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

Bladimir Carrillo

July 21, 2017

Abstract

This paper provides estimates of the long-term impacts of prenatal exposure to rainfall shocks using Colombian data. I find that individuals prenatally exposed to excessive precipitation have fewer years of schooling, display increased rates of illiteracy, have smaller cohort size, are less likely to work in the market, and are more likely to report serious mental and physical illness. These effects are generally larger for males, especially when considering health outcomes. This paper then uses geographical disaggregations of weather and agricultural cultivation to examine the extent to which agricultural income shocks may be driving the relationship between early rainfall conditions and later-life outcomes. The patterns I find are generally consistent with this hypothesis.

Keywords: Drought; heavy precipitation; early life health; Colombia

Resumo

Este artigo fornece estimativas dos impactos de longo prazo da exposição pré-natal a secas extremas e inundações sobre o capital humano em saúde e educação na Colômbia. Encontra-se que individuos expostos pré-natalmente a chuvas intensas têm menos anos de escolaridade, exibem taxas mais elevadas de analfabetismo, têm menor tamanho de coorte, são mais provaveis a ficar desempregadas, e são mais propensos a relatar graves doenças mentais e físicas. Esses efeitos geralmente são maiores para os homens, especialmente considerando indicadores de saúde. Em seguida, este artigo usa desagregações geográficas do clima e do cultivo agrícola para examinar até que ponto os choques da renda agrícola na infância podem ser um mecanismo de impacto relevante. Os padrões nos dados sugerem evidencia consistente com a existência de um mecanismo de renda agrícola.

Palavras chaves: Secas; Chuvas intensas; saúde na infância; Cõlombia
Classificação JEL: I15, O13, O15

Área ANPEC: Economia Social e Demografia Econômica

---

[*] Support for this research was provided by the Latin American and Caribbean Environmental Economics Program. We thank Danyelle Branco, Carlos Charris, Micheliana Costa, Wescley Freitas, Wilman Iglesias, Juan Trujillo, Raul Velilla, Ian Trotter, and participants at various conferences and seminars for helpful comments. Contact: bladimir.bermudez@ufv.br, address: DER, UFV.

1 Introduction

It is now widely recognized that emissions of greenhouse will alter global climate, causing extreme weather events, such as droughts and floods, to become more frequent. One prominent body of work highlights that climate change may have serious repercussions for children's health and human capital acquisition, 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 the effects of specific diseases (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] Yet, despite this plethora of evidence about the pervasive role of poor conditions in early life, surprisingly little attention has been devoted to the long-run consequences of extreme weather events. Estimates of the welfare consequences of early exposure to weather shocks are crucial for the most efficient design of climate change mitigation policies.

Existing research suggests several channels through which early rainfall shocks can have intergenerational consequences on human capital and welfare. It is well established that heavy precipitation increases the optimal conditions for infectious and parasitic diseases, which could adversely affect the health of pregnant mothers and fetal maturation and thus increases the risk of poor health in early life. At the same time, lower yields of subsistence crops and reduced income from cash crops due water scarcity or excess 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 in 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). Less educated individuals, in turn, have poorer labor market prospects.[2]

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 later-life 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 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 effected by the impacts of 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

---

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

[2] See Currie (2009) and Cutler and Lleras-Muney (2010).

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 × 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 examine several dimensions, including health, education and labor market outcomes, and find strong evidence that prenatal exposure to rainfall shocks leads to poorer adult outcomes. I find that individuals prenatally exposed to excessive precipitation have fewer years of schooling, display increased rates of illiteracy, have smaller cohort size, are less likely to work in the market, and are more likely to report serious mental and physical illness. When I examine the impacts separately for males and females, I find striking differences, especially when considering health outcomes. For instance, the treatment effect of excess rainfall on mental disability is approximately 20 times larger for males than for females. These gender heterogeneities are consistent with literature pointing out that male fetuses are more vulnerable to in utero shocks than female fetuses (Almond and Mazumder 2011; Eriksson et al. 2010; Kraemer 2000).

I then explore a set of additional heterogeneities that may provide insights on the mechanisms at play. Understanding of specific mechanisms is critical for guiding the targeting of policies. Policy implications when infectious and parasitic diseases are the relevant mechanism may be different from those when the relevant mechanism is reduced agricultural income. In this paper, I focus on the role of agricultural income in driving the associations between early rainfall conditions and later-life income. If the agricultural income is in fact an important mechanism, one would expect to see larger effects among cohorts born in areas with a high fraction of population depending on farming and related agricultural activities for their livelihoods. I find evidence generally consistent with the agricultural income hypothesis. I show, for example, that the treatment effects of excessive rainfall on years of schooling is about 20 times larger for cohorts born in areas having a high share of population that is rural compared to their counterpart born in low rural population rate areas. Moreover, the effects tend to be smaller among more recent cohorts, which were more likely to live in urban areas and experience technological advances in agriculture that shield from weather events. While tentative, this evidence is only suggestive of the agricultural income hypothesis, since I cannot completely rule out alternative interpretations.

I am aware of only two papers, one by Maccini and Yang (2009) and one by Dinkelman (2017), investigating the long-term consequences of rainfall shocks on adult wellbeing. Using data from Indonesia, the former finds evidence that exposure to early droughts is associated with poorer self-reported health and less grades of schooling. Remarkably, the authors show that the effects were largely driven by females. The latter study shows that early drought exposure raises later-life disability rates in South Africa, with the effects concentrated in physical and mental disabilities.

Aside from distinguishing between heterogeneous effects across different geographical disaggregations of weather and agricultural cultivation, this study contributes to the existing literature in two ways. First, I investigate the long-term effects of rainfall shocks in a context where both positive and negative precipitation shocks are likely to have adverse consequences. Previous studies have focused exclusively on the long-run consequences of droughts, but it is unclear whether one should expect the estimated effects to be the same across different contexts. More rainfall may increase agricultural productivity and household income in areas with water scarcity, improving nutrition and health. In other contexts, however, heavy rainfall may reduce agricultural productivity and increase the incidence of infectious diseases, contributing to a poorer fetal environment. In fact, when I measure early rainfall shocks in the same way Maccini and Yang (2009) do, which assumes that more heavy precipitation are unequivocally beneficial events, I find weaker and generally not statistically significant effects.

Second, I focus on a country with no known son preferences, differently from Maccini and Yang (2009) and Dinkelman (2017). This distinction is particularly important to understand the gender heterogeneities of the effects of early rainfall exposure. The evidence on gender differences in the effect of early rainfall on later-life outcomes has been mixed. While Maccini and Yang (2009) find that the effects are the largest for females, 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. An investigation of the gender differences in the impacts of early rainfall shocks in country with no gender bias would allow to highlight the importance of other mechanisms in driving the gender gap.

