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STORMS, EARLY EDUCATION AND HUMAN CAPITAL

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Storms, Early Education and Human Capital*

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Abstract/Résumé

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.

Cet article étudie l'impact de l'exposition aux tempêtes à l'âge scolaire sur l'éducation et le statut d'activité primaire des jeunes adultes en Inde. À l'aide d'une étude de cohorte transversale basée sur l'historique de l'exposition au vent, nous trouvons des preuves d'une déqualification significative des zones vulnérables aux risques liés au changement climatique. Plus précisément, nos résultats montrent une augmentation de 2,4 points de pourcentage de la probabilité d'accumuler des retards dans l'éducation, une baisse de 2 points de pourcentage de la réussite dans l'enseignement post-secondaire et une réduction de 1,6 point de pourcentage de l'obtention d'un emploi salarié régulier. En outre, notre étude montre que la dégradation des infrastructures scolaires et la baisse des revenus des ménages contribuent à ces résultats.

Keywords/Mots-clés: climate change, storms, education, human capital / changement climatique, tempêtes, éducation, capital humain

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1 Introduction

Low- and middle-income nations confront an inordinately elevated risk of natural catastrophes, as they are both more exposed to climate-related hazards and possess diminished resilience (Dell et al., 2014).[1] This deficiency in resilience renders children particularly susceptible, a concerning fact given that extreme weather events are anticipated to escalate due to climate change (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 & 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 & 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 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

[1] 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.

share of individuals involved in domestic duties. Additionally, we observe that an average exposure incurs a 3.9% reduction in hourly wages. Severe tropical storm, 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 & Russ, 2019; Rosales-Rueda, 2018; Akresh et al., 2017; Dinkelman, 2017; Maccini & 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 Deuchert & Felfe (2015) and Groppo & Kraehnert (2017), 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, natural 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 & Felfe (2015), which 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.[2]

[2]In a related study, Shah & 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

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.[3] Apart from Barattieri et al. (2023), which investigates employment and wage effects in Puerto Rico, we are unaware of other papers that utilize this methodology to study environmental shocks.

Regarding the infrastructure channel, there is limited evidence on the impacts of natural disasters on educational facilities. Our results indirectly highlight the importance of adequate school infrastructure for long-term education and labor market outcomes, thus contributing to the literature on the effects of constructing new schools in developing countries (Damon et al., 2018; Glewwe & Kremer, 2006). For a recent study that focuses on the construction of disaster-resistant schools, see Herrera-Almanza & 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. In Section 3 we discuss the extent to which storms can be considered as exogenous and examine the potential measurement error generated by storm-related migration. Section 4 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 Section 4.1 and robustness tests in Section 4.4. Finally, Section 5 discusses the channels through which storms may affect education in the long-run and Section 6 concludes.

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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.

[3]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; Naguib et al., 2022; Roth Tran & Wilson, 2021). As discussed by Dube et al. (2022), 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.

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.[4] Column (1) displays the number of years required to complete each individual category of schooling. For graduate and postgraduate levels, the figures 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).[5] 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

[4]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

[5]Although it would be intriguing to differentiate between the two types of delays, the PLFS lacks sufficient information to do so. In Section 5.3, we utilize auxiliary data to investigate the impact of storms on school attendance and performance.

the value of 1 for individuals with positive delays and 0 otherwise.

The PLFS supplies information on individuals' primary activity status.[6] 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.

Crucially, the PLFS offers information 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. Given the very small proportion of individuals migrating outside of their birth district (see, for instance, Edmonds et al., 2010; Topalova, 2010; Munshi & Rosenzweig, 2009), we assume that individuals completed their compulsory schooling in the same district where they reside in 2018. This assumption is critical for constructing the school-age storm exposure index and is further explored in Section 3.

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 indi-

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

viduals 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.

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.[7] 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.

School-age exposure to storms is computed as follows:

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

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:

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

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 A of the Online Appendix. The term w^{max} refers to the highest wind speed observed throughout the entire sample. We assume a quadratic damage function in order to capture the force exerted by winds on structures, following e.g. Pelli & Tschopp (2017).[8] Considering the poor quality of construction materials in India, infrastructures and

[7] 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.

[8] In Section 4.4, 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

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. By definition, $x_{dt} \in (0, \mid H_t \mid)$, where a value of 0 indicates zero district exposure to storms (i.e., winds in district d are below the threshold limit), and $\mid H_t \mid$ represents the number of elements (storms) in set H_t.

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.

The left panel of Figure E.2 in the Online Appendix displays the measure of school-age exposure to storms at the state level for our sample.[9] 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, Orissa, and Telangana displaying the highest median exposures.

The right panel of Figure E.2 offers a visualization of the distribution of C_{bd} across districts for the 1987 birth cohort. Darker shades of red represent higher exposures, with the darkest shade indicating districts where the index of school-age exposure to storms exceeds the 90th percentile in the distribution of C_{bd} for individuals born in 1987. Each shade encompasses 15% of the districts with a positive exposure. The northern landlocked region of India demonstrates virtually zero exposure, which aligns with storm best track data,

of compulsory schooling.

[9] Only states with positive exposures are included.

typically highlighting a high concentration of storms along coastal areas. The map reveals that the 1987 cohort residing in the remaining areas of India experienced positive exposure to storms, with districts along the South-Eastern coast being most affected.

