The Long-Term Effects of Unexpected Interruptions in Compulsory Schooling *

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Abstract

In this paper we quantify the long-run impacts of childhood exposure to storms on education and labor market activities in urban and rural India. The identification strategy relies on an original continuous measure of exposure to storms during compulsory schooling that varies by birth-year cohort and district. Our results suggest that storms have substantial disruptive impacts on education and labor market prospects, causing a deskilling of the affected regions. The estimates are particularly sizable in the case of severe cyclonic storms which, because of climate change, have surged over the past few years. In this case, our findings imply an increase of 18 percentage points of the probability of accumulating an educational delay of at last one year, a 4.8 percentage points increase in the probability of no formal schooling and a fall of 8.1 percentage points in the probability of completing post-secondary education. In the long run, childhood exposure to storms has an impact on the type of labor market activity performed, causing a reduction in the probability of accessing regular salaried jobs while increasing the odds of performing domestic duties as primary activity.

Keywords: natural disasters, climate change, storms, schooling interruptions, education, labor markets
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1 Introduction

Over the last 10 years, weather-related natural catastrophes generated on average over 200 billion US dollars of damages per year globally. The main culprits of these damages are tropical cyclones.[1] Emanuel (2021) shows that an increase in greenhouse gases in the atmosphere is directly responsible for an increase in frequency and intensity of tropical cyclones. Evidence of this link and growing concerns about climate change imply that the cost of these events is likely to increase dramatically over the coming years and call for a thorough understanding of their impacts on the economy.

The short- and long-term effects of natural disasters on economic growth have been studied extensively (e.g. Cavallo & Noy, 2010; Strobl, 2011; Cavallo et al., 2013; Dell et al., 2014). While there is a general consensus on the negative contemporaneous consequences of natural disasters, recent findings are in disagreement regarding their long-term effects.[2] The majority of studies relates the path of GDP growth to physical capital reconstruction and potential technological upgrading, yet, to the best of our knowledge, causal evidence of the long-run effects of the impact of natural disasters on human capital formation is scant.

The literature has long established that education is an important determinant of an individual's earnings and that, in the aggregate, human capital contributes to the economic growth of a nation.[3] As a consequence, if natural disasters hinder academic achievements, we may still expect economic growth to slow down in the long run, even if environmental disruption stimulates innovation and assets are replaced with newer and more productive vintages.

In this paper we quantify the long-run impacts of childhood exposure to storms over the course of compulsory schooling on both educational attainments and the type of activity performed by individuals in young adulthood in urban and rural India. We focus on India which is one of the most impacted regions in the world, with over 370 million people affected yearly (roughly one in four people).[4] Natural disasters can disrupt education through two channels: the supply and the demand for schooling. The former channel likely operates

[1] https://www.swissre.com/media/news-releases/nr-20201215-sigma-full-year-2020-preliminary-natcat-loss-estimates.html

[2] Hsiang & Jina (2014) summarize the literature that describes the long-term evolution of GDP per capita in the aftermath of a natural disaster. The authors put foward four hypotheses: *(i) creative destruction*, *(ii) build back better*, *(iii) recovery to trend* and, *(iv) no recovery*. In the long run, each of these hypotheses predicts a different level of GDP per capita.

[3] See for instance Topel (1999) for a study on the role of human capital in economic growth. See Card (2001) for a survey of papers that attempt to identify the impact of education on labor market earnings using supply-side features of the education system (e.g. compulsory schooling laws or differences in the accessibility of schools) as determinants of schooling outcomes. For more recent evidence see Chetty et al. (2011).

[4] See https://ncrmp.gov.in/cyclones-their-impact-in-india/

through the destruction of schools and road infrastructures, which have been shown to play a key role in promoting education (see for instance Duflo, 2001; Jaume & Willèn, 2019). The demand channel may be linked to the impact of storms on the psychological health of children (see Kar & Bastia, 2006; Neria et al., 2008) and/or households' income.

To study the long-run impacts of childhood exposure to storms, data requirements are high. For each individual we need information on both, current outcomes and exposure to storms during compulsory schooling. We combine data from the 2018 release of the Periodic Labour Force Survey (PLFS) with storms' best tracks data from the National Oceanic and Atmospheric Administration (NOAA) over the period 1990-2010. Our identification strategy relies on an original measure of childhood exposure to storms constructed from exogenous variations in wind exposure across birth-year cohorts and districts during compulsory schooling.[5] We proceed in two steps. First, for each year between 1990 and 2010, we build an index of yearly district exposure to storms that accounts for the force exerted by winds on physical structures at the district's geographical centroid. Second, for each district and birth-year cohort aged between 23 and 33 in 2018, we aggregate the index over years of compulsory schooling and obtain a continuous treatment that varies by birth-year cohort and district.

We find that exposure to storms over the course of compulsory schooling impacts long-term educational attainments and the type of activity performed in young adulthood. In the case of severe tropical storms, the estimates imply an average schooling delay of close to three months and an 18 percentage points increase in the probability of repeating a year or dropping out of school. While affected children still complete primary schooling, the delays accumulated over time translate in a lower propensity to complete higher education. Estimates suggest that children exposed to storms face a decrease of 8.1 percentage points in the probability of completing post-secondary education. Concurrently, the probability of having no formal schooling increases by 4.8 percentage points. These delays also manifest themselves later on, once affected individuals reach young adulthood. Positive exposure reduces the likelihood of working a formal job or being self-employed, while it increases the probability of performing domestic duties as primary activity. Hence, our results suggest that tropical storms can lead to a deskilling of future generations and, in consequence, a widening of the skill and wage gaps across affected and sheltered regions. If, on the one hand, storms foster innovations and technological advancement, on the other hand, our findings indicate that they have negative effects on human capital formation, therefore potentially

[5]The occurrence of storms is random and unpredictable (see for instance Elsner & Bossak, 2001; Pielke et al., 2008), and the literature has shown that individuals do not account for it in their decision process (Wu et al., 2014; Dessaint & Matray, 2017; Elsner & Bossak, 2001; Pielke et al., 2008). We provide a detailed discussion regarding the exogeneity of storms to economic activity in Pelli & Tschopp (2017) and Pelli et al. (2020).

compromising economic growth in the long run.

Our paper is closely related to Maccini & Yang (2009) who adopt a similar methodology to quantify the impact of early-life (0 to 5 years) rainfall shocks on adult outcomes (health, schooling and wealth) in Indonesia. While our identification strategy is similar in essence, we use a different type of shock, focusing on exposure to storms during years of compulsory schooling. Our paper informs on public policies that respond to extreme events whereas their findings arise from more typical year-to-year variation in rural households' economic conditions (variation in agricultural output) around the time of birth. The authors find evidence that early-life rainfall is an important determinant of adult socioeconomic status.

The literature on the effect of natural disasters on education has mainly focused on the contemporaneous effects of disasters. An exception is Caruso (2017) which, in line with our results, finds that children suffer long-lasting negative effects from natural disasters. However, the paper examines all types of natural disasters occurring in Latin America over the last 100 years and identifies them with dummy variables. Deuchert & Felfe (2015) look at a super typhoon on Cebu island in the Philippines and show a negative effect on children's education, probably due to a shift in households' spending towards reconstruction. Groppo & Kraehnert (2017) look at the impact of severe winters in Mongolia and find that children's education suffers, likely because severe winters act as a negative income shock. Rosales-Rueda (2018) shows lower test score results for children affected by floods while in utero in Ecuador. Spencer et al. (2016) look at the contemporaneous effect of cyclones on educational results in the Caribbeans, which turn out to be negative.[6]

Finally, this paper also speaks to the literature on education in developing countries. Evidence suggests that improving school attendance, subsidizing textbooks (Glewwe et al., 2009) or even increasing the number of teachers (Banerjee et al., 2004) does not necessarily ameliorate learning. Therefore, recent findings suggest that improving the quality of teaching is a first-order concern (see for instance Banerjee et al., 2007) and that policies promoting school enrolment should, at the very least, be coupled with interventions improving the pedagogy or curriculum of schooling.[7] While we do not provide direct evidence on the quality

[6] Many papers focus on developed countries. Karbownik & Wray (2019) investigates the impact of exposure to cyclones on fetal and early life in the US and find a negative income effect in adulthood for white males. Billings et al. (2020) show a decrease in enrolment numbers and in graduation rates and, Sacerdote (2012) finds an initial decrease in test scores of students affected by Hurricanes Katrina and Rita but a subsequent increase for students moving out of Louisiana to states with better school systems. Groen et al. (2020) study the effects of the same hurricanes on employment and earnings. While job losses generate short-term fall in earnings, they find that, in the long run, affected regions experience gains which are due to both a contraction of labor supply and an increase in labor demand.

[7] Duflo (2001) and Duflo (2004) estimate the impact of school construction on education and labor market outcomes in Indonesia. For more references, see Glewwe & Kremer (2006). See Chetty et al. (2011) for a study on the long-run impacts of early childhood education in the U.S.

of teaching, our results seem to indicate that school attendance remains very important as the delays which appear to result from absenteeism or school removals do have long-term consequences on the probability of completing higher education and on future labor market outcomes.

2 Data

Our empirical analysis uses two sources of data: *i)* the 2018 release of the PLFS – used to measure educational delays and labor market variables, and *ii)* tropical storms data from the NOAA – used to construct an index of childhood exposure to storms.

2.1 Individual and household data

The PLFS is an individual- and household-level representative survey of the Indian population collected by the National Sample Survey Office (NSSO) of the Ministry of Statistics and Program Implementation. The survey provides a variety of information on individuals' characteristics such as age, gender, educational level and the number of years spent at school. In India, children typically start school at the age of 6. Without delays, compulsory schooling lasts 9 years, i.e. until a child is 15 years old. Table 1 summarizes the schooling system, including the various paths to higher education.[8] Column (1) indicates the number of years needed to complete each individual category of schooling. For graduate and postgraduate levels, the numbers correspond to the modal duration across disciplines. Column (2) shows the total cumulated number of years needed to complete any given level of education. For instance, middle school lasts 3 years. At the end of middle school, a child should have accumulated 8 years of education; 5 years of primary and 3 years of middle school. The PLFS provides information on the highest level of education completed and on whether an individual earned a diploma/certificate. These two pieces of information allow us to infer the path of individuals who continued into higher education and compute the corresponding theoretical number of years of education (in the absence of an educational delay).

[Table 1 here]

For each individual, we measure educational delay as the difference between the actual number of years in formal education and the minimum number of years needed in the schooling system to achieve the reported level of education. For example, suppose an individual

[8] More details on the educational system of India and how it compares to other systems can be found here: https://wenr.wes.org/2018/09/education-in-india

reports seven years of formal schooling but has only completed primary school. This individual has a two-years educational delay, which may be caused either by repeating grades or by dropping out from a higher educational level (middle school, in this particular example).[9] Thus, our analysis will inform on whether storms increase educational delays but it will not be able to tell us anything about the likelihood of repeating grades versus dropping out of school. As an alternative measure of educational delay we also construct an indicator variable taking the value of 1 for individuals with positive educational delays and 0 otherwise.

The PLFS provides information on the primary activity status of individuals.[10] For instance, we know whether an individual's primary activity takes place in the formal labor market or at home (e.g. performing domestic duties – collecting vegetables, firewood, cattle feed, sewing, etc.). Included among formal labor market activities are regular work (i.e. work associated with a formal job and an employment contract), casual work (i.e. work with a daily or periodic contract only), self-employment, and unpaid family work (e.g. work in the family business/farm without pay). The survey also contains labor market indicators such as hours of work and earnings, yet this information only pertains to individuals who perform paid activities and report being part of the labor force.

