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**Working Paper**

## Storms, early education, and human capital

ADB Economics Working Paper Series, No. 743

**Provided in Cooperation with:**

Asian Development Bank (ADB), Manila

*Suggested Citation:* Pelli, Martino; Tschopp, Jeanne (2024) : Storms, early education, and human capital, ADB Economics Working Paper Series, No. 743, Asian Development Bank (ADB), Manila, <https://doi.org/10.22617/WPS240462-2>

This Version is available at:

<https://hdl.handle.net/10419/305430>

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# STORMS, EARLY EDUCATION, AND HUMAN CAPITAL

*Martino Pelli and Jeanne Tschopp*

**NO. 743**

October 2024

**ADB ECONOMICS  
WORKING PAPER SERIES**

ADB Economics Working Paper Series

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No. 743 | October 2024

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ISSN 2313-6537 (print), 2313-6545 (PDF)  
Publication Stock No. WPS240462-2  
DOI: <http://dx.doi.org/10.22617/WPS240462-2>

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## ABSTRACT

This paper explores how school-age exposure to storms impacts the education and primary activity status of young adults in India. Using a cross-sectional cohort study based on wind exposure histories, we find evidence of a significant deskilling of areas vulnerable to climate change-related risks. Specifically, our results show a 2.4 percentage point increase in the probability of accruing educational delays, a 2 percentage point decline in post-secondary education achievement, and a 1.6 percentage point reduction in obtaining regular salaried jobs. Additionally, our study provides evidence that degraded school infrastructure and declining household income contribute to these findings.

**Keywords:** climate change, storms, education, human capital

**JEL codes:** I25, O12, Q54

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We are grateful to the Social Sciences and Humanities Research Council (SSHRC, grant number 039367), and the Swiss National Foundation (SNF, grant number 100018\_192553) for their financial support. We thank, without implicating them, Teevrat Garg, Blaise Melly, Ben Sand, Eric Strobl. All remaining errors are ours.

# 1 Introduction

Low- and middle-income nations confront an inordinately elevated risk of catastrophes, as they are both more exposed to climate-related hazards and possess diminished resilience (Dell et al., 2014).<sup>1</sup> This deficiency in resilience renders children particularly susceptible, a concerning fact given that extreme weather events are anticipated to escalate due to climate change (IPCC, 2023; Emanuel, 2021). Our study delves into the enduring adverse consequences of school-age exposure to tropical storms and cyclones on education and pursuits during early adulthood in India. We scrutinize potential causal pathways spanning the school years, a crucial phase demonstrated to shape lifetime earnings (e.g., Oreopoulos, 2007; Angrist and Krueger, 1991).

In this paper, we begin by evaluating the ramifications of storm exposure during school-age on educational outcomes in both the short and long run, using a cross-sectional cohort study based on the 2018 release of the Periodic Labour Force Survey (PLFS). We assess educational outcomes by considering years of schooling and the highest educational level achieved. To capture the cumulative effects of storms throughout school years, we devise a continuous treatment that aggregates wind exposure histories for each district and cohort born between 1985-1995. Contrasting with other environmental impact studies (e.g., Ebenstein et al., 2016; Deuchert and Felfe, 2015), our focus centers on long-term exposure to storms, which enables us to gauge the consequences of climate change as opposed to mere weather variability. Our results indicate that long-run storm exposure during school years causes educational delays, and exerts significant lasting effects on educational attainment and career choices in early adulthood.

An average storm exposure during school years yields an increased likelihood of experiencing an educational delay by 2.4 percentage points, which translates to a 7.25% rise in the fraction of delayed individuals. We also observe a notable decline in the number of individuals attaining post-secondary education. An average exposure leads to a decrease in this probability by 2 percentage points, corresponding to a 7.35% reduction in the proportion of individuals with this level of education.

Furthermore, we identify detrimental effects of storm exposure on labor market outcomes. An average school-age exposure results in a 1.6 percentage-point decrease

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<sup>1</sup>Developing countries' reduced resilience stems from factors such as insufficient infrastructure, weak social safety nets, market failures like absent credit and insurance markets, and an absence of effective early warning systems and comprehensive disaster risk management. Refer to Hallegatte et al. (2020) for a comprehensive review of why economically disadvantaged populations are disproportionately impacted by natural hazards and related disasters.

in the proportion of individuals employed as regular workers, while concurrently causing a similar increase in the share of individuals occupied with domestic duties. These statistics equate to an 8% reduction in the share of individuals employed as regular workers and a 4.8% rise in the share of individuals involved in domestic duties. Additionally, we observe that an average exposure incurs a 3.9% reduction in hourly wages. Severe tropical storms, once uncommon but now progressively prevalent due to climate change, amplify all these findings.

Our results remain robust through a series of checks, encompassing a falsification test and alternative specifications of school-age exposure to storms. We also demonstrate that our estimates are not influenced by early-life exposure to storms or other environmental factors such as precipitation and temperature during ages 5-15.

These findings show that prolonged exposure to extreme environmental shocks during school years may contribute to the progressive deskilling of regions more susceptible to climate-change related risks. This degradation of skills will exacerbate inequalities, underscoring additional costs of climate change that have not been extensively examined thus far. To devise appropriate mitigating policies, we must comprehend the underlying pathways that drive our primary results.

In the second part of the paper, we employ supplementary datasets (Consumer Pyramids and District Information System for Education) to probe the short-term mechanisms by which storms may influence long-term educational outcomes, focusing on their impact on household income and school infrastructure damage. Our findings reveal the presence of both demand and supply shocks in the schooling sector. Firstly, using panel local projections, we find that following an average storm, household income progressively declines, reaching levels approximately 8% below pre-disaster incomes 10 months post-shock. Secondly, we show that school closures significantly escalate in the aftermath of an average storm, with the proportion of closed schools surging by 7.4% within two years. However, the impact of an average exposure on the proportion of well-maintained classrooms and reliable electricity availability at schools is modest, albeit statistically significant. Lastly, we observe a reduction in primary school attendance and a decline in academic performance among middle school students, which is consistent with a negative income shock and a reduction in schooling demand.

These findings offer indirect evidence that the enduring consequences of storms on education extend beyond the mere physical damages to schools, highlighting the significance of broadening post-disaster policies beyond reconstruction efforts and enhancing social safety nets (see Deryugina, 2017, for the importance of social safety nets in developed countries). Specifically, our results propose that financial transfers

ought to be paired with policies advocating sustained education and post-disaster school enrollment, potentially by conditioning cash transfers on school attendance. Furthermore, social policies, such as unemployment insurance, could prove instrumental in fostering resilience and risk management in urban areas, which are observed to be particularly vulnerable following storms.

Our paper enhances the body of research examining the economic consequences of environmental disturbances during childhood in developing nations. Thus far, the literature on this subject has concentrated on three primary analytical approaches. Firstly, a significant portion of research investigates the short-term effects of concurrent shocks (e.g., Spencer et al., 2016; Björkman-Nyqvist, 2013; Jensen, 2000).

Secondly, another branch of literature explores the deleterious repercussions of environmental shocks experienced in utero or early life (up to 4 years old). These shocks have been linked to a range of short and long-term outcomes, including various aspects of adult life, such as health, education, wealth, and offspring outcomes (e.g., Chang et al., 2022; Hyland and Russ, 2019; Rosales-Rueda, 2018; Akresh et al., 2017; Dinkelman, 2017; Maccini and Yang, 2009).

The third analytical approach delves into the long-term consequences of short-term incidents occurring later in life, beyond ages 0-4. This line of research typically focuses on either singular events, as illustrated by Shidiqi et al. (2023), Groppo and Kraehnert (2017), and Deuchert and Felfe (2015), or on short-term occurrences coinciding with crucial moments for individuals, such as high-stakes exam days, as seen in studies by Park (2022) and Ebenstein et al. (2016).

Our paper makes two main contributions to this body of research. First, by examining long-term exposure, our findings indicate a potential progressive deskilling in areas more susceptible to climate change-related risks – an additional cost associated with climate change that has not yet been emphasized in the literature. Second, we concentrate on the impact of these risks during the school-age years, highlighting the significance of this period in shaping human capital formation. Recognizing the distinction between infancy and school-age exposure is crucial, as the mechanisms through which adverse shocks affect long-term education likely differ. While in utero disruptions are known to influence human capital via children's health, disasters during school-age years are more likely to impact long-term educational attainment through changes in household income and schooling infrastructure.

Our study is most closely related to Deuchert and Felfe (2015) and Shidiqi et al. (2023). Deuchert and Felfe (2015) investigates the short- and long-term effects of Super Typhoon Mike on educational outcomes in Cebu Island, Philippines. The study reveals a



negative and enduring impact on education, alongside a gradual reallocation of funds from education to reconstruction efforts. We build upon these findings by concentrating on long-term exposure through a continuous measure rather than a binary damage indicator, offering valuable insights into the labor market outcomes of affected children in early adulthood and presenting an in-depth analysis of the factors underlying long-term educational delays.<sup>2</sup> In a similar vein, Shidiqi et al. (2023) examines the aftermath of a potent earthquake in Yogyakarta, Indonesia, in 2006, observing its repercussions on educational delays and attainments. Like cyclones, the earthquake instigated educational setbacks and decreased the likelihood of fulfilling mandatory education. However, contrary to cyclones, earthquakes appear to hinder education predominantly through the destruction of school facilities.

Lastly, our study contributes to the literature by conducting an extensive examination of the mechanisms connecting school-age exposure to storms with long-term human capital degradation, specifically focusing on income and schooling infrastructure channels. To our knowledge, these channels have predominantly been explored independently (see Baez et al., 2010, for a review).

Our findings on the income channel relate to the body of research that reveals the negative impact of economic recessions on education (e.g., Stuart, 2022). The majority of studies addressing this channel have employed difference-in-difference estimations. In contrast, local projections offer advantages in cases of multiple treatments, making them a suitable alternative to the difference-in-difference approach for addressing dynamic treatment effects. To the best of our knowledge, few papers have utilized this methodology to study environmental shocks.<sup>3</sup>

Although it is undeniable that disasters lead to infrastructure disruptions and damages (see e.g. Hallegatte et al., 2019), there are limited studies specifically estimating the effects of disasters on educational facilities. Our results indirectly highlight the importance of adequate school infrastructure for long-term education and labor

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<sup>2</sup>In a related study, Shah and Steinberg (2017) employs rainfall as a proxy for wages in rural India, demonstrating that higher rainfalls, which correlate with higher wages, influence human capital accumulation differently depending on the child's age. This research is part of a broader literature on the impact of economic downturns on education (e.g., Stuart, 2022). While the study also scrutinizes the school-age period, its primary objective differs from ours, as it seeks to evaluate how favorable economic conditions alter the opportunity cost of schooling and, consequently, the incentives for children to attend school.

<sup>3</sup>Local projections have been first proposed by Jorda (2005). They have been used widely in empirical macroeconomics and more recently in the context of environmental economics (see e.g. Barattieri et al., 2023; Roth Tran and Wilson, 2023; Naguib et al., 2022). As discussed by Dube et al. (2023), they can be used as an alternative methodology to deal with the issue of dynamic treatment effects that arises with the difference-in-difference approach in the case of multiple treatments.

market outcomes, thus contributing to the literature on the effects of constructing new schools in developing countries (Damon et al., 2018; Glewwe and Kremer, 2006). For a recent study that focuses on the construction of disaster-resistant schools, see Herrera-Almanza and Cas (2021). The authors demonstrate that building typhoon-resistant secondary schools in the Philippines can help alleviate the long-term detrimental effects of extreme weather events on the education and labor market outcomes of school-age children.