Table D.2 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 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.[10]

3 Exogeneity of Storms and Migration

In this section, we discuss two potential concerns that may affect the validity of our results. First, we examine the extent to which storms can be considered exogenous. Second, we investigate the possibility of measurement error in storm exposure due to migration.

Exogeneity of Storms One potential concern with our empirical strategy is the possibility that storms are not random. Figure E.3 in the Online Appendix displays hazard-prone districts based on the frequency of cyclones and their severity. The figure shows that most storm activity occurs on both coasts of the country, with certain areas being more susceptible to storms than others. However, despite being more prone to storms, these areas are also characterized by a high level of industrialization and economic activity.

To obtain a conditionally exogenous measure of storm exposure, we include district fixed

[10] 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.

effects (FE) in all of our specifications. 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 & 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.[11] 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, 2018).

Measurement Error in Storm Exposure One potential source of bias is measurement error in storm exposure due to migration. We construct storm exposure using individuals' place of residence at the time of the survey, as we do not observe their place of birth or residence during their school years. If individuals migrate out-of-district by 2018, the measure may contain error if either the origin or destination districts were exposed to storms during their compulsory schooling. This error could bias our results if migration is selective, such as if a particular educational group systematically moves out-of-district or if movers are concentrated in a specific educational category. Additionally, our estimates could be biased if the probability of moving to another district increases after a storm. We alleviate this concern in three ways, using alternative data sources.

First, we provide evidence that out-of-district migration is negligible in India. Using the 64th round of the National Sample Survey (NSS) for the years 2007-2008, we estimate that only 3.5% of households (out of 125,578) had migrated within the last 365 days, and only 1.3% had migrated permanently, out of which only about half migrated out-of-district. We also 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 5.1 for more details on the Pyramids Dx data). To be consistent with the PLFS estimation sample, we focus on individuals aged 5-33 years old. We find that less than 3% (10,424 out of 367,378) of individuals in the sample moved across districts over a two-year period, and the yearly share of movers is 1.75% on average. These results are consistent with Topalova (2010), who finds that less than 4% (13%) of rural (urban) individuals migrate out-of-district.

Second, we use the same sample from the Consumer Pyramids Dx to show that mobility

[11] 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.

is not concentrated among any particular category of education. Table D.3 in the Online Appendix displays the number of individuals by category of schooling as a share of out-of-district movers (Column 2) and the share of out-of-district movers within each category of education (Column 4). The table reveals that only 0.7% of movers have no education, with about 6.8%, 9.3%, 44.6%, and 38.6% of movers being in primary, middle, secondary, and graduate-level education, respectively. As expected, the share of movers is higher in the 12th grade and at the graduate level, corresponding to moments where students move for higher education or specific graduate programs. Moreover, the share of movers within each schooling category is small, representing less than 2% of individuals within each grade that falls below middle school, less than 3% of individuals within each grade at the secondary level, and about 6% of graduates.

Overall, 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 can reasonably assume that the current district of residence is a good proxy for the place of residence during school age. Furthermore, since mobility does not appear to be concentrated among any particular educational category, any measurement error is unlikely to be systematically biased towards a particular group of education. Hence, we do not expect measurement error to substantially affect our estimates.

Third, we present additional evidence suggesting that any selection bias from endogenous migration responses to disaster shocks is likely to be small, if not absent. Using the same auxiliary database, we estimate the probability of impacted individuals migrating out of their district of residence within a year following the shock. As described in Section B of the Online Appendix, we find that storm exposure does not increase the likelihood of moving to another district. In fact, our estimates indicate that individuals tend to migrate less after a storm.

We interpret this result as indicating that the period following a strike is typically not an opportune time for individuals to start anew, severing ties with their support networks, family, and friends. As we demonstrate in Section 5.1, storms typically do not entirely destroy people's homes and possessions, but they do cause substantial income losses that diminish individuals' ability to move. Therefore, we do not anticipate post-disaster migration to bias our estimates.

This finding aligns with recent studies indicating that natural disasters and migration have a negative or statistically insignificant relationship (e.g., Shakya et al., 2022; Beine et al., 2019; Gröschl & Steinwachs, 2017; Cattaneo & Peri, 2016). Mueller et al. (2014) and Gray & Mueller (2012) show that persistent and slowly worsening natural disasters, such as heat, may result in migration, while floods and storms do not significantly alter individuals' propensity to migrate. This may be because, during times of distress, individuals who are economically disadvantaged are often reluctant to abandon their local adaptation capacity.

4 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.

4.1 Educational Delay

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

$$Y_i = \alpha_0 + \alpha_1 C_{bd} + \mathbf{X_i'}\boldsymbol{\beta} + \delta_d + \delta_b + \xi_d + \epsilon_i, \tag{3}$$

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 indicating if the individual is a female, a first-born child and Hindu respectively.[12] δ_d and δ_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, ξ_d, that accounts for differential trends in district-level education policies and regional disparities in economic growth. Finally, ϵ_i is the error term.[13]

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.[14] State-level clustering also takes into account spatial correlations within a state and time correlations in the exposure

[12] 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.

[13] 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.

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

index resulting from the fact that the same storm affects multiple birth-year cohorts simultaneously.[15]

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. The left panel of 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 Orissa 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 Orissa 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 Orissa 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.[16]

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 under the government of Rajiv Gandhi 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

[15] We work with 35 clusters, including 28 states and 7 union territories. Tables D.5 and D.6 in the Online Appendix show that our results remain consistent when using district clustering or district-cohort clustering.

[16] 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 $0.43 \cdot 0.1 \cdot 42 \cdot 5$.