Importantly, the PLFS provides information on the district of residence of individuals, which, combined with information on individuals' age, allows us to create a unique measure of childhood exposure to storms that vary by birth-year cohort and district. As we describe below, our measure is a continuous treatment taking into account the intensity of the storms to which children of a given cohort and living in a specific district were exposed over the course of compulsory schooling. Given the very small proportion of individuals migrating outside of their birth's district (see, for instance, Gupta, 1987; Munshi & Rosenzweig, 2009; Edmonds et al., 2010; Topalova, 2010), we assume that individuals completed their compulsory schooling in the same district in which they are living in 2018.[11] This assumption is important for the construction of the childhood exposure index.

As benchmark age for young adulthood we choose the age of an individual at the time of completing postgraduate education (master degree). Without educational delays, obtaining a postgraduate degree takes 17 years. Children usually start school at the age of 6 and,

[9] While it would be interesting to distinguish between both types of delays, the PLFS does not provide sufficient information to distinguish between the two cases.

[10] Details and definitions can be found here (p.35): http://mospi.nic.in/sites/default/files/publication$_$reports/Annual$_$Report$_$PLFS$_$2018$_$19$_$HL.pdf

[11] Migration in India is low and, according to Topalova (2010), only less than 4% (13%) of rural (urban) individuals migrate out of district. We also compute our own migration numbers using the 64th round of the National Sample Survey (NSS) for the years 2007-2008. We find that only 3.5% of households (out of 125,578) have migrated within the last 365 days and that 1.3% have migrated permanently, out of which about half migrate out-of-district.

therefore, young adulthood is reached at the age of 23. As a consequence, the youngest cohort considered in the paper was born in 1995 and should have completed compulsory schooling in 2010. The oldest cohort examined is dictated by the quality of satellite coverage. World Meteorological Organization (WMO)-sanctioned cyclone data for the North Indian Ocean only goes back to 1990.[12] As illustrated in Figure 1, this means that the oldest cohort we consider was born in 1985, and is 33 years old in 2018.

[Figure 1 here]

Therefore, our analysis focuses on the 77,737 individuals born between 1985 and 1995 (i.e. the cohorts aged 23-33 in 2018) and storms which took place between 1990 and 2010.

2.2 Childhood exposure to tropical storms

In order to understand how childhood exposure to storms impacts long-term education levels and labor market outcomes, we create an index based on storms' wind speed that varies by birth-year cohort and district. This measure captures storms occurring in the first nine years of compulsory schooling (starting at age six) and in the pre-school year. This additional year allows us to account for children born early in the year and, therefore, integrating school a year earlier. Childhood exposure to storms is computed as follows:

$$C_{bd} = \sum_{t=b+5}^{t=b+15} x_{dt}, \tag{1}$$

where b denotes a birth-year cohort, d a district, t a year. The variable x_{dt} is an index of yearly district exposure to storms and accounts for the force exerted by winds on physical structures. Details on the construction of x_{dt} are presented in Appendix A. Consider for instance the timeline of the oldest cohort (born in 1985). As illustrated in Figure 1, the index of childhood exposure, C_{bd}, sums district exposure to storms from 1990 (the pre-school year) up to 2000 (the end of the nine years of compulsory schooling); i.e. $C_{1985,d} = \sum_{t=1990}^{t=2000} x_{dt}$. Within birth-year cohort across district variation in the index results from the fact that at a given point in time, the exact same storm exerts different windspeed intensities at various locations, while some areas are sheltered. Accounting for wind speed lends us with a continuous treatment that varies across space, which is a considerable advantage in terms of identifying variation in state of relying on dummies or categorical treatments (e.g. a measure taking the value of one if an individual was exposed to a storm during the period of compulsory schooling).

[12] See https://climatedataguide.ucar.edu/climate-data/ibtracs-tropical-cyclone-best-track-data.

Within district across birth-year cohorts variation results from the fact that different cohorts may be subject to different storms over the course of compulsory schooling.

The left panel of Figure 2 presents the measure of childhood exposure to storms at the state level.[13] In our sample, children living in 28 out of the 35 Indian states experienced tropical storms between the ages of 5 and 15. Importantly, the boxplots show substantial variation in childhood exposure to storms within and across states, with Andhra Pradesh, Gujarat, Maharashtra, Orissa and Telangana displaying the largest median exposures. The right panel of Figure 2 provides a visualization of the distribution of C_{bd} across districts for the cohort born in 1987, with darker shades of red indicating higher exposures. The darkest shade indicates districts for which the index of childhood exposure falls above the 90th percentile in the distribution of C_{bd} for individuals born in 1987. Each shade contains 15% of the districts with a positive childhood exposure. The landlocked part of India in the North exhibits nearly zero exposure, which is consistent with storms' best track data which typically indicates a high concentration of storms along coastal areas. The map reveals that the cohort of 1987 living in the remainder of India experienced positive exposure to storms, with districts around the South-Eastern coast being the most affected.

[Figure 2 here]

In Table 2 we provide summary statistics for the main variables used in the paper. Panel A shows figures for the measure of childhood exposure to storms. 23,547 out of the 77,373 individuals included in the sample – roughly 30% – were exposed to storms over the course of compulsory schooling. In Panel B we present the list of individual controls that are used in the analysis. The sample is evenly split across genders, with a majority of Hindu households and 30% of first-born individuals.[14] Columns (1) and (2) of Panel C show the mean and standard deviation of dummy variables for the highest category of schooling completed by individuals. About 12% of our sample falls in the category *below primary*, 76% of these individuals did not attend (formal) school at all, while the remaining 24% are primary school dropouts. 9% completed primary school only, 22% middle school and 33% secondary school. Finally, 24% of the sample obtained a diploma (completed a certificate course) or obtained a post-/graduate degree. Columns (3) and (4) of Panel B show means of the variables with zero and positive exposure, respectively. The last column displays the difference between the latter two means and tests its statistical significance. Means do not statistically differ from each other for individuals falling in the categories *below primary* and *primary*. However,

[13]Only states with positive exposures are included.

[14]The set of controls that we can use is highly restricted, because most household- and individual-level controls are likely to be affected by storms and would, therefore, be bad controls.

they differ in a statistically significant manner for individuals who completed higher levels of education.

Looking at Panel D, prima facie evidence suggests that, on average, the educational delay tends to be greater for individuals with positive exposure. Individuals with zero exposure experience an average delay of 0.47 years, which amounts to about 20 weeks.[15] The delay is on average two weeks longer for individuals with positive exposure (0.53 years, i.e. about 22 weeks). We check whether the same difference exists within educational categories, distinguishing between individuals who have completed at most primary, middle, secondary or higher educational levels. The difference in educational delays between the zero and positive exposure groups is particularly marked for individuals who did not go past middle school. For secondary and categories of schooling that fall into *higher education*, the sign of the relationship is opposite. With compulsory schooling lasting roughly until completion of secondary school, results in Panel C suggest that delays associated with storms tend to occur quite immediately, rather than appearing gradually over the years. Combining the results from Panel C and D, it appears that while the delays accumulated over the period of compulsory schooling do not prevent its completion, they have long-term impacts by reducing the likelihood of going past *middle* education levels. It is important to note that these are simple observations based on descriptive statistics, without accounting for different storm exposures and possible confounders.

Regarding labor market indicators, the descriptive statistics suggest that for the subsample of individuals who have positive salaries and report belonging to the labor force, positive exposure is associated with longer workweeks. Finally, the bottom of Panel D presents binary variables for the primary activity status of individuals. Among these individuals, the largest share, 35%, carries out domestic duties while only 18% has a formal job with a regular employment contract and salary.

[Table 2 here]

3 Baseline Results

3.1 Educational delay

Specification

We evaluate the impact of early childhood exposure to storms on individuals' educational delay using the following specification:

[15] The school year lasts 42 weeks.

$$Y_i = \alpha_0 + \alpha_1 C_{bd} + \mathbf{X_i'}\boldsymbol{\beta} + \delta_d + \delta_b + \epsilon_i, \tag{2}$$

where i is an individual's subscript. Y_i captures an individual's educational delay, measured either using the number of years of educational delay or a dummy variable taking the value of one in the case of an educational delay of at least one year. $\mathbf{X_i'}$ is a vector of individual characteristics including dummy variables indicating if the individual is a female, a first-born child and Hindu respectively. While we drop subscripts where possible, it is understood that $i = (b, d)$, where b denotes the birth-year cohort and d the district.[16]

δ_d is a set of district FE to control for fixed district characteristics that may affect the education level of individuals. δ_b is a set of birth-year cohort FE. The inclusion of both district and birth-year cohort FE implies that identification is achieved using two sources of data variation, i.e. by comparing the educational delays of first, cohorts with different levels of exposure within districts, and second, the same birth-year cohort across districts facing differential exposures. Finally, ϵ_i is the error term.

Importantly, note that our sample only contains individuals who were actually enrolled in the schooling system. Therefore, the effect on the most vulnerable children (e.g. those belonging to scheduled castes who most likely did not attend school at all) is not captured by α_1.

Results

Table 3 shows results for equation (2). Standard errors are clustered at the state level. Clustering at the state level accounts for the fact that the largest part of funding for education and the coordination of education programs are administered at the state level.[17] In addition, state-level clustering also takes into consideration spatial correlations within state as well as time correlations in the exposure index that result from the fact that the same storm affects multiple birth-year cohorts simultaneously.

In Panel A we measure educational delay as the difference between reported years of schooling and the minimum number of years corresponding to the reported educational attainment. Column (1) shows the baseline result and suggests that exposure to storms leads to a statistically significant delay in completing a given level of education. The estimate indicates that a child with unit exposure will be delayed by 0.31 years on average, which amounts to a delay of approximately 13 weeks (3 months). The left panel of Figure 2

[16] We specifically do not include in the regression controls such as being the household head, married, living in a rural area and for the size of the individual's household. In principle, each of these variables could be affected by childhood exposure to storm and, in consequence, be a bad control.

[17] http://countrystudies.us/india/37.htm

indicates that while unit values in the exposure index are exceptional in our sample period, they are actually observed in Orissa. These values result from the 1999 BOB 06 super cyclone (rather than a series of small-scale storms during compulsory schooling), the most intense and destructive tropical cyclone recorded over the period 1990-2000 in India. Extremely severe cyclonic storms are certainly rarer, yet they have been observed again recently, e.g. storm Phailin and Fani made landfall in Orissa in 2013 and 2019 respectively, and super cyclone Amphan hit West Bengal in 2020. In 2021, the extremely severe cyclonic storm Tauktae caused severe damages in Gujarat and ten days later, the severe cyclonic storm Yaas led to grave destruction in West Bengal and Orissa. In light of the recent surge of extremely severe cyclonic events, it is important to provide an interpretation of our estimates for large (unit) values of the exposure index, as it informs on the educational long-term delays that the current generation of school-attending kids may suffer. If instead we use average exposure in the sample to interpret our results, we obtain an educational delay of roughly six and a half school days.[18]

In columns (2)-(4) we use different sets of FE to investigate the robustness of our results. In column (2) we include state trends to control for trends in state-level education policies and regional disparities in economic growth. Results are quantitatively similar to the baseline estimate. In column (3) we incorporate state-(birth)-year FE, thereby allowing state-level economic conditions at the time of birth of a cohort to affect long-term educational delays. Although the estimate is imprecise, it is worth noting that, even in this demanding specification, the size of the estimate remains nearly identical to the baseline.