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. Finally, Section 5 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 × 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. Following Rocha and Soares (2015), I construct a municipality-by-month of weather panel. I begin by computing the centroid for each of the municipalities and then located the four closest nodes to build 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.

Using this consolidated dataset, I define a negative (positive) rainfall shock for a given month if rainfall was one standard deviation below (above) historical average for that calendar month within municipality. Since I am not comparing municipalities, the "extreme" rainfall shock should not be taken in an absolute sense. These are simply extreme rainfall shock months for each municipality within the given period. The historical average rainfall is calculated for each municipality and calendar month over the 1900-2010 period. Since early years did not have weather stations, Matsuura and Willmott (2012) impute the data for missing years using a meteorological model. Thus, a natural concern is bias from measurement error. However, I find very similar results when considering the

1942-2010 period to construct both the historical mean and standard deviation.

I then measure prenatal exposure to droughts and floods according to the frequency with which a given shock occurs in the 12 months prior to the individual's birth. For example, if an individual was born on December, then prenatal exposure to extreme floods is computed as the share of months exposed to any positive rainfall shocks between January and December. The same logic is used for prenatal drought exposure. The use of 12 months prior to birth is important given the evidence suggesting that women's health status immediately before conception affect infant health (Rocha and Soares, 2015; Kudamatsu et al., 2012). I also present results that consider only the 9 months prior to birth.

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. This information allows me to match individuals with drought and wet shocks of the municipality where they were born to identify prevailing rainfall conditions in early-life. I focus on adults aged 25-65 at the time we see them 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 or speech, mental or physical disability. These disability measures have been widely used in the literature linking early life shocks to later outcomes (Almond, 2006; Almond and Mazumder, 2011; Lin and Liu, 2014). In addition to disability outcomes, I consider also log cohort size. Previous studies have shown that cohort size is a reasonable proxy for survival, so it can be used as additional measure of health human capital. Unfortunately, the 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 18,843,493 individuals. Since the analysis exploits the municipality-by-month-by-year variation in rainfall shocks, I collapse the data into municipality-of-birth × month-of-birth × 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. For the cohort size analysis, each cell contains the total number of individuals born in a given place and date. 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.

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 0.07.

The most common disability in the data is related to vision. The fraction of individuals suffering from this condition is 3 percent. In contrast, the prevalence of mental disability is relatively lower, with 0.8 percent of individuals reporting 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 7.91. About 8 percent of individuals declared that do not know how to read or write, and 56 percent of people have a job.

2.3 Agricultural data

Historical data on cultivation patterns at the municipality level are unavailable from official statistics registries in Colombia. Hence, I rely on interpolated data from the Hystory Database of the Global Environment (HYDE). This database provides worldwide decennial estimates of land used for crops on a 5' latitude/longitude grid resolution. I use a geo-spatial spatial sotware to aggregate the data to the municipality level. In particular, the HYDE provides data on area under crops for calendar years in which the majority of the crop is harvested. I focus on the amount of cultivated land as well as the amount of cultivated land by rain-fed crops (both measured per capita). I then classify municipalities into "low" and "high" categories based on the agricultural cultivation patterns observed in 1940. Specifically, I will refer to low and high categories as municipalities in the first and third tertiles of distribution.[3]

I also classify municipalities into low and high categories based on the fraction of population residing in rural areas. During the study period, a considerable fraction of population was residing in rural areas, so there is relatively little variability across municipalities. Indeed, the median of the proportion of rural population in 1940 is 100 percent. So, I use data from the 1993 Census, the earliest full population census available for which there is sufficient variation in rural population rates. If areas with a relatively high fraction of population depending on farming and related agricultural activities for their livelihoods in the past continue to be the same areas with a relatively high fraction of population depending on agriculture today, then the use of data recent on rural population rate will be informative. The evidence suggests that this may be reasonable. Conditional on having a rural population rate lower than 100 percent in 1940, the correlation between 1940 and 1993 rural population rates is higher than 0.50.

2.4 Variation in rainfall shocks and outcomes

Because the statistical approach relies on within-municipality variation, I confirm that there is in fact substantial within-municipality variability in the data for identification. Figure 1 shows the spatial distribution of the incidence of rainfall shocks over time. Panels (a) and (b) plots the frequency with which extreme drought and flood shocks occur over time and space, respectively. The figure reveals that the incidence of droughts and floods varies sharply across municipalities within a given month. Episodes of rainfall shocks occur, on average, in 10 percent of the Colombian municipalities. Yet, there are periods

---

[3] Although the HYDE provides geographically detailed historical data on cultivation patterns, they are interpolated and thus likely subject to considerable measurement error. As a robustness check, I use data on cultivation practices which are readily available for 1993 from the Colombian Bureau of Statistics (DANE). I find results that are in line with those derived from the HYDE data.

with pervasive rainfall shocks hitting almost 80 percent of the municipalities as well as periods with no municipality experiencing such a shock.

To evaluate the within-municipality variability in the data more formally, I regress the measures of early rainfall shocks for a cell on a full set of municipality fixed effects and month-by-year fixed effects. The residual variation of these regressions is a direct measure of within-municipality variability. An R-squared close to 0 is counted as evidence of substantial within-municipality variation. I find that about 75 percent of the total variation in drought and flood shocks cannot be explained by this set of fixed effects. When I account for specific-municipality linear time trends in addition, I find still substantial within-municipality variation, with 60 percent of the variation due to within-municipality differences.

I also compute the within-municipality variation for adult outcomes. Municipality and time fixed effects cannot explain 70 percent of the variation in years in schooling, and this hardly changes when specific-municipality time trends are accounted for. I find also that a substantial portion of the total variation of employment status is due to within-municipality differences, about 80 percent. Conditional on specific-municipality-of-birth linear time trends, and fixed effects for municipality-of-birth and month-of-birth × year-of-birth, the variation in disability outcomes ranges from 88 to 95 percent. The within-municipality variation for log cohort size is notably lower, but it is still relevant for identification. Indeed, 12 percent of the variation in log cohort size is within-municipality.

3 Empirical Strategy

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

$$\begin{aligned} Outcome_{jmt} = {} & \alpha + \beta_1 Flood_{jmt} + \beta_2 Drought_{jmt} + \gamma Z_{jmt} + \theta Trend_{tm} \times M_j \\ & + \eta_j + \mu_{mt} + \xi_{jmt} \end{aligned} \tag{1}$$

for cohorts born in municipality j, month m and year t. *Outcome* is the dependent variable of interest, either an education or health outcome. *Drought* and *Flood* are the fraction of negative and positive rainfall shocks during the 12 months prior to birth, respectively. The covariates Z include a set of predetermined individual characteristics, such as sex and race. In all specifications, I control for specific-municipality 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 specific-area risks of diseases. The set of month-of-birth × year-of-birth fixed effects controls for common time trends such as seasonal fluctuation in later outcomes, macroeconomic conditions and common national policies. All our models use robust standard errors adjusted for clustering at the municipality level to account for serial correlation (Bertrand et al., 2004).