The remainder of the paper is organized as follows. Section 2 describes the PLFS data and the construction of our main measure of school-age exposure to storms. Section 3 examines the long-term effects of school-age exposure to storms on educational delays, educational attainment and the type of labor market activity performed by young adults. The main results are presented in Sections 3.1-3.3 and robustness tests in Section 3.4. Finally, Section 4 discusses the channels through which storms may affect education in the long-run and Section 5 concludes.

## **2 Data**

In our primary empirical analysis, we rely on two data sources: *i*) the 2018 release of the PLFS, which provides measurements for educational delays and labor market variables, and *ii*) tropical storm data from the NOAA, employed to construct a school-age exposure index for 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. This survey offers a range of information on individual characteristics, including age, gender, educational level, and the number of years spent in school.

In India, children typically begin school at the age of 6, with compulsory schooling lasting for 9 years, until they reach 15 years old. Table D.1 in the online appendix outlines the Indian schooling system, including the various pathways to higher education.<sup>4</sup> Column (1) displays the number of years required to complete each individual category of schooling. For graduate and postgraduate levels, the figures

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<sup>4</sup>For more details on the Indian educational system and its comparison to other systems, please visit: <https://wenr.wes.org/2018/09/education-in-india>. The Appendix is available at <http://dx.doi.org/10.22617/WPS240462-2>.

correspond to the modal duration across disciplines. Column (2) presents the total cumulative number of years needed to complete any given level of education. For example, middle school spans 3 years, and by 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 data on the highest level of education completed as well as whether an individual earned a diploma or certificate, enabling us to infer the path of those who pursued higher education.

For each individual, we measure educational delay by comparing the actual number of years spent in formal education to the minimum number of years required within the schooling system to attain the reported level of education. For instance, consider an individual who reports seven years of formal schooling but has only completed primary school. This individual would have a two-year educational delay, stemming from either grade repetition or dropping out from a higher educational level (middle school, in this specific example).<sup>5</sup> Consequently, our analysis sheds light on whether storms increase educational delays, but it does not offer insights into the likelihood of repeating grades versus dropping out of school. As an alternative, we also construct an indicator variable for educational delay, which takes the value of 1 for individuals with positive delays and 0 otherwise.

The PLFS supplies information on individuals' primary activity status.<sup>6</sup> This data reveals whether an individual's primary engagement is in 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, unpaid family work (e.g., working in the family business/farm without pay), or domestic duties (e.g., collecting vegetables, firewood, cattle feed, sewing, etc.). Additionally, the survey includes labor market indicators such as hours worked and earnings; however, this information is only available for individuals engaged in paid activities and who report being part of the labor force.

The data also informs on individuals' district of residence, which, when combined with their age, allows us to create a unique measure of school-age exposure to storms that varies by birth-year cohort and district. As we discuss further below, our measure is a continuous treatment that considers the intensity of the storms experienced by children from a specific cohort living in a particular district throughout their schooling years.<sup>7</sup>

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<sup>5</sup>Although it would be intriguing to differentiate between the two types of delays, the PLFS lacks sufficient information to do so. In Section 4.3, we utilize auxiliary data to investigate the impact of storms on school attendance and performance.

<sup>6</sup>Details and definitions can be found here (p.35): [https://www.mospi.gov.in/sites/default/files/publication\\_reports/Annual\\_Report\\_PLFS\\_2018\\_19\\_HL.pdf](https://www.mospi.gov.in/sites/default/files/publication_reports/Annual_Report_PLFS_2018_19_HL.pdf)

<sup>7</sup>Given the very small proportion of individuals migrating outside of their birth district (see, for instance, Munshi and Rosenzweig, 2016; Edmonds et al., 2010; Topalova, 2010), we assume that individuals

We select the age of an individual upon completing postgraduate education (master's degree) as the benchmark for early adulthood. Without educational delays, obtaining a postgraduate degree takes 17 years (path 4 in Table D.1 of the Online Appendix). Children typically start school at the age of 6, so early adulthood is reached at the age of 23. Consequently, the youngest cohort considered in this paper was born in 1995 and should have completed compulsory schooling in 2010. The oldest cohort examined is determined by the availability of WMO-sanctioned North Indian Ocean data, which only goes back to 1990 (Knapp et al., 2010). As a result, the oldest cohort we consider was born in 1985 and is 33 years old in 2018 (refer to Figure E.1 in the Online Appendix for an illustration).

Our analysis concentrates on individuals born between 1985 and 1995 (i.e., cohorts aged 23-33 in 2018) and storms occurring between 1990 and 2010. We focus on individuals who have received at least some formal education. This includes those who attended school but did not complete primary education, while excluding individuals who were never enrolled in the schooling system. As a result, our analysis does not capture the effects on the most vulnerable children (e.g., those belonging to scheduled castes). Moreover, we exclude individuals who dropped out of school *and* were exposed to tropical storms after leaving school; individuals who were never exposed or were exposed before dropping out are included in the sample. This ensures that our storm measure accurately reflects cohort-district exposures while attending school. This restriction affects fewer than 1% of the observations aged 23-33 in 2018.

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completed their compulsory schooling in the same district where they reside in 2018. Using auxiliary dataset, we also compute our own out-of-district migration figures. Using the 64th round of the National Sample Survey (NSS) for the years 2007-2008, we estimate that only 0.65% of households had migrated out-of-district permanently. As an alternative data source, we use the Pyramids Dx (panel People of India) dataset, a panel survey of individuals belonging to approximately 200,000 households interviewed three times a year, to compute migration figures for the period 2020-2021 (see Section 4.1 for more details on the Pyramids Dx data). To be consistent with our final estimation sample, we focus on individuals aged 5-33 years old. We find that less than 1.75% of individuals moved across districts from one year to another. This number is higher than our NSS estimate as it includes both permanent and temporary migration.

Using the same database, we also present additional evidence suggesting that any selection bias from endogenous migration responses to disaster shocks is likely to be small. We estimate the probability of impacted individuals migrating out of their district of residence within a year following the shock. As described in Section A of the Online Appendix, we find that storm exposure does not increase the likelihood of moving to another district. To the contrary, our estimates indicate that individuals tend to migrate less after a storm. Hence, although we cannot completely rule out the possibility of measurement error in exposure for some individuals, the low levels of migration suggest that any such error is likely to be small. Consequently, we believe that we reasonably assume that the current district of residence is a good proxy for the place of residence during school age.

## 2.2 School-age Exposure to Tropical Storms

To investigate the impact of school-age exposure to storms on long-term education levels and labor market outcomes, we create an index based on storm wind speeds, which varies by birth-year cohort and district.<sup>8</sup>

We use wind speed as the primary indicator of storm hazards. Wind speed not only correlates with real water-related risks such as wet landslides, floods, and storm surges but has also served as an incidence measure in numerous prior studies (e.g., Hsiang, 2010; Hsiang and Jina, 2014). As discussed in Hsiang and Jina (2014), wind speed stands out as the critical dimension for assessing destruction because the materials used in the construction of durable capital structures tend to fail at a critical pressure threshold.

Our measure accounts for storms occurring during the first nine years of compulsory schooling (starting at age six) and the pre-school year. By including the pre-school year, we accommodate children born early in the year who may begin school earlier. Specifically, school-age exposure to storms is computed as follows:

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

where  $b$  represents a birth-year cohort,  $d$  indicates a district, and  $t$  denotes a year. The variable  $x_{dt}$  represents a yearly district exposure to storms index, taking into account the force exerted by winds on physical structures. It is determined by the following quadratic specification:

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

where  $H_t$  represents the set of storms in year  $t$ , and  $w_{dh}$  is the maximum wind speed associated with storm  $h$  to which district  $d$  was exposed. We compute  $w_{dh}$  using a wind field model, as detailed in Section B of the Online Appendix. The term  $w^{max}$  refers to the highest wind speed observed throughout the entire sample. Normalizing the measure using  $w^{max}$  enables us to rescale it such that  $x_{dt} \in (0, |H_t|)$ , where a value of 0 indicates zero district exposure to storms (i.e., winds in district  $d$  are below the threshold limit), and  $|H_t|$  represents the number of elements (storms) in set  $H_t$ . We assume a quadratic

<sup>8</sup>As of August 1, 2022, India was composed of 766 districts. With a surface area of approximately 3.3 million square kilometers, the average district spans roughly 4,300 square kilometers or a square of 65 x 65 kilometers. Based on the most recent data from the World Development Indicators, India had a population density of 469.7 people per square kilometer of land area in the year 2020, equating to an average of approximately 2 million people per district.

damage function in order to capture the force exerted by winds on structures, following e.g., Pelli and Tschopp (2017).<sup>9</sup> Considering the poor quality of construction materials in India, infrastructures and buildings are vulnerable even at low wind intensities. Thus, we focus on a threshold of 50 knots, as in Emanuel (2005), rather than 64 knots – the threshold for a category 1 cyclone according to the Saffir-Simpson scale.

Consider, for example, the timeline of the oldest cohort (born in 1985). As depicted in Figure E.1 of the Online Appendix, the index of school-age exposure to storms,  $C_{bd}$ , sums district exposure to storms from 1990 (the pre-school year) up to 2000; i.e.,  $C_{1985,d} = \sum_{t=1990}^{t=2000} x_{dt}$ . Variation in the index within birth-year cohorts across districts arises due to differences in wind speed intensities at various locations during the same storm, while some areas remain sheltered. Incorporating wind speed provides a continuous treatment that varies spatially, offering significant advantages in terms of identifying variation compared to using dummy variables 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). Variation within districts across birth-year cohorts results from different cohorts being subject to distinct storms over the course of compulsory schooling.

In our sample, 70.5% of individuals have an exposure index of zero, indicating no exposure to storms during the compulsory schooling period. On the other hand, 22.5% experienced only one storm, and 6% were hit by two storms between ages 5-15. A mere 1% of individuals encountered three or four storms during this time. Among those with a positive exposure, 75.5% were affected by a single storm, 20.5% experienced two storms, and 4% faced three or four storms.

Figure E.2 in the Online Appendix displays the measure of school-age exposure to storms at the state level for our sample.<sup>10</sup> Children residing in 28 out of the 35 Indian states experienced tropical storms between the ages of 5 and 15. Notably, the boxplots exhibit significant variation in school-age exposure to storms both within and across states, with Andhra Pradesh, Gujarat, Maharashtra, Odisha, and Telangana displaying the highest median exposures.

Table D.3 in the Online Appendix presents summary statistics for the main variables used in the paper. Panel A shows that 20,750 out of the 70,003 individuals in the sample, approximately 30%, experienced exposure to storms throughout compulsory

<sup>9</sup>In Section 3.4, we demonstrate that our baseline estimates remain robust under various alternative specifications of district exposure to storms. Specifically, we employ a cubic damage function, conduct robustness tests using other wind field models, and suggest a different aggregation of exposures across years of compulsory schooling.