In the last column we add state-period FE. The period FE used in the interaction term include three periods: 1985, 1986-1991 and post 1991. This additional set of FE accounts, at least to some extent, for the introduction of the new National Policy on Education introduced under the government of Rajiv Gandhi in 1986, and for its amendment in 1992. An important constituent of the policy is that it called for fulfilling compulsory education for all children up to the age of 14. Although it was introduced at the national level, the policy got effectively adopted at the state level. Results remain similar to the baseline estimates.

In Panel B, we run a linear probability model to explore the effect of childhood exposure to storms on the probability of having an educational delay of at least on year. The estimates obtained are considerably stable across specifications and suggest that being exposed to storms during compulsory schooling increases the probability of a delay. Focusing on the baseline specification (column 1) and examining the impact of a unit exposure (which is likely to be driven by severe events, as describe earlier), the estimate implies an increase

[18] Given an average storm exposure of 0.1, 42 weeks per year and assuming that the benchmark school week contains 5 days, this number is computed as $0.31 \cdot 0.1 \cdot 42 \cdot 5$.

of 18 percentage points of the probability of accumulating a delay (i.e. repeating a year or dropping out). Meanwhile, the average exposure increases this probability by 1.8 percentage points. To give a sense of the importance of these findings, consider the states that never experienced storms. Among these states, the share of individuals with an educational delay of at least one year is 0.26. From the baseline estimate in Panel B we can infer that this share would increase by 69% (to a share of 0.44) in the case of an extreme cyclonic storm exposure and by 6.9% (to a share of 0.278) in the case of an average exposure. Hence, these are sizable changes in the probability of being delayed.

[Table 3 here]

3.2 Educational attainment

In Table 4 we examine whether, in addition to creating educational delays, storms also impact the probability of completing a given level of education. To answer this question we run an ordered logit estimation using a categorical variable indicating the reported educational attainment (0=no formal schooling, 1=primary school, 2=middle school, 3=secondary education, 4=above-secondary education). As in equation (2), we include individual controls, a set of district and birth-year FE and cluster standard errors at the state level.

The first column of the table shows the ordered logit estimates. In column (2)-(6) we report the marginal effects of childhood exposure to storms for each category of schooling. The estimates, which are all statistically significant, yield percentage points changes in the probability of completing at most a given category of schooling in the case of unit childhood exposure to storms. As a general statement, positive exposure increases the probability of having no formal schooling and of completing at most primary or middle school. Concurrently, exposure to storms reduces the probability of attaining secondary or post-secondary education.

The estimates are the largest for the highest and lowest schooling categories, implying a fall of 8.1 and an increase of 4.8 percentage points in the probability of completing post-secondary education and no formal schooling in the case of unit exposure respectively. Consider for instance kids who were exposed to the 1999 BOB 06 super cyclone during years of compulsory schooling (i.e. $C_{bd} = 1$). Our sample indicates that in Orissa about 17% of individuals have gone past secondary education. Taking this number as benchmark, the estimate translates into a 47% decrease in the probability of completing post-secondary education. If instead we consider the average exposure of 0.1, the decrease would be much smaller and reduce to 4.75%. In the same state, the share of individuals with no formal schooling is 13%. Hence, one can infer that exposure to the 1999 super cyclone led to a 37% increase of individuals lacking basic education, and, as a consequence, a substantial drop in literacy and numeracy rates. Average exposures instead led to a 3.7% increase, which is smaller but still sizable.

In Figures 3 to 7 we use the estimates from the ordered logit and plot the predicted probabilities of attaining a given category of schooling over the interval [0, 1] of exposures to storms, and their 95% confidence bands, using as benchmark the share of individuals (in the full sample) that belong to a given category. For instance in Figure 3, the probability corresponding to an exposure of zero is the share of individuals with no formal education (about 12%, which is in line with the summary statistics in Table 2). Overall, the results from this exercise clearly indicate that storms shift the distribution of educational attainment to the left, which is particularly worrisome for developing countries like India whose distribution of skills is already quite skewed to the left.

[Table 4 and Figures 3-7 here]

3.3 Type of activity

We expect this educational disruption to be reflected in the type of labor market activity performed in young adulthood, as certain categories of jobs require higher levels of education or at the very least adequate reading, writing and computing skills. We investigate this issue in Table 5 by estimating a reduced-form specification of childhood exposure to storms on an indicator variable for each activity. For instance, in column (1) the dependent variable is a dummy variable equal to 1 if the main activity of individual i is to perform regular work. For each type of activity, we include individual controls as in equation (2), district FE, birth-year FE, as well as an additional set of state trends to account for economic tendencies that may impact labor markets across states differentially. Estimates suggest that individuals who were exposed to storms during compulsory schooling are less likely to work as regular salaried worker, to be self-employed and more likely to perform domestic duties. However, we find no statistically significant effect on the likelihood to be a casual worker or an unpaid family worker.

Our results suggest that the long-term effect of childhood exposure to storms on the type of labor market activity is sizable. To give a sense of the magnitudes, consider the long-term labor market impacts associated with the average (positive) exposure in our sample (i.e. C_{bd}=0.1). The estimate in column (1) implies a reduction of the probability of being a regular worker of 1.6 percentage points. Among states which never experienced storms over the period 1990-2010, the average share of individuals performing regular work is 0.16. Taking this number as benchmark, the estimate in column (1) implies a 10% reduction in the probability of being employed as a regular worker. For self-employment, column (3) indicates a probability drop of 1.2 percentage points. Using the average share of self-employed workers among states with zero exposure (i.e. a share of 0.14) as a starting point, this percentage points drop amounts to an 8.5% long-term decline in the probability of being self-employed. Finally, the estimate in column (5) implies a 1.8 percentage points raise in the probability of performing domestic duties as primary activity in young adulthood. This raise translates in a 5% change when taking the share of individuals involved in domestic duties in states with zero exposure (i.e. a share of 0.37) as baseline. These changes are larger for kids who were exposed to stronger storms. For instance, with unit exposures and taking the previous baseline shares, the estimates imply changes of approximately 100%, 86% and 49% for regular work, self-employment and domestic duties respectively.

[Table 5 here]

Finally, Table 6 examines whether positive exposure is associated with lower wages and longer hours of employment. In column (1) the sample is restricted to workers who receive a salary, which explains the drop in sample size. In the second column the sample contains all the individuals who report positive hours of work, including people performing unpaid tasks. We find no evidence that, conditional on being employed as regular labor, childhood exposure to storms has permanent effects on wages. By disrupting the education of children, storms are likely to exacerbate income and social inequalities across districts and cohorts in the long run. This increase in inequalities occurs not so much through a reduction in wages but through a change in qualifications and type of activity, inducing less regular jobs and relatively more unpaid work (e.g. domestic duties) which do not provide the social security that formal jobs can supply. Disparities along the income distribution will also widen as individuals with the largest delays tend to belong to particularly vulnerable social groups.

[Table 6 here]

4 Robustness

In this section we propose a series of robustness checks involving two falsification tests, the addition of educational controls, the removal of extreme exposures and finally the use of alternative measures of storm exposure. In each case, we start with the discussion of the results for education and then briefly present the estimates obtained on the type of activities performed by individuals.

4.1 Falsification tests

Table 7 presents the results of two falsification tests on the educational delay and attainment regressions. In Panel A educational delay is measured by the number of years of delay, while in Panel B it is measured by a dummy variable taking the value of one in the case of an educational delay of at least one year. In Panel C we report the results of ordered logit estimations on educational attainment.

The first falsification test consists in a placebo exercise in which we replace the index of childhood exposure to storms in equation (2) with a random exposure obtained by reshuffling C_{bd} over the entire sample. We perform this operation 1000 times and in columns (1)-(3) report the share of replications that produce statistically significant estimates at the 1%, 5% and 10% levels respectively. Overall, results suggest that our coefficients do not capture spurious correlations. The numbers in column (1) indicate that in only 1.2% (Panel C) to 2.6% (Panel A) of the cases, the randomization produces estimates which are statistically significant at the 1% level. Not surprisingly, this share increases when considering higher levels of statistical significance, reaching maximums of 7.9% (Panels A and B) at the 5% level and 13.2% (Panel B) at the 10% level.

In column (4) of the same table we perform a second falsification test. We assign our exposure index to cohorts 10 years older (which were not included in our sample initially). One would expect educational attainments of older cohorts to be unaffected by the occurrence of future storms. Specifically, for each birth-year cohort and district, we assign the value of C_{bd} to the cohort born 10 years earlier. We then examine the effect of this artificial exposure measure on the educational delay of the cohorts born between 1975 and 1985. Regardless of the measure of education used, the estimates obtained are highly statistically insignificant.

In Table 8 we repeat these two falsification tests on the type of activities performed by individuals. The results are in line with the ones obtained in Table 7, leading us to the same conclusions that our baseline estimates do not capture spurious relationships.

[Tables 7 and 8 here]

4.2 Education controls

The summary statistics in Table 2 indicate that, on average, educational delays differ across categories of schooling. In particular, the table shows that while delays are pretty high among individuals who completed at most primary schooling, they tend to decrease as we move to higher educational categories. Hence, it appears that within educational categories, individuals may share observable characteristics or have similar abilities which make them more or less likely to accumulate a delay. Accounting for this fact is challenging given that exposure to storms impacts both educational delay and educational attainment. While using educational categories fixed effects would cause a bad-control problem, we propose two alternative ways of approaching this issue, acknowledging that each approach has its own limitations.

First, we proxy educational attainment using the predicted probability of completing the reported level of education, conditional on observable individual characteristics. Specifically, for each education category we run a linear probability model on a set of individual characteristics: the individual's gender, year of birth and whether she is a first born child and Hindu. Interactions of these variables are also included in each regression. To avoid a bad control issue in the final regression, we focus on the sub-sample of states with zero exposure over the period 1990-2010 and restrict ourselves to individual characteristics that are unlikely to be affected by storms, which admittedly will generate a proxy based on a limited set of observable characteristics. We then use the estimates to predict the probability of completing each level of education for each individual in the full (baseline) sample and use the probability corresponding to the reported level of education as a proxy for educational attainment in our final regression. The result from this exercise is presented in column (2) of Table 9 and is exceptionally close to the baseline results (column 1, for comparability purposes).

Second, we include fixed effects capturing parental education. The latter has been shown to be an important predictor of children educational achievements (Guryan et al., 2008; Björklund & Salvanes, 2011; Kim et al., 2021) but is far less likely to be affected by children exposure to storms. While it is rather implausible that parental education overlaps with children compulsory schooling at low levels of education, one should keep in mind that it may nevertheless be possible that parents enrolled at university have kids attending primary school. This may be particularly true for relatively young parents. One disadvantage of this approach is that parental education is only observed in our sample if both the individual and the parents still live in the same household. Moreover, as married women tend to move in with their husbands' family, the sample will reduce to a sub-sample that contains relatively more males than in the baseline. For this reason, we start by replicating the baseline approach on the sub-sample for which parental education is available (column 3 of the table). Although the sample only represents 42% of the initial sample, the coefficients are very similar to the baseline estimates. In column (4) we proceed to include parental FE and obtain nearly identical estimates.

In Table 10 we also perform these two exercises on the type of activities. Panel A shows the baseline results and Panel B includes the predicted probability of completing the reported level of education. Including this predicted probability does not alter the baseline estimates; exposure to storms during compulsory schooling reduces the probability of being employed as a regular worker, be self-employed and increases the likelihood of performing domestic duties in a statistically significant manner. Panel C replicates the baseline regression on the sub-sample that includes information on parental education. When estimated over the sub-sample the probability to be self-employed appears to be no longer affected by childhood exposure to storms. Importantly, however, including parental education (Panel D) as control produces estimates which are very similar to those obtained in Panel C.

