[0.0014]	[0.0008]	[0.0405]	[0.0017]	[0.0038]**
2nd trimester - floods	0.0011	0.0003	0.0004	0.001	0.0002	-0.0012	-0.0349	0.0015	-0.007
	[0.0019]	[0.0025]	[0.0006]	[0.0014]	[0.0015]	[0.0009]	[0.0354]	[0.0017]	[0.0039]*
1st trimester - floods	0.0000	0.0020	0.0017	0.0009	-0.0005	-0.0001	-0.0725	0.0045	0.0004
	[0.0017]	[0.0026]	[0.0008]**	[0.0012]	[0.0015]	[0.0008]	[0.0369]*	[0.0016]***	[0.0035]
3rd trimester - droughts	-0.0005	-0.0025	-0.0010	-0.0003	-0.0005	-0.0008	0.0316	0.0005	-0.0119
	[0.0017]	[0.0022]	[0.0007]	[0.0011]	[0.0014]	[0.0009]	[0.0324]	[0.0015]	[0.0037]***
2nd trimester - droughts	0.0013	0.0030	0.0007	0.0017	-0.0005	0.0011	-0.0098	0.0002	0.0017
	[0.0019]	[0.0025]	[0.0007]	[0.0011]	[0.0013]	[0.0009]	[0.0335]	[0.0019]	[0.0040]
1st trimester - droughts	0.0013	0.0026	0.0005	0.0004	0.0002	0.0015	-0.0787	0.0029	-0.0008
	[0.0017]	[0.0024]	[0.0006]	[0.0011]	[0.0013]	[0.0008]*	[0.0324]**	[0.0015]**	[0.0035]

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. The total number of observations is 232962. Sample includes 524 municipalities. *Floods* (*droughts*) is the fraction of months during the 9 months before birth that the *floods* (*droughts*) indicator equals one. 1st trimester *floods* (*droughts*) is the fraction of months during the 6-8 months before birth that the *floods* (*droughts*) indicator equal one. 2nd trimester *floods* (*droughts*) is the fraction of months during the 3-5 months before birth that the *floods* (*droughts*) indicator equal one. 3rd trimester *floods* (*droughts*) is the fraction of months during the 0-2 months before birth that the *floods* (*droughts*) indicator equal one. Flood and drought shocks are defined as $\pm$ 1 standard deviations with respect to the historical monthly mean of each municipality. All regressions control for municipality-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 4: The effects of early rainfall shocks on later-life outcomes
selective fertility

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Months 0-2 before birth	0.0000	0.0005	0.0005	-0.0005	0.0003	0.0002	-0.0126	-0.0015	0.0104
	[0.0012]	[0.0017]	[0.0005]	[0.0009]	[0.0011]	[0.0006]	[0.0285]	[0.0011]	[0.0030]***
Months 3-5 before birth	-0.0012	-0.0018	-0.0006	-0.0014	0.0001	0.0000	0.0198	-0.0008	0.0022
	[0.0015]	[0.0019]	[0.0005]	[0.0010]	[0.0010]	[0.0007]	[0.0266]	[0.0013]	[0.0028]
Months 6-8 before birth	-0.0007	-0.0024	-0.001	-0.0006	0.0001	-0.0008	0.0761	-0.0036	0.0001
	[0.0014]	[0.0019]	[0.0005]*	[0.0009]	[0.0011]	[0.0006]	[0.0265]***	[0.0011]***	[0.0029]
Months 9-11 before birth	-0.0007	-0.0007	-0.0003	-0.0002	0.0000	-0.0002	0.0204	-0.0016	0.0014
	[0.0013]	[0.0018]	[0.0005]	[0.0007]	[0.0010]	[0.0006]	[0.0310]	[0.0014]	[0.0025]
Months 12-14 before birth	-0.0013	-0.0011	0.0000	-0.0006	-0.0006	0.0000	-0.0044	0.0012	0.0033
	[0.0016]	[0.0020]	[0.0004]	[0.0009]	[0.0014]	[0.0006]	[0.0269]	[0.0013]	[0.0027]
Months 15-17 before birth	-0.0005	0.0000	-0.0002	-0.0002	0.0012	-0.0009	0.0177	0.0003	-0.0004
	[0.0014]	[0.0021]	[0.0006]	[0.0007]	[0.0010]	[0.0007]	[0.0283]	[0.0014]	[0.0028]

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth $\times$ month-of-birth $\times$ year-of-birth level. The total number of observations is 232962. Sample includes 524 municipalities. Estimates represent the effects of exposure to normal rainfall conditions in different trimesters during and before pregnancy. All regressions control for municipality-of-birth fixed effects, month-of-birth $\times$ year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 5: The effects of prenatal and postnatal rainfall shocks on later-life outcomes

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Months 1-12 after birth	0.0027	0.0036	0.0007	0.0003	0.0015	0.0011	0.0844	-0.0056	0.0000
	[0.0021]	[0.0028]	[0.0007]	[0.0013]	[0.0016]	[0.0009]	[0.0508]*	[0.0019]***	0.0000
Months 0-2 before birth	-0.0001	0.0004	0.0005	-0.0005	0.0003	0.0002	-0.0133	-0.0014	0.0105
	[0.0012]	[0.0018]	[0.0005]	[0.0009]	[0.0011]	[0.0007]	[0.0283]	[0.0011]	[0.0030]***
Months 3-5 before birth	-0.0013	-0.0019	-0.0006	-0.0014	0.0002	-0.0001	0.0184	-0.0006	0.0022
	[0.0014]	[0.0019]	[0.0005]	[0.0010]	[0.0010]	[0.0007]	[0.0269]	[0.0013]	[0.0028]
Months 6-8 before birth	-0.0008	-0.0025	-0.0011	-0.0006	0.0001	-0.0009	0.0753	-0.0035	0.0001
	[0.0013]	[0.0019]	[0.0005]**	[0.0009]	[0.0011]	[0.0006]	[0.0265]***	[0.0011]***	[0.0029]

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. The total number of observations is 232962. Sample includes 524 municipalities. Estimates represent the effects of exposure to normal rainfall conditions before and after birth. All regressions control for municipality-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 6: The effects of early rainfall shocks on later-life outcomes
gender heterogeneities