<sup>10</sup>Only states with positive exposures are included.

schooling. Individuals accumulate up to 6 years of delay, with 33% reporting at least a one-year delay (Panel B). Panel C displays summary statistics of dummy variables for the highest category of schooling completed by individuals in our sample, excluding those with no formal schooling. Approximately 3% of the sample did not complete primary school (but received some primary education), 10% completed at most primary school, 24% middle school, and 37% secondary school. The remaining individuals received education beyond secondary level, obtaining either a diploma (*certificate course*) or a post-/graduate degree. Binary variables for the primary activity status of individuals in our sample, shown in Panel D, indicate that the largest share, 33%, engages in domestic duties, while only 20% have a formal job with a regular employment contract and salary. For the subsample with available data, individuals earn, on average, a log hourly wage of 3.7 rupees and work 54 hours per week (Panel E). The bottom of the table lists individual controls used in the analysis. The sample is nearly evenly split across genders, predominantly comprising Hindu households, with 30% being first-born individuals.<sup>11</sup>

### 3 Educational Delay and Long-term Effects

Below we examine the long-term effects of school-age exposure to storms on educational delays, educational attainment and the type of labor market activity pursued by young adults.

#### 3.1 Educational Delay

**Specification.** To assess the impact of storm exposure during school years on educational delays, we run the following specification:

$$Y_i = \alpha_0 + \alpha_1 C_{bd} + \mathbf{X}_i' \boldsymbol{\beta} + \delta_d + \delta_b + \tau_d + \epsilon_i, \quad (3)$$

where  $i$  denotes an individual. While we drop subscripts where possible, note that  $i = (b, d)$ , where  $b$  denotes a cohort and  $d$  the district. The variable  $Y_i$  measures educational delay, either as the number of years of delay or as a dummy variable indicating at least one year of delay.  $C_{bd}$  is our cohort-district-specific measure of exposure to storms between the ages of 5-15.  $\mathbf{X}_i'$  is a vector of individual characteristics, including dummy variables

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<sup>11</sup>The set of controls that we can use is restricted because most household- and individual-level controls are likely to be affected by storms and would, therefore, be bad controls.

indicating if the individual is a female, a first-born child and Hindu respectively.<sup>12</sup>  $\delta_d$  and  $\delta_b$  are sets of district FE and cohort FE, respectively. Although our data are cross-sectional, we introduce a district-specific linear relationship across cohorts,  $\tau_d$ , that accounts for differential trends in district-level education policies and regional disparities in economic growth. Finally,  $\epsilon_i$  is the error term.<sup>13</sup>

Incorporating district fixed effects in all our model specifications provides us with a measure of storm exposure that is conditionally exogenous. This allows us to account for the possibility that certain areas, such as coastal regions, may be more susceptible to hazards. Earlier studies have demonstrated that the occurrence of a cyclone does not provide any information on the probability of observing a similar event in the same location in the future (see e.g., Pielke et al., 2008; Elsner and Bossak, 2001). Therefore, it is impossible to predict the occurrence and exact path of storms, conditional on location. By using district FE, we are left with random realizations of storms, purging any correlation between locational economic decisions and the local distribution of storm exposure.<sup>14</sup> Furthermore, our use of winds exclusively to construct exposures can be considered exogenous. Although other storms' hazards include floods and surges, their impacts are affected by land management and deforestation, which have been shown to factor into people's settling decisions (Petkov, 2022).

We use the PLFS survey weights to weight our observations, and we cluster standard errors at the state level. Clustering at the state level is appropriate because education funding and programs are primarily administered at the state level.<sup>15</sup> State-level clustering also takes into account spatial correlations within a state and time correlations in the exposure index resulting from the fact that the same storm affects multiple birth-year cohorts simultaneously.<sup>16</sup>

<sup>12</sup>We include birth order as a control variable in our analysis, as certain studies have indicated that birth order may have an impact on parental investments in education (see e.g., Black et al., 2005). We do not include controls for household headship, marital status, rural residency, or household size, as each of these variables could be affected by school-age exposure to storms and may lead to a bad-control issue if included.

<sup>13</sup>It is worth noting that our outcomes are observed only once for each individual in the cross-sectional PLFS dataset, and thus our specifications are neither dynamic nor staggered difference-in-differences models despite the inclusion of different cohorts. Instead, we adopt a cross-sectional cohort study approach, where we retrospectively evaluate individuals' exposure histories between ages 5-15.

<sup>14</sup>Evidence suggests that, even with climate change, any signal will appear in the distribution of storm activity very gradually (see e.g., Emanuel, 2011). Therefore, it is unlikely that economic agents are aware of changes in the local distribution of storm exposure.

<sup>15</sup><http://countrystudies.us/india/37.htm>

<sup>16</sup>We work with 35 clusters, including 28 states and 7 union territories. Tables D.4 and D.5 in the Online Appendix show that our results remain consistent when using district clustering or district-cohort clustering.



**Results.** In Table 1, Panels A and B present the results for equation (3). Panel A reports the results for educational delay measured as the difference between the reported years of schooling and the expected number of years based on reported educational attainment.

Column (1) presents the baseline results, which suggest that exposure to storms leads to a statistically significant delay in completing a given level of education. The estimated delay for a child with unit exposure is 0.43 years on average, which translates to roughly a 5-month delay. Figure E.2 of the Online Appendix shows that while unit values in the exposure index are exceptional in our sample period, they are observed in Odisha due to the 1999 BOB 06 super cyclone, the most severe and destructive tropical cyclone recorded in India from 1990 to 2000. Although extremely severe cyclonic storms are rare, recent events such as storms Phailin and Fani in Odisha in 2013 and 2019 respectively, super cyclone Amphan in West Bengal in 2020, and severe cyclonic storms Tauktae and Yaas in Gujarat and West Bengal and Odisha respectively in 2021, indicate an increasing frequency of such events. Therefore, 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 face. If we use the average exposure in the sample to interpret our results, we find that the educational delay is approximately nine school days.<sup>17</sup>

We examine the robustness of our results by using different sets of fixed effects (FE) in columns (3) and (4). In column (3), we include state-cohort FE, which allows the economic conditions of a state at the time of a cohort's birth to affect long-term educational delays. Although the estimate is less precise and smaller than the baseline, it remains qualitatively similar. In the last column, we add state-policy FE, which account for the introduction of the new National Policy on Education in 1986 and its amendment in 1992. The policy aimed to provide compulsory education for all children up to the age of 14 and was effectively adopted at the state level. The interaction term covers cohorts born in 1985, those born between 1986 and 1991, and those born after 1991. Results are similar to those in column (2) and remain statistically significant at the 5% level.

In Panel B of Table 1, we estimate a linear probability model to examine the impact of school-age exposure to storms on the likelihood of experiencing an educational delay of at least one year. The results are consistent across specifications and indicate that exposure to storms during schooling years increases the likelihood of experiencing an educational delay. Specifically, in the baseline specification (column 1), a unit exposure (which is likely to be driven by severe events) is associated with a 24 percentage point increase in the

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<sup>17</sup>With an average storm exposure of 0.1 in our sample, 42 weeks per year, and a 5-day school week, this number is computed as  $0.43 \cdot 0.1 \cdot 42 \cdot 5$ .

probability of accumulating an educational delay (i.e. repeating a year or dropping out). On the other hand, an average exposure increases this probability by 2.4 percentage points. In our sample, the proportion of individuals with an educational delay of at least one year is 0.331 (as shown in Panel B of Table D.3 of the Online Appendix). Based on the baseline estimate in Panel B, we can infer that this proportion would increase by approximately 72.5% (to a share of 0.571) in the case of an extreme cyclonic storm exposure, and by 7.25% (to a share of 0.355) in the case of an average exposure.

### 3.2 Educational Attainment

In Panel C of Table 1, we investigate whether storms not only cause educational delays, but also affect the likelihood of completing a given level of education. We use an ordered logit model with a categorical variable representing reported educational attainment (0=below primary, 1=primary school, 2=middle school, 3=secondary education, 4=above-secondary education), where category 0 includes individuals who received some education but did not complete primary school. The same set of variables as in equation (3) is included as controls.

The first column of the table displays the ordered logit estimates, while columns (2)-(6) report the marginal effects of school-age exposure to storms for each category of schooling. All of the estimates are statistically significant and represent the percentage point changes in the probability of completing a certain level of education in the case of unit school-age exposure to storms. In general, positive exposure to storms increases the probability of not completing primary school and completing at most primary and middle school, while decreasing the probability of achieving secondary and post-secondary education. Specifically, our findings show that unit exposure reduces the likelihood of achieving post-secondary education by 20 percentage points. This translates to a 2 percentage point reduction in the event of an average exposure.

To give a sense of the scale of our findings, let us consider children who experienced the 1999 BOB 06 super cyclone during their schooling years (i.e.,  $C_{bd} = 1$ ). Based on the educational attainment proportions in Table D.3 of the Online Appendix, the estimates in Panel C of Table 1 suggest that the percentage of individuals who did not complete primary school (with at most primary education) would increase from 2.7% to 7.3% (or 9.8% to 20.8%), while the percentage of individuals who obtained post-secondary education (with at most secondary education) would decline from 27.2% to 7.2% (or 36.5% to 31%). Therefore, it can be inferred that exposure to the 1999 super cyclone likely resulted in a significant increase in individuals lacking basic education. Even for an

average storm exposure, these percentages remain significant. For example, the proportion of individuals with post-secondary education would decrease by 7.35%, while the percentage of individuals with only primary school education would increase by 11%.

In Figure E.3 in the Online Appendix, we utilize the estimates obtained from the ordered logit model to visualize the predicted probabilities of achieving a particular level of education across the range of storm exposures from 0 to 1, along with their 95% confidence intervals. The overall findings from this analysis reveal that storms result in a leftward shift in the distribution of educational attainment, which is particularly concerning for developing nations such as India, where the distribution of skills is already heavily skewed to the left.

### 3.3 Type of Activity

We expect that the educational disruption caused by storms during compulsory schooling would affect the type of labor market activities individuals perform in early adulthood, as certain types of jobs require higher levels of education or at least basic reading, writing, and computing skills. To investigate this issue, we estimate a reduced-form specification of school-age exposure to storms on an indicator variable for each type of activity in Panel A of Table 2. For example, in column (1), the dependent variable is a dummy variable equal to 1 if the main activity of individual  $i$  is regular work. We include the same set of controls as in equation (3) for each type of activity.

Our estimates suggest that individuals who were exposed to storms during their schooling years are less likely to work as regular salaried workers and more likely to perform domestic duties. However, we find no statistically significant effect on the likelihood of being a casual worker, self-employed, or an unpaid family worker. To illustrate the magnitude of our results, let us consider the 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 1.6 percentage point reduction in the probability of being a regular worker. According to Panel D of Table D.3 of the Online Appendix, 19.6% of individuals in our sample are engaged in regular work. Thus, the estimate in column (1) implies an 8% decrease in the probability of being employed as a regular worker. The estimate in column (5) indicates a 1.6 percentage point increase in the likelihood of performing domestic duties as the primary activity in early adulthood, which corresponds to a 4.8% change when taking the share of individuals involved in domestic duties (i.e., a share of 0.33) as a baseline. These effects are more pronounced for children who experienced more severe exposures. For example, with unit exposures and taking the

same baseline shares, the estimates imply changes of approximately 80% and 48% for regular work and domestic duties, respectively.

In summary, an average storm increases the probability of experiencing a schooling delay by 2.4 percentage points, while concurrently reducing the likelihood of completing post-secondary education by 2 percentage points. These findings are congruent with a 1.6 percentage-point decrease in the probability of securing a regular salaried position, suggesting that both delays in schooling and a skill reduction are likely contributing factors.