[Tables 9 and 10 here]

4.3 Excluding extreme values

In Table 11 we evaluate the sensitivity of our results to extreme values of exposure. The panel structure of the table is similar to Table 7. Column (1) shows the baseline results. In column (2) we exclude individuals located in Orissa, which, as discussed earlier, exhibits exceptionally large values of exposures due to the 1999 super cyclone BOB 06. Results obtained on this sub-sample are not different from the baseline estimates. In column (3) we interact the exposure index and a dummy taking the value of one for individuals who live in Orissa and obtain estimates which align with those obtained on the sub-sample. Hence, it appears that our baseline results are not driven by the super cyclonic event that took place in Orissa in 1999. Finally, in the last column we consider all severe events and recompute the exposure index, removing all the winds with values falling above the 95th percentile of the wind distribution. Not surprisingly, taking out extremes causes the estimates to shrink, albeit by a relatively small extent. The estimates remain qualitatively similar and precise.

In Table 12 we perform a similar exercise for the type of activities performed by individuals as they reach young adulthood. The column structure of the table is identical to Table 5, Panel A reports the baseline results, Panel B and C examine whether the baseline results are driven by observations in the state of Orissa and the last panel removes severe winds. The conclusions we achieve are in line with those obtained in Table 11, hence our results are not sensitive to extreme values of exposure.

[Tables 11 and 12 here]

4.4 Alternative measures of childhood exposure to storms

In Tables 13 and 14 we experiment with alternative specifications of C_{bd}. First, we change the functional form that captures the force exerted by winds on structures (we replace the square with a cube in equation 3 of Appendix A.1). A few studies in the U.S. claim that the energy released by a storm and the force exerted by winds on physical structures are related in a cubic and not a quadratic manner (see the technical HAZUS manual of the Federal Emergency Management Agency – FEMA – of the U.S. Department of Homeland Security and Emanuel, 2005). We account for this fact in column (2) and (4) of Table 13 and in Panels B and D of Table 14, where we use a cubic specification.

Second, we alter the threshold above which a wind is classified as a storm. Throughout the paper, we define the benchmark windspeed threshold likely to generate damages at 50 knots, following Emanuel (2011). In columns (3) and (4) of Table 13 and in Panels C and D of Table 14, we move the storm threshold up to 64 knots (corresponding to a category 1 cyclone on the Saffir-Simpson scale). In column (5) of table 13 and in Panel E of Table 14 we completely drop the notion of threshold and use all the winds sweeping the country during a cyclone.

Finally, in column (6) of Table 13 and in Panel F of Table 14, we use the HURRECON model (see Appendix A.2.2 for more details) in order to compute the maximum windspeed hitting each district, following Boose et al. (2004) instead of Deppermann (1947).

[Table 13 here]

In each column of Table 13 the estimated coefficients remain positive and precisely estimated (with the exception of column 5) and, as expected, the magnitudes of the estimates become larger as the threshold used to compute district exposure increases to capture less frequent but more powerful winds. Using cubic specifications also tend to inflate the estimates. For instance, in column (3) of Panel A, where district exposure is computed using a threshold of 64 and a quadratic function, the estimate grows to 0.42 against 0.31 in the baseline (column 1). This number corresponds to an increase in delay of 8.8 school days for the average exposure index and of 17.7 weeks in districts with the highest exposure. The corresponding estimate in Panel B implies that in these districts, individuals are 25 percentage points more likely to have an educational delay of one year or more, in comparison to storm-sheltered places. The same pattern is observed for the ordered logit estimates in Panel C, the magnitude is slightly bigger but signs and statistical significance are left untouched. Including all winds when computing storm exposure (column 5) introduces a downward bias on our estimates and causes them to be statistically insignificant. This change is not surprising as accounting for all winds means attributing non-zero childhood exposure values to many districts that were only exposed to very mild windspeeds.

[Tables 14 here]

In Table 14 we look at how the impact of childhood exposure on the type of activity changes when we modify the specification of the index. The magnitudes of the results are similar to the baseline but the estimates are less precise. The impact on casual labor, self-employment, unpaid family work and involvement in domestic duties is not affected by the different specifications. Instead, we find that the negative and statistically significant impact on regular work is estimated less precisely when the definition of storm is altered. It is worth mentioning that the coefficient is still borderline significant in some cases: in Panel C the p-value is 0.18 and in Panel F it is 0.11.

5 Heterogeneity of the effects

In this section we study how the effects of interest differ across males and females, and urban and rural areas. First, we look at the impact on education across the four groups. Columns (1) and (2) of Table 15 show results for educational delay, while columns (3) to (6) present results for educational attainments. The four subsamples have roughly the same size. Results for males (Panel A) are similar in sign and statistical significance to those for females (Panel B). Yet, the table points towards significant differences in terms of the magnitude of the effects. For educational delay, the results for males are roughly 64% larger. These results, even if less pronounced, are similar to what Takasaki (2017) finds regarding boys and girls' school enrolment in the aftermath of a cyclone in the Fiji Islands. This difference in the effect may be due to the different speed of physical development between genders. Often, boys 10 years and older are already physically capable to help their parents in reconstruction activities. If storms were to damage the family farm or fields, a household may decide to keep boys home to help and send girls to school, causing boys to accumulate a delay but not girls. Regarding educational attainment, columns (3) to (6) tell us that storms tend to increase the probability of not having formal education and of completing at most middle school, while they decrease the probability of completing secondary or higher education. While the impact on the educational delay is smaller for females, their probability of not going further than primary school is 178% larger than for males (0.078 vs 0.028) and is statistically significant at the 1% level. These results seem to indicate that while males accumulate an educational delay, females may just end their educational career earlier.

[Table 15 here]

Panel C and D show results for rural and urban areas, respectively. In rural areas, the majority of households are involved in farming. These households are likely to take their children out of school in the aftermath of a storm to perform reconstruction and field activities, generating an educational delay, as found in columns (1) and (2) of Table 15. In addition, we find that the probability of completing higher education decreases. Maybe surprisingly, in urban areas, the story seems to be similar. We find an increase in educational delay, and a higher probability of attaining lower educational levels, starting with primary school.

In Table 16, we look at what happens to the type of activity undertaken in young adulthood for each of the four subsamples. We find that the probability of being involved in regular work (i.e. high quality salaried jobs) and to be self-employed decreases across the four subgroups, while the probability of performing domestic duties increases across the board.

[Table 16 here]

6 Conclusion

In this paper we look at how exposure to storms during compulsory schooling affects long-term educational attainments and the type of activity performed in early adulthood. Using storm data from NOAA, we construct a measure of exposure to storms over the course of compulsory schooling for individuals aged 23 to 33 in the 2018 release of the PLFS. Individuals hit by a storm during these important years tend to accumulate an educational delay and are less likely to complete higher education. We also find that exposure to storms reduces the probability of obtaining a regular salaried job, or being self-employed and increases the probability of performing domestic duties as primary activity in young adulthood.

Duflo (2001) finds that economic returns to education range from 6.8 to 10.6 percent in Indonesia. Applying these numbers to India, our estimates of educational delay imply that a unit exposure to storms during the years of compulsory schooling could cause a lifelong 2.1% to 3.3% fall in returns on average. For the average exposure in our sample, the corresponding lifelong fall in returns ranges between 0.13% and 0.21%.[19] This is an important number

[19] A unit exposure to storms causes an average delay of 13 weeks out of the 42 weeks of a school year, which amounts to 31% of the year. Multiplying this number by Duflo (2001)'s estimates we obtain a reduction of 2.1% to 3.3% in returns. For the average exposure, educational delays represent 2% of the year, which translates into a fall of 0.13% to 0.21% in returns.

considering that storms typically last less than a week. Although we cannot test this formally, our results also suggest that the skill distribution of cohorts subjected to positive exposures during compulsory schooling are more skewed to the left. Our findings consequently hint towards a deskilling of the geographical areas prone to natural catastrophes in the long-term and towards a widening of wage inequality within and across districts. The effects we find are rather moderate for the average exposure over the period 1990-2010 – yet they reach sizable magnitudes for more severe exposures which recently became the norm rather than the exception (Emanuel, 2021).

As we pointed out earlier, there are two main channels through which storms may affect education. On the schooling supply side, a disaster may destroy public infrastructure, like roads and schools, creating punctual delays in schooling due to the impossibility to attend classes (see for instance Duflo, 2001; Jaume & Willèn, 2019). A storm can also impact the demand for schooling, for instance by generating PTSD in children, a condition that has been shown to hinder their scholastic and labor market performance (e.g. Cutter et al., 2003; Kar & Bastia, 2006; Neria et al., 2008; Blaikie et al., 2014). Or it may result in a negative income shock for the household, e.g. by destroying crops and farms in rural areas or part of production facilities in both, rural and urban areas (see for instance Pelli et al., 2020). Negative income shocks can be temporary and last until physical assets are rebuilt, or permanent, if for instance the loss of a season's crop puts a farming household in debt and cripple it financially for years to come.

While it is likely that our findings reflect the interaction of all these different channels, our data do not allow us to identify their relative contribution in increasing educational delays, making it harder to formulate precise policy interventions. Nevertheless, the results from Section 5 suggests that the detrimental effects of storms are present not only in rural but also in urban areas, therefore evoking the need for wide-scale policies aimed at helping both regions. In our opinion however, the set of policies should differ across regions and be adapted to their intrinsic economic activities. In rural areas, we recommend an emergency system of Conditional Cash Transfers (CCT) covering reconstruction and the loss of agricultural and farm income. To limit educational delays, this policy should be conditional on regular and uninterrupted school attendance. In urban areas, individuals are more likely to be laid off as storms destroy firms' facilities and impair production networks. These layoffs and job losses can drag a family down a spiral of poverty especially if unemployment insurance and social assistance programs are dysfunctional, as it is often the case in developing countries. In consequence, when social safety nets are weak, children may be taken out of school earlier, jeopardizing their future employment prospects. A possible policy in urban areas would be to subsidize firms in order for them to keep paying their employees during the reconstruction of their physical infrastructure. These subsidies could be modeled on the policies that have been implemented by many countries during the COVID-19 pandemic and should be complemented with social assistance programs targeted at individuals who are not regular salaried workers.

The results obtained in this paper help us predict part of the long-term impacts of the COVID-19 pandemic that started towards the end of 2019. Government-mandated school closures led to a reduction in the schooling supply, at least temporarily. On the demand side, the literature documents that the pandemic gave rise to PTSD (see for instance Phelps & Sperry, 2020; Yue et al., 2020; Zhou, 2020) and, by causing heterogeneous losses of employment, heterogeneous negative income shocks (see Di Pietro et al., 2020). In light of our findings, we can infer that the pandemic will likely generate an educational delay for many students, compromise educational attainment and increase the drop-out rate. Consistent with this argument, Kuhfeld et al. (2020) predict that returning students will have only approximately 63-68% of the reading capacity and 37-50% of the mathematical knowledge with respect to a usual school year. Moreover, our results also suggest a change in the type of activity that current students will engage in during early adulthood. This change will likely be reflected in the share of individuals with regular salaried work and also in the type of occupations or tasks individuals will perform. Furthermore, we expect the ensuing fall (increase) in the supply of high-skill (low-skill) workers for the cohorts attending school during 2020-2021 to contribute to increased wage inequality. In light of this evidence, it should be paramount for policymakers to find a way to allow these students to make up for the delay accumulated. In the absence of such an effort, economic inequalities are meant to increase for individuals who were in school during the COVID-19 pandemic.