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel (a): Males				
3rd trimester	0.0005	0.0001	0.0005	-0.0011	0.0004	0.0003	-0.0528	0.0006	0.0054
	[0.0020]	[0.0026]	[0.0008]	[0.0014]	[0.0016]	[0.0009]	[0.0374]	[0.0015]	[0.0042]
2nd trimester	-0.0009	-0.0015	-0.0014	-0.0013	0.0018	-0.0006	0.0461	-0.0001	0.0002
	[0.0022]	[0.0028]	[0.0006]**	[0.0015]	[0.0014]	[0.0010]	[0.0376]	[0.0018]	[0.0036]
1st trimester	-0.0006	-0.0033	-0.0022	0.0003	0.0002	-0.0016	0.1223	-0.0051	0.0025
	[0.0021]	[0.0026]	[0.0008]***	[0.0013]	[0.0017]	[0.0009]*	[0.0399]***	[0.0017]***	[0.0032]
N	204728	204728	204728	204728	204728	204728	204728	204728	204728
					Panel (b): Females				
3rd trimester	-0.0006	0.0006	0.0003	0.0000	0.0002	0.0000	0.0292	-0.0031	0.0155
	[0.0016]	[0.0023]	[0.0007]	[0.0011]	[0.0012]	[0.0008]	[0.0384]	[0.0018]*	[0.0043]***
2nd trimester	-0.0014	-0.0021	0.0002	-0.0014	-0.0012	0.0003	-0.0026	-0.0015	0.0038
	[0.0017]	[0.0022]	[0.0006]	[0.0010]	[0.0012]	[0.0009]	[0.0339]	[0.0018]	[0.0042]
1st trimester	-0.0008	-0.0016	0.0000	-0.0015	0.0001	-0.0001	0.0385	-0.0024	-0.0017
	[0.0018]	[0.0024]	[0.0006]	[0.0012]	[0.0014]	[0.0007]	[0.0352]	[0.0017]	[0.0043]
N	207544	207544	207544	207544	207544	207544	207544	207544	207544

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. Sample includes 524 municipalities. 1st trimester is the fraction of months during the 6-8 months before birth that the *normal* rainfall indicator equals one. 2nd trimester is the fraction of months during the 3-5 months before birth that the *normal* rainfall indicator equals one. 3rd trimester is the fraction of months during the 0-2 months before birth that the *normal* rainfall indicator equals one. All regressions control for municipality-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 7: The effects of early rainfall shocks on later-life outcomes
heterogeneities in treament effects (1)

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Normal rainfall	-0.0002	-0.0003	-0.0006	-0.0014	0.0019	-0.0001	0.0708	-0.0045	0.0135
	[0.0027]	[0.0036]	[0.0008]	[0.0016]	[0.0021]	[0.0012]	[0.0530]	[0.0023]**	[0.0048]***
Normal rainfall x Malaria risk	-0.0024	-0.0052	-0.0018	-0.0023	0.0007	-0.0018	0.1168	-0.0045	0.006
	[0.0021]	[0.0029]*	[0.0008]**	[0.0015]	[0.0016]	[0.0009]**	[0.0498]**	[0.0021]**	[0.0047]
Normal rainfall x Rural population	0.0016	0.0002	-0.001	0.0008	0.0003	0.0001	0.0267	-0.0019	0.0012
	[0.0029]	[0.0040]	[0.0010]	[0.0017]	[0.0025]	[0.0013]	[0.0735]	[0.0029]	[0.0062]
Normal rainfall x Population	-0.0006	-0.0034	-0.0004	-0.0015	-0.0009	-0.0006	0.0566	-0.0029	-0.0002
	[0.0021]	[0.0031]	[0.0006]	[0.0011]	[0.0016]	[0.0010]	[0.0375]	[0.0017]*	[0.0049]
Normal rainfall x Income	0.0007	0.0013	-0.0017	0.0016	0.0005	0.0009	-0.0681	-0.0005	-0.0004
	[0.0029]	[0.0041]	[0.0011]	[0.0019]	[0.0025]	[0.0013]	[0.0662]	[0.0031]	[0.0059]

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. The total number of observations is 232962. Sample includes 524 municipalities. *Normal* rainfall is the fraction of months during the 9 months before birth that the *normal* rainfall indicator equals one. Rural population rate, income and population at the municipality level are computed from the 1973 Census. Malaria risk at the municipality level is obtained from Bleakley (2010). Each municipality-specific variable are normalized to have mean 0 and standard deviation 1. All regressions control for municipality-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 8: The effects of early rainfall shocks on later-life outcomes
heterogeneities in treament effects (2)

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Floods	0.0006	0.0012	0.0015	0.0022	-0.0018	-0.0008	-0.1244	0.0083	-0.0181
	[0.0034]	[0.0047]	[0.0012]	[0.0021]	[0.0028]	[0.0014]	[0.0658]*	[0.0032]**	[0.0067]***
Floods x Malaria risk	0.0026	0.0055	0.002	0.0017	0.0007	0.001	-0.1121	0.0036	-0.0079
	[0.0031]	[0.0040]	[0.0010]**	[0.0019]	[0.0021]	[0.0012]	[0.0601]*	[0.0029]	[0.0061]
Floods x Rural population	-0.0033	-0.0017	0.0011	-0.0009	-0.0007	-0.0013	-0.0212	0.0039	0.0113
	[0.0039]	[0.0051]	[0.0013]	[0.0022]	[0.0034]	[0.0017]	[0.0826]	[0.0036]	[0.0077]
Floods x Population	0.0033	0.008	0.0014	0.0022	0.0038	0.0005	-0.0378	0.0021	-0.0007
	[0.0027]	[0.0046]*	[0.0011]	[0.0016]	[0.0019]**	[0.0015]	[0.0455]	[0.0019]	[0.0054]
Floods x Income	-0.0063	-0.0082	0.001	-0.0032	-0.0037	-0.0023	0.0753	0.0027	0.0157
	[0.0036]*	[0.0052]	[0.0014]	[0.0022]	[0.0032]	[0.0018]	[0.0760]	[0.0033]	[0.0078]**
Droughts	0.0001	-0.0002	-0.0002	0.0008	-0.0018	0.0009	-0.0278	0.0012	-0.0102
	[0.0033]	[0.0043]	[0.0010]	[0.0018]	[0.0025]	[0.0014]	[0.0629]	[0.0030]	[0.0054]*
Droughts x Malaria risk	0.0022	0.005	0.0016	0.0028	-0.0019	0.0025	-0.1224	0.0053	-0.0049
	[0.0023]	[0.0030]	[0.0009]*	[0.0016]*	[0.0019]	[0.0011]**	[0.0570]**	[0.0023]**	[0.0048]
Droughts x Rural population	-0.0004	0.001	0.0009	-0.0007	0.0000	0.0008	-0.0294	0.0004	-0.0104
	[0.0032]	[0.0044]	[0.0012]	[0.0019]	[0.0027]	[0.0015]	[0.0861]	[0.0034]	[0.0065]
Droughts x Population	-0.0014	0.0002	-0.0003	0.001	-0.0012	0.0007	-0.0701	0.0036	0.0008
	[0.0023]	[0.0031]	[0.0007]	[0.0013]	[0.0019]	[0.0010]	[0.0453]	[0.0021]*	[0.0052]
Droughts x Income	0.0033	0.0036	0.0022	-0.0005	0.0018	0.0001	0.0639	-0.0012	-0.0106
	[0.0033]	[0.0046]	[0.0013]*	[0.0021]	[0.0026]	[0.0014]	[0.0765]	[0.0038]	[0.0066]