In Panel B of Table 2, we investigate whether positive exposure to storms is associated with lower wages and longer hours of work. In column (1), we restrict the sample to workers who receive a salary, which explains the drop in sample size. We find no evidence that, conditional on being employed as a regular worker, school-age exposure to storms has a permanent effect on wages. However, the subsample of workers with a positive salary is a selected one since exposure to storms reduces the probability of being a regular worker (see column 1 of Panel A). To address this issue, we run a Tobit estimation and report the average marginal effect (AME) on wages, evaluated at the means of the covariates, in column (2) of Panel B. The estimate shows a negative effect on wages, which is statistically significant at the 10% level. This suggests that, on average, a super storm causes a 39% decline in hourly wages, or taking the average exposure, a 3.9% wage drop. This result is consistent with the fact that storms increase educational delays and reduce the probability of completing higher education.

Column (3) of Panel B shows the results on hours of work, focusing on individuals reporting positive hours of work and a positive salary, as in column (1). In column (4), we report the corresponding AME from a Tobit estimation, once again evaluated at the means of the covariates. We find no evidence that school-age exposure to storms has a permanent effect on hours of work.

The disruption of education caused by storms is likely to widen income and social disparities across different districts and age groups in the long run. Our findings suggest that this increase in inequality is primarily driven by changes in qualifications and types of employment, leading to less secure and potentially lower-paying work. Additionally, disparities along the income distribution may further increase as those who experience the largest delays in education often come from vulnerable social groups.

### 3.4 Robustness

In this section, we present a series of robustness checks. We focus on the results related to education and refer readers to Online Appendix C for an analysis of the primary activity status of individuals.

**Early-life Exposure to Storms.** It is well-documented in the literature that shocks experienced in early life can have long-lasting negative impacts on health, education, and labor market outcomes (see e.g., Almond et al., 2018, for a recent survey of this literature). To verify that our findings are attributable to shocks during school-age years rather than earlier shocks, we augment our baseline specification by incorporating a measure of storm exposure during the early years of life (0-4 years old), a period considered critical for skill formation in developmental psychology, epidemiology, and economics (see e.g., Duque et al., 2019; Heckman, 2008; Knudsen et al., 2006). The measure is similar to our baseline index (see equation 1), but specifically focuses on early life.

Table 3 presents results from this exercise. In column (1), we present our baseline specification. Including storms which took place in early years reduces the sample to birth-year cohorts 1990-1995. Column (2) shows that restricting the sample to individuals born after 1989 does not affect our baseline results. In column (3), we replace school-age ( $C_{bd}$ ) with early-life exposure. Estimates are statistically insignificant for all outcomes, except for educational attainment, suggesting that for individuals who actually received some formal schooling, storms in early life have little impact on educational delays. However, this does not necessarily mean that early-life exposure to storms has no impact on education; it may still reduce the probability of receiving a formal education, which we are unable to assess. Finally, in column (4), we include both early-life and school-age exposures simultaneously. The inclusion of the early-life measure does not impact the estimates of interest, suggesting that our results are not driven by storm shocks that occurred in years 0-4, and that school-age years are crucial for long-term human capital formation.<sup>18</sup>

**Falsification Test.** To confirm the validity of our identification strategy, we conduct a falsification test by randomly assigning storms to the sample. We shuffle the measure of exposure to storms across the entire sample and substitute this randomized variable for the actual exposure measure in the baseline specification. We anticipate that the results

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<sup>18</sup>We also control for after-school storm exposure by summing yearly exposures over the after-school period up to 2018 in the analysis of primary activity status (see Table D.7 in the Online Appendix). Our baseline estimates remain unchanged even after adding this control.

of this exercise will yield mostly statistically insignificant estimates for the variable of interest, while leaving the statistical significance of the estimates for other variables largely unchanged. We repeat this process 1,000 times and record the t-statistics and p-values for each iteration.

We present the results of the falsification test in Figure 1, which visually shows the distribution of t-statistics for the coefficient of interest. Panel A focuses on the regression on years of educational delay, while Panel B considers educational delay as a binary variable. Panel C shows the results for educational attainment. The histograms represent the distribution of t-statistics across the 1,000 repetitions of the falsification exercise, with the red vertical line indicating the t-statistic of the baseline estimates (3.01, 2.96, and -3.79, respectively). We observe that the majority of the distribution falls within the -1.96 and 1.96 boundaries, indicating that most of the coefficients obtained through the falsely-attributed storms are statistically insignificant.

**Removing Extreme Exposures.** Table 4 examines the sensitivity of our results to extreme values of exposure. The baseline results are shown in column (1). In column (2), we exclude individuals from Odisha, which has unusually high values of exposure due to the 1999 super cyclone BOB 06. The results obtained from this subsample are similar to the baseline estimates. Finally, in column (3), we exclude all winds with values above the 95th percentile of the wind speed distribution. As anticipated, this leads to smaller effect sizes and less precise estimates.

**Climate Controls.** To account for the potential influence of general climate conditions during childhood on human capital formation and long-term outcomes, we include controls for local climate effects such as precipitation and temperature in columns (4) and (5) of Table 4.

To this end, we augment the baseline specification by introducing a district-specific variable measuring the average annual precipitation (in millimeters) between ages 5 and 15. Additionally, we include controls for the average temperature (in °C) and the number of days that children in a particular district were exposed to different temperature ranges (0-10, 10-20, 20-30, and above 30°C) during their school-age years. We obtain the raw temperature and precipitation data from the ERA5-Land archive, accessed through the Google Earth Engine, and then aggregate them at the district level.<sup>19</sup>

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<sup>19</sup>The ERA5-Land data is generated by researchers at the European Centre for Medium-Term Weather Forecasting (Muñoz Sabater et al., 2019). It is a climate reanalysis dataset that provides hourly weather information with a spatial resolution of 0.1×0.1 degrees, which is approximately 10×10 kilometers, covering the period from 1981 to the present.

Results are shown in columns (4) and (5) of Table 4. Column (4) reports the baseline specification estimated on the subset of the sample for which climate variables are available. In column (5), we include controls for precipitation and temperature, as described earlier. Across all panels, the coefficient of interest remains precisely estimated and qualitatively similar to the baseline results, albeit with a slightly smaller size effect.

**Migration.** One potential source of bias arises from measurement error in storm exposure attributed to migration. In fact, we determine storm exposure based on individuals' residential locations at the time of the survey, as we lack information about their residence during their school years. If individuals have relocated to a different district by 2018, our measure may contain errors, particularly if either the district they left or the one they moved to had experienced storms during their compulsory schooling.

To address this concern, we conducted an analysis using supplementary data sets, demonstrating that permanent out-of-district migration is relatively rare and that the occurrence of a storm tends to reduce the probability of migration (see Online Appendix A and Footnote 7). As an additional test, focusing on the PLFS sample, we propose excluding married females from the analysis (column 6 of Table 4). In India, marriage is a primary driver of migration, especially for women who typically move to live with their husband's family. However, this test is admittedly approximate, as it is unclear whether all married women have migrated out of their district, and it is also uncertain whether unmarried women have not migrated. Similarly, we observe that educated individuals had to move more frequently, often for educational purposes. Thus, we also suggest excluding highly educated individuals from the sample (column 7 of Table 4), noting that estimated delays will only pertain to individuals with at most secondary education. As observed in columns (6) and (7) of Table 4, the key estimates remain qualitatively similar to the baseline results, although they are somewhat less precisely estimated in the former column, possibly due to the exclusion of approximately 40% of the sample.

**Education Controls.** To account for the fact that individuals within educational categories may share observable characteristics or have similar abilities that make them more or less likely to experience educational delay, we propose two alternative approaches.<sup>20</sup>

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<sup>20</sup>Since school-age exposure to storms impacts both educational delay and educational attainment, using educational categories fixed effects would cause a bad-control problem

The first approach involves using the predicted probability of completing a reported level of education, conditional on observable individual characteristics, as a proxy for educational attainment. Specifically, we estimate a linear probability model on a set of individual characteristics, such as the individual's gender, year of birth, whether they are a first-born child, and their religion (Hindu or non-Hindu) as predictors, along with interactions of these variables. To avoid a bad control issue in the final regression, we focus on a subsample of states with zero exposure to storms between 1990 and 2010, and restrict ourselves to individual characteristics that are unlikely to be affected by storms. We use these estimates to predict the probability of completing each level of education for each individual in the full (baseline) sample and use the corresponding probability as a proxy for educational attainment in the final regression. The result of this exercise is presented in column (2) of Table 5 and is highly comparable to the baseline estimates presented in column (1).

Second, we propose including fixed effects capturing parental education, which has been shown to be a significant predictor of children's educational achievements (Björklund and Salvanes, 2011; Guryan et al., 2008). Parental education is less likely to be affected by children's exposure to storms, although there is a possibility that parents enrolled in university may have young children attending primary school. This may be particularly true for relatively young parents. However, it is implausible that parental education overlaps with children's compulsory schooling at low levels of education.

One potential drawback of this approach is that parental education is only observable if both the individual and their parents live in the same household. Additionally, since married women often move in with their husbands' families, the sample may include relatively more males than in the baseline. We begin by replicating the baseline approach on the subset of the sample with available data on parental education (column 3 of the table). Despite the smaller sample size (representing only 45% of the initial sample), the coefficients are very similar to the baseline estimates. In column (4), we include fixed effects for parental education, and the estimates remain nearly identical across specifications.

**Alternative Measures of School-age Exposure to Storms.** In Table 6, we explore alternative specifications of  $C_{bd}$ . The first alternative (column 2) measures exposure by summing over the squares of yearly exposures (i.e.,  $\sum_{t=b+5}^{t=b+15} x_{dt}^2$ ), which assigns more weight to stronger exposures. In contrast, the baseline measure simply sums over exposures, treating a district-cohort exposed to multiple small storms the same as a district-cohort exposed to one violent and potentially destructive storm. By summing



across squares, we can differentiate between storm intensities when aggregating over the years. However, multiple exposures over time in a given district are rare (75.5% of individuals with positive exposure experienced only one storm between ages 5 and 15), so we do not expect this alternative specification to substantially alter our results.

Second, we experiment with a different functional form to capture the relationship between the force exerted by winds on structures and wind speed exposure. Emanuel (2011) notes that there are physical reasons to believe that damages to building infrastructure are related to wind speed exposure in a cubic manner. To account for this, we use a cubic specification in columns (3) and (5) of Table 6, where we replace the square in Equation 2 with a cube.

Third, we modify the wind speed threshold that defines a storm. In the paper, we use a benchmark threshold of 50 knots, based on Emanuel (2011), which is likely to cause damages. However, in columns (4) and (5) of Table 6, we raise the threshold to 64 knots, equivalent to a category 1 cyclone on the Saffir-Simpson scale. In column (6) of Table 6, we eliminate the threshold altogether and include all winds that occur during a cyclone event.

Finally, in column (7) of Table 6, we compute the maximum wind speed hitting each district using the HURRECON wind field model (see Online Appendix B.2 for more details), following Boose et al. (2004) instead of Deppermann (1947).

Overall, the estimates in Table 6 largely resemble the baseline results, except for column (6), where measuring exposure directly with winds introduces a downward bias. This is likely due to the assignment of non-zero exposure values for districts with only mild wind speeds, which hardly cause any damage.

## **4 Income and Infrastructure Channels**

Our study indicates that for school-age children, exposure to storms leads to educational delays and has negative long-term consequences for both academic achievements and career prospects. This is concerning, as a decline in human capital formation contributes to a deskilling of the population, potentially impeding economic growth.