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 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.2 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.

4.2 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.2 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.4 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.

4.3 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.2 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.

4.4 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.[17]

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 of this exercise will yield mostly statistically insignificant estimates for the variable of interest, while leaving

[17] 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.8 in the Online Appendix). Our baseline estimates remain unchanged even after adding this control.

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 Orissa, 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 5-15. 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.[18]

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.

[18]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.

Across all panels, the coefficient of interest remains precisely estimated and qualitatively similar to the baseline results, albeit with a slightly smaller size effect.

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.[19]

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 & 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.

[19] Since school-age exposure to storms impacts both educational delay and educational attainment, using educational categories fixed effects would cause a bad-control problem

Alternative Measures of School-age Exposure to Storms In Table 6, we explore alternative specifications of C_{bd}. The first alternative 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 5-15), 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 (2) and (4) 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 (3) and (4) of Table 6, we raise the threshold to 64 knots, equivalent to a category 1 cyclone on the Saffir-Simpson scale. In column (5) of Table 6, we eliminate the threshold altogether and include all winds that occur during a cyclone event.

Finally, in column (6) of Table 6, we compute the maximum wind speed hitting each district using the HURRECON wind field model (see Online Appendix A.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 (5), 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.

5 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.[20]

Demand shifts can occur due to the harmful effects of storms on i) household income (or consumption) and ii) children's mental 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.

Financial stress may also lead to malnutrition, which can hinder learning, although malnutrition's effects are more pronounced among younger children. It is essential to emphasize that our study concentrates on school-age shocks, which are less likely to influence long-term educational outcomes through children's health than prenatal or early-life events. However, storms may still impact children's mental health and trigger Post-Traumatic Stress Disorder (PTSD), a condition known to hamper academic and career performance (see, for example, 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. Unfortunately, we cannot explore the PTSD channel due to a lack of publicly available mental health data in India. 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.

5.1 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 approximately 200,000

[20] See Baez et al. (2010) for a comprehensive review of the literature on the channels through which disasters damage human capital.

households between 2014 and 2021.[21] 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.13 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:

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

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 τ (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 τ. 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). δ_{dt}, δ_τ and δ_h represent district-year, time and household fixed effects. Our object of interest, γ_1^k, captures the average response of households' monthly income at horizon k to a storm at time τ. In combination, the sequence of γ_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 γ_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 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

[21] The Consumer Pyramid Dx database is produced by the Center for Monitoring the Indian Economy (CMIE).

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.5 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.5 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 B.

The existing literature on the short-term dynamic effects of natural disasters on household income in developing countries is sparse. However, 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 natural disasters. Our findings for urban wage income in India suggest that implementing similar policies could considerably alleviate the negative impacts as well.[22]

5.2 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.[23] The data also includes the school code identifier and the block, postal code (*pincode*), village and district in which the school is located.[24]

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.13 of the Online Appendix.

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

[22]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 & Polachek, 2009). Research on developing countries primarily focuses on aggregate outcomes as well. For example, Keerthiratne & Tol (2018) in Sri Lanka discover that natural disasters reduce income inequalities as higher-income groups experience a larger share of damages.

[23]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.

[24]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.

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).[25] 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), 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 natural disasters, our findings align with the general understanding that natural disasters increase children's vulnerability

[25]The number of observations decreases between column (1) and (2) due to singleton observations.

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 natural disasters rise. To date, only a few studies have provided direct evidence that constructing more resistant schools can mitigate the detrimental effects of natural disasters on long-term education and labor market outcomes. For example, see Herrera-Almanza & Cas (2021) and Cas (2016) for studies examining the impact of building typhoon-resistant schools in the Philippines.

5.3 Scholastic Outcomes on Impact

The results presented in Section 4 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.[26] Panel II of Table D.13 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 natural 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 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 natural disasters 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

[26] 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.

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 natural 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.[27] 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.

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%

[27] Examination results for other years of schooling are not available.

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 natural disasters and extreme weather shocks on education in developing countries (see, e.g., Deuchert & Felfe, 2015; Spencer et al., 2016).

6 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 natural 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.

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Table 1: Educational Delay and Educational Attainment

	Educational delay (1)	Educational delay (2)	Educational delay (3)
Panel A: # of years			
School-age exposure	0.43***	0.22*	0.28**
	(0.14)	(0.13)	(0.10)
Panel B: yes=1, no=0			
School-age exposure	0.24***	0.20**	0.18**
	(0.080)	(0.084)	(0.072)
Individual controls	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
District trends	Yes	No	No
State-cohort FE	No	Yes	No
State-policy FE	No	No	Yes
Observations	70003	70003	70003
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)
Panel C: Educ. attainment						
School-age exposure	-1.18***	0.046***	0.11***	0.096***	-0.055***	-0.20***
	(0.31)	(0.011)	(0.029)	(0.027)	(0.015)	(0.052)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	70003	70003	70003	70003	70003	70003
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. Individual controls include dummy variables indicating if the individual is female, the first-born child, 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.

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)
Panel A: Type of activity					
School-age exposure	-0.16**	-0.0093	-0.046	0.016	0.16***
	(0.076)	(0.075)	(0.059)	(0.067)	(0.056)
Individual controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
District trends	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)
Panel B: Wage & hours worked				
School-age exposure	0.018	-0.393*	3.57	-4.33
	(0.18)	(0.230)	(3.70)	(4.684)
Individual controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes
Observations	29089	70003	29089	70003
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). 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. Individual controls include dummy variables indicating if the individual is female, first-born child, and Hindu, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level.