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. The total number of observations is 232962. Sample includes 524 municipalities. *Floods/droughts* is the fraction of months during the 9 months before birth that the *floods/droughts* indicator equals one. Rural population rate, income and population at the municipality level are computed from the 1973 Census. Malaria risk at the municipality level is obtained from Bleakley (2010). Each municipality-specific variable are normalized to have mean 0 and standard deviation 1. All regressions control for municipality-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 9: The effects of early rainfall shocks on postnatal investment

	Was child ever breastfed?	# months breastfed	Ln(# months breastfed + 1)	Breastfed for more than six months	BCG	DPT	Polio	Measles
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel (a): Full gestational period				
Normal rainfall	-0.0322	-0.4878	-0.0512	-0.0633	-0.0869	0.0002	-0.0226	-0.0167
	[0.0119]***	[0.4555]	[0.0396]	[0.0241]***	[0.1337]	[0.0118]	[0.0264]	[0.0254]
N	29924	27888	27888	28063	30470	30489	30488	30488
				Panel (b): Effects of rainfall in each trimester				
3rd trimester	-0.0111	0.009	-0.0156	-0.0091	-0.0163	-0.004	0.0035	0.0082
	[0.0066]*	[0.2045]	[0.0185]	[0.0121]	[0.0591]	[0.0057]	[0.0111]	[0.0121]
2nd trimester	-0.0076	-0.4022	-0.0227	-0.0285	0.0509	0.0016	0.0037	0.0042
	[0.0070]	[0.2397]*	[0.0216]	[0.0135]**	[0.0580]	[0.0065]	[0.0119]	[0.0114]
1st trimester	-0.0139	-0.0705	-0.0123	-0.0249	-0.1325	0.0025	-0.0316	-0.0309
	[0.0070]**	[0.2155]	[0.0197]	[0.0137]*	[0.0591]**	[0.0061]	[0.0115]***	[0.0114]***
N	29924	27888	27888	28063	30470	30489	30488	30488

Notes. Demographic Health Surveys: 1986, 1990, 1995, 2000, 2004-05, and 2009-10 waves. The sample consists of children in families that have been living in the current municipality for a greater time than child's age. Children in the five main capitals of the country are excluded. Dependent variable in column (1) is dummy variable indicating whether the child was ever breastfed. Columns (2)-(3) represent the duration of breastfeeding by using linear and log-linear regressions, respectively. Dependent variable in column (4) is a dummy variable indicating whether the child was breastfed for more than six months. Dependent variables in columns (5)-(8) are dummies indicating whether the child has all the recommended vaccination doses for specific diseases. $Normal$ is the fraction of months during the 9 months before birth that the *normal* indicator equals one. 1st trimester is the fraction of months during the 6-8 months before birth that the *normal* rainfall indicator equal one. 2nd trimester is the fraction of months during the 3-5 months before birth that the *normal* rainfall indicator equal one. 3rd trimester is the fraction of months during the 0-2 months before birth that the *normal* rainfall indicator equal one. All regressions control for municipality fixed effects, month-of-birth $\times$ year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, mother's age at first birth, indicators for mother's education level, an indicator for marital status (married), an indicator for child's sex, and birth order. The regressions use the data at the child level. Robust standard errors (in brackets) are clustered at the grid level (255 grids). The number of municipalities in the sample is 443. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

For Online Publication: Early Rainfall Shocks and Later-Life Outcomes: Evidence from Colombia*

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A Appendix

Table A.1: Summary statistics - Demographic Health Surveys

	Mean	Standard deviation	Min	Max
Neonatal mortality	0.016	0.12	0	1
Infant mortality	0.024	0.15	0	1
Was child ever breastfed?	0.93	0.25	0	1
# months breastfed	12.33	9.08	1	59
Breastfed for more than six months	0.64	0.48	0	1
BCG immunization	0.94	0.23	0	1
DPT immunization	0.67	0.47	0	1
Polio immunization	0.73	0.44	0	1
Measles immunization	0.70	0.46	0	1
Specific doses				
Polio 1	0.89	0.32	0	1
Polio 2	0.81	0.40	0	1
Polio 3	0.67	0.47	0	1
DPT 1	0.91	0.28	0	1
DPT 2	0.82	0.39	0	1
DPT 3	0.74	0.44	0	1

Notes. Demographic Health Surveys (1986, 1990, 1995, 2000, 2004-05, 2009-10). The sample consists of children in families that have been living in the current municipality for a greater time than child's age. Children in the five main capitals of the country are excluded. Neonatal and infant mortality outcomes focus on births that occurred less than ten years prior to the date of the survey. This includes children born between 1976 and 2010. Breastfeeding and vaccination outcomes is based on a sample of children under five. This includes births that occurred between 1981 and 2010.