To develop effective policy recommendations addressing the consequences of storms, we next explore the ways in which education may be impacted. Storms can affect human capital through two primary channels: shifts in schooling demand and changes in schooling supply.<sup>21</sup>

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<sup>21</sup>See Baez et al. (2010) for a comprehensive review of the literature on the channels through which disasters damage human capital.

Demand shifts can occur due to the harmful effects of storms on i) household income (or consumption) and ii) children's health. For instance, a storm might cause a negative income shock by destroying crops and farms in rural areas or damaging production facilities in both rural and urban settings. These shocks can be temporary, lasting until physical assets are restored, or permanent, as when the loss of a season's crop puts a farming household in debt and causes financial hardship for years. As a result, children may have less time to study and complete homework if they need to work to supplement their family's income. In extreme cases, parents may no longer be able to afford sending their children to school, causing them to drop out.

After cyclones, there is also a potential risk of people contracting water-borne diseases such as typhoid and cholera, as well as mosquito-borne illnesses like malaria and dengue. Additionally, cyclones can reduce agricultural productivity due to increased salinity in irrigation water and rising sea levels. This decline, combined with economic strain, might contribute to malnutrition, which in turn can impede learning. Storms can also adversely affect children's mental well-being, possibly leading to Post-Traumatic Stress Disorder (PTSD), a condition known to impair academic and professional outcomes (refer, for example, to Neria et al., 2008).

On the supply side, disasters can damage public infrastructure, such as roads and schools, causing temporary disruptions in schooling due to the inability to attend classes. Storms may also reduce electricity supply to schools, degrade classroom conditions, or even destroy buildings, preventing children from attending school and teachers from providing instruction in suitable environments.

In what follows, we examine the income and schooling supply channels.<sup>22</sup> To better understand the links between storm exposure and long-term human capital losses, we utilize auxiliary data and investigate the short-term effects of storms on household income, school infrastructure, and academic outcomes. Additional data are necessary because the PLFS database does not offer detailed information on school and household income for individuals during their school-age years.

## **4.1 Income Channel**

We begin by examining the impact of storms on income, focusing on the dynamic income effects over a two-year period. Our data is sourced from the Consumer Pyramid Dx database, a comprehensive panel containing monthly income information for

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<sup>22</sup>We will reserve the analysis of the health and nutrition channel for future research. Regrettably, examining the PTSD channel is not feasible due to the unavailability of public mental health data in India.

approximately 200,000 households between 2014 and 2021.<sup>23</sup> The panel differentiates between rural and urban households, offers details on income sources (such as monthly household wages and business profits), and includes information on each household's district location.

Income data, presented in lakhs of rupees, is converted in real terms using the Indian Consumer Price Index (CPI), base year 2010, from the World Development Indicators. Summary statistics for these variables can be found in Panel I of Table D.12 of the Online Appendix.

We investigate the short-term dynamic income effects of storms using panel local projections over a 24-month period. Specifically, we conduct a series of  $k$ -step ahead panel predictive regressions:

$$\Delta Y_{h,\tau+k} = \alpha^k + \gamma_1^k x_{d\tau} + \delta_{dt} + \delta_\tau + \delta_h + \epsilon_{dh,\tau+k}, \quad (4)$$

where  $\Delta Y_{h,\tau+k} \equiv \log Y_{h,\tau+k} - \log Y_{h,\tau-1}$  and  $Y$  represents the monthly income of household  $h$  (located in district  $d$ ) at time  $\tau$  (a month-year pair). Thus,  $\Delta Y_{h,\tau+k}$  denotes the cumulative growth of households' monthly income from time  $\tau - 1$  to time  $\tau + k$ , signifying that our estimates capture cumulative effects up to period  $k$ .  $x_{d\tau}$  captures district storm exposure at time  $\tau$ . Similar to the school-age exposure to storms,  $x_{d\tau}$  is calculated using a quadratic damage function of winds and a 50 knots threshold (refer to Section 2.2 for details).  $\delta_{dt}$ ,  $\delta_\tau$  and  $\delta_h$  represent district-year, time and household fixed effects. Our object of interest,  $\gamma_1^k$ , captures the average response of households' monthly income at horizon  $k$  to a storm at time  $\tau$ . In combination, the sequence of  $\gamma_1^k$  up to  $k = 24$  traces the dynamic response to the disaster shock. We weight each regression using household weights and cluster standard errors at the state level.

Results are presented in the first panel of Figure 2. The figure plots the estimated coefficients  $\gamma_1^k$  at the different time horizons  $k \in [0, 24]$ , normalized for the average storm exposure in our sample (i.e., 0.023). Consequently, at each point in time, the local projection (the blue line) provides the estimated direct total impact of the average storm. The shaded area represents the 95% confidence interval. The figure shows that the income effects of an average storm are substantial.

Panel (a) of Figure 2 reveals that, following an average storm, household income experiences a brief yet statistically significant increase. After the initial peak, household income gradually declines, reaching levels nearly 8% below the no-disaster counterfactual 10 months after the disaster. This effect is negative and statistically

<sup>23</sup>The Consumer Pyramid Dx database is produced by the Center for Monitoring the Indian Economy (CMIE).

significant between the 10th and 15th months following the storm. Considering that this captures the impact of an average storm, 8% is an economically significant figure. Subsequently, income recovers, returning to pre-disaster levels 15 months after the shock. Panels (b) and (c) of Figure 2 demonstrate that this initial result conceals some heterogeneity, as differentiating between urban and rural households suggests that only rural income may revert to pre-disaster levels.

Panel (b) of Figure 2 indicates that in urban areas, household income declines immediately and continues to fall until the 5th month, where the total effect reaches 12%. In the second semester, income recovers slightly and then stabilizes at a level 8% below pre-disaster levels around the 14th month. After two years, income still has not recovered, suggesting that storms may act as a permanent income shock for urban households. Rural areas, in contrast, adjust differently. Income initially increases sharply and begins to fall gradually after 3 months, which aligns with immediate aid and the fact that the destruction of agricultural land may only be felt during the subsequent growing season. After 10 months, income starts to rebound, reaching pre-disaster levels after approximately 21 months.

Figure E.4 in the Online Appendix offers a more comprehensive analysis of income by source, displaying impulse response functions (IRFs) for wages in the top panels and business profits (i.e., self-employment) in the bottom panels. Panels (a) and (c) reveal that the decline in urban incomes is primarily driven by a decrease in wages. In contrast, Panels (b) and (d) demonstrate that in rural areas, as wages begin to fall, income from small business activities rises. This shift could result from a substitution effect, where individuals experiencing wage loss increase their involvement in small business activities or start new businesses. Alternatively, it could be due to new business opportunities arising from the storm. Profits generated by these activities can help compensate, at least in part, for lost wages, enabling recovery for individuals in rural areas, unlike those in urban areas.

In summary, Figure E.4 in the Online Appendix demonstrates that wages in both urban and rural areas decline within a year following a storm. A decrease in household wages does not necessarily indicate job loss, reduced hours worked, or a drop in hourly wages; it may also signify a shift in the types of jobs held by household members. Nevertheless, despite the potential expansion of some industries due to reconstruction efforts, the data suggests that such growth is unable to prevent a decline in household wages. Consequently, increased demand might be insufficient to accommodate workers from shrinking industries. Furthermore, workers could encounter mobility barriers due to

challenges in transferring skills between jobs or geographic constraints, an hypothesis that aligns with the migration results presented in Online Appendix A.

Our results align with Feng et al. (2016), who finds that the 2008 Wenchuan earthquake in rural Sichuan decreased household incomes by 14%. In developed countries, Deryugina (2017) shows that government transfers, such as unemployment insurance, can fully counteract the potential income effects of disasters. Our findings for urban wage income in India suggest that implementing similar policies could considerably alleviate the negative impacts as well.<sup>24</sup>

## 4.2 School Infrastructure Channel

We next investigate the impact of storms on school infrastructure, using data from the District Information System for Education (DISE) collected by the National Institute of Educational Planning and Administration (NIEPA). This comprehensive dataset covers all schools offering elementary education in India between 2010 and 2018, including information on classroom conditions, school attendance, and examination results at the school and academic year levels.<sup>25</sup> The data also includes the school code identifier and the block, postal code (*pincode*), village and district in which the school is located.<sup>26</sup>

At the school level, our primary variables of interest include the number of classrooms in good condition, the school's access to electricity (reliable, unreliable, or none), whether the school is under construction, and the school's construction date. To suit our analysis, we aggregate pertinent school data at the postal code-academic year level and match it with a storm exposure index. This index's construction is similar to that of  $x_{dt}$  but is calculated using wind speed exposures at the postal code level during an academic year (refer to Section 2.2 for more details). Summary statistics for the main variables can be found in Panel II of Table D.12 of the Online Appendix.

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<sup>24</sup>Several studies have examined the local labor market impacts of hurricanes in the U.S., focusing exclusively on the effects of hurricane evacuee in-migration (see e.g., De Silva et al., 2010). For an analysis of long-term effects on earnings in affected regions, see Groen et al. (2020). Much of the literature on wages in developed countries employs more aggregated measures (see e.g., Belasen and Polachek, 2009). Research on developing countries primarily focuses on aggregate outcomes as well. For example, Keerthiratne and Tol (2018) in Sri Lanka discover that disasters reduce income inequalities as higher-income groups experience a larger share of damages.

<sup>25</sup>DISE covers all schools, including unrecognized schools. Recognized establishments can be managed by the Department of Education, the Local Body, the Social/Tribal Welfare Department, and by a Private-Aided or -Unaided Body.

<sup>26</sup>In comparison to the Annual Status of Education Report survey (ASER), the DISE data enables a more extensive analysis, encompassing all of India and using panel data at a highly granular geographic level (postal code). Conversely, the ASER survey primarily targets rural areas, is not released annually, and only offers district-level information.

To estimate the first-order contemporaneous effects of storm exposure on school facilities and infrastructure, we regress outcomes on the storm exposure index, postal code fixed effects, and district-year fixed effects, with standard errors clustered at the state level.

Panel A of Table 7 investigates the impact of storms on school facilities. In column (1), the dependent variable is the log average number of classrooms in good condition. The coefficient obtained is negative and precisely estimated, indicating that classroom conditions worsen following a storm. The average number of classrooms in good condition declines by 0.5% after an average storm (with a force of 0.053). This reduction increases to 3% following a storm at the 95th percentile of the distribution (with a value of 0.3) and to 10% after a super storm (with a value of 1).

The remaining columns in the table examine the proportion of schools with electricity (column 2), without electricity (column 3), and with unreliable electricity (column 4).<sup>27</sup> The estimates are consistently precise, indicating that storms raise the percentage of schools without electricity or with unreliable electricity. Specifically, we find that the share of schools with electricity drops by 5.6 percentage points (a 7.7% decrease) following a super storm. For the average storm, this reduction is 0.28 percentage points.

In Panel B of Table 7, we explore the extent to which storms lead to school destruction. Columns (1)-(3) examine the number of school closures (exits), expressed as a percentage of the total number of schools in 2010. The results indicate that the share of school closures rises in the year following a storm, with this effect persisting over two periods, as evident in column (3). For an average storm, the cumulative effects up to two periods suggest an increase in the share of school closures by approximately 0.37 percentage points, translating to a 7.4% rise in the probability of exit. For a super storm, the cumulative effects result in a 7.3 percentage point increase, equating to a 146% surge in the probability of exit.