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

	Baseline	Sub-sample	Early-life	School & early-life
	(1)	(2)	(3)	(4)
Panel A:				
Educ. delay: # of years				
School-age exposure	0.43***	0.35**		0.35**
	(0.14)	(0.15)		(0.14)
Early-life exposure			-0.16	-0.16
			(0.14)	(0.14)
Panel B:				
Educ. delay: yes=1, no=0				
School-age exposure	0.24***	0.38***		0.38***
	(0.080)	(0.088)		(0.087)
Early-life exposure			-0.086	-0.088
			(0.080)	(0.080)
Panel C:				
Educ. attainment				
School-age exposure	-1.18***	-1.22***		-1.22***
	(0.31)	(0.44)		(0.43)
Early-life exposure			0.82***	0.83***
			(0.13)	(0.15)
Individual controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes
Observations	7003	41892	41892	41892
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. Individual controls include dummy variables indicating if the individual is female, first-born child, and Hindu, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level.

Table 4: Removing Extreme Exposures and Controlling for Climate Variables (Education)

	Baseline	Excl. Orissa	Excl. extremes	Sub-sample	Climate controls
	(1)	(2)	(3)	(4)	(5)
Panel A:					
Educ. delay: # of years					
School-age exposure	0.43***	0.53**	0.29**	0.44***	0.22**
	(0.14)	(0.22)	(0.15)	(0.16)	(0.097)
Panel B:					
Educ. delay: yes=1, no=0					
School-age exposure	0.24***	0.23*	0.15*	0.24***	0.18**
	(0.080)	(0.14)	(0.077)	(0.084)	(0.071)
Panel C:					
Educ. attainment					
School-age exposure	-1.18***	-0.74***	-0.65#	-1.30***	-0.81**
	(0.31)	(0.27)	(0.41)	(0.24)	(0.39)
Individual controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes	Yes
Climate controls	No	No	No	No	Yes
Observations	70003	67770	70003	66702	66702
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. Individual controls include dummy variables indicating if the individual is female, first-born child, and Hindu, respectively. # $p < 0.012$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level.

Table 5: Educational Controls (Education)

	Baseline	Predicted educ. attainment	Sub-sample	Parental education
	(1)	(2)	(3)	(4)
Panel A:				
Educ. delay: # of years				
School-age exposure	0.43***	0.43***	0.37*	0.37**
	(0.14)	(0.15)	(0.19)	(0.18)
Panel B:				
Educ. delay: yes=1, no=0				
School-age exposure	0.24***	0.24***	0.24**	0.24**
	(0.080)	(0.080)	(0.11)	(0.11)
Panel C:				
Educ. attainment				
School-age exposure	-1.18***	-1.17***	-0.79	-0.91*
	(0.31)	(0.31)	(0.59)	(0.47)
Individual controls	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes
Predicted educ. attainment	No	Yes	No	No
Parental education	No	No	No	Yes
Observations	70003	70003	31243	31243
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. Individual controls include dummy variables indicating if the individual is female, first-born child, and Hindu, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level.

Table 6: Alternative Measures (Education)

	Baseline	Sum of squares	50, cubic	64, square	64, cubic	All winds	HURRECON
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A:							
Educ. delay: # of years							
School-age exposure	0.43***	0.43**	0.48**	0.48**	0.42**	0.077***	0.39**
	(0.14)	(0.19)	(0.21)	(0.18)	(0.19)	(0.024)	(0.16)
Panel B:							
Educ. delay: yes=1, no=0							
School-age exposure	0.24***	0.30***	0.30***	0.29***	0.30***	0.044***	0.27***
	(0.080)	(0.077)	(0.084)	(0.078)	(0.076)	(0.014)	(0.064)
Panel C:							
Educ. attainment							
School-age exposure	-1.18***	-1.69***	-1.65***	-1.59***	-1.68***	0.013	-1.24***
	(0.31)	(0.27)	(0.14)	(0.11)	(0.26)	(0.085)	(0.39)
Individual controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	70003	70003	70003	70003	70003	70003	70003
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. Individual controls include dummy variables indicating if the individual is female, first-born child, and Hindu, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level.

Table 7: Damages to School Facilities and School Destruction

	Log avg. # of classrooms in good conditions	Share of schools with: electricity	w/o electricity	unreliable electricity
	(1)	(2)	(3)	(4)
Panel A:				
Damages to school facilities				
Storm exposure	-0.101**	-0.056***	0.045***	0.011**
	(0.047)	(0.0082)	(0.011)	(0.0045)
Postal code FE	yes	yes	yes	yes
District-year FE	yes	yes	yes	yes
Observations	153789	153789	153789	153789
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)
Panel B:						
School destruction						
Storm exposure	-0.0068	-0.0046	0.0084	-0.019***	-0.025***	-0.029***
	(0.0089)	(0.0091)	(0.011)	(0.0023)	(0.0036)	(0.0048)
Storm exposure$_{(t-1)}$		0.016***	0.029***		-0.053***	-0.056***
		(0.0036)	(0.0054)		(0.0084)	(0.0093)
Storm exposure$_{(t-2)}$			0.067***			-0.023***
			(0.019)			(0.0056)
Postal code FE	yes	yes	yes	yes	yes	yes
District-year FE	yes	yes	yes	yes	yes	yes
Observations	109831	92918	76109	109831	92918	76109
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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level.