The coefficients β_1 and β_2 measure the effects of early-life exposure to rainfall shocks 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.

While the empirical design is compelling in principle, I still 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. To address this issue, I explore the robustness of the results when considering rainfall shocks occurring in the 9 months before month, when fertility decisions are likely made. As shown below, the results from these models are in line with the main findings, supporting the validity of the research design.

Second, a bias may arise if different types of women are likely to migrate away from areas affected by extreme droughts or floods while pregnancy. It seems implausible that this is the case given I focus on temporary variations in rainfall and by the low migration rates of pregnant women. Fortunately, the Census collects information on the municipality where an individual lived five years earlier. I then compares rainfall shocks between migrant and non-migrant families during the five years prior to census. Consistent with the view that migration is unlikely to be related temporary variations in rainfall, I find no statistically significant differences in rainfall shocks during the five years prior to census between migrants and non-migrants. Although I cannot entirely rule out the possibility of migration during pregnancy, these data suggest that it is unlikely to be a major source of bias.

Third, as the sample is based on surviving (and presumably higher quality) individuals, a potential issue is selectively 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 or early infancy 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 of the effects early-life rainfall shocks towards zero. If so, my estimates should be taken to be lower bound of the effect of the true effect and large impacts would even become more telling. Therefore, I am less concerned about bias from selective mortality.

4 Results

4.1 Main findings

Table 2 reports the estimates of the effect of rainfall shocks on adult outcomes. All regression results are based on the full specification that adjusts for municipality-of-birth fixed effects, month-of-birth × year-of-birth fixed effects, specific-municipality time trends and the set of predetermined individual characteristics. Sample sizes and R-squared's of the regressions are shown at the bottom of the table.

Column (1) shows evidence that greater prenatal exposure to extreme floods reduces cohort size. For this outcome, the estimate of β_1 is -0.0499 (standard error = 0.0177), which is statistically different from zero at the 1 percent level of significance. This estimate implies that an increase of one standard deviation in the fraction of early extreme floods leads to a 0.67 percent decrease in cohort size. With cohort size interpreted as cumulative survival, this result indicates that exposure to excess rainfall in utero is associated with decreased mortality.

Columns (2)-(3) look at an indicator for any serious disability and the number of disabilities, respectively. There is no evidence that early-life rainfall shocks are associated with changes in the prevalence of disabilities, with estimates of the parameter of interest statistically indistinguishable from zero at the conventional levels of significance. However, these aggregate measures of disabilities may mask important form of heterogeneities across disability types. With this in mind, columns (4)-(7) explores the effects of early rainfall shocks on disability types. I find a statistically significant effect of excessive rainfall on mental disability, with an estimated coefficient of interest of 0.0030 and standard error of 0.0011. Thus, an increase of one standard deviation in the fraction of early excess rainfall implies an increase of 0.047 percentage points in the probability of adult mental disability. Relative to the mean mental disability rate of 0.79, this implies an increase of 5.1 percent in the incidence of this disability. I find also a statistically significant relationship between prenatal floods and physical disability. The point estimate of the coefficient of interest 0.0047 (standard error = 0.0021) indicates that an increase of one standard deviation in the fraction of prenatal floods exposure is associated with an increase of 0.063 percentage points in the prevalence of physical disabilities. Relative to the mean, this estimate suggests a 2.6-percent increase. I do not find any evidence that early rainfall shocks affect other disability types.

To better place the 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.[4] In our sample, an increase of one standard deviation in years of education is associated with a increase of 0.48 in the probability of reporting a serious mental disability.[5] Relative to this difference, the estimated effect of prenatal flood on this outcome is about 10 percent. The same comparison for physical disability reveals a similar relative difference.

Column (8) investigates the relationship between early rainfall shocks and years of schooling. I find evidence that greater exposure to excessive rainfall in early-life leads to fewer years of schooling, with β_1 estimate at -0.1183 (standard error = 0.0616). For comparison, Duflo (2001) finds that a large school construction program leads to an increase of 0.15 years of education in Indonesia. Column (9) presents the results for illiteracy and suggests that a significantly significant effect of early extreme wets. The rate of illiteracy among individuals experiencing a 50 percent prenatal exposure to excessive rainfall is increased by 0.38 percentage points relative to those who were not exposed to any positive rainfall shock in utero. This is about 4.7 percent at the mean illiteracy rate. Finally, column (10) examines early rainfall impacts on employment. I find statistically significant effects of both extreme droughts and floods. Indeed, exposure to positive or negative rainfall shocks results in lower employment rate. The effect of excessive rainfall is larger in magnitude than that of extreme droughts. The estimated coefficients

---

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

[5] This estimate is obtained by regressing mental disability on years of schooling, and controls for age, sex, and race.

indicate that individuals spent 50 percent of their prenatal period experiencing positive and negative rainfall shocks are 1.08 and 0.7 percentage points less likely to work in adulthood, respectively.

4.2 Heterogeneity in treatment effects

4.2.1 Gender heterogeneities

I now investigate the gender specificity of the main results. In Table 3, I show the results from running regressions separately for males and females. The results for the log cohort regressions indicate larger impacts of excess rainfall for males than for females. An increase of one standard deviation in the extreme flood exposure in the 12 months prior to birth results in a 0.87 percent smaller cohort for males. For females, the corresponding treatment effect is about 0.48 percent smaller cohort size, although it is indistinguishable from zero. The results for disabilities also suggest larger impacts for males. In contrast to the baseline estimates, I now observe statistically significant impacts of prenatal floods on the probability of reporting any serious disability and the number of disabilities. The results indicate that males who spent 50 percent of the prenatal period in floods are 0.40 percentage points more likely to report any serious disability and have on average 0.006 more disabilities, which represent increases of 6 and 8 percent relative to the baseline mean, respectively. These effects are largely drive by mental and physical disabilities. The differences between males and females in point estimates are generally statistically significant and strikingly large. In particular, I find that the estimated effect of prenatal flood on the number of disabilities among males is 135 times as large as among females.

There are also striking differences in the the effect of prenatal flood on years of schooling between males and females. The corresponding effect of exposure to extreme negative rainfall shocks is about 3 times larger for males. Now, one standard deviation increase in the early excess rainfall exposure results in 0.024 fewer years of schooling for males. In contrast, in the case of illiteracy, I find that the effect of prenatal exposure to excessive rainfall is somewhat larger for females, with point estimates of 0.0053 (standard error =0.0039) for males and of 0.009 (standard error=0.0040) for females. However, I cannot reject the null hypothesis that both estimates are the same. Finally, I find that the estimates of rainfall shock effects on employment are larger in magnitude for females than for males. The results suggest that females who spent 50 percent of the prenatal period in floods and droughts are 1.3 and 1.09 less likely to working, respectively. For males, the coefficients are imprecisely estimated and are therefore not able to reject the hypothesis of treatment effects equal to zero.