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Tables

Table 1: Schooling System in India

	Duration (1)	Cumulated Years of Education (2)
Lower education:		
Primary	5	5
Middle	3	8
Secondary	2	10
Higher secondary	2	12
Higher education:		
Path 1:		
Diploma/certificate course	1	13
Path 2:		
Graduate	3	15
Path 3:		
Diploma/certificate course	1	13
Graduate	3	16
Path 4:		
Graduate	3	15
Postgraduate and above	2	17
Path 5:		
Diploma/certificate course	1	13
Graduate	3	16
Postgraduate and above	2	18

Notes: Column (1) shows the duration of each category of schooling. For *Graduate* and *Postgraduate*, the duration corresponds to the mode across disciplines. Column (2) gives the total number of years of education accumulated after completion of each category of schooling (and path in the case of higher education).

Table 2: Summary Statistics

	Mean (1)	Std. Dev. (2)	Min (3)	Max (4)	N (5)	
Panel A: Exposure to storms						
C_{bd}	0.03	0.096	0	1.003	77,737	
$C_{bd} > 0$	0.099	0.154	1.23e-08	1.003	23,547	
Panel B: Controls						
Gender	0.49	0.50	0	1	77,737	
First born	0.29	0.45	0	1	77,737	
Hinduism	0.74	0.44	0	1	77,737	
	All		Zero exp.	Pos. exp.	Diff.	Diff.
	Mean (1)	Std. Dev. (2)	Mean (3)	Mean (4)	(3)-(4) (5)	in weeks (6)
Panel C:						
Highest category of schooling completed (yes=1, no=0)						
Below primary♠	0.12	0.32	0.12	0.12	-0.001	
Primary	0.09	0.29	0.09	0.09	0.003	
Middle	0.22	0.41	0.22	0.21	0.01***	
Secondary	0.33	0.47	0.33	0.33	-0.004	
Above♣	0.24	0.43	0.24	0.25	-0.01***	
Observations	77,737		54,190	23,547		
Panel D: Main variables						
Educational delay						
Educational delay (yes=1, no=0)	0.30	0.46	0.29	0.33	-0.04***	
Educational delay (# of years)	0.48	0.88	0.47	0.53	-0.06***	2.52***
Primary	0.74	0.93	0.70	0.86	-0.16***	6.72***
Middle	0.59	0.67	0.53	0.75	-0.22***	9.24***
Secondary	0.30	0.71	0.31	0.26	0.05***	2.1***
Above	0.39	0.75	0.39	0.38	0.01	0.42
Observations	77,737		54,190	23,547		
Labor market						
Log hourly wage	3.69	0.64	3.69	3.69	0.002	
Weekly hours worked	53.59	13.09	53.23	54.28	-1.05***	
Observations	31,535		20,896	10,639		
Primary activity status						
Regular work	0.18	0.39	0.17	0.22	-0.05***	
Casual labor	0.10	0.30	0.09	0.11	-0.02***	
Self-employment	0.13	0.34	0.13	0.13	0.003*	
Unpaid family work	0.08	0.27	0.08	0.09	-0.009**	
Domestic duties	0.35	0.48	0.35	0.34	0.01***	
Observations	77,737		54,190	23,547		

Notes: The following categories *not literate*, *literate without formal schooling* and *literate below primary* are grouped into the category *below primary*. ♠ Among those individuals who did not complete primary schooling, approximately 76% did not attend school at all and 24% are primary school dropouts. ♣ This category includes all the categories that fall into higher education: *Diploma/certificate course*, *graduate*, *postgraduate and above*. Columns (1) and (2) show the mean and standard deviation of the main variables for the entire sample. Columns (3)-(5) distinguish between individuals with zero childhood exposure to storms from those with positive exposure. Column (5) tests whether means statistically differ from each other across these two groups of individuals. The variable *gender* is equal to one for female individuals. *First born* is equal to one for first born individuals. Finally, *Hinduism* is equal to one for Hindus. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3: Educational delay

	Educational delay			
	(1)	(2)	(3)	(4)
Panel A:				
# of years				
Childhood exposure	0.31***	0.27**	0.20	0.29***
	(0.079)	(0.13)	(0.15)	(0.097)
Panel B:				
yes=1, no=0				
Childhood exposure	0.18***	0.20***	0.20**	0.19***
	(0.052)	(0.067)	(0.082)	(0.065)
Individual controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes
State trends	No	Yes	No	No
State-(birth)-year FE	No	No	Yes	No
State-period FE	No	No	No	Yes
Observations	77,737	77,737	77,737	77,737

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. In Panel A educational delay is computed as the difference between reported years of schooling and the minimum number of years corresponding to the reported educational attainment. In Panel B, the measure of educational delay is given by a dummy variable taking the value of one in the case of an educational delay of at least one year. Individual controls include dummy variables indicating if the individual is a female, the first born child and Hindu respectively. Period FE used in the interaction terms include three FE corresponding to the year 1985, the periods 1986-1991 and post 1991.

Table 4: Educational attainment

	Logit estimates	No formal schooling	Primary school	Middle school	Secondary education	Above-secondary education
	(1)	(2)	(3)	(4)	(5)	(6)
Childhood exposure	-0.48***	0.048***	0.028***	0.031***	-0.026***	-0.081***
	(0.130)	(0.012)	(0.008)	(0.009)	(0.007)	(0.022)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	77,737	77,737	77,737	77,737	77,737	77,737

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. Column (1) shows the results from an ordered logit estimation where the dependent variable is a categorical variable indicating the reported educational attainment (0=no formal schooling, 1=primary school, 2=middle school, 3=secondary education, 4=above-secondary education). Columns (2)-(6) report the marginal effects of childhood exposure to storms for each category of schooling. Individual controls include dummy variables indicating if the individual is a female, the first born child and Hindu respectively.

Table 5: Type of activity

	Regular work	Casual labor	Self-employed	Unpaid family work	Domestic duties
	(1)	(2)	(3)	(4)	(5)
Childhood exposure	-0.16**	-0.059	-0.12***	0.046	0.18**
	(0.069)	(0.049)	(0.020)	(0.045)	(0.073)
Individual controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes	Yes
State trends	Yes	Yes	Yes	Yes	Yes
Observations	77,737	77,737	77,737	77,737	77,737

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. Individual controls include dummy variables indicating if the individual is a female, the first born child and Hindu respectively.

Table 6: Wages and hours of work

	Log hourly wages	Hours of work
	(1)	(2)
Childhood exposure	-0.021	5.72
	(0.19)	(4.55)
Individual controls	Yes	Yes
District FE	Yes	Yes
Birth-year FE	Yes	Yes
State trends	Yes	Yes
Observations	31,534	31,534

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. Individual controls include dummy variables indicating if the individual is a female, the first born child and Hindu respectively.

Table 7: Falsification tests, education

	Placebo Share of estimations with statistical significance at:			**Older cohort assignment**
	1%	5%	10 %	+ 10 years
	(1)	(2)	(3)	(4)
Panel A:				
Educ. delay: # of years				
Childhood exposure	0.026	0.079	0.128	-0.040
				(0.12)
Panel B:				
Educ. delay: yes=1, no=0				
Childhood exposure	0.024	0.079	0.132	-0.038
				(0.064)
Panel C:				
Educ. attainment				
Childhood exposure	0.012	0.060	0.108	-0.370
				(0.410)
Individual controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes
Observations	77,737	77,737	77,737	67,765

Notes: In Panel A educational delay is computed as the difference between reported years of schooling and the minimum number of years corresponding to the reported educational attainment. In Panel B the measure of educational delay is given by a dummy variable taking the value of one in the case of an educational delay of at least one year. In Panel C educational attainment is a categorical variable indicating the reported educational attainment (0=no formal schooling, 1=primary school, 2=middle school, 3=secondary education, 4=above-secondary education). Columns (1)-(3) show the share of statistically significant results over 1000 randomizations, where the childhood exposure measure is randomized over the entire sample. Column (4) shows the estimates obtained using the synthetic index of childhood exposure over the sample consisting of cohorts born between 1975 and 1985. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. Individual controls include dummy variables indicating if the individual is a female, the first born child and Hindu respectively.

Table 8: Falsification tests, type of activities

	Placebo Share of estimations with statistical significance at:			Older cohort assignment
	1%	5%	10 %	+ 10 years
	(1)	(2)	(3)	(4)
Panel A:				
Regular work				
Childhood exposure	0.016	0.061	0.131	-0.053
				(0.058)
Panel B:				
Casual work				
Childhood exposure	0.016	0.085	0.142	-0.058
				(0.047)
Panel C:				
Self-employed				
Childhood exposure	0.024	0.089	0.144	0.097
				(0.080)
Panel D:				
Unpaid family work				
Childhood exposure	0.023	0.074	0.131	0.022
				(0.032)
Panel E:				
Domestic duties				
Childhood exposure	0.028	0.093	0.146	0.032
				(0.054)
Individual controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes
State trends	Yes	Yes	Yes	Yes
Observations	77,737	77,737	77,737	67,765

Notes: Notes: In Panels A-E, the dependent variable is a dummy taking the value of one if an individual's primary activity is regular work, casual work, self-employment, unpaid family work or domestic duties respectively. Columns (1)-(3) show the share of statistically significant results over 1000 randomizations, where the childhood exposure measure is randomized over the entire sample. Column (4) shows the estimates obtained using the synthetic index of childhood exposure over the sample consisting of cohorts born between 1975 and 1985. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. Individual controls include dummy variables indicating if the individual is a female, the first born child and Hindu respectively.

Table 9: Educational controls, education

	Baseline	Predicted educ. attainment	Sub-sample	Parental education
	(1)	(2)	(3)	(4)
Panel A:				
Educ. delay: # of years				
Childhood exposure	0.31***	0.31***	0.36**	0.36**
	(0.079)	(0.079)	(0.15)	(0.15)
Panel B:				
Educ. delay: yes=1, no=0				
Childhood exposure	0.18***	0.18***	0.21***	0.21***
	(0.052)	(0.051)	(0.075)	(0.075)
Individual controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes
Predicted educ. attainment	No	Yes	No	No
Parental education	No	No	No	Yes
Observations	77,737	77,737	32,581	32,581

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. Individual controls include dummy variables indicating if the individual is a female, the first born child and Hindu respectively. In Panel A educational delay is computed as the difference between reported years of schooling and the minimum number of years corresponding to the reported educational attainment. In Panel B the measure of educational delay is given by a dummy variable taking the value of one in the case of an educational delay of at least one year.