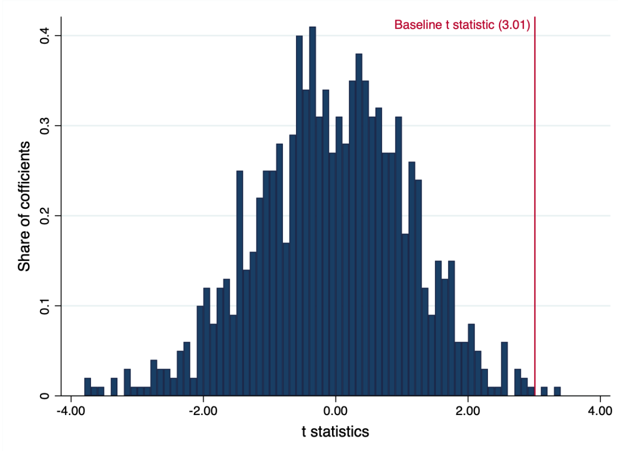
Table 8: School Attendance and Examination Results

	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)
Panel A:								
School attendance								
Storm exposure	-0.022	-0.18	-0.11*	-0.15***	-0.15*	-0.075	-0.054	-0.47
	(0.085)	(0.16)	(0.062)	(0.035)	(0.085)	(0.16)	(0.17)	(0.44)
Postal code FE	yes	yes	yes	yes	yes	yes	yes	yes
District-year FE	yes	yes	yes	yes	yes	yes	yes	yes
Observations	143579	143579	143579	143579	143579	143579	143579	143579
Mean dep. var. (w/o logs)	20.102	19.588	19.524	19.264	19.11	17.917	17.603	16.388

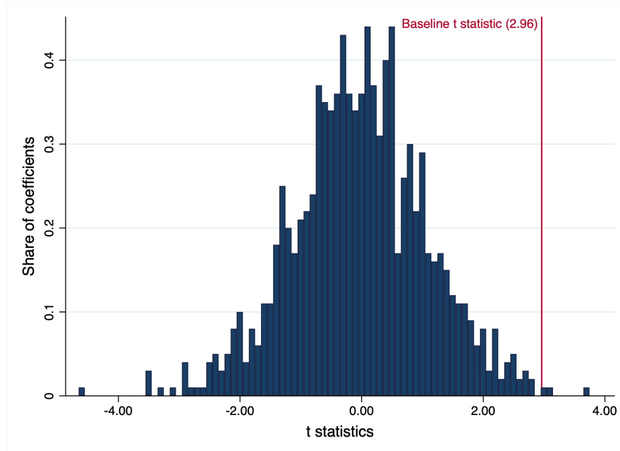
	Log avg. # of kids who appeared at exam.		passed exam.		got a grade > 60%	
	C5	C8	C5	C8	C5	C8
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B:						
Examination results						
Storm exposure	-0.17	-3.73***	-0.19	-3.74***	0.032	-3.85***
	(0.14)	(0.63)	(0.14)	(0.59)	(0.14)	(0.76)
Postal code FE	yes	yes	yes	yes	yes	yes
District-year FE	yes	yes	yes	yes	yes	yes
Observations	126737	65195	126737	65195	126737	65195
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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level.

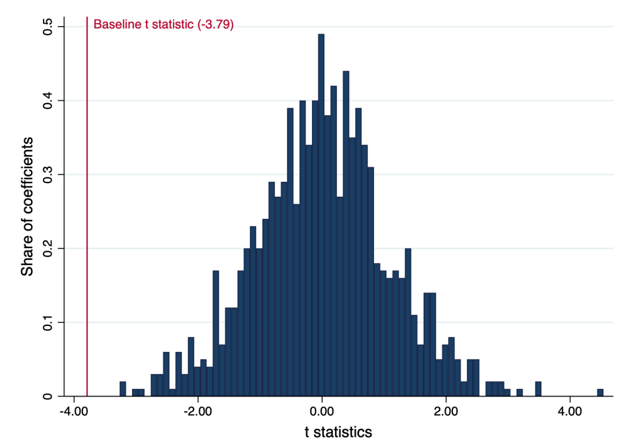
Figure 1: Falsification Tests – Distribution of t-statistics



(a) Educ. delay: # of years



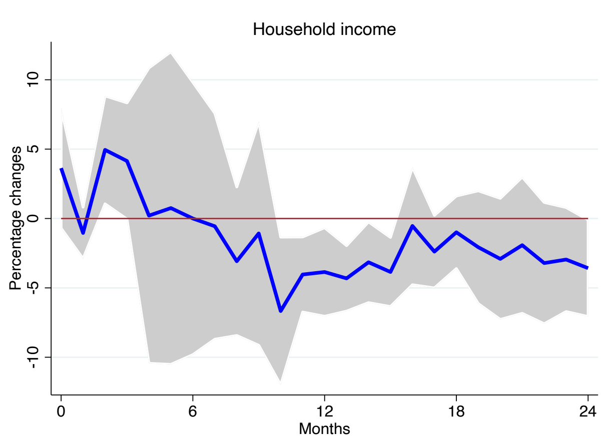
(b) Educ. delay: dummy



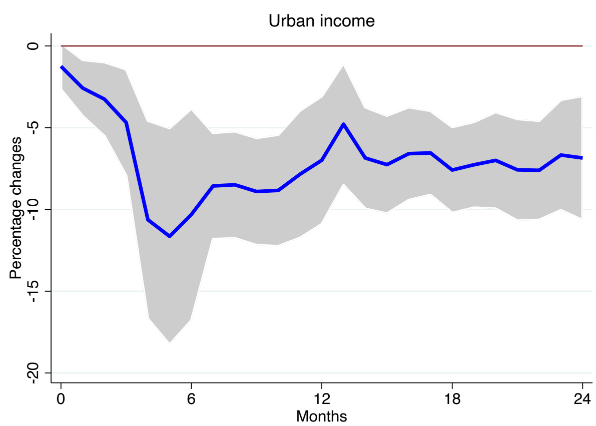
(c) Educ. attainment

Note: 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).