Overall, the results suggest strong gender heterogeneities. Taken in their entirety, the results tend to show larger treatment effects for males than for females when considering health and education outcomes. The patterns I find here 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 attributes gender differences to differences in ability to produce nutrients in the placenta. This is supported by studies documenting gender-specific effects of different shocks during pregnancy (Ross and Desai, 2005).

It is interesting that effects on employment are larger for females. Since employment is an outcome largely determined by employers, and thus supplier control over it is relatively limited, this finding may be taken as supporting evidence for the hypothesis of gender discrimination in the labor market. This is consistent with a large literature in developing countries showing sharp gender gaps in labor market outcomes for a women and men with similar observable characteristics.[6] In our data, women are about 40 percentage points more likely to be engaged in the labor market than men are, even conditional on education, age and race. While my findings are suggestive of sex discrimination in the labor market, further research is required to clarify these relationships.

4.2.2 Agro-climatic heterogeneities

Next, I assess the extent to which agricultural income shocks may drive the relationship between prenatal rainfall and later-life outcomes. I first explore whether adult outcomes are more responsive to rainfall shocks in dry and wet areas. Reductions in precipitation may lead to larger reductions in agricultural production and higher increases in food prices in areas facing extremely dry conditions and constant moisture deficit than in areas with higher baseline rainfall. In contrast, the negative effects of heavy rainfall periods on agriculture are likely to be more salient in areas with high long-term mean rainfall and relying on crops which are more sensitive to excessive precipitation. If the effects of prenatal rainfall shocks on later-life outcomes operate through an agricultural income channel, one would expect to see larger effects among cohorts born in areas with low and high long-term mean rainfall relative to those born in areas with less extreme rainfall conditions.

To investigate this formally, I disaggregate the sample into municipalities characterized by long-term rainfall that is in the first and third tertile of distribution. I refer to these as the "dry" and "wet" samples. The results of this exercise are presented in Table 4. Compared to the baseline estimates in Table 2, I find that the effects of excess rainfall in utero are larger in magnitude in the wet sample. A striking example is years of schooling. While complete exposure to excessive rainfall in utero leads to 0.30 fewer years of schooling in the wet sample, it has an effect that is about 3 times smaller in the entire sample. In contrast, I find generally statistically insignificant effects of rainfall shocks in the dry sample, suggesting that excessive rainfall is the main driver of the relationship between early rainfall shocks and later-life outcomes.

Table 5 examines the long-term effects of rainfall shocks in areas with low and high amount of cultivated land (per inhabitants). Again, if agricultural income is an important mechanism behind my findings, one would expect to see larger effects among cohorts born in areas depending directly on farming for a living. This prediction is supported by the regression estimates. In general, I observe larger estimates in magnitude among cohorts born in areas with high amount of cultivated land per capita. For example, the effect of excess rainfall in utero on the number of adult disabilities among individuals born in high cultivated areas is 176 times as large as among those born in low cultivated areas.

---

[6] For a general review of literature, see Weichselbaumer and Winter-Ebmer (2005). Prominent studies on the gender gap in Colombia include Angel-Urdinola and Wodon (2006), Hoyos et al. (2010), and Galvis (2010).

Table 6 explores heterogeneities among cohorts born in areas with low and high amount of cultivated land by rain-fed crops (per inhabitants). I continue to find results consistent with the agricultural income hypothesis: the effects are the largest among cohorts born in areas depending on rain-fed crops. Striking large differences in the treatment effects are observed for cohort size and disabilities outcomes. When I consider years of schooling, the coefficient on floods is imprecisely estimated, possibly because of the reduced sample size, but it is negative and slightly larger in high rain-fed crop areas relative to the baseline estimate in Table 2.

I now compare the treatment effects in areas with high and low share of population that is rural. The results in Table 7 show that the effects of floods on later-life outcomes are larger in areas with higher rural population rate. In particular, I find substantial differences when I examine years of schooling. The estimated coefficient on flood for this outcome is -0.44 (standard error=0.11) for individuals born in high rural population rate areas and -0.022 (standard error=0.085) for those born in low rural population rate areas. I also find larger effects of droughts in areas with high fraction of rural population, although the differences are statistically significant in some few cases.

Between 1940 and 1980, Colombia experienced a substantial transformation 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. Indeed, more individuals living in urban areas and employed in non-farm activities implies fewer families directly depending on agriculture for a living. These changes would imply smaller effects of rainfall shocks among more recent cohorts. To evaluate this, I run the regressions separately for cohorts born before and after 1960. A natural 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 patterns. But since I focus on individuals aged 25-65, which are likely to have completed their schooling, the "age" channel should be a minor issue for education outcomes. Although the coefficients are estimated very imprecisely likely due to reduced sample sizes, I find a consistent pattern showing larger effects in magnitude for more recent cohorts (Table 8). The estimated coefficient on flood for years of schooling is about 10 times larger for individuals born before 1960 than for those born after 1960. In the same vein, complete exposure to floods in utero reduces the probability of being illiterate by 0.9 percentage points among individuals born before 1960, while it implies a reduction of 0.46 percentage points among individuals born after 1960.

Taken in their entirety, these results are consistent with the existence of an agricultural income mechanism. Of course, this evidence is only suggestive since I cannot completely rule out other alternative interpretations. In particular, areas with different agricultural and weather patterns are likely to differ in dimensions that may contribute to the treatment effects of rainfall shocks. For example, rural areas are likely to have less access to health care and higher long-term poverty, so families residing in such areas may be less able to remediate the adverse effects of poor neonatal health. In addition, I do not find significant heterogeneities in some few cases, which suggests that other mechanisms may be also important. In any case, my findings can be taken as initial evidence on the role of agricultural in the relationship between prenatal rainfall shocks and later-life outcomes.

4.3 Robustness checks

I conduct a number of other specification checks to test the robustness of the main results (Not shown here due to space limitations). As mentioned above, a natural concern with the research designed is that different parents may change fertility decisions if they are exposed to a severe rainfall shock around conception time. Since I have information on the month of birth, I can explore this possibility by examining the robustness of the results when considering exposure measures based on rainfall shocks in the 9 months prior to birth. The use of this type of exposure measures should largely diminish the potential bias induced by parents changing fertility decisions around conception. The results of this exercise and suggests evidence highly consistent with the baseline findings. Indeed, point estimates are very similar to the ones that consider exposure measures based on rainfall shocks in the 12 months prior to birth. As an additional check, I estimate models that include exposure to rainfall shocks in 12 and 13-24 months before birth. Interestingly, the estimated coefficients of interest are virtually identical to the ones in the benchmark specification, while the effects of exposure to droughts and floods 13-24 months before birth are statistically indistinguishable from zero. This finding suggests that changes in fertility around conception time is unlikely to be a major issue.