Table A.2: The effects of early rainfall shocks on cohort size

	Baseline		Younger cohorts		Older cohorts	
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel (a): full sample			
Normal rainfall	0.0068		-0.0026		0.0068	
	[0.0127]		[0.0155]		[0.0165]	
Floods		-0.0315		-0.0267		-0.025
		[0.0175]*		[0.0193]		[0.0278]
Droughts		0.0133		0.0295		0.0053
		[0.0140]		[0.0202]		[0.0184]
N	232962	232962	128301	128301	104661	104661
			Panel (b): males			
Normal rainfall	0.0071		0.0026		-0.011	
	[0.0162]		[0.0221]		[0.0264]	
Floods		-0.0401		-0.0256		-0.0459
		[0.0200]**		[0.0273]		[0.0353]
Droughts		0.0198		0.0185		0.0486
		[0.0199]		[0.0278]		[0.0310]
N	204728	204728	117122	117122	87606	87606
			Panel (c): females			
Normal rainfall	0.0157		-0.0009		0.0303	
	[0.0168]		[0.0215]		[0.0250]	
Floods		-0.0235		-0.026		-0.0099
		[0.0250]		[0.0286]		[0.0413]
Droughts		-0.0093		0.0256		-0.0438
		[0.0185]		[0.0282]		[0.0308]
N	207544	207544	118908	118908	88636	88636

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. Younger cohorts refers to individuals born between 1942 and 1960, while older cohorts refers to those born between 1961 and 1981. The data are collapsed to municipality-of-birth $\times$ month-of-birth $\times$ year-of-birth level. Sample includes 524 municipalities. $Normal/floods/droughts$ is the fraction of months during the 9 months before birth that the $normal/floods/droughts$ indicator equals one. All regressions control for municipality-of-birth fixed effects, month-of-birth $\times$ year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A.3: The effects of early rainfall shocks on neonatal and infant mortality

	Full sample		Males		Females	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel (a): dependent variable is neonatal mortality					
Normal rainfall	-0.0065		-0.0128		-0.0005	
	[0.0040]		[0.0059]**		[0.0050]	
Floods		0.0108		0.0168		0.0062
		[0.0055]**		[0.0076]**		[0.0072]
Droughts		0.003		0.0095		-0.0043
		[0.0054]		[0.0085]		[0.0068]
N	61220	61220	31264	31264	29956	29956
	Panel (b): dependent variable is infant mortality					
Normal rainfall	-0.0026		-0.0102		0.0049	
	[0.0047]		[0.0071]		[0.0056]	
Floods		-0.0001		0.0131		0.0003
		[0.0059]		[0.0097]		[0.0083]
Droughts		0.0059		0.0079		-0.0093
		[0.0066]		[0.0098]		[0.0076]
N	61220	61220	31264	31264	29956	29956

Notes. Demographic Health Surveys: 1986, 1990, 1995, 2000, 2004-05, and 2009-10 waves. The sample consists of children in families that have been living in the current municipality for a greater time than child's age. Children in the five main capitals of the country are excluded. The sample includes births that occurred less than ten years prior to the date of the survey. This includes children born between 1976 and 2010. $Normal/floods/droughts$ is the fraction of months during the 9 months before birth that the $normal/floods/droughts$ indicator equals one. All regressions control for municipality fixed effects, month-of-birth $\times$ year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, mother's age at first birth, indicators for mother's education level, an indicator for marital status (married), an indicator for child's sex, and birth order. The regressions use the data at the child level. Robust standard errors (in brackets) are clustered at the grid level (256 grids). The number of municipalities in the sample is 446. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A.4: The effects of early rainfall shocks on neonatal and infant mortality heterogeneity

	Neonatal mortality		Infant mortality	
	(1)	(2)	(3)	(4)
Normal rainfall	-0.0074		-0.003	
	[0.0050]		[0.0056]	
Floods		0.011		0.0097
		[0.0077]		[0.0094]
Droughts		0.0071		-0.0003
		[0.0079]		[0.0089]
Normal rainfall x Married	-0.0015		-0.0042	
	[0.0064]		[0.0079]	
Normal rainfall x High education	0.0125		0.0111	
	[0.0101]		[0.0104]	
Normal rainfall x Teenage	0.0001		0.0007	
	[0.0019]		[0.0023]	
Floods x Married		0.0104		0.0103
		[0.0094]		[0.0126]
Floods x High education		-0.0148		-0.0124
		[0.0130]		[0.0140]
Floodsl x Teenage		-0.0027		-0.0094
		[0.0072]		[0.0087]
Droughts x Married		-0.0048		0.0000
		[0.0076]		[0.0086]
Droughts x High education		-0.0124		-0.0114
		[0.0131]		[0.0138]
Droughts x Teenage		-0.0024		0.0029
		[0.0065]		[0.0078]

Notes. Demographic Health Surveys: 1986, 1990, 1995, 2000, 2004-05, and 2009-10 waves. The sample consists of children in families that have been living in the current municipality for a greater time than child's age. Children in the five main capitals of the country are excluded. The sample includes births that occurred less than ten years prior to the date of the survey. This includes children born between 1976 and 2010. The total number of observations is 61220. $Normal/floods/droughts$ is the fraction of months during the 9 months before birth that the $normal/floods/droughts$ indicator equals one. Teenage is an indicator for mothers who were under the age of 20 at the time they give their first birth. High education is an indicator for mothers with some college or more. All regressions control for municipality fixed effects, month-of-birth $\times$ year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, mother's age at first birth, indicators for mother's education level, an indicator for marital status (married), an indicator for child's sex, and birth order. The regressions use the data at the child level. Robust standard errors (in brackets) are clustered at the grid level (256 grids). The number of municipalities in the sample is 446. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A.5: The effects of early rainfall shocks on later life outcomes
alternative standard errors