Table 10: Educational controls, type of activities

	Regular work	Casual labor	Self-employed	Unpaid family work	Domestic duties
	(1)	(2)	(3)	(4)	(5)
Panel A:					
Baseline					
Childhood exposure	-0.16**	-0.059	-0.12***	0.046	0.18**
	(0.069)	(0.049)	(0.020)	(0.045)	(0.073)
Observations	77,737	77,737	77,737	77,737	77,737
Panel B:					
Predicted educ. attainment					
Childhood exposure	-0.15**	-0.055	-0.12***	0.044	0.18**
	(0.069)	(0.050)	(0.020)	(0.045)	(0.073)
Predicted educ. attainment	Yes	Yes	Yes	Yes	Yes
Observations	77,737	77,737	77,737	77,737	77,737
Panel C:					
Sub-sample					
Childhood exposure	-0.21**	-0.0094	-0.11	0.067	0.12***
	(0.10)	(0.089)	(0.070)	(0.087)	(0.040)
Observations	32,581	32,581	32,581	32,581	32,581
Panel D:					
Parental education					
Childhood exposure	-0.21*	0.00017	-0.11	0.069	0.13***
	(0.11)	(0.088)	(0.074)	(0.087)	(0.039)
Parental education	Yes	Yes	Yes	Yes	Yes
Observations	32,581	32,581	32,581	32,581	32,581
Individual controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes	Yes
State trends	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. Individual controls include dummy variables indicating if the individual is a female, the first born child and Hindu respectively. Panel A shows baseline estimates. Panel B includes the predicted probability of completing the reported level of education. Panel C replicates the baseline specification on the sub-sample for which parental education is available. Panel D controls for parental education.

Table 11: Sensitivity to extreme values of exposure, education

	Baseline	Excluding Orissa	Interaction: Orissa × C_{bd}	Excluding extreme winds
	(1)	(2)	(3)	(4)
Panel A:				
Educ. delay: # of years				
Childhood exposure (C_{bd})	0.31***	0.30***	0.30***	0.20***
	(0.079)	(0.10)	(0.10)	(0.067)
Orissa × C_{bd}			0.016	
			(0.10)	
Panel B:				
Educ. delay: yes=1, no=0				
Childhood exposure (C_{bd})	0.18***	0.16**	0.16**	0.10**
	(0.052)	(0.061)	(0.061)	(0.040)
Orissa × C_{bd}			0.055	
			(0.061)	
Panel C:				
Educ. attainment				
Childhood exposure (C_{bd})	-0.48***	-0.53***	-0.55***	-0.32***
	(0.13)	(0.18)	(0.19)	(0.10)
Orissa × C_{bd}			0.23	
			(0.19)	
Individual controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes
Observations	77,737	75,192	77,737	77,737

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. Individual controls include dummy variables indicating if the individual is a female, the first born child and Hindu respectively. In Panel A educational delay is computed as the difference between reported years of schooling and the minimum number of years corresponding to the reported educational attainment. In Panel B the measure of educational delay is given by a dummy variable taking the value of one in the case of an educational delay of at least one year. In Panel C educational attainment is a categorical variable indicating the reported educational attainment (0=no formal schooling, 1=primary school, 2=middle school, 3=secondary education, 4=above-secondary education). C_{bd} in the interaction term denotes childhood exposure to storms.

Table 12: Sensitivity to extreme values of exposure, type of activities

	Regular work	Casual labor	Self-employed	Unpaid family work	Domestic duties
	(1)	(2)	(3)	(4)	(5)
Panel A:					
Baseline					
Childhood exposure (C_{bd})	-0.16**	-0.059	-0.12***	0.046	0.18**
	(0.069)	(0.049)	(0.020)	(0.045)	(0.073)
Observations	77,737	77,737	77,737	77,737	77,737
Panel B:					
Excluding Orissa					
Childhood exposure (C_{bd})	-0.23***	-0.088	-0.099***	0.069	0.18*
	(0.039)	(0.062)	(0.020)	(0.065)	(0.10)
Observations	75,192	75,192	75,192	75,192	75,192
Panel C:					
Interaction: Orissa $\times$ C_{bd}					
Childhood exposure (C_{bd})	-0.23***	-0.087	-0.10***	0.070	0.18*
	(0.039)	(0.063)	(0.019)	(0.065)	(0.10)
Orissa $\times$ C_{bd}	0.23***	0.092	-0.061***	-0.080	-0.025
	(0.041)	(0.067)	(0.018)	(0.066)	(0.099)
Observations	77,737	77,737	77,737	77,737	77,737
Panel D:					
Excluding extremes winds					
Childhood exposure C_{bd}	-0.13***	-0.059	-0.072***	0.046	0.12**
	(0.041)	(0.041)	(0.021)	(0.035)	(0.056)
Observations	77,737	77,737	77,737	77,737	77,737
Individual controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes	Yes
State trends	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. Individual controls include dummy variables indicating if the individual is a female, the first born child and Hindu respectively. C_{bd} in the interaction term denotes childhood exposure to storms.

Table 13: Alternative measures, education

	Baseline	50, cubic	64, square	64, cubic	All winds	HURRECON
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:						
Educ. delay: # of years						
Childhood exposure	0.31***	0.47***	0.42***	0.50***	0.0033	0.33***
	(0.079)	(0.12)	(0.094)	(0.17)	(0.0079)	(0.090)
Panel B:						
Educ. delay: yes=1, no=0						
Childhood exposure	0.18***	0.28***	0.25***	0.31***	0.0006	0.21***
	(0.052)	(0.052)	(0.049)	(0.070)	(0.0042)	(0.054)
Panel C:						
Educ. attainment						
Childhood exposure	-0.48***	-0.60***	-0.58***	-0.54***	0.006	-0.60***
	(0.13)	(0.20)	(0.15)	(0.21)	(0.013)	(0.15)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	77,737	77,737	77,737	77,737	77,737	77,737

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. Individual controls include dummy variables indicating if the individual is a female, the first born child and Hindu respectively. In Panel A educational delay is computed as the difference between reported years of schooling and the minimum number of years corresponding to the reported educational attainment. In Panel B the measure of educational delay is given by a dummy variable taking the value of one in the case of an educational delay of at least one year. In Panel C educational attainment is a categorical variable indicating the reported educational attainment (0=no formal schooling, 1=primary school, 2=middle school, 3=secondary education, 4=above-secondary education).

Table 14: Alternative measures, type of activities

	Regular work	Casual labor	Self-employed	Unpaid family work	Domestic duties
	(1)	(2)	(3)	(4)	(5)
Panel A:					
Baseline					
Childhood exposure	-0.16**	-0.059	-0.12***	0.046	0.18**
	(0.069)	(0.049)	(0.020)	(0.045)	(0.073)
Panel B:					
50, cubic					
Childhood exposure	-0.16	-0.044	-0.20***	0.036	0.25*
	(0.15)	(0.061)	(0.016)	(0.048)	(0.13)
Panel C:					
64, quadratic					
Childhood exposure	-0.16	-0.045	-0.18***	0.036	0.23**
	(0.12)	(0.056)	(0.0092)	(0.049)	(0.11)
Panel D:					
64, cubic					
Childhood exposure	-0.12	-0.016	-0.23***	0.016	0.25
	(0.16)	(0.049)	(0.042)	(0.030)	(0.16)
Panel E:					
All winds					
Childhood exposure	0.0039	0.0017	-0.011***	-0.0024	0.0012
	(0.0045)	(0.0040)	(0.0037)	(0.0018)	(0.0038)
Panel F:					
HURRECON					
Childhood exposure	-0.16	-0.036	-0.14***	0.031	0.19**
	(0.10)	(0.055)	(0.022)	(0.041)	(0.083)
Observations	77,737	77,737	77,737	77,737	77,737
Individual controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes	Yes
State trends	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. Individual controls include dummy variables indicating if the individual is a female, the first born child and Hindu respectively. In Panel A we find baseline results. In Panel B the measure of childhood exposure is computed using a threshold of 50 knots and a cube. In Panel C the measure of childhood exposure is computed using a threshold of 64 knots and a square. In Panel D the measure of childhood exposure is computed using a threshold of 64 knots and a cube. In Panel E the measure of childhood exposure is computed using all winds. In Panel F the measure of childhood exposure is computed using the HURRECON model, a threshold of 50 knots and a square.

Table 15: Education: gender, rural and urban sub-samples

	Educational delay		Category of schooling completed: yes=1, no=0				
	# of years	yes=1, no=0	No educ.	Primary	Middle	Secondary	Above
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A:							
Male							
Childhood exposure	0.41***	0.23***	0.028**	0.025**	0.038**	-0.014**	-0.077**
	(0.13)	(0.061)	(0.011)	(0.010)	(0.015)	(0.0056)	(0.031)
Observations	39,272	39,272	39,272	39,272	39,272	39,272	39,272
Panel B:							
Female							
Childhood exposure	0.25**	0.14***	0.078***	0.031***	0.025***	-0.040***	-0.095***
	(0.12)	(0.049)	(0.028)	(0.012)	(0.0095)	(0.015)	(0.034)
Observations	38,465	38,465	38,465	38,465	38,465	38,465	38465
Panel C:							
Rural							
Childhood exposure	0.27**	0.19***	0.054**	0.038**	0.059**	0.0030**	-0.15**
	(0.10)	(0.060)	(0.024)	(0.016)	(0.025)	(0.0014)	(0.066)
Observations	42,281	42,281	35,456	35,456	35,456	35,456	35456
Panel D:							
Urban							
Childhood exposure	0.33***	0.10**	0.051**	0.025**	0.020**	-0.045**	-0.052**
	(0.12)	(0.046)	(0.022)	(0.011)	(0.0086)	(0.019)	(0.023)
Observations	35,454	35,454	42,281	42,281	42,281	42,281	42,281
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth-year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. Individual controls include dummy variables indicating if the individual is a female, the first born child and Hindu respectively. In Panel A we present results for the sub-sample of males, and in Panel B, C, and D for females, urban, and rural individuals respectively.

Table 16: Gender, rural and urban sub-samples, type of activities

	Regular work	Casual labor	Self-employed	Unpaid family work	Domestic duties
	(1)	(2)	(3)	(4)	(5)
Panel A:					
Male					
Childhood exposure	-0.29**	-0.025	0.0036	0.094	0.025*
	(0.14)	(0.082)	(0.056)	(0.068)	(0.013)
Observations	39,272	39,272	39,272	39,272	39,272
Panel B:					
Female					
Childhood exposure	-0.079***	-0.071	-0.20***	0.019	0.29**
	(0.028)	(0.083)	(0.044)	(0.036)	(0.12)
Observations	38,465	38,465	38,465	38,465	38,465
Panel C:					
Rural					
Childhood exposure	-0.098*	-0.062	-0.18***	0.093	0.16**
	(0.054)	(0.058)	(0.019)	(0.065)	(0.074)
Observations	42,281	42,281	42,281	42,281	42,281
Panel D:					
Urban					
Childhood exposure	-0.36***	-0.026	0.028	-0.045*	0.24***
	(0.093)	(0.033)	(0.038)	(0.024)	(0.043)
Observations	35,454	35,454	35,454	35,454	35,454
Individual controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Educ.-Birth year FE	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. Individual controls include dummy variables indicating if the individual is a female, the first born child and Hindu respectively. In Panel A we present result for the sub-sample of males, and in Panel B, C, and D for females, urban, and rural individuals respectively.