I also consider alternative measures of rainfall shocks. First, I define early rainfall shocks by the deviation of rainfall 12 months before birth from the average historical yearly rainfall in each municipality. More specifically, the variable is the natural log of prenatal rainfall minus the natural log of mean annual rainfall in the given municipality. This is the measure used by Maccini and Yang (2009). When I follow this specification, I find estimates of the effect of prenatal rainfall shocks that are statistically significant only in some few cases. Indeed, I find that higher rainfall relative to the normal local rainfall is associated with fewer years of schooling and increased rates of illiteracy, but there is no evidence of significant effects for the rest of outcomes. These weaker estimates is perhaps unsurprising given the non-linear relationship between early rainfall and adult outcomes I document above.

Second, I define 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 early drought and excess rainfall are computed using these definitions of extreme drought and wet months. The results are in general consistent with the baseline, although are imprecisely estimated in some cases.

Third, I define 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 measures. Using exposure measures based on the SPI leads to results that are very similar to the baseline findings.

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 × year-of-birth fixed effects in all regressions, one could be even concerned if there is specific-regional seasonal variation in adult outcomes spuriously correlated with variation in rainfall shocks. I examine this issue by estimating models that control for a full set of municipality-of-birth × 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.

5 Conclusion

The health and other consequences of extreme weather events are an increasing 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. Yet, despite its importance in the public debate, there is even little research to date documenting the long-run effects of rainfall shocks on human capital and welfare. This paper uses Colombian data to gain new insights into the effects of early floods and droughts on later-life welfare. I show that prenatal exposure to floods result in fewer years of schooling, increased rates of illiteracy, smaller cohort size, higher unemployment rate, and increased rates of disabilities. Remarkably, these effects are generally larger for males, especially when considering health outcomes.

To test the extent to which agricultural income shocks may be an important driver behind the relationship between early rainfall shocks and later-life outcomes, I explore heterogeneities in treatment effects across an array of geographical disaggregations. I find substantially larger effects of prenatal floods among cohorts born in areas having a high fraction of rural population and high amount of cultivated land by rain-fed crops. Furthermore, the effects are smaller among more recent cohorts, when a higher fraction of population were living in urban areas and did not depend on farm related activities. Together, these results provide evidence for the operation of an agricultural channel. Naturally, other mechanisms, such as shocks in the prevalence of diseases, may be also important. Future studies, perhaps combining exogenous changes in the agricultural practices and the incidence of specific diseases, would allow to draw more definitive conclusions about the relative roles of both channels of impacts.

References

Anna Aizer, Laura Stroud, and Stephen Buka. Maternal Stress and Child Outcomes: Evidence from Siblings. *Journal of Human Resources*, 51:523–555, 2016.

Douglas Almond. Is the 1918 Influenza Pandemic Over? Long-Term Effects of In Utero Influenza Exposure in the Post 1940 U.S. Population. *Journal of Political Economy*, 114(4):672–712, 2006.

Douglas Almond and Janet Currie. Killing Me Softly: The Fetal Origins Hypothesis. *Journal of Economic Perspectives*, 25(3):153–172, 2011.

Douglas Almond and Bhashkar Mazumder. Health Capital and the Prenatal Environment: The Effect of Ramadan Observance during Pregnancy. *American Economic Journal: Applied Economics*, 3(4):56–85, 2011.

Mabel Andalon, Joao Pedro Azevedo, Carlos Rodriguez-Castelan, Viviane Sanfelice, and Daniel Valderrama-Gonzalez. Weather Shocks and Health at Birth in Colombia. *World Development*, 82:69–82, 2016.

Diego F. Angel-Urdinola and Quentin Wodon. The gender wage gap and poverty in colombia. *Labour*, 20(4):721–739, 2006.

Sarah Baird, Joan Hamory Hicks, Michael Kremer, and Edward Miguel. Worms at Work: Long-run Impacts of a Child Health Investment. *The Quarterly Journal of Economics*, 131(4):1637–1680, 2016.

D J P Barker. The fetal origins of coronary heart disease. *Acta Paediatrica*, 86(S422): 78–82, 1997.

Alan I Barreca. The Long-Term Economic Impact of In Utero and Postnatal Exposure to Malaria. *Journal of Human Resources*, 45(4):865–892, 2010.

Gary S Becker. Health as human capital: synthesis and extensions. *Oxford Economic Papers*, 59(3):379–410, 2007.

Marianne Bertrand, Esther Duflo, and Sendhil Mullainathan. How Much Should We Trust Differences-In-Differences Estimates? *The Quarterly Journal of Economics*, 119 (1):249–275, 2004.

Kasey S Buckles and Daniel M Hungerman. Season of Birth and Later Outcomes: Old Questions, New Answers. *The Review of Economics and Statistics*, 95(3):711–724, 2013.

Janet Currie. Healthy, Wealthy, and Wise: Socioeconomic Status, Poor Health in Childhood, and Human Capital Development. *Journal of Economic Literature*, 47(1):87–122, 2009.

David M Cutler and Adriana Lleras-Muney. Understanding differences in health behaviors by education. *Journal of Health Economics*, 29(1):1–28, 2010.

Taryn Dinkelman. Long-run health repercussions of drought shocks: Evidence from south african homelands. *The Economic Journal*, 2017. doi: 10.1111/ecoj.12361.

Stephen G Donald and Kevin Lang. Inference with Difference-in-Differences and Other Panel Data. *The Review of Economics and Statistics*, 89(2):221–233, 2007.

Esther Duflo. Schooling and labor market consequences of school construction in indonesia: Evidence from an unusual policy experiment. *American Economic Review*, 91(4): 795–813, September 2001.

Luis Galvis. Wage Gap by Gender and Region in Colombia: A Quantile Regression Approach. *Revista de Economia del Rosario*, 13(2):235–277, 2010.

Germanwatch. Global Climate Risk Index 2012: Who suffers most from extreme weather events? Weather-related loss events in 20010 and 1991-2010. Technical report, Bonn, Germany, 2011.

Alejandro Hoyos, Hugo Ñopo, and Ximena Peña. The Persistent Gender Earnings Gap in Colombia, 1994-2006. 2010.

Masayuki Kudamatsu, Torsten Persson, and David Strömberg. Weather and Infant Mortality in Africa. 2012.

Ming-Jen Lin and Elaine M Liu. Does in utero exposure to Illness matter? The 1918 influenza epidemic in Taiwan as a natural experiment. *Journal of Health Economics*, 37:152–163, 2014.

Maarten Lindeboom, France Portrait, and Gerard J van den Berg. Long-run effects on longevity of a nutritional shock early in life: The Dutch Potato famine of 18461847. *Journal of Health Economics*, 29(5):617–629, sep 2010.