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel (a): Full gestational period				
Normal rainfall	-0.002	-0.0039	-0.0012	-0.0026	0.0006	-0.0007	0.085	-0.0057	0.0127
SE clustered by grid level	[0.0027]	[0.0038]	[0.0008]	[0.0014]*	[0.0021]	[0.0012]	[0.0524]	[0.0020]***	[0.0047]***
Conley SE (cuttof of 100 km)	[0.0025]	[0.0034]	[0.0008]	[0.0016]	[0.0019]	[0.0011]	[0.0409]**	[0.0030]*	[0.0039]***
Conley SE (cuttof of 500 km)	[0.0024]	[0.0034]	[0.0008]	[0.0017]	[0.0019]	[0.0011]	[0.0386]**	[0.0030]*	[0.0038]***
					Panel (b): Effects of rainfall in each trimester				
3rd trimester	-0.0001	0.0004	0.0005	-0.0005	0.0003	0.0002	-0.0126	-0.0014	0.0105
SE clustered by grid level	[0.0012]	[0.0018]	[0.0005]	[0.0009]	[0.0011]	[0.0007]	[0.0283]	[0.0011]	[0.0030]***
Conley SE (cuttof of 100 km)	[0.0015]	[0.0020]	[0.0005]	[0.0010]	[0.0011]	[0.0007]	[0.0250]	[0.0017]	[0.0024]***
Conley SE (cuttof of 500 km)	[0.0015]	[0.0020]	[0.0005]	[0.0009]	[0.0011]	[0.0007]	[0.0255]	[0.0017]	[0.0023]***
2nd trimester	-0.0012	-0.0018	-0.0006	-0.0014	0.0002	-0.0001	0.0207	-0.0007	0.0022
SE clustered by grid level	[0.0015]	[0.0019]	[0.0005]	[0.0010]	[0.0010]	[0.0007]	[0.0268]	[0.0013]	[0.0028]
Conley SE (cuttof of 100 km)	[0.0014]	[0.0020]	[0.0005]	[0.0009]	[0.0011]	[0.0006]	[0.0242]	[0.0017]	[0.0024]
Conley SE (cuttof of 500 km)	[0.0016]	[0.0021]	[0.0005]	[0.0010]	[0.0012]	[0.0006]	[0.0242]	[0.0016]	[0.0024]
1st trimester	-0.0007	-0.0025	-0.001	-0.0006	0.0001	-0.0009	0.077	-0.0037	0.0001
SE clustered by grid level	[0.0013]	[0.0019]	[0.0005]**	[0.0009]	[0.0011]	[0.0006]	[0.0266]***	[0.0011]***	[0.0029]
Conley SE (cuttof of 100 km)	[0.0015]	[0.0020]	[0.0005]**	[0.0009]	[0.0011]	[0.0006]	[0.0247]***	[0.0017]**	[0.0024]
Conley SE (cuttof of 500 km)	[0.0015]	[0.0021]	[0.0005]**	[0.0009]	[0.0012]	[0.0007]	[0.0243]***	[0.0018]**	[0.0023]

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. The total number of observations is 232962. Sample includes 524 municipalities. *Normal* rainfall is the fraction of months during the 9 months before birth that the *normal* rainfall indicator equals one. 1st trimester is the fraction of months during the 6-8 months before birth that the *normal* rainfall indicator equals one. 2nd trimester is the fraction of months during the 3-5 months before birth that the *normal* rainfall indicator equals one. 3rd trimester is the fraction of months during the 0-2 months before birth that the *normal* rainfall indicator equals one. All regressions control for municipality-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A.6: The effects of early rainfall shocks on later-life outcomes controlling for conflict intensity (1)

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel (a): Baseline				
Normal rainfall	-0.002	-0.0039	-0.0012	-0.0026	0.0006	-0.0007	0.085	-0.0057	0.0127
	[0.0027]	[0.0038]	[0.0008]	[0.0014]*	[0.0021]	[0.0012]	[0.0524]	[0.0020]***	[0.0047]***
					Panel (b): Add guerrilla and paramalitary attacks × linear time trends				
Normal rainfall	-0.002	-0.0039	-0.0012	-0.0026	0.0006	-0.0007	0.085	-0.0057	0.0127
	[0.0027]	[0.0038]	[0.0008]	[0.0014]*	[0.0021]	[0.0012]	[0.0523]	[0.0020]***	[0.0047]***
					Panel (c): Add guerrilla and paramalitary massacres × linear time trends				
Normal rainfall	-0.002	-0.0039	-0.0012	-0.0026	0.0006	-0.0007	0.085	-0.0057	0.0127
	[0.0027]	[0.0038]	[0.0008]	[0.0014]*	[0.0021]	[0.0012]	[0.0523]	[0.0020]***	[0.0047]***
					Panel (d): Add guerrilla and paramalitary political kidnappings × linear time trends				
Normal rainfall	-0.002	-0.0039	-0.0012	-0.0026	0.0006	-0.0007	0.085	-0.0057	0.0127
	[0.0027]	[0.0038]	[0.0008]	[0.0014]*	[0.0021]	[0.0012]	[0.0523]	[0.0020]***	[0.0047]***
					Panel (e): Add the complete set of interactions				
Normal rainfall	-0.002	-0.0039	-0.0012	-0.0026	0.0006	-0.0007	0.085	-0.0057	0.0127
	[0.0027]	[0.0038]	[0.0008]	[0.0014]*	[0.0021]	[0.0012]	[0.0523]	[0.0020]***	[0.0047]***

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. The total number of observations is 232962. Sample includes 524 municipalities. *Normal* rainfall is the fraction of months during the 9 months before birth that the *normal* rainfall indicator equals one. Guerrilla and paramalitary attacks refer to the annual average number of attacks perpetrated by either guerrilla or paramalitary groups in a given municipality between 1988 and 2005. The same logic is used for political kidnappings and massacres. All regressions control for municipality-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A.7: The effects of early rainfall shocks on later-life outcomes controlling for conflict intensity (2)