Columns (4)-(6) concentrate on the proportion of school buildings under construction. The findings reveal that this share decreases immediately after a storm and persists in the following year, resulting in a total effect of -0.35 percentage points following an average storm. This corresponds to a 39% reduction in the share of buildings under construction. The immediate effect is expected, as construction sites can be severely damaged even at relatively low wind intensities. Additionally, construction may decelerate if the projects' initial budgets need revising. The negative impact of storms endures for an additional year, implying that school (re)construction only resumes after two years and that destroyed school buildings may not be immediately rebuilt. This contrasts with Pelli et al. (2023),

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<sup>27</sup>The number of observations decreases between column (1) and (2) due to singleton observations.

which finds that firms quickly rebuild and replace destroyed capital within a year after the storm. Our results suggest that rebuilding public infrastructures takes a longer time.

In summary, our results show that storms disrupt daily school operations by damaging classrooms and causing power outages. Furthermore, storms lead to school closures and a decrease in school buildings under construction, potentially preventing teachers from conducting and students from attending classes for an extended period. While we are not aware of studies specifically estimating school destruction due to disasters triggered by natural hazards, our findings align with the general understanding that disasters increase children's vulnerability in developing countries by diminishing the availability of adequate schooling infrastructure, and consequently, the supply of education. Unless the resilience of school buildings improves over time, the infrastructure channel will likely become increasingly significant as the frequency and severity of disasters rise. To date, only a few studies have provided direct evidence that constructing more resistant schools can mitigate the detrimental effects of disasters on long-term education and labor market outcomes. For example, see Herrera-Almanza and Cas (2021) and Cas (2016) for studies examining the impact of building typhoon-resistant schools in the Philippines.

### **4.3 Scholastic Outcomes on Impact**

The results presented in Section 3 suggest that exposure to storms during school-age increases the likelihood of educational delays. However, it remains unclear whether these delays are caused by a higher probability of dropping out or failing a course of study, as the PLFS data does not contain the necessary information to determine the exact cause. To shed light on these possible mechanisms, we turn to the DISE data and analyze school attendance and examination results at the postal code-academic year level, using the same measure of postal code exposure to storms as before.<sup>28</sup> Panel II of Table D.12 in the Online Appendix presents summary statistics for the relevant variables.

Our analysis reveals a stark contrast in how children in different stages of schooling respond to disasters. Specifically, for those attending primary school, the adjustment occurs at the extensive margin, reflected in changes to attendance rates. In contrast, for middle school students, the adjustment takes place at the intensive margin, resulting in changes to their academic performance.

Panel A of Table 8 examines school attendance by examining the log average number of children in each postal code-year and school level, ranging from primary to

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<sup>28</sup>It is worth noting that the DISE data only covers elementary schools, limiting our ability to examine attendance and performance at the secondary or upper-secondary level.

middle school. The findings demonstrate that storms have a substantial impact on attendance for primary school children in levels C3 to C5, corresponding to ages 8 to 11. The estimated reductions in attendance are sizable, with a drop of approximately 15% for a super storm and 0.75% for an average storm. However, no statistically significant impacts on attendance are observed for any other levels.

Overall, our findings align with previous research indicating that weather shocks and disasters triggered by natural hazards lead to a drop in school enrollments in developing countries (see, for instance, Jensen, 2000). Furthermore, our findings lend support to the idea that a disaster-induced negative income shock prompts parents to withdraw their children from school. This could be due to financial constraints, as families may no longer have the means to afford schooling, or because their children are required to work to supplement the household income. This hypothesis that disasters increase the incidence of working children is well-supported by the literature (see, for instance, Baez et al., 2010; De Janvry et al., 2006, for a review).

The imprecise estimates for levels C1 and C2 suggest that children at these levels may remain in school because they are either too young to work or unable to contribute to reconstruction efforts due to physical limitations. Additionally, the lack of effects on levels C6-C8 may be attributed to the dedication of poorer parents whose children have advanced to this level to ensure that their children complete middle school. Furthermore, since wealthier children are more commonly found in middle school, they may be less affected by the income shock, as our income shock story suggests.

Despite the lack of impact on attendance at the middle school level, older students may still be compelled to work or work more frequently after school and on weekends. This may reduce the amount of time available for studying, increase fatigue, reduce concentration in school, and ultimately lead to a decline in academic performance. We explore this possibility in Panel B of Table 8, which examines examination results in the final year of primary (C5) and middle (C8) school.<sup>29</sup> Columns (1) and (2) focus on the log average number of students who appeared for the exam, while the subsequent two columns examine those who passed the exam. Columns (6) and (7) show the effect on the log average number of students who scored above 60

Our results indicate that there is no effect on exam appearance or performance for students at level C5. However, in accordance with our income shock narrative, we discover that middle school students experience a decline in academic performance. Fewer students appear for the exam, and even fewer pass it. Additionally, the number of students who pass with a grade above 60% also decreases following a storm.

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<sup>29</sup>Examination results for other years of schooling are not available.



The estimates are consistent across columns, and unlike the results for school attendance, they reveal substantial effects of about 15% for an average storm. This implies that even a moderate storm can have significant impacts on education. These findings are particularly important given that a decline in grades may ultimately result in educational delays, although we cannot formally investigate this possibility with our available data.

Taken together, our results suggest that the primary reason for the estimated long-term educational delays is the significant decrease in income, resulting in reduced attendance for primary school children and decreased academic performance for middle school children. While the decline in attendance for an average storm is relatively small, the deterioration of academic performance is substantial. Specifically, the average storm results in a 15% reduction in the number of children who appear for the exam, pass the exam, and receive a good grade. These results align with previous research that has demonstrated the adverse contemporaneous effects of disasters triggered by natural hazards and extreme weather shocks on education in developing countries (see e.g., Deuchert and Felfe, 2015; Spencer et al., 2016).

## **5 Conclusion**

In this study, we examine the impact of storm exposure during school years on long-term educational attainment and primary activity status among young adults in India. Our findings indicate that individuals who experienced a storm during these critical years are more likely to experience educational delays and less likely to complete higher education. Furthermore, we observe a decrease in the likelihood of securing regular salaried employment and an increase in the probability of engaging in domestic duties as a primary activity.

Our results also provide indirect evidence that the enduring effects of school-age storm exposure can be attributed to both the degradation of educational infrastructure and a decline in household demand for schooling due to reduced income. These findings align with the notion that adverse income shocks can lead to an increase in working children, manifested as decreased school attendance for primary students and diminished academic performance for middle schoolers.

Overall, our study underscores the importance of robust social safety nets and the need to extend post-disaster policies beyond mere reconstruction efforts. Such policies should integrate financial transfers with educational initiatives, such as cash transfers conditional on school attendance and enhanced school support. Local projections of

household income reveal that social policies, including unemployment insurance, could be instrumental in building resilience and managing risk in urban areas, which are particularly vulnerable following storms.

While our results offer valuable insights, two caveats warrant consideration, suggesting that our findings should be interpreted as lower bounds of the true effects. First, our estimations do not account for the poorest individuals who are likely not enrolled in school due to their families' low-income status. Including this segment, which tends to be disproportionately affected by disasters, would likely yield larger estimates. Second, our data only accounts for individuals who survived the storm and its aftermath until 2018. Although storm-related fatalities have been relatively contained in recent years, it is important to recognize that our results may be subject to survivorship bias. However, we do not anticipate this issue to significantly affect our findings.

## Tables and Figures

Table 1: Educational Delay and Educational Attainment

	Educational delay		
	(1)	(2)	(3)
<b>Panel A: # of years</b>			
School-age exposure	0.43*** (0.14)	0.22* (0.13)	0.28** (0.10)
<b>Panel B: yes=1, no=0</b>			
School-age exposure	0.24*** (0.080)	0.20** (0.084)	0.18** (0.072)
Controls	Yes	Yes	Yes
State-cohort FE	No	Yes	No
State-policy FE	No	No	Yes
Observations	70,003	70,003	70,003
Panel A: Mean dep. var.	0.52	0.52	0.52
Panel B: Mean dep. var.	0.33	0.33	0.33

	Educational attainment					
	Logit estimates	Below primary	Primary school	Middle school	Secondary education	Above-secondary education
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel C: Educ. attainment</b>						
School-age exposure	-1.18*** (0.31)	0.046*** (0.011)	0.11*** (0.029)	0.096*** (0.027)	-0.055*** (0.015)	-0.20*** (0.052)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	70,003	70,003	70,003	70,003	70,003	70,003
Mean dep. var.		0.027	0.098	0.239	0.365	0.272

Notes: Panel A and B show results on educational delay. In Panel A, educational delay is calculated as the difference between the reported years of schooling and the minimum number of years required in the schooling system to attain the reported educational level. In Panel B, educational delay is measured using a dummy variable that takes a value of one if the delay is at least one year. In Panel C, educational attainment is a categorical variable indicating the reported level of education (0=no formal schooling, 1=primary school, 2=middle school, 3=secondary education, 4=above-secondary education), with category 0 including individuals who received some education but did not complete primary school. Column (1) shows the results from an ordered logit estimation where the dependent variable is a categorical variable indicating the reported educational attainment. Columns (2) to (6) report the marginal effects of childhood exposure to storms for each category of schooling. Controls include district FE, cohort FE, district trends, and individual controls, including dummy variables indicating if the individual is female, first-born, and Hindu. Policy FE used in the interaction terms include three FE corresponding to cohorts born in 1985, those born between 1986 and 1991, and those born after 1991. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors are clustered at the state level.

Source: Authors' estimates.

Table 2: Type of Activity, Wages and Hours Worked

	Regular work	Casual labor	Self-employed	Unpaid family work	Domestic duties
	(1)	(2)	(3)	(4)	(5)
<b>Panel A: Type of activity</b>					
School-age exposure	-0.16** (0.076)	-0.0093 (0.075)	-0.046 (0.059)	0.016 (0.067)	0.16*** (0.056)
Controls	Yes	Yes	Yes	Yes	Yes
Observations	70003	70003	70003	70003	70003
Mean dep. var	0.196	0.093	0.132	0.079	0.329
	Log hourly wages	Log hourly wages AME Tobit	Hours of work	Hours of work AME Tobit	
	(1)	(2)	(3)	(4)	
<b>Panel B: Wage &amp; hours worked</b>					
School-age exposure		0.018 (0.18)	-0.393* (0.230)	3.57 (3.70)	-4.33 (4.684)
Controls		Yes	Yes	Yes	Yes
Observations		29,089	70,003	29,089	70,003
Mean dep. var		3.71		53.60	

Notes: In Panel A, the dependent variable is a dummy variable that takes a value of 1 if the main activity of the individual is to perform regular work (column 1), casual labor (column 2), self-employment (column 3), work as an unpaid family worker (column 4), or perform domestic duties (column 5), as described in the Section 2. In Panel B, the dependent variable is the individual's logarithm of (real) hourly wage in rupees (columns 1 and 2) and hours worked (columns 3 and 4). Columns (1) and (3) estimate the effect of childhood exposure to storms using a subsample of individuals who report wages and hours worked. This subsample mainly consists of individuals engaged in regular work and casual labor, and the number of observations may differ slightly from that presented in the summary statistics due to singleton observations. In columns (2) and (4), we report the average marginal effects (AME) on wages and hours worked, respectively, evaluated at the means of the covariates, using a Tobit estimation. Controls include district FE, cohort FE, district trends, and individual controls, including dummy variables indicating if the individual is female, first-born, and Hindu. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors are clustered at the state level.

Source: Authors' estimates.