Sharon Maccini and Dean Yang. Under the Weather: Health, Schooling, and Economic Consequences of Early-Life Rainfall. *American Economic Review*, 99(3):1006–1026, 2009.

K Matsuura and C. Willmott. Terrestrial Air Temperature and Precipitation: 1900-2010 Gridded Monthly Time Series, Version 3.01. Technical report, University of Delaware, 2012.

Edward Miguel and Michael Kremer. Worms: Identifying impacts on education and health in the presence of treatment externalities. *Econometrica*, 72(1):159–217, 2004.

Rudi Rocha and Rodrigo R Soares. Water scarcity and birth outcomes in the Brazilian semiarid. *Journal of Development Economics*, 112(0):72–91, 2015.

Michael G Ross and Mina Desai. Gestational programming: population survival effects of drought and famine during pregnancy. *American Journal of Physiology - Regulatory, Integrative and Comparative Physiology*, 288(1):R25–R33, 2005.

Steven Ruggles and Matthew Sobek. Integrated Public Use Microdata Series: Version 2.0. Minneapolis, Minn: Historical Census Projects. 1997. URL `http://www.ipums.umn.edu`.

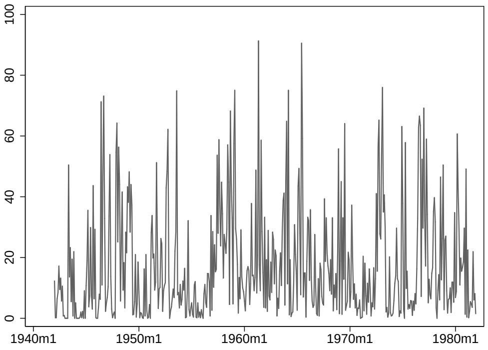
J R Seckl. Physiologic programming of the fetus. *Clinics in perinatology*, 25(4):939–62, vii, 1998.

Matthew Sobek, Steven Ruggles, Robert McCaa, Miriam King, and Deborah Levison. Integrated Public Use Microdata Series-International: Preliminary Version 1.0. 2012. URL `http://www.ipums.org/international`.

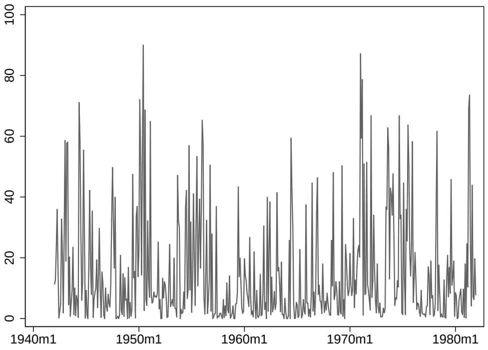
Atheendar S Venkataramani. Early life exposure to malaria and cognition in adulthood: Evidence from Mexico. *Journal of Health Economics*, 31(5):767–780, 2012.

Doris Weichselbaumer and Rudolf Winter-Ebmer. A meta-analysis of the international gender wage gap. *Journal of Economic Surveys*, 19(3):479–511, 2005.

Figure 1: Rainfall shocks across time and place



(a) Droughts



(b) Floods

Notes. Panels (a) and (b) present the percentage of municipalities with droughts and floods in each month, respectively. Author's calculation based on data from the Terrestrial Air Temperature and Terrestrial Precipitation: 1900-2010 Gridded Monthly Time Series, Version 3.02.

Table 1: The effects of early rainfall shocks on later-life outcomes

	Mean	Standard deviation	Min	Max
ln cohort size	5.4839	1.7559	0	9.1683
Any diability	0.0619	0.1231	0	1
Number of disabilities	0.0759	0.1668	0	4
Mental disability	0.0079	0.0402	0	1
Physical disability	0.0244	0.0771	0	1
Vision disability	0.0332	0.0915	0	1
Hearing/speech disability	0.0121	0.0534	0	1
Years of schooling	7.9115	3.0374	0	17
Illiteracy	0.0846	0.1833	0	1
Employment	0.5632	0.2617	0	1
Floods	0.1421	0.1357	0	1
Droughts	0.1703	0.1478	0	1

Notes. Floods and Droughts represent the fraction of months during the 12 months before birth that the *flood* and *drought* indicators equal one, respectively. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. Sample restricted to 2005 Census data on individuals born between 1942 and 1981.

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

	Ln cohort size	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)	(10)
Floods	-0.0499	0.0037	0.0064	0.003	0.0047	-0.0008	-0.0006	-0.1183	0.0077	-0.0216
	[0.0177]***	[0.0032]	[0.0046]	[0.0011]***	[0.0021]**	[0.0025]	[0.0014]	[0.0616]*	[0.0030]**	[0.0076]***
Droughts	0.003	0.0000	0.0003	0.0003	0.0011	-0.002	0.0008	-0.1032	0.0021	-0.0149
	[0.0137]	[0.0034]	[0.0045]	[0.0011]	[0.0018]	[0.0024]	[0.0018]	[0.0745]	[0.0032]	[0.0057]***
N	236062	236062	236062	236007	236007	236007	236007	235426	236062	236062
R-sq	0.9	0.106	0.093	0.016	0.054	0.085	0.031	0.53	0.297	0.311

Notes. Floods and Droughts represent the fraction of months during the 12 months before birth that the *flood* and *drought* indicators equal one, respectively. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. 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 12 months before birth, sex and race. The regressions weights the observations by the cell size to adjust for precision with which the cell means are estimated. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. Robust standard errors clustered at the municipality level are presented in brackets. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 3: The effects of early rainfall shocks on later-life outcomes by gender

	Ln cohort size (1)	Any disability (2)	Number of disabilities (3)	Mental disability (4)	Physical disability (5)	Vision disability (6)	Hearing/speech disability (7)	Years of schooling (8)	Illiteracy (9)	Employment (10)
					Panel (a): Males					
Floods	-0.0667	0.0081	0.0135	0.0059	0.0055	0.0007	0.0014	-0.1893	0.0053	-0.0167
	[0.0215]***	[0.0041]**	[0.0065]**	[0.0016]***	[0.0033]*	[0.0032]	[0.0022]	[0.0795]**	[0.0039]	[0.0118]
Droughts	0.0134	-0.002	0.0004	0.0011	0.0006	-0.0029	0.0015	-0.1398	0.0039	-0.0079
	[0.0199]	[0.0041]	[0.0056]	[0.0018]	[0.0025]	[0.0030]	[0.0023]	[0.0997]	[0.0039]	[0.0085]
N	208346	208346	208346	208269	208269	208269	208269	207157	208346	208346
R-sq	0.844	0.072	0.065	0.018	0.038	0.06	0.029	0.427	0.211	0.165
					Panel (b): Females					
Floods	-0.0371	-0.0003	0.0001	0.0003	0.004	-0.0018	-0.0022	-0.0515	0.0097	-0.026
	[0.0254]	[0.0045]	[0.0059]	[0.0015]	[0.0025]	[0.0033]	[0.0018]	[0.0822]	[0.0040]**	[0.0084]***
Droughts	-0.0236	0.0014	-0.0001	-0.0005	0.0014	-0.0014	0.0003	-0.0774	0.0001	-0.0218
	[0.0189]	[0.0045]	[0.0060]	[0.0013]	[0.0026]	[0.0034]	[0.0022]	[0.0787]	[0.0047]	[0.0078]***
N	210685	210685	210685	210598	210598	210598	210598	209954	210685	210685
R-sq	0.845	0.088	0.077	0.016	0.048	0.07	0.027	0.45	0.24	0.208