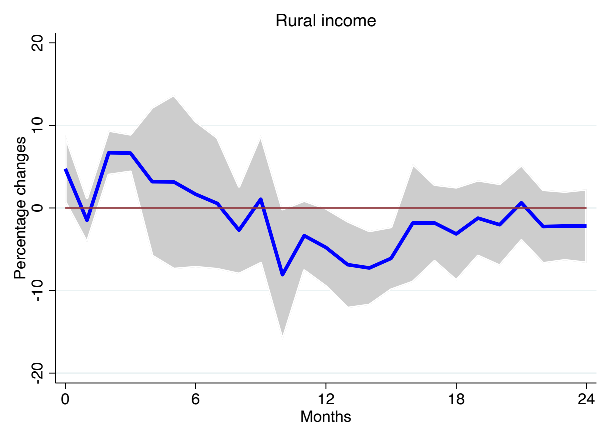
Figure 2: Local Projections on Household Income



(a) Household income



(b) Household income, urban areas



(c) Household income, rural areas

Note: 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.

Online Appendix for:

Storms, Early Education and Human Capital

Martino Pelli (Sherbrooke University)
Jeanne Tschopp (University of Bern)

A Appendix. Computing Wind Speed

A.1 Baseline Wind Field Model: the Rankine-combined Formula (Deppermann, 1947)

In this appendix, we explain how we calculate w_{dh}, which represents the maximum wind speed associated with storm h in district d. We use data from the National Oceanic and Atmospheric Administration (NOAA) Tropical Prediction Center, specifically the best tracks of storms in the North Indian and South Indian basins from 1990 to 2010 (Knapp et al., 2010). The best tracks provide a comprehensive record of each storm, including latitude, longitude, date, and wind speed at 6-hour intervals for the eye of the storm.

We begin by linearly interpolating the best tracks of storms at every kilometre, resulting in a set of landmarks k, each with a set of coordinates and a corresponding wind speed at the eye of the storm, denoted as e_k. Next, for each district falling within the vortex associated with a landmark, we use the Rankine-combined formula (Deppermann, 1947) to compute the winds at the district's centroid. This formula describes wind fields as follows:

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

where D_{dk} is the distance between the centroid of district d and landmark k. The number 26.9978 corresponds to the Simpson and Riehl radius of maximum wind speed in knots, which is the distance between the eye and the point where wind reaches its maximum speed.[1] According to this formula, winds first increase exponentially up to a maximum and then decrease rapidly.

Finally, we obtain a single wind speed measure per district and storm by selecting the maximum wind speed to which the district was exposed, i.e.:

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

[1] In reality, the radius of maximum wind speed for each cyclone varies, and is calculated using the difference in barometric pressure between the center and the outskirts of the storm. However, cyclone data are often characterized by a high number of missing data when it comes to barometric pressure. Therefore, we follow the approach of Simpson & Riehl (1981) and Hsu & Zhongde (1998) and apply the average radius of maximum wind speed, 50 km, to all the cyclones considered in this paper.

A.2 Alternative Wind Field Model: the HURRECON Model (Boose et al., 1994)

As an alternative wind field model, we use the HURRECON model (see Boose et al., 1994, 2001, 2004). The model uses information on the track, size, intensity, and cover type (land or water) of a hurricane to describe sustained wind velocity at any point within a cyclone's vortex. Specifically, the computation of sustained wind velocity at each district centroid is done using the following equation:[2]

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

where F is a scaling parameter capturing the effect of friction. Usually this parameter is set to 1 for points over water and to 0.8 for points over land. In our case, $F = 0.8$. V_k captures the wind velocity at the eye at landmark k, which we linearly interpolate from the best track data. S is a scaling parameter for the asymmetry due to the forward motion of the storm, set to 1 as in Boose et al. (2001); T is the clockwise angle between the forward path of the hurricane and a radial line connecting the eye of the hurricane to the centroid of a district;V_f denotes the forward velocity of the hurricane; R_m is the radius of maximum winds, set as in the baseline at 26.9978; R is the Euclidean distance from the center of the hurricane to the centroid of a district. Finally, the parameter B controls for the shape of the wind profile curve and is set at 1.35. The parameters of this equation are chosen following Boose et al. (2004) who parameterized and validated the model.

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

B Appendix. Storms and Migration

As discussed in Section 3 of the paper, one of the threats to our identification is the possibility that people may migrate out of their district in response to storms. In this section, we use the Pyramids Dx dataset to investigate whether individuals do in fact migrate after storms. While the Pyramids Dx only introduced migration questions in their survey in 2020, we still have a panel of 367,378 individuals aged 5 to 33, who are interviewed every four months, providing us with six observations over the two years of available data.