Figures

Figure 1: Oldest Cohort

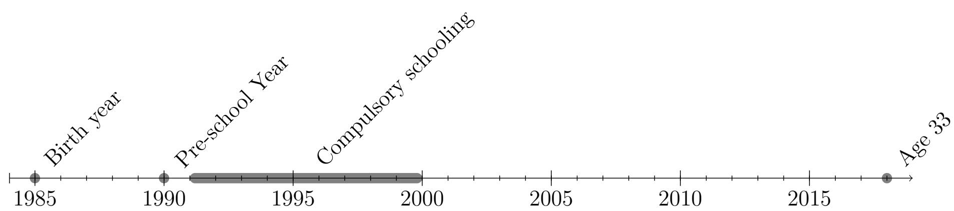
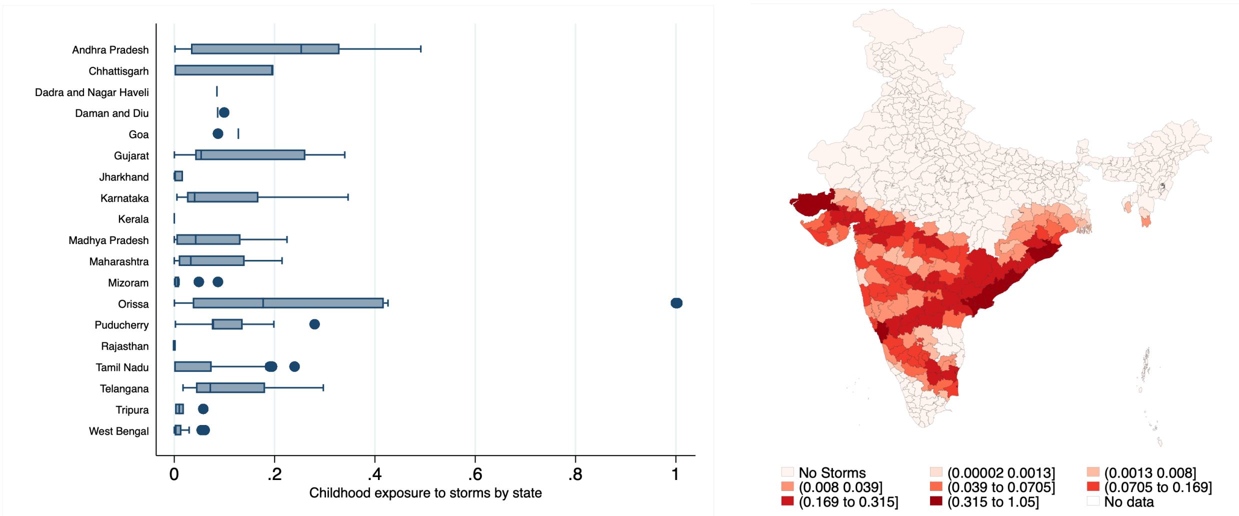
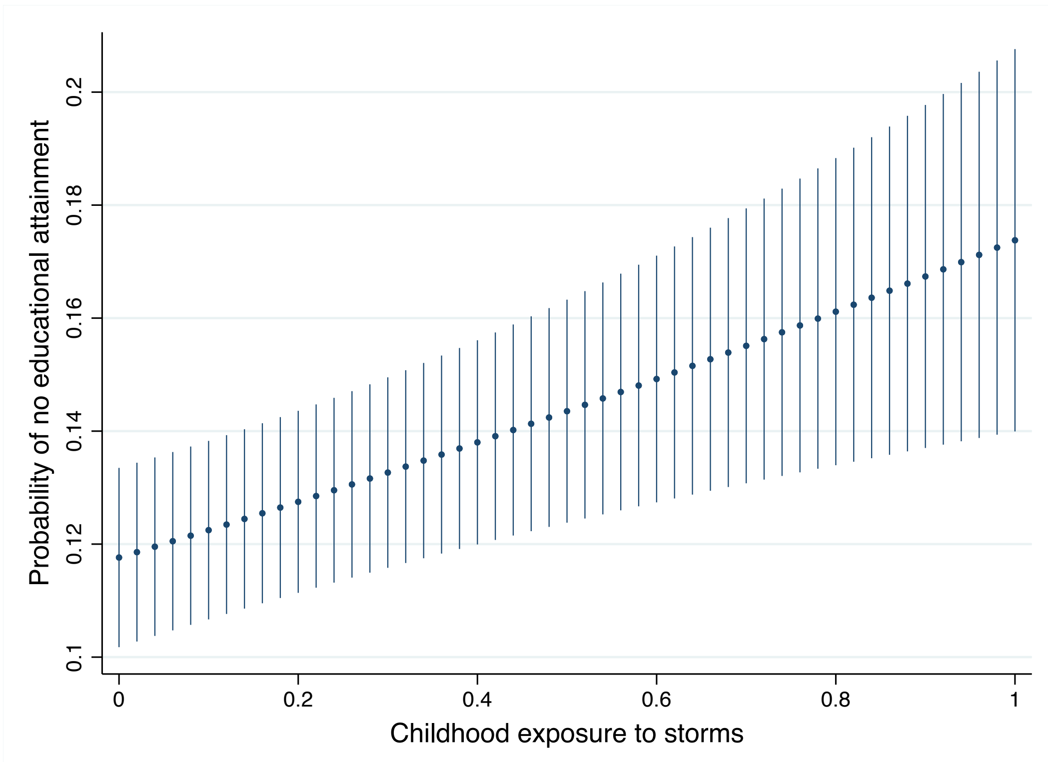


Figure 2: Childhood Exposure to Storms



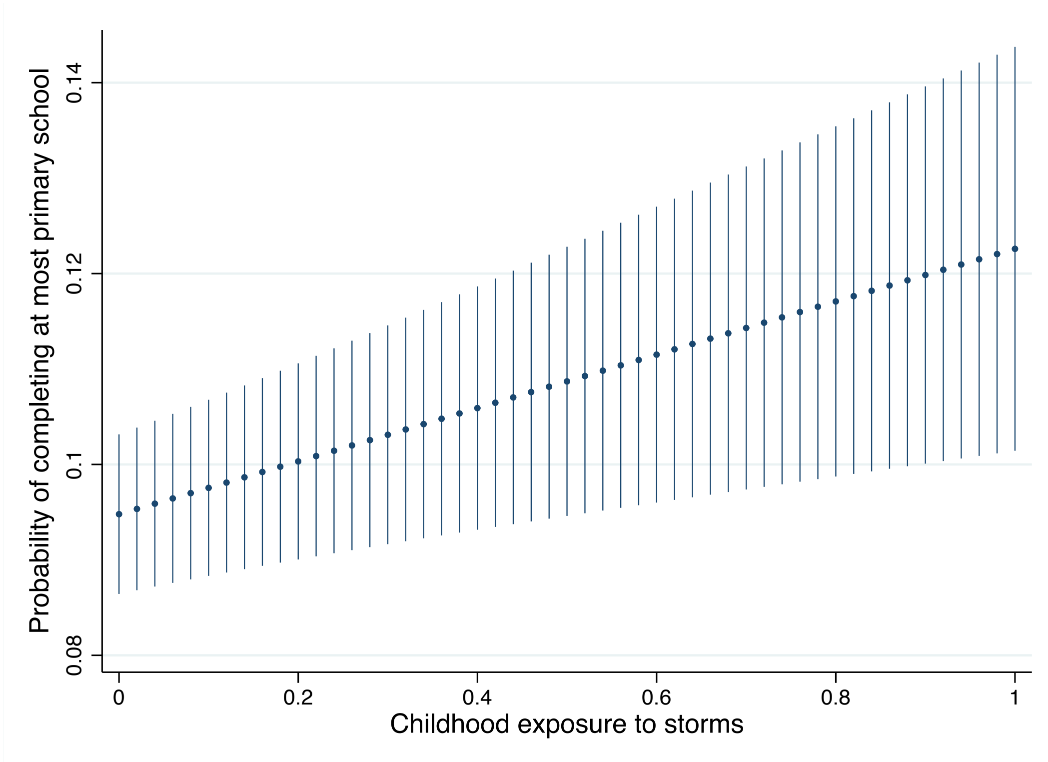
Notes: The boxplots (left panel) describe the measure of childhood exposure to storms for positive values of exposure ($C_{bd} > 0$) and individuals born between 1985 and 1995 by state, listed in alphabetical order. The figure only shows states with positive exposure. The blue line in each box is the median. The lower bound of a box is the first quartile and the higher bound is the third quartile. The end of the left (right) whisker is the 1st percentile (99th percentile). Circles without a box mean that all observations are clustered around the median. The circles outside of the box represent outliers. The map (right panel) provides a visual illustration of childhood exposure to storms across districts for the cohort born in 1987. The darkest shades correspond to districts for which the index of childhood exposure falls above the 90th percentile in the distribution of C_{bd} in 1987. The other shades corresponding to positive exposures contain 15% of the districts each.

Figure 3: Effect of childhood exposure on the probability of no educational attainment



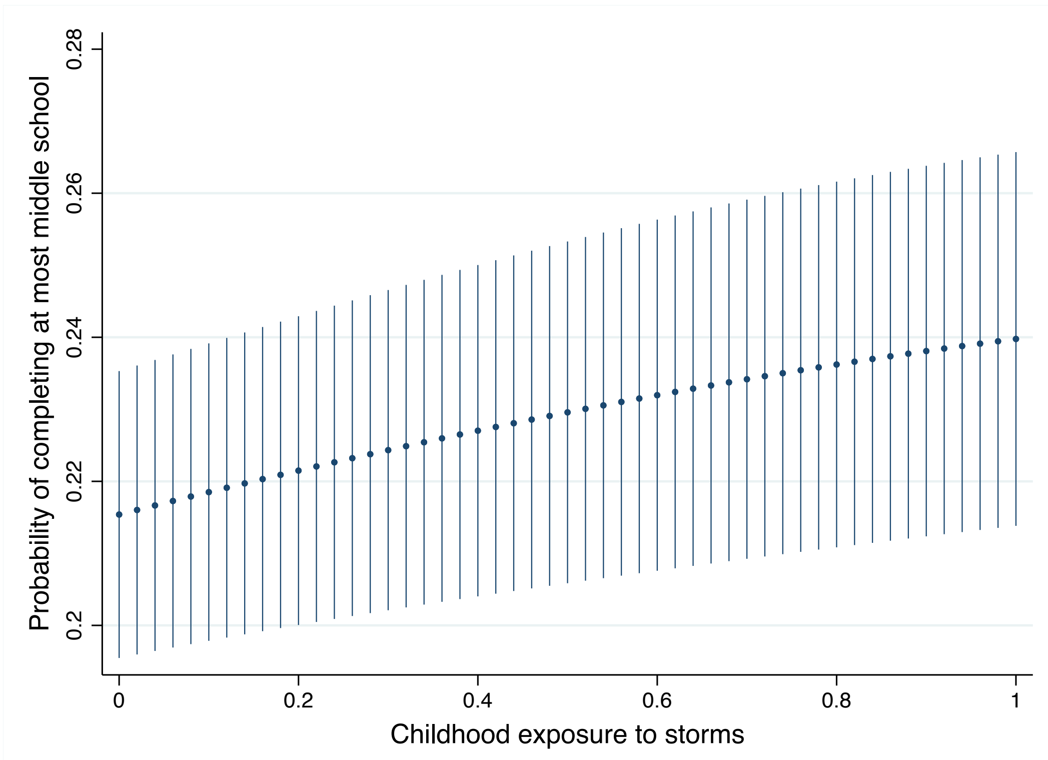
Note: The figure shows the predicted probabilities (and their 95% confidence intervals) of no educational attainment, over the interval [0,1] of exposures, using as benchmark the share of individuals that belong to a given category. Estimates are obtained from Table 4, an ordered logit estimation of educational attainment.