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel (a): Baseline				
3rd trimester	-0.0001	0.0004	0.0005	-0.0005	0.0003	0.0002	-0.0126	-0.0014	0.0105
	[0.0012]	[0.0018]	[0.0005]	[0.0009]	[0.0011]	[0.0007]	[0.0283]	[0.0011]	[0.0030]***
2nd trimester	-0.0012	-0.0018	-0.0006	-0.0014	0.0002	-0.0001	0.0207	-0.0007	0.0022
	[0.0015]	[0.0019]	[0.0005]	[0.0010]	[0.0010]	[0.0007]	[0.0268]	[0.0013]	[0.0028]
1st trimester	-0.0007	-0.0025	-0.001	-0.0006	0.0001	-0.0009	0.077	-0.0037	0.0001
	[0.0013]	[0.0019]	[0.0005]**	[0.0009]	[0.0011]	[0.0006]	[0.0266]***	[0.0011]***	[0.0029]
					Panel (b): Add guerrilla and paramalitary attacks × linear time trends				
3rd trimester	-0.0001	0.0004	0.0005	-0.0005	0.0003	0.0002	-0.0126	-0.0014	0.0105
	[0.0012]	[0.0018]	[0.0005]	[0.0009]	[0.0011]	[0.0007]	[0.0283]	[0.0011]	[0.0030]***
2nd trimester	-0.0012	-0.0018	-0.0006	-0.0014	0.0002	-0.0001	0.0207	-0.0007	0.0022
	[0.0014]	[0.0019]	[0.0005]	[0.0010]	[0.0010]	[0.0007]	[0.0268]	[0.0013]	[0.0028]
1st trimester	-0.0007	-0.0025	-0.001	-0.0006	0.0001	-0.0009	0.077	-0.0037	0.0001
	[0.0013]	[0.0019]	[0.0005]**	[0.0009]	[0.0011]	[0.0006]	[0.0265]***	[0.0011]***	[0.0029]
					Panel (c): Add guerrilla and paramalitary massacres × linear time trends				
3rd trimester	-0.0001	0.0004	0.0005	-0.0005	0.0003	0.0002	-0.0126	-0.0014	0.0105
	[0.0012]	[0.0018]	[0.0005]	[0.0009]	[0.0011]	[0.0007]	[0.0283]	[0.0011]	[0.0030]***
2nd trimester	-0.0012	-0.0018	-0.0006	-0.0014	0.0002	-0.0001	0.0207	-0.0007	0.0022
	[0.0014]	[0.0019]	[0.0005]	[0.0010]	[0.0010]	[0.0007]	[0.0268]	[0.0013]	[0.0028]
1st trimester	-0.0007	-0.0025	-0.001	-0.0006	0.0001	-0.0009	0.077	-0.0037	0.0001
	[0.0013]	[0.0019]	[0.0005]**	[0.0009]	[0.0011]	[0.0006]	[0.0265]***	[0.0011]***	[0.0029]
					Panel (d): Add guerrilla and paramalitary political kidnappings × linear time trends				
3rd trimester	-0.0001	0.0004	0.0005	-0.0005	0.0003	0.0002	-0.0126	-0.0014	0.0105
	[0.0012]	[0.0018]	[0.0005]	[0.0009]	[0.0011]	[0.0007]	[0.0283]	[0.0011]	[0.0030]***
2nd trimester	-0.0012	-0.0018	-0.0006	-0.0014	0.0002	-0.0001	0.0207	-0.0007	0.0022
	[0.0014]	[0.0019]	[0.0005]	[0.0010]	[0.0010]	[0.0007]	[0.0268]	[0.0013]	[0.0028]
1st trimester	-0.0007	-0.0025	-0.001	-0.0006	0.0001	-0.0009	0.077	-0.0037	0.0001
	[0.0013]	[0.0019]	[0.0005]**	[0.0009]	[0.0011]	[0.0006]	[0.0265]***	[0.0011]***	[0.0029]
					Panel (e): Add the complete set of interactions				
3rd trimester	-0.0001	0.0004	0.0005	-0.0005	0.0003	0.0002	-0.0126	-0.0014	0.0105
	[0.0012]	[0.0018]	[0.0005]	[0.0009]	[0.0011]	[0.0007]	[0.0283]	[0.0011]	[0.0030]***
2nd trimester	-0.0012	-0.0018	-0.0006	-0.0014	0.0002	-0.0001	0.0207	-0.0007	0.0022
	[0.0014]	[0.0019]	[0.0005]	[0.0010]	[0.0010]	[0.0007]	[0.0268]	[0.0013]	[0.0028]
1st trimester	-0.0007	-0.0025	-0.001	-0.0006	0.0001	-0.0009	0.077	-0.0037	0.0001
	[0.0013]	[0.0019]	[0.0005]**	[0.0009]	[0.0011]	[0.0006]	[0.0265]***	[0.0011]***	[0.0029]

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. The total number of observations is 232962. Sample includes 524 municipalities. 1st trimester is the fraction of months during the 6-8 months before birth that the *normal* rainfall indicator equals one. 2nd trimester is the fraction of months during the 3-5 months before birth that the *normal* rainfall indicator equal one. 3rd trimester is the fraction of months during the 0-2 months before birth that the *normal* rainfall indicator equal one. Guerrilla and paramalitary attacks refer to the annual average number of attacks perpetrated by either guerrilla or paramalitary groups in a given municipality between 1988 and 2005. The same logic is used for political kidnappings and massacres. All regressions control for municipality-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A.8: The effects of early rainfall shocks on later-life outcomes
alternative rainfall shock measures

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel (a): Rainfall				
Rainfall	0.0004	0.0018	0.0014	0.0005	0.0009	-0.001	-0.0737	0.0033	0.0049
	[0.0029]	[0.0037]	[0.0008]*	[0.0015]	[0.0023]	[0.0012]	[0.0440]*	[0.0030]	[0.0053]
					Panel (b): Rainfall shocks based on 10 and 90 percentiles				
Floods	-0.0004	0.0007	0.0009	0.0001	0.0000	-0.0002	-0.0357	0.0018	-0.0034
	[0.0013]	[0.0018]	[0.0004]**	[0.0008]	[0.0010]	[0.0005]	[0.0229]	[0.0012]	[0.0025]
Droughts	-0.0004	-0.0005	-0.0001	-0.0003	-0.0004	0.0003	-0.0174	0.0011	-0.0034
	[0.0011]	[0.0015]	[0.0004]	[0.0005]	[0.0009]	[0.0005]	[0.0205]	[0.0010]	[0.0018]*
					Panel (c): Rainfall shocks based on SPI index				
Floods	0.0008	0.0009	0.0003	0.0003	-0.0001	0.0004	-0.0194	0.0021	-0.0057
	[0.0011]	[0.0015]	[0.0004]	[0.0006]	[0.0008]	[0.0005]	[0.0205]	[0.0009]**	[0.0018]***
Droughts	0.0003	0.0011	0.0007	0.0007	0.0000	-0.0003	-0.0211	0.0024	-0.0041
	[0.0011]	[0.0016]	[0.0004]	[0.0007]	[0.0008]	[0.0004]	[0.0184]	[0.0010]**	[0.0021]*