We run the following specification:

$$M_{i\tau} = \zeta_0 + \zeta_1 x_{d\tau} + \zeta_2 x_{d\tau-1} + \zeta_3 x_{d\tau-2} + \delta_i + \delta_\tau + \delta_{dt} + \varepsilon_i,$$

where i denotes an individual and τ denotes time (a quadrimester-year pair). M_{it} is a dummy equal to one if the individual migrated out of their district of residence in the last four months, and $x_{d\tau}$ ($x_{d\tau-1}$) captures district storm exposure over the period $\tau - 1$ to τ ($\tau - 2$ to $\tau - 1$). δ_i, δ_τ and δ_{dt} represent individual, time, and district-year FE, respectively. ζ_1 captures the percentage points changes in the contemporaneous probability of out-of-district migration. We use weights provided by Pyramids Dx and cluster standard errors at the state level.

Table D.4 displays the results from estimating the above equation. Panel A pertains to individuals aged 5 to 33, while Panel B focuses on children aged 5 to 15, the compulsory schooling age range. Column (1) shows the contemporaneous effect of storm exposure on the probability of migration. In column (2), we add the first lag of storm exposure (4 months prior), and in column (3), we include a second lag (8 months prior). The negative coefficients are generally precisely estimated, indicating that the occurrence of a storm reduces the probability of migration.

C Appendix. Robustness

In this Appendix, we show the robustness results for the type of activity performed by individuals once they reach early adulthood.

Table D.7 evaluates the importance of early-life exposure to storm on individuals' type of activity. The measure is constructed as described in Section 4.4 of the paper. Panel A presents the baseline specification, Panel B reports results on the restricted subsample of individuals born after 1989. Panel C replaces school-age (C_{bd}) with early-life exposure, and Panel D includes both early-life and school-age exposures simultaneously. Notably, compared to the subsample, the inclusion of a measure that accounts for early-life shocks does not affect the estimate on the school-age exposure index, indicating that the period of school years is also highly responsive to shocks.

Table D.9 presents the results of the falsification test and reports the proportion of replications that yield statistically significant estimates at the 1%, 5%, and 10% levels, respectively. Overall, the results suggest that our coefficients do not capture spurious correlations. The numbers in column (1) indicate that statistically significant estimates at the 1% level are produced in only 1.8% (Panel E) to 2.7% (Panel B) of the cases. As expected, the proportion of significant estimates increases when considering higher levels of statistical significance, reaching a maximum of 8.5% (Panel D) at the 5% level and 13.8% (Panel B) at the 10% level.

In Table D.10, we explore the sensitivity of our results to extreme values of exposure. Panel A shows the baseline results, and Panel B excludes individuals residing in Orissa. In Panel C, we recompute the exposure index by removing winds at the top 5% of the wind distribution. The estimates obtained in Panel B and C are consistent with the baseline results in Panel A. The last two panels of the table control for climate variables, constructed as described in Section 4.4 of the paper. Panel D runs the baseline specification on the subsample for which rainfall and temperature data are available, and Panel E includes the climate controls in the regression. Adding these additional variables does not change the estimates of interest.

In Table D.11 we account for individuals' education as described in Section 4.4 of the paper. Panel A of the table shows the baseline results and Panel B includes the predicted probability of completing the reported level of education as a control variable. The inclusion of this variable does not affect the baseline estimates; exposure to storms during compulsory schooling reduces the probability of being employed as a regular worker and increases the likelihood of performing domestic duties in a statistically significant manner. Panel C replicates the baseline regression on the subsample that includes information on parental education. The estimate of the probability of performing regular work is slightly less precise in this subsample. However, including parental education as a control variable (Panel D) produces estimates that are very similar to those obtained in Panel C.

Finally, Table D.12 shows results obtained with using alternative specifications of our measure of school-age exposure to storms. While the magnitudes of the estimates are similar to the baseline, they are generally less precise. Notably, the impact on casual labor, unpaid family work, and involvement in domestic duties is consistent across all specifications. However, we find that the negative and statistically significant effect on regular work is estimated imprecisely when the definition of storm is altered. In terms of self-employment, we obtain negative and precise estimates with all alternative measures except for the one computed from all winds.

D Appendix. Tables

Table D.1: Schooling System in India

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

Notes: Column (1) shows the duration of each category of schooling, which is the standard time required to complete each level of education. For categories such as *Graduate* and *Postgraduate*, the duration corresponds to the mode across disciplines. Column (2) gives the total number of years of education accumulated after completion of each category of schooling (and path in the case of higher education).

Table D.2: Summary Statistics

	Mean (1)	Std. Dev. (2)	Min (3)	Max (4)	N (5)
A. School-age exposure to storm					
C_{bd}	0.029	0.095	0	1.003	70,003
$C_{bd} > 0$	0.098	0.154	1.23e-08	1.003	20,750
B. Educational delay					
Educational delay (# of years)	0.52	0.89	0	6	70,003
Educational delay (yes=1, no=0)	0.331	0.471	0	1	70,003
C. Educational attainment (yes=1, no=0)					
Below primary	0.027	0.161	0	1	70,003
Primary	0.098	0.297	0	1	70,003
Middle	0.239	0.427	0	1	70,003
Secondary	0.365	0.481	0	1	70,003
Above secondary	0.272	0.445	0	1	70,003
D. Primary activity status (yes=1, no=0)					
Regular work	0.196	0.397	0	1	70,003
Casual labor	0.093	0.29	0	1	70,003
Self-employment	0.132	0.338	0	1	70,003
Unpaid family work	0.079	0.269	0	1	70,003
Domestic duties	0.329	0.47	0	1	70,003
E. Wages and hours worked					
(Real) log hourly wage	3.713	0.654	-1.142	7.796	29399
Weekly hours worked	53.603	13.148	2	105	29399
F. Controls (yes=1, no=0)					
Female	0.472	0.499	0	1	70,003
First born	0.308	0.462	0	1	70,003
Hindu	0.740	0.439	0	1	70,003

Note: The category *below primary* refers to individuals who received some education but did not complete primary school. This category excludes illiterate individuals and those without any formal education. Wages are reported in rupees.