Table 3: Controlling for Early-life Exposure (Education)

	Baseline (1)	Sub-sample (2)	Early-life (3)	School & early-life (4)
<b>Panel A:</b>				
<b>Educ. delay: # of years</b>				
School-age exposure	0.43*** (0.14)	0.35** (0.15)		0.35** (0.14)
Early-life exposure			-0.16 (0.14)	-0.16 (0.14)
<b>Panel B:</b>				
<b>Educ. delay: yes=1, no=0</b>				
School-age exposure	0.24*** (0.080)	0.38*** (0.088)		0.38*** (0.087)
Early-life exposure			-0.086 (0.080)	-0.088 (0.080)
<b>Panel C:</b>				
<b>Educ. attainment</b>				
School-age exposure	-1.18*** (0.31)	-1.22*** (0.44)		-1.22*** (0.43)
Early-life exposure			0.82*** (0.13)	0.83*** (0.15)
Controls	Yes	Yes	Yes	Yes
Observations	70,003	41,892	41,892	41,892
Panel A: Mean dep. var.	0.52			
Panel B: Mean dep. var.	0.33			

Notes: The table presents results when controlling for early-life exposure to storms. In Panel A, educational delay is calculated as the difference between reported years of schooling and the minimum number of years required in the schooling system to attain the reported educational level. In Panel B, educational delay is measured using a dummy variable that takes a value of 1 if the delay is at least one year. In Panel C, educational attainment is a categorical variable indicating the reported level of education (0=no formal schooling, 1=primary school, 2=middle school, 3=secondary education, 4=above-secondary education), with category 0 including individuals who received some education but did not complete primary school. Column (1) shows baseline estimates, while column (2) presents results for the baseline specification estimated on a subsample of individuals born after 1989. In columns (3) and (4), the focus is on the same subsample. Column (3) replaces the school-age exposure measure with the early-life exposure index, and column (4) includes both measures simultaneously. Controls include district FE, cohort FE, district trends, and individual controls, including dummy variables indicating if the individual is female, first-born, and Hindu. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors are clustered at the state level.

Source: Authors' estimates.

Table 4: Additional Robustness (Education)

	Baseline	Excl. Orissa	Excl. extremes	Sub-sample (climate)	Climate controls	Excl. females	Excl. high educ.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<b>Panel A:</b>							
<b>Educ. delay:</b>							
<b># of years</b>							
School-age exposure	0.43*** (0.14)	0.53** (0.22)	0.29** (0.15)	0.44*** (0.16)	0.22** (0.097)	0.40* (0.20)	0.52** (0.23)
<b>Panel B:</b>							
<b>Educ. delay:</b>							
<b>yes=1, no=0</b>							
School-age exposure	0.24*** (0.080)	0.23* (0.14)	0.15* (0.077)	0.24*** (0.084)	0.18** (0.071)	0.24** (0.10)	0.28** (0.11)
<b>Panel C:</b>							
<b>Educ. attainment</b>							
School-age exposure	-1.18*** (0.31)	-0.74*** (0.27)	-0.65# (0.41)	-1.30*** (0.24)	-0.81** (0.39)	-0.86# (0.53)	-1.20*** (0.41)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Climate controls	No	No	No	No	Yes	No	No
Observations	70,003	67,770	70,003	66,702	66,702	43,989	50,981
Panel A: Mean dep. var.	0.52						
Panel B: Mean dep. var.	0.33						

Notes: The table presents results after removing extreme exposures and controlling for climate variables. In Panel A, educational delay is calculated as the difference between reported years of schooling and the minimum number of years required in the schooling system to attain the reported educational level. In Panel B, educational delay is measured using a dummy variable that takes a value of 1 if the delay is at least one year. In Panel C, educational attainment is a categorical variable indicating the reported level of education (0=no formal schooling, 1=primary school, 2=middle school, 3=secondary education, 4=above-secondary education), with category 0 including individuals who received some education but did not complete primary school. Column (1) shows baseline estimates, while column (2) presents results for the baseline specification estimated on a subsample of individuals located outside Orissa. In column (3), we recompute the exposure index by removing all winds with values above the 95th percentile of the wind distribution. Column (4) replicates the baseline specification on a subsample for which climate variables are available. In column (5), we include climate controls, such as a district-specific measure capturing the average yearly precipitation (in millimeters) experienced between ages 5-15. Additionally, we include the average temperature (in °C) and the number of exposure days within temperature bins (0-10, 10-20, 20-30, and above 30°C) to which children of a given district were exposed during school age. Column (6) excludes married females and column (7) removes individuals with above-secondary education from the sample. Controls include district FE, cohort FE, district trends, and individual controls, including dummy variables indicating if the individual is female, first-born, and Hindu. #  $p < 0.012$ , \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors are clustered at the state level.

Source: Authors' estimates.

Table 5: Educational Controls (Education)

	Baseline	Predicted educ. attainment	Sub-sample	Parental education
	(1)	(2)	(3)	(4)
<b>Panel A:</b>				
<b>Educ. delay: # of years</b>				
School-age exposure	0.43*** (0.14)	0.43*** (0.15)	0.37* (0.19)	0.37** (0.18)
<b>Panel B:</b>				
<b>Educ. delay: yes=1, no=0</b>				
School-age exposure	0.24*** (0.080)	0.24*** (0.080)	0.24** (0.11)	0.24** (0.11)
<b>Panel C:</b>				
<b>Educ. attainment</b>				
School-age exposure	-1.18*** (0.31)	-1.17*** (0.31)	-0.79 (0.59)	-0.91* (0.47)
Controls	Yes	Yes	Yes	Yes
Predicted educ. attainment	No	Yes	No	No
Parental education	No	No	No	Yes
Observations	70,003	70,003	31,243	31,243
Panel A: Mean dep. var.	0.52			
Panel B: Mean dep. var.	0.33			

Notes: The table presents results on educational delay with the addition of educational controls. In Panel A, educational delay is calculated as the difference between reported years of schooling and the minimum number of years required in the schooling system to attain the reported educational level. In Panel B, educational delay is measured using a dummy variable that takes a value of 1 if the delay is at least one year. In Panel C, educational attainment is a categorical variable indicating the reported level of education (0=no formal schooling, 1=primary school, 2=middle school, 3=secondary education, 4=above-secondary education), with category 0 including individuals who received some education but did not complete primary school. Column (1) shows baseline estimates, while column (2) controls for the individual's predicted probability of completing the reported level of education. Column (3) replicates the baseline specification on a subsample for which parental education is available. In column (4), the same sample as in column (3) is used, and parental education is additionally controlled for. Controls include district FE, cohort FE, district trends, and individual controls, including dummy variables indicating if the individual is female, first-born, and Hindu.\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors are clustered at the state level.

Source: Authors' estimates.

Table 6: Alternative Measures of Storm Exposure (Education)

	Baseline	Sum of squares	50, cubic	64, square	64, cubic	All winds	HURRECON
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<b>Panel A:</b>							
<b>Educ. delay: # of years</b>							
School-age exposure	0.43*** (0.14)	0.43** (0.19)	0.48** (0.21)	0.48** (0.18)	0.42** (0.19)	0.077*** (0.024)	0.39** (0.16)
<b>Panel B:</b>							
<b>Educ. delay: yes=1, no=0</b>							
School-age exposure	0.24*** (0.080)	0.30*** (0.077)	0.30*** (0.084)	0.29*** (0.078)	0.30*** (0.076)	0.044*** (0.014)	0.27*** (0.064)
<b>Panel C:</b>							
<b>Educ. attainment</b>							
School-age exposure	-1.18*** (0.31)	-1.69*** (0.27)	-1.65*** (0.14)	-1.59*** (0.11)	-1.68*** (0.26)	0.013 (0.085)	-1.24*** (0.39)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	70,003	70,003	70,003	70,003	70,003	70,003	70,003
Panel A: Mean dep. var.	0.52	0.52	0.52	0.52	0.52	0.52	0.52
Panel B: Mean dep. var.	0.33	0.33	0.33	0.33	0.33	0.33	0.33

Notes: The table presents results on educational delay with the use of alternative specifications of the school-age exposure to storms. In Panel A, educational delay is calculated as the difference between reported years of schooling and the minimum number of years required to attain the reported level of education. In Panel B, educational delay is measured using a dummy variable that takes a value of 1 if the delay is at least one year. In Panel C, educational attainment is a categorical variable indicating the reported level of education (0=no formal schooling, 1=primary school, 2=middle school, 3=secondary education, 4=above-secondary education), with category 0 including individuals who received some education but did not complete primary school. Column (1) shows baseline estimates, while columns (2)-(7) present results based on alternative specifications of storm exposure. Specifically, in column (2), storm exposure is calculated using the sum of the squares of yearly exposures. In column (3), storm exposure is calculated using a threshold of 50 knots and a cube. In column (4), exposure is calculated using a threshold of 64 knots and a square, and in column (5), exposure is calculated using a threshold of 64 knots and a cube. Column (6) computes exposure using all winds, and finally, in column (7), exposure is computed using the HURRECON model, a threshold of 50 knots, and a square. Controls include district FE, cohort FE, district trends, and individual controls, including dummy variables indicating if the individual is female, first-born, and Hindu.\*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors are clustered at the state level.

Source: Authors' estimates.



Table 7: Damages to School Facilities and School Destruction

	Log avg. # of classrooms in good conditions	with electricity	Share of schools without electricity	with unreliable electricity		
	(1)	(2)	(3)	(4)		
<b>Panel A:</b>						
<b>Damages to school facilities</b>						
Storm exposure	-0.101** (0.047)	-0.056*** (0.0082)	0.045*** (0.011)	0.011** (0.0045)		
Controls	Yes	Yes	Yes	Yes		
Observations	153,789	153,789	153,789	153,789		
Mean dep. var.	4.36	0.688	0.285	0.026		
	Exit share of schools			Share of buildings under construction		
	(1)	(2)	(3)	(4)	(5)	(6)
<b>Panel B:</b>						
<b>School destruction</b>						
Storm exposure	-0.0068 (0.0089)	-0.0046 (0.0091)	0.0084 (0.011)	-0.019*** (0.0023)	-0.025*** (0.0036)	-0.029*** (0.0048)
Storm exposure <sub>(t-1)</sub>		0.016*** (0.0036)	0.029*** (0.0054)		-0.053*** (0.0084)	-0.056*** (0.0093)
Storm exposure <sub>(t-2)</sub>			0.067*** (0.019)			-0.023*** (0.0056)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	109,831	92,918	76,109	109,831	92,918	76,109
Mean dep. var.	0.026			0.008		

Notes: Panel A presents results on damages to school facilities. In column (1), the dependent variable is the log of the average number of classrooms in good conditions, with the average taken across schools at the pincode-year level. In columns (2) and (3), the dependent variable is the share of schools with and without electricity, respectively, in a postal code-year. In the last column, the dependent variable is the share of schools with unreliable electricity. Panel B shows results on school destruction. The dependent variable is the share of existing schools and the share of school buildings under construction in columns (1)-(3) and (4)-(6), respectively, with shares expressed relative to the number of schools in 2010 and taken within a postal code-year. In both panels, storm exposure is computed from wind exposures at the postal code level using a quadratic damage function and a 50 knots threshold. In Panel B, the number of observations differs slightly from that presented in the summary statistics due to singleton observations that are dropped in the estimations. In column (1) of Panel A, the mean dependent variable at the bottom of the table is presented without logs. Controls include postal code FE and district-year FE. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors are clustered at the state level.

Source: Authors' estimates.