Notes. Panel (a) considers the natural log of rainfall 9 months before birth minus the natural log of historical mean rainfall. Panel (b) defines flood (droughts) for a given month if rainfall was above (below) the 90th (10th) percentile of rainfall distribution for that calendar month within municipality. Panel (c) follows the same logic of our baseline measures, but z-scores are computed using the Spatial Precipitation Index distribution. Panels (b) and (c) measures exposure to droughts and floods according to the frequency with which a given shock occurs in the 9 months prior to the individual's birth. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth $\times$ month-of-birth $\times$ year-of-birth level. The total number of observations is 232962. Sample includes 524 municipalities. All regressions control for municipality-of-birth fixed effects, month-of-birth $\times$ year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A.9: The effects of early rainfall shocks on later-life outcomes specific seasonal variation (1)

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel (a): Baseline				
Normal rainfall	-0.002	-0.0039	-0.0012	-0.0026	0.0006	-0.0007	0.085	-0.0057	0.0127
	[0.0027]	[0.0038]	[0.0008]	[0.0014]*	[0.0021]	[0.0012]	[0.0524]	[0.0020]***	[0.0047]***
					Panel (b): Controlling for municipality-of-birth × month-of-birth fixed effects				
Normal rainfall	-0.0021	-0.0038	-0.0012	-0.0024	0.0005	-0.0008	0.0835	-0.006	0.0125
	[0.0027]	[0.0038]	[0.0008]	[0.0014]*	[0.0021]	[0.0012]	[0.0524]	[0.0020]***	[0.0047]***

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. The total number of observations is 232962. Sample includes 524 municipalities. *Normal* rainfall is the fraction of months during the 9 months before birth that the *normal* rainfall indicator equals one. All regressions control for municipality-of-birth × month-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A.10: The effects of early rainfall shocks on later-life outcomes specific seasonal variation (2)

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel (a): Baseline				
3rd trimester	-0.0001	0.0004	0.0005	-0.0005	0.0003	0.0002	-0.0126	-0.0014	0.0105
	[0.0012]	[0.0018]	[0.0005]	[0.0009]	[0.0011]	[0.0007]	[0.0283]	[0.0011]	[0.0030]***
2nd trimester	-0.0012	-0.0018	-0.0006	-0.0014	0.0002	-0.0001	0.0207	-0.0007	0.0022
	[0.0015]	[0.0019]	[0.0005]	[0.0010]	[0.0010]	[0.0007]	[0.0268]	[0.0013]	[0.0028]
1st trimester	-0.0007	-0.0025	-0.001	-0.0006	0.0001	-0.0009	0.077	-0.0037	0.0001
	[0.0013]	[0.0019]	[0.0005]**	[0.0009]	[0.0011]	[0.0006]	[0.0266]***	[0.0011]***	[0.0029]
					Panel (b): Controlling for municipality-of-birth x month-of-birth fixed effects				
3rd trimester	-0.0002	0.0006	0.0004	-0.0002	0.0002	0.0003	-0.0098	-0.0014	0.0102
	[0.0012]	[0.0018]	[0.0005]	[0.0009]	[0.0011]	[0.0007]	[0.0299]	[0.0011]	[0.0030]***
2nd trimester	-0.0013	-0.0019	-0.0006	-0.0014	0.0000	0.0000	0.0182	-0.0004	0.0016
	[0.0015]	[0.0019]	[0.0005]	[0.0010]	[0.0010]	[0.0007]	[0.0267]	[0.0013]	[0.0027]
1st trimester	-0.0005	-0.0025	-0.001	-0.0008	0.0003	-0.001	0.0752	-0.0041	0.0008
	[0.0013]	[0.0019]	[0.0005]*	[0.0009]	[0.0011]	[0.0006]*	[0.0278]***	[0.0011]***	[0.0030]

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. The total number of observations is 232962. Sample includes 524 municipalities. 1st trimester is the fraction of months during the 6-8 months before birth that the *normal* rainfall indicator equals one. 2nd trimester is the fraction of months during the 3-5 months before birth that the *normal* rainfall indicator equals one. 3rd trimester is the fraction of months during the 0-2 months before birth that the *normal* rainfall indicator equals one. All regressions control for municipality-of-birth × month-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A.11: The effects of early rainfall shocks on later-life outcomes across time (1)

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel (a): Cohorts born during 1942-1960				
Normal rainfall	-0.0034	-0.0071	-0.001	-0.0042	0.0007	-0.0026	0.0977	-0.0084	0.0073
	[0.0053]	[0.0067]	[0.0014]	[0.0029]	[0.0041]	[0.0022]	[0.0958]	[0.0050]*	[0.0076]
					Panel (a): Cohorts born during 1961-1981				
Normal rainfall	-0.0022	-0.0039	-0.0015	-0.0017	-0.0004	-0.0003	0.0255	-0.0016	0.0088
	[0.0027]	[0.0041]	[0.0011]	[0.0018]	[0.0017]	[0.0013]	[0.0669]	[0.0025]	[0.0072]

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. The total number of observations in panels (a) and (b) is 104661 and 128301, respectively. Sample includes 524 municipalities. *Normal* rainfall is the fraction of months during the 9 months before birth that the *normal* rainfall indicator equals one. All regressions control for municipality-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A.12: The effects of early rainfall shocks on later-life outcomes across time (2)