Notes. Floods and Droughts represent the fraction of months during the 12 months before birth that the *flood* and *drought* indicators equal one, respectively. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. 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 12 months before birth, and race. The regressions weights the observations by the cell size to adjust for precision with which the cell means are estimated. Sample restricted to 2005 Census data on individuals born between 1942 and 1981. Robust standard errors clustered at the municipality level are presented in brackets. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 4: The effects of early rainfall shocks on later-life outcomes in wet and dry areas

	Ln cohort size (1)	Any disability (2)	Number of disabilities (3)	Mental disability (4)	Physical disability (5)	Vision disability (6)	Hearing/speech disability (7)	Years of schooling (8)	Illiteracy (9)	Employment (10)
					Panel (a): Wet areas					
Floods	-0.0236	0.0068	0.0144	0.0056	0.001	0.005	0.0028	-0.3094	0.0182	-0.0227
	[0.0310]	[0.0063]	[0.0082]*	[0.0022]**	[0.0041]	[0.0047]	[0.0028]	[0.1220]**	[0.0061]***	[0.0146]
Droughts	-0.0073	0.0094	0.0187	0.0025	-0.0004	0.0082	0.0083	-0.172	0.007	-0.0201
	[0.0270]	[0.0063]	[0.0087]**	[0.0018]	[0.0040]	[0.0041]**	[0.0030]***	[0.1167]	[0.0057]	[0.0121]*
N	78692	78692	78692	78652	78652	78652	78652	78230	78692	78692
R-sq	0.484	0.117	0.107	0.026	0.059	0.096	0.045	0.273	0.212	0.26
					Panel (b): Dry areas					
Floods	-0.0521	0.0047	0.0085	0.0025	0.0063	0.0024	-0.0027	0.0392	0.0038	-0.022
	[0.0238]**	[0.0046]	[0.0066]	[0.0016]	[0.0030]**	[0.0033]	[0.0020]	[0.0763]	[0.0042]	[0.0103]**
Droughts	0.0117	-0.0027	-0.0031	0.0011	0.0014	-0.0039	-0.0016	-0.1336	0.0031	-0.0191
	[0.0189]	[0.0045]	[0.0060]	[0.0019]	[0.0027]	[0.0034]	[0.0029]	[0.1035]	[0.0050]	[0.0082]**
N	78484	78484	78484	78478	78478	78478	78478	78409	78484	78484
R-sq	0.941	0.105	0.09	0.024	0.054	0.088	0.033	0.587	0.4	0.377

Notes. "Wet" areas refers to the sub-sample of municipalities with a historical mean rainfall above the 66th percentile of the distribution. "Dry" areas refers to the sub-sample of municipalities with a historical mean rainfall below the 33th percentile of the distribution. Floods and Droughts represent the fraction of months during the 12 months before birth that the *flood* and *drought* indicators equal one, respectively. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. 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 12 months before birth, sex and race. The regressions weights the observations by the cell size to adjust for precision with which the cell means are estimated. The sub-samples restricted to 2005 Census data on individuals born between 1942 and 1981. Robust standard errors clustered at the municipality level are presented in brackets. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 5: The effects of early rainfall shocks on later-life outcomes by cultivated land

	Ln cohort size	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)	(10)
Panel (a): High per capita cultivated land										
Floods	-0.1105	0.0137	0.0176	0.0031	0.009	0.0042	0.0012	-0.1258	0.0093	-0.0228
	[0.0360]***	[0.0066]**	[0.0099]*	[0.0025]	[0.0037]**	[0.0060]	[0.0031]	[0.1217]	[0.0064]	[0.0114]**
Droughts	-0.0148	0.0039	0.0066	0.0007	0.0023	0.0024	0.0011	-0.1552	0.0046	-0.0251
	[0.0322]	[0.0060]	[0.0094]	[0.0022]	[0.0035]	[0.0043]	[0.0041]	[0.1420]	[0.0060]	[0.0089]***
N	75731	75731	75731	75710	75710	75710	75710	75447	75731	75731
R-sq	0.776	0.102	0.087	0.021	0.056	0.086	0.034	0.42	0.272	0.264
Panel (b): Low per capita cultivated land										
Floods	-0.0346	-0.0014	0.0001	0.003	0.0016	-0.0044	0.0001	-0.1624	0.0058	-0.0233
	[0.0252]	[0.0047]	[0.0066]	[0.0018]*	[0.0029]	[0.0032]	[0.0022]	[0.0701]**	[0.0036]	[0.0116]**
Droughts	0.0028	-0.0033	-0.003	-0.0014	0.0003	-0.0019	-0.0001	-0.148	0.0024	-0.0184
	[0.0209]	[0.0045]	[0.0058]	[0.0019]	[0.0026]	[0.0035]	[0.0021]	[0.1034]	[0.0043]	[0.0099]*
N	80181	80181	80181	80162	80162	80162	80162	79969	80181	80181
R-sq	0.938	0.117	0.101	0.025	0.065	0.093	0.034	0.604	0.24	0.349

Notes. Low and high per capita crop land refer to municipalities with a per capita cultivated land below and above the 33 and 66th percentile of distribution of per capita cultivated land in 1940, respectively. Floods and Droughts represent the fraction of months during the 12 months before birth that the *flood* and *drought* indicators equal one, respectively. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. 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 12 months before birth, sex and race. The regressions weights the observations by the cell size to adjust for precision with which the cell means are estimated. The sub-samples restricted to 2005 Census data on individuals born between 1942 and 1981. Robust standard errors clustered at the municipality level are presented in brackets. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 6: The effects of early rainfall shocks on later-life outcomes by rainfed crop areas