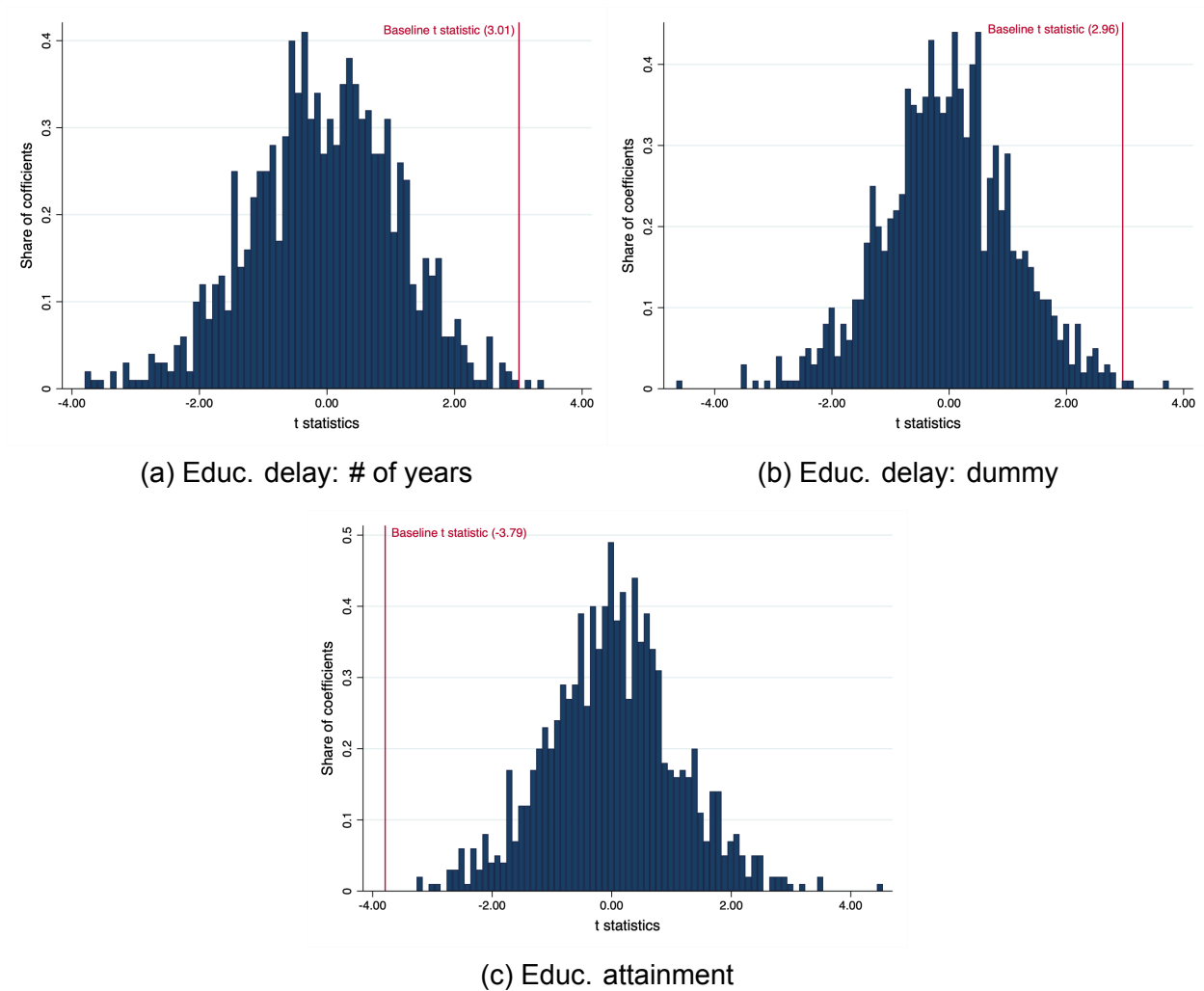
Table 8: School Attendance and Examination Results

	Log avg. # of kids in primary school					Log avg. # of kids in middle school		
	C1	C2	C3	C4	C5	C6	C7	C8
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<b>Panel A:</b>								
<b>School attendance</b>								
Storm exposure	-0.022 (0.085)	-0.18 (0.16)	-0.11* (0.062)	-0.15*** (0.035)	-0.15* (0.085)	-0.075 (0.16)	-0.054 (0.17)	-0.47 (0.44)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	143,579	143,579	143,579	143,579	143,579	143,579	143,579	143,579
Mean dep. var. (w/o logs)	20.102	19.588	19.524	19.264	19.11	17.917	17.603	16.388
	Log avg. # of kids who							
	appeared at exam		passed exam		got a grade > 60%			
	C5	C8	C5	C8	C5	C8		
	(1)	(2)	(3)	(4)	(5)	(6)		
<b>Panel B:</b>								
<b>Examination results</b>								
Storm exposure		-0.17 (0.14)	-3.73*** (0.63)	-0.19 (0.14)	-3.74*** (0.59)	0.032 (0.14)	-3.85*** (0.76)	
Controls		Yes	Yes	Yes	Yes	Yes	Yes	
Observations		126,737	65,195	126,737	65,195	126,737	65,195	
Mean dep. var. (w/o logs)		8.140	0.976	8.048	0.912	5.665	0.473	

Notes: Panel A shows results on school attendance. In columns (1)–(5), the dependent variable is the log average number of primary school children in levels C1–C5, while in columns (6)–(8), the dependent variable is the log average number of middle school children in levels C6–C8. Averages are taken across schools at the postal code-year level. Storm exposure is computed from wind exposures at the postal code level using a quadratic damage function and a 50 knots threshold. Panel B presents results on children's examination results. In column (1) (column 2), the dependent variable is the log average number of children who appeared at the C5 primary school level (C8 middle school level) examination. In column (3) (column 4), the dependent variable is the log average number of children who passed the examination at the C5 level (C8 level). Finally, in column (5) (column 6), the dependent variable is the log average number of children who passed the C5 level (C8 level) examination with a grade above 60%. In both panels, averages are taken across schools at the postal code-year level. Storm exposure is computed from wind exposures at the postal code level using a quadratic damage function and a 50 knots threshold. The number of observations differs slightly from that presented in the summary statistics due to singleton observations that are dropped in the regressions. Controls include postal code FE and district-year FE. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Standard errors are clustered at the state level.

Source: Authors' estimates.

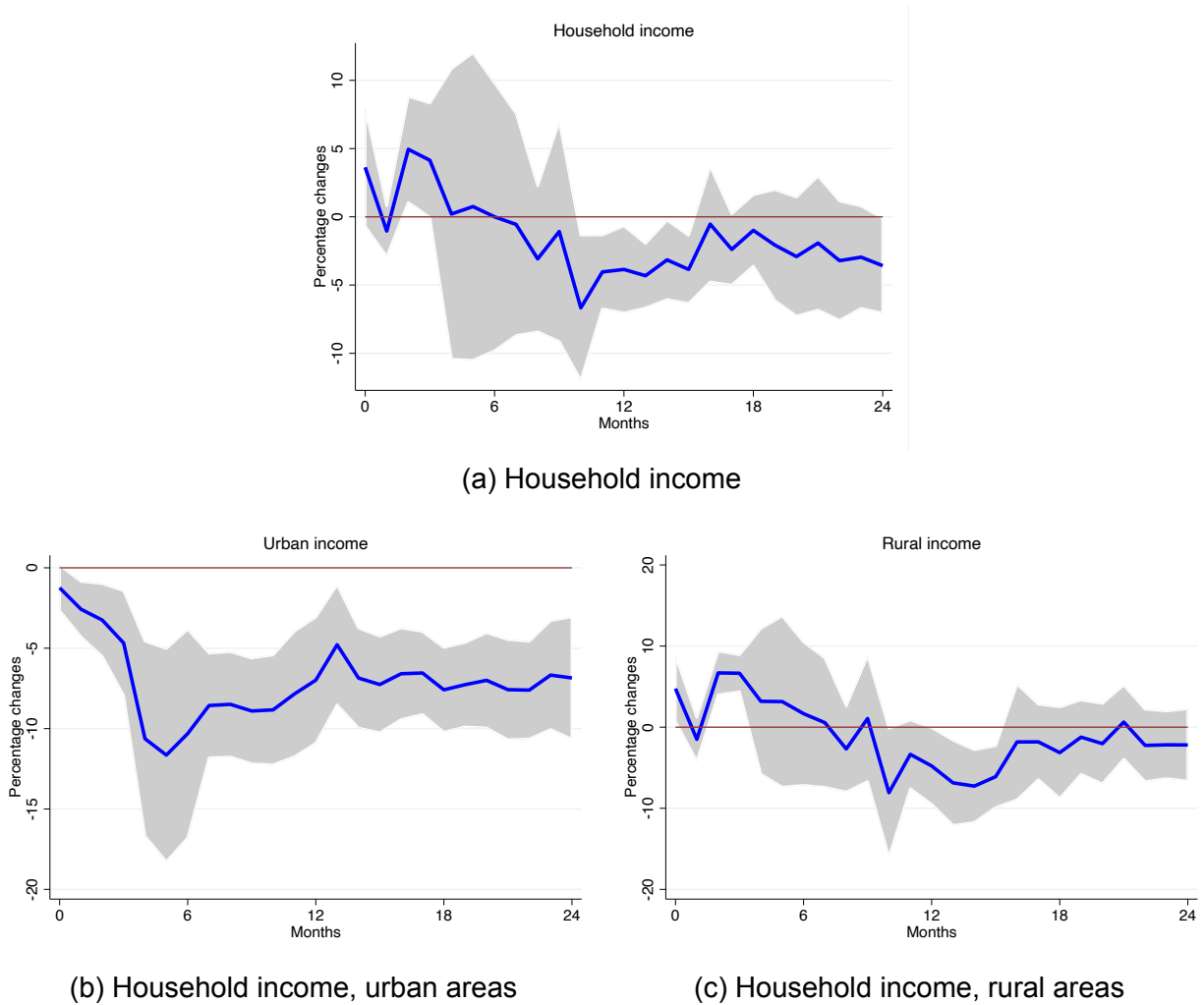
Figure 1: Falsification Tests – Distribution of t-statistics



Notes: The figures display the distribution of t-statistics across 1,000 replications, while the red vertical lines show the t-statistics from our baseline regressions. Panel (a) shows the results for educational delay measured in number of years (baseline t-statistics = 3.01); Panel (b) shows the results for educational delay measured with a dummy variable (1=yes, 0=no, baseline t-statistics = 2.96); finally, Panel (c) shows the result for the ordered logit estimation on educational attainment (baseline t-statistics = -3.79).

Source: Authors' estimates.

Figure 2: Local Projections on Household Income



Notes: The figure shows the results of local projections (direct effect) on household income, both overall (Panel a) and by urban/rural area (Panels b and c), on a 24-month time horizon for the average cyclone exposure. The analysis includes district-year FE, time FE, and household FE. Storm exposure is computed from wind exposures at the district-month level using a quadratic damage function and a 50 knots threshold. The figure presents 95% confidence intervals.

Source: Authors' estimates.

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## **Storms, Early Education, and Human Capital**

This paper examines the impact of school-age exposure to storms on education and employment outcomes in India. Using wind exposure histories, the paper shows that exposure to an average storm can cause a 2.4 percentage point increase in educational delays, a 2 percentage point drop in post-secondary education attainment, and a 1.6 percentage point decline in regular salaried employment. The paper also highlights the role of damaged school infrastructure and declining household income in driving these outcomes.

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