	Any disability	Number of disabilities	Mental disability	Physical disability	Vision disability	Hearing/speech disability	Years of Schooling	Illiteracy	Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Panel (a): Cohorts born during 1942-1960				
3rd trimester	0.0011	0.0008	0.0011	0.001	-0.0004	-0.0009	0.0041	-0.0039	0.0122
	[0.0026]	[0.0032]	[0.0009]	[0.0019]	[0.0021]	[0.0013]	[0.0524]	[0.0028]	[0.0044]***
2nd trimester	-0.0033	-0.0039	-0.0009	-0.0034	0.001	-0.0005	0.0019	-0.0001	-0.0055
	[0.0030]	[0.0039]	[0.0008]	[0.0015]**	[0.0023]	[0.0014]	[0.0527]	[0.0028]	[0.0053]
1st trimester	-0.0011	-0.0039	-0.0011	-0.0017	0.0001	-0.0012	0.0924	-0.0045	0.0011
	[0.0028]	[0.0039]	[0.0008]	[0.0020]	[0.0022]	[0.0012]	[0.0411]**	[0.0023]**	[0.0043]
					Panel (a): Cohorts born during 1961-1981				
3rd trimester	-0.0011	-0.0006	0.0000	-0.0014	0.0003	0.0006	-0.0359	0.0008	0.0075
	[0.0015]	[0.0021]	[0.0006]	[0.0009]	[0.0010]	[0.0008]	[0.0404]	[0.0013]	[0.0037]**
2nd trimester	-0.0004	-0.0013	-0.0004	-0.0003	-0.0005	0.0000	0.0135	-0.0002	0.0042
	[0.0014]	[0.0018]	[0.0006]	[0.0012]	[0.0008]	[0.0008]	[0.0348]	[0.0015]	[0.0034]
1st trimester	-0.0007	-0.0021	-0.0011	0.0001	-0.0002	-0.0008	0.0476	-0.0022	-0.0029
	[0.0014]	[0.0022]	[0.0008]	[0.0009]	[0.0010]	[0.0007]	[0.0307]	[0.0013]*	[0.0040]

Notes. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. The total number of observations in panels (a) and (b) is 104661 and 128301, respectively. Sample includes 524 municipalities. 1st trimester is the fraction of months during the 6-8 months before birth that the *normal* rainfall indicator equal one. 2nd trimester is the fraction of months during the 3-5 months before birth that the *normal* rainfall indicator equal one. 3rd trimester is the fraction of months during the 0-2 months before birth that the *normal* rainfall indicator equal one. All regressions control for municipality-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. The regressions weight the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A.13: The effects of early rainfall shocks on postnatal investment
specific vaccines

	Polio 1	Polio 2	Polio 3	DPT1	DPT2	DPT3
	(1)	(2)	(3)	(4)	(5)	(6)
			Panel (a): Full gestational period			
Normal rainfall	-0.0075	-0.0059	-0.019	0.0072	-0.0069	-0.0279
	[0.0176]	[0.0232]	[0.0251]	[0.0158]	[0.0228]	[0.0262]
N	30488	30478	30478	30488	30476	30476
			Panel (b): Effects of rainfall in each trimester			
3rd trimester	0.0024	0.0001	0.0076	-0.0117	-0.005	0.0034
	[0.0087]	[0.0111]	[0.0120]	[0.0081]	[0.0109]	[0.0112]
2nd trimester	0.0069	0.0125	0.0038	0.0117	0.0126	0.0002
	[0.0097]	[0.0108]	[0.0111]	[0.0090]	[0.0118]	[0.0115]
1st trimester	-0.0182	-0.0205	-0.0323	-0.0041	-0.0164	-0.0331
	[0.0100]*	[0.0101]**	[0.0112]***	[0.0095]	[0.0110]	[0.0115]***
N	30488	30478	30478	30488	30476	30476

Notes. Demographic Health Surveys: 1986, 1990, 1995, 2000, 2004-05, and 2009-10 waves. The sample consists of children in families that have been living in the current municipality for a greater time than child's age. Children in the five main capitals of the country are excluded. Dependent variables in each column are dummies indicating whether the child has all the recommended polio or DPT vaccination dose. $Normal$ is the fraction of months during the 9 months before birth that the *normal* indicator equals one. 1st trimester is the fraction of months during the 6-8 months before birth that the *normal* rainfall indicator equal one. 2nd trimester is the fraction of months during the 3-5 months before birth that the *normal* rainfall indicator equal one. 3rd trimester is the fraction of months during the 0-2 months before birth that the *normal* rainfall indicator equal one. All regressions control for municipality fixed effects, month-of-birth $\times$ year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, mother's age at first birth, indicators for mother's education level, an indicator for marital status (married), an indicator for child's sex, and birth order. The regressions use the data at the child level. Robust standard errors (in brackets) are clustered at the grid level (255 grids). The number of municipalities in the sample is 443. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table A.14: Prenatal rainfall and migration

	Dependent variable: was child born in the current municipality?			
	Rural		Urban	
	(1)	(2)	(3)	(4)
	Panel (a): Ages 0-2 years			
Normal rainfall	0.0128		0.0204	
	[0.0158]		[0.0178]	
3rd trimester		0.0091		0.0027
		[0.0082]		[0.0098]
2nd trimester		-0.0022		0.014
		[0.0065]		[0.0098]
1st trimester		0.0059		0.0028
		[0.0069]		[0.0101]
N	586272	586272	965163	965163
	Panel (b): Ages 0-5 years			
Normal rainfall	0.0072		0.0087	
	[0.0085]		[0.0101]	
3rd trimester		0.0057		0.0033
		[0.0042]		[0.0065]
2nd trimester		-0.0010		0.0092
		[0.0044]		[0.0065]
1st trimester		0.0026		-0.0050
		[0.0049]		[0.0054]
N	1194047	1194047	1988939	1988939

Notes. Sample restricted to 2005 Census data on children under five. Individuals born in the five main capitals of the country are excluded. *Normal* rainfall is the fraction of months during the 9 months before birth that the *normal* rainfall indicator equasl one. 1st trimester is the fraction is the fraction of months during the 6-8 months before birth that the *normal* rainfall indicator equals one. 2nd trimester is the fraction of months during the 3-5 months before birth that the *normal* rainfall indicator equals one. 3rd trimester is the fraction of months during the 0-2 months before birth that the *normal* rainfall indicator equals one. The regressions are at the child level and are based on the expanded sample. All regressions control for municipality-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, municipality-specific linear time trends, average temperature in 9 months before birth, sex and race. Robust standard errors (in brackets) are clustered at the grid level (216 grids). Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.