	Ln cohort size	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)	(10)
Panel (a): High per capita rainfed crops										
Floods	-0.1185	0.0131	0.0181	0.0033	0.0085	0.0046	0.0017	-0.1334	0.0085	-0.0223
	[0.0356]***	[0.0066]**	[0.0100]*	[0.0026]	[0.0036]**	[0.0061]	[0.0031]	[0.1231]	[0.0065]	[0.0112]**
Droughts	-0.0197	0.0022	0.0053	0.0004	0.0011	0.0029	0.0009	-0.1686	0.0051	-0.0234
	[0.0323]	[0.0061]	[0.0095]	[0.0022]	[0.0034]	[0.0043]	[0.0042]	[0.1417]	[0.0061]	[0.0088]***
N	75779	75779	75779	75758	75758	75758	75758	75496	75779	75779
R-sq	0.778	0.104	0.088	0.021	0.057	0.088	0.034	0.42	0.27	0.262
Panel (b): Low per capita rainfed crops										
Floods	-0.0401	-0.0002	0.0009	0.0031	0.0023	-0.0038	-0.0006	-0.1683	0.0068	-0.0297
	[0.0245]	[0.0047]	[0.0066]	[0.0018]*	[0.0029]	[0.0032]	[0.0022]	[0.0692]**	[0.0036]*	[0.0110]***
Droughts	-0.0033	-0.0031	-0.0029	-0.0012	0.0007	-0.0023	-0.0002	-0.1408	0.0023	-0.0192
	[0.0207]	[0.0046]	[0.0058]	[0.0019]	[0.0026]	[0.0035]	[0.0021]	[0.1025]	[0.0043]	[0.0097]**
N	80112	80112	80112	80093	80093	80093	80093	79899	80112	80112
R-sq	0.938	0.118	0.101	0.025	0.065	0.093	0.034	0.6	0.241	0.348

Notes. Low and high per capita rainfed crops refer to municipalities with a per capita rainfed crop below and above the 33 and 66th percentile of distribution of per capita rainfed crop in 1940, respectively. Floods and Droughts represent the fraction of months during the 12 months before birth that the *flood* and *drought* indicators equal one, respectively. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. 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 12 months before birth, sex and race. The regressions weights the observations by the cell size to adjust for precision with which the cell means are estimated. The sub-samples restricted to 2005 Census data on individuals born between 1942 and 1981. Robust standard errors clustered at the municipality level are presented in brackets. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 7: The effects of early rainfall shocks on later-life outcomes by rural population rate

	Ln cohort size	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)	(10)
Panel (a): High rural population rate										
Floods	-0.1056	0.0128	0.0166	0.0037	0.0069	0.0052	0.0008	-0.4407	0.0207	-0.0609
	[0.0343]***	[0.0068]*	[0.0088]*	[0.0026]	[0.0046]	[0.0052]	[0.0029]	[0.1106]***	[0.0073]***	[0.0136]***
Droughts	-0.0397	0.0156	0.0224	0.0015	0.0055	0.0114	0.004	-0.177	0.0076	-0.0328
	[0.0293]	[0.0065]**	[0.0088]**	[0.0020]	[0.0044]	[0.0048]**	[0.0027]	[0.1140]	[0.0069]	[0.0109]***
N	78807	78807	78807	78776	78776	78776	78776	78421	78807	78807
R-sq	0.259	0.125	0.117	0.028	0.066	0.102	0.05	0.25	0.355	0.28
Panel (b): Low rural population rate										
Floods	-0.0535	0.0033	0.0056	0.0039	0.0053	-0.0018	-0.0018	-0.0221	0.005	-0.017
	[0.0243]**	[0.0046]	[0.0067]	[0.0015]**	[0.0027]*	[0.0034]	[0.0019]	[0.0850]	[0.0038]	[0.0105]
Droughts	0.0011	0	0.0004	0.0006	0.0018	-0.0034	0.0014	-0.0569	0.0004	-0.015
	[0.0184]	[0.0045]	[0.0059]	[0.0016]	[0.0025]	[0.0033]	[0.0028]	[0.1172]	[0.0042]	[0.0072]**
N	77933	77933	77933	77921	77921	77921	77921	77836	77933	77933
R-sq	0.938	0.097	0.083	0.022	0.051	0.082	0.029	0.504	0.241	0.34

Notes. Low and high rural population rate refer to municipalities with a rural popylation rate below and above the 33 and 66th percentile of distribution of rural population rate in 1993, respectively. Floods and Droughts represent the fraction of months during the 12 months before birth that the *flood* and *drought* indicators equal one, respectively. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. 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 12 months before birth, sex and race. The regressions weights the observations by the cell size to adjust for precision with which the cell means are estimated. The sub-samples restricted to 2005 Census data on individuals born between 1942 and 1981. Robust standard errors clustered at the municipality level are presented in brackets. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.

Table 8: The effects of early rainfall shocks on later-life outcomes across time

	Ln cohort size	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)	(10)
Panel (a): Cohorts born during 1942-1960										
Floods	-0.0583	0.0017	0.007	0.0048	0.008	-0.0078	0.002	-0.1614	0.0095	-0.0205
	[0.0332]*	[0.0086]	[0.0121]	[0.0029]*	[0.0056]	[0.0063]	[0.0037]	[0.1138]	[0.0079]	[0.0139]
Droughts	0.0152	0.0091	0.01	0.0022	0.0035	0.0024	0.0018	-0.1481	0.0041	-0.0097
	[0.0277]	[0.0069]	[0.0092]	[0.0018]	[0.0042]	[0.0051]	[0.0029]	[0.1191]	[0.0066]	[0.0095]
N	100198	100198	100198	100170	100170	100170	100170	99825	100198	100198
R-sq	0.82	0.072	0.07	0.025	0.048	0.054	0.04	0.402	0.264	0.291
Panel (b): Cohorts born during 1961-1981										
Floods	-0.0468	0.0023	0.0044	0.0022	0.0029	0.0011	-0.0017	-0.0165	0.0046	-0.0135
	[0.0184]**	[0.0033]	[0.0054]	[0.0014]	[0.0022]	[0.0023]	[0.0017]	[0.0782]	[0.0028]	[0.0083]
Droughts	0.0203	-0.0012	0.0006	0.0003	0.0011	-0.002	0.0012	-0.0341	0.0019	-0.0064
	[0.0213]	[0.0044]	[0.0059]	[0.0015]	[0.0020]	[0.0028]	[0.0022]	[0.0954]	[0.0031]	[0.0083]
N	135864	135864	135864	135837	135837	135837	135837	135601	135864	135864
R-sq	0.921	0.049	0.041	0.023	0.03	0.043	0.021	0.532	0.297	0.282

Notes. Floods and Droughts represent the fraction of months during the 12 months before birth that the *flood* and *drought* indicators equal one, respectively. The data are collapsed to municipality-of-birth × month-of-birth × year-of-birth level. 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 12 months before birth, sex and race. The regressions weights the observations by the cell size to adjust for precision with which the cell means are estimated. Robust standard errors clustered at the municipality level are presented in brackets. Significance: * $p < 0.10$ ** $p < 0.05$, *** $p < 0.01$.