Figure 4: Effect of childhood exposure on the probability of attaining at most primary school



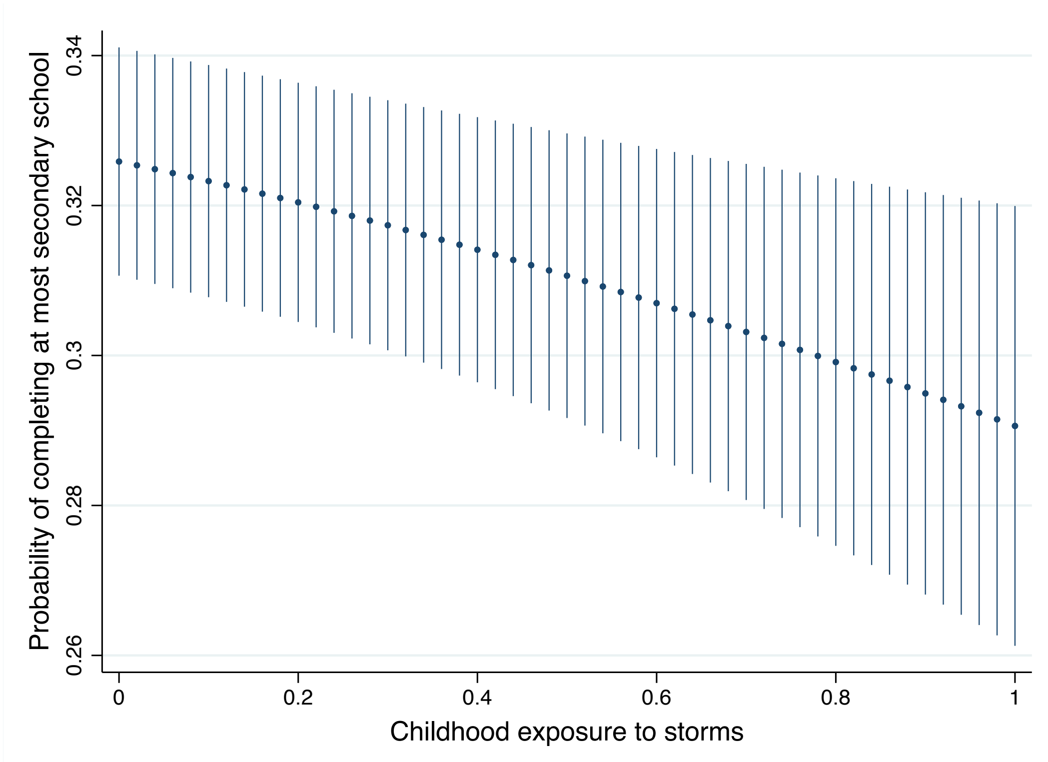
Note: The figure shows the predicted probabilities (and their 95% confidence intervals) of attaining at most primary school, over the interval [0,1] of exposures, using as benchmark the share of individuals that belong to a given category. Estimates are obtained from Table 4, an ordered logit estimation of educational attainment.

Figure 5: Effect of childhood exposure on the probability of attaining at most middle school



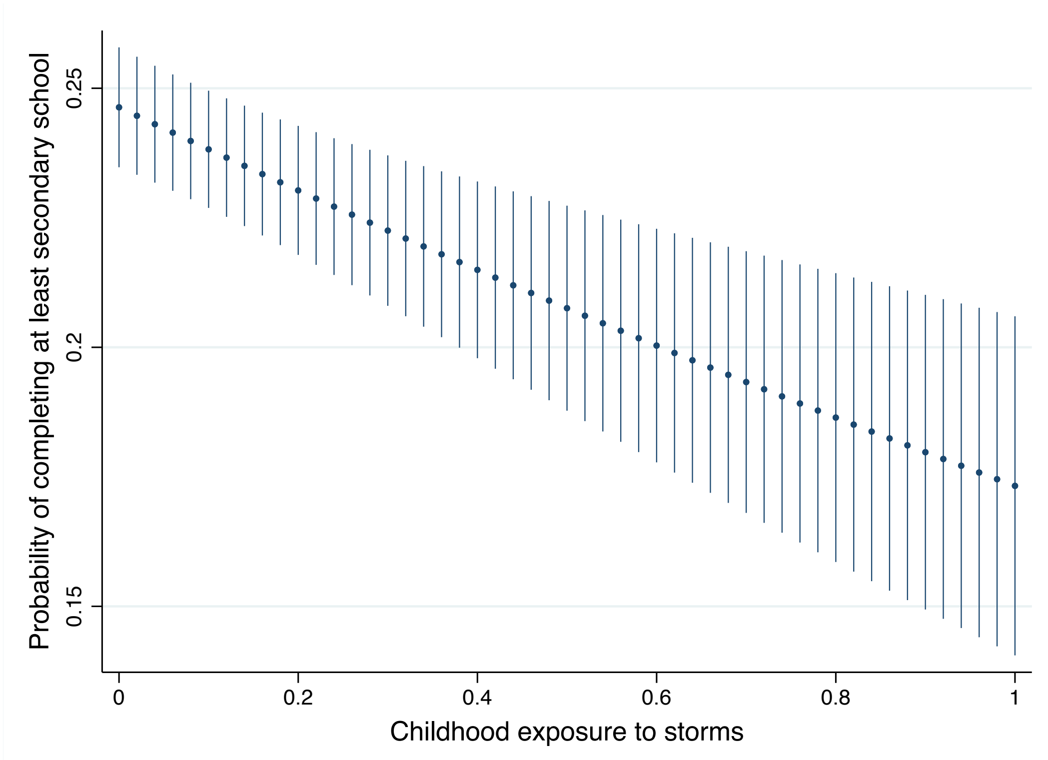
Note: The figure shows the predicted probabilities (and their 95% confidence intervals) of attaining at most middle school, over the interval [0,1] of exposures, using as benchmark the share of individuals that belong to a given category. Estimates are obtained from Table 4, an ordered logit estimation of educational attainment.

Figure 6: Effect of childhood exposure on the probability of attaining at most secondary school



Note: The figure shows the predicted probabilities (and their 95% confidence intervals) of attaining at most secondary school, over the interval [0,1] of exposures, using as benchmark the share of individuals that belong to a given category. Estimates are obtained from Table 4, an ordered logit estimation of educational attainment.

Figure 7: Effect of childhood exposure on the probability of attaining at least secondary school



Note: The figure shows the predicted probabilities (and their 95% confidence intervals) of attaining at least secondary school, over the interval [0,1] of exposures, using as benchmark the share of individuals that belong to a given category. Estimates are obtained from Table 4, an ordered logit estimation of educational attainment.

A Appendix. District exposure and wind speed

A.1 District exposure to tropical storms

In what follows we describe how we construct x_{dt}, the index of exposure to storms of district d in year t. This index is given by the following quadratic specification:

$$x_{dt} = \sum_{h \in H_t} \frac{(w_{dh} - 50)^2}{(w^{max} - 50)^2} \quad \text{if} \quad w_{dh} > 50, \tag{3}$$

where H_t is the set of storms in year t and w_{dh} is the maximum wind speed associated with storm h and to which district d was exposed. We describe the construction of w_{dh} below. The term w^{max} denotes the maximum wind speed observed over the entire sample. In order to capture the force exerted by winds on structures, we assume a quadratic functional form between district exposure to storms and winds, as in Yang (2008) and Pelli & Tschopp (2017).[20] Given the poor quality of construction materials, infrastructures and buildings in India are already vulnerable at low wind intensities. For these reasons, we focus on a threshold of 50 knots, as in Emanuel (2005), as opposed to 64 knots – the threshold for a category 1 cyclone according to the Saffir-Simpson scale. By definition, $x_{dt} \in (0, \mid H_t \mid)$, with a value of 0 indicating zero district exposure to storms (i.e. winds in district d are below the threshold limit) and with $\mid H_t \mid$ indicating the number of elements (storms) in set H_t.

A.2 Wind speed at the district level

A.2.1 Baseline: the Rankine-combined formula (Deppermann, 1947)

We now turn to the construction of w_{dh}, i.e. the maximum wind speed associated with storm h in district d. The variable is constructed using data from the National Oceanic and Atmospheric Administration (NOAA) Tropical Prediction Center and specifically using storms' best tracks in the North Indian and South Indian basins over the period 1990-2010. Best tracks contains the full history of each storm, with information at 6-hours intervals on the latitude, longitude, date and wind speed at the eye of each storm.

We first linearly interpolate storms' best tracks at every kilometre and obtain, for each interpolated kilometre, a landmark k with a set of coordinates and e_k, the windspeed at the eye of the storm. For each district that falls in the vortex associated with a landmark we use the Rankine-combined formula (Deppermann, 1947) and compute winds at the district's

[20] In Section 4.4 we experiment with a variety of alternative specifications of district exposure to storms.

centroid. The formula describes wind fields in the following way:

$$\begin{aligned} w_{dk} &= e_k \cdot \left(\frac{D_{dk}}{26.9978}\right) \text{ if } D_{dk} \leq 26.9978 \\ w_{dk} &= e_k \cdot \left(\frac{26.9978}{D_{dk}}\right)^{0.5} \text{ if } D_{dk} > 26.9978, \end{aligned} \tag{4}$$

where D_{dk} is the distance between the centroid of district d and landmark k. The number 26.9978 corresponds to Simpson and Riehl radius of maximum wind speed in knots, i.e. the distance between the eye and the point where wind reaches its maximum speed.[21] Hence, according to this formula, winds first increase exponentially up to a maximum and then, decrease rapidly. Finally, we obtain one measure of windspeed per district and storm by retaining the maximum windspeed to which a district was exposed:

$$w_{dh} = \max_{k \in H_t}\{w_{dk}\}.$$

A.2.2 Alternative specification: the HURRECON model (Boose et al., 1994)

The HURRECON model (see Boose et al., 1994, 2001, 2004) describes sustained wind velocity at any point within a cyclone's vortex using information on the track, size, intensity, and cover type (land or water) of a hurricane. Using this model, we compute sustained wind velocity at each district centroid as follows:[22]

$$w_{dk} = F\left[V_k - S(1-\sin T)\frac{V_f}{2}\right]\left[\left(\frac{R_m}{R}^{B} e^{1-\left[\frac{R_m}{R}\right]^B}\right)\right]^{1/2} \tag{5}$$

where F is a scaling parameter capturing the effect of friction set at 0.8, since all the point of interest to us are situated on land (this parameter is usually set equal to 1 for points over water and to 0.8 for points over land); V_k is the wind velocity at the eye at landmark k, which we linearly interpolate from the best track data; S is a scaling parameter for the asymmetry due to the forward motion of the storm, set to 1 (i.e. peak wind speed on the right side minus peak wind speed on the left side equals the forward velocity of the hurricane – V_f, as defined in Boose et al., 2001); T is the clockwise angle between the forward path of the hurricane and a radial line connecting the eye of the hurricane to the population-weighted

[21]In reality, each cyclone has a different radius of maximum windspeed, which is calculated using the difference in barometric pressure between the center and the outskirts of the storm. Unfortunately, cyclone data are characterized by a high number of missing data when it comes to barometric pressure. For this reason we decided to follow Simpson & Riehl (1981) and Hsu & Zhongde (1998) and apply the average radius of maximum windspeed, 50 km, to all the cyclones considered in this paper.

[22]Velocity and wind direction are measured relative to the surface of the Earth, and angles are measured in degrees.

centroid of a county; V_f is the forward velocity of the hurricane, i.e. the speed at which the hurricane is moving forward; R_m is the radius of maximum winds, set as in the previous approach at 26.9978; R is the radial (or Euclidean) distance from the center of the hurricane to the population-weighted centroid of a county; and B is a scaling parameter controlling for the shape of the wind profile curve (usually included between 1.2 and 1.5, and set at 1.35). The parameters of this equation, adapted from Holland's equation for the cyclostrophic wind (Holland, 1980), are set following Boose et al. (2004) that parameterized and validated the model.