Table D.3: Migration by Educational Attainment

	Movers (1)	**Share of movers** (2)	**Individuals** (3)	**Share of individuals** (4)
No education:				
No education	69	0.7	5,447	1.3
Primary:				
1^{st} grade	82	0.8	8,137	1.0
2^{nd} grade	99	1.0	10,413	0.9
3^{rd} grade	115	1.1	11,638	1.0
4^{th} grade	151	1.5	11,785	1.3
5^{th} grade	252	2.4	18,591	1.3
Middle:				
6^{th} grade	249	2.4	17,430	1.4
7^{th} grade	243	2.3	18,157	1.3
8^{th} grade	483	4.6	24,847	1.9
Secondary:				
9^{th} grade	383	3.7	20,964	1.8
10^{th} grade	1,213	11.6	48,071	2.5
11^{th} grade	386	3.7	18,516	2.1
12^{th} grade	2,673	25.6	88,567	3.0
Above secondary:				
Graduate and above	4,026	38.6	64,815	6.2
Total	10,424	100	367,378	

Notes: The table provides information on out-of-district movers by category of schooling for individuals aged 5-33 years old. Column (1) shows the number of individuals who moved out of their district by category of schooling. Column (2) shows the share of individuals who moved by category out of the total number of movers. Column (3) displays the total number of individuals by category of schooling and column (4) shows the share of out-of-district movers within each category of schooling.

Table D.4: Probability of migration

	Probability of migration		
	(1)	(2)	(3)
Panel A: 5 to 33 years old			
Storm exposure	-0.020***	-0.012***	-0.014
	(0.0062)	(0.0022)	(0.0083)
L.Storm exposure		-0.0087**	-0.012***
		(0.0037)	(0.0023)
L2.Storm exposure			-0.0095***
			(0.00076)
Observations	1,490,058	1,189,250	908,242
Panel B: 5 to 15 years old			
Storm exposure	-0.011***	-0.017***	-0.011***
	(0.0038)	(0.0022)	(0.0038)
L.Storm exposure		-0.010*	-0.020***
		(0.0052)	(0.0037)
L2.Storm exposure			-0.018***
			(0.0012)
Observations	479,970	375,652	280,078
Individual FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
District-year FE	Yes	Yes	Yes

Notes: The table estimates the effect of storm exposure on the probability of an individual migrating. The dependent variable is a binary variable that takes the value of one if the individual migrated and zero otherwise. The data come from the Consumer Pyramids DX, in which each individual is surveyed every four months. The first lag, *L.Storm exposure*, corresponds to a four-month lag, and the second lag, *L2.Storm exposure*, corresponds to an eight-month lag. Time FE represent a FE for each four-month-year cell. In each regression, the weights provided by the Consumer Pyramids DX dataset are used. Standard errors are clustered at the state level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.5: Alternative Clustering (Education)

	Baseline	District	District-cohort
	(1)	(2)	(3)
Panel A:			
Educ. delay: # of years			
School-age exposure	0.43***	0.43**	0.43**
	(0.14)	(0.17)	(0.17)
Panel B:			
Educ. delay: yes=1, no=0			
School-age exposure	0.24***	0.24***	0.24***
	(0.080)	(0.072)	(0.079)
Panel C:			
Educ. attainment			
School-age exposure	-1.18***	-1.18***	-1.18***
	(0.31)	(0.35)	(0.31)
Individual controls	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
District trends	Yes	Yes	Yes
Observations	70003	70003	70003
Panel A: Mean dep. var.	0.52		
Panel B: Mean dep. var.	0.33		

Notes: The table presents results on educational delay and attainment, with alternative clustering methods. In Panel A, educational delay is measured as the difference between reported years of schooling and the minimum number of years needed in the schooling system to achieve the reported educational attainment. In Panel B, educational delay is measured with a dummy variable taking the value of one in the case of an educational delay of at least one year. In Panel C, educational attainment is a categorical variable indicating the reported educational attainment (0=no formal schooling, 1=primary school, 2=middle school, 3=secondary education, 4=above-secondary education), where category 0 includes individuals who received some education but did not complete primary school. Column (1) shows the baseline estimates with state clustering, column (2) uses district clustering, and in column (3) standard errors are clustered at the district-cohort level. Individual controls include dummy variables indicating if the individual is female, the first-born child, and Hindu, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.6: Alternative Clustering (Type of Activities)

	Regular work	Casual labor	Self-employed	Unpaid family work	Domestic duties
	(1)	(2)	(3)	(4)	(5)
Panel A:					
Baseline					
School-age exposure	-0.16**	-0.0093	-0.046	0.016	0.16***
	(0.076)	(0.075)	(0.059)	(0.067)	(0.056)
Observations	70003	70003	70003	70003	70003
Panel B:					
District					
School-age exposure	-0.16**	-0.0093	-0.046	0.016	0.16***
	(0.079)	(0.064)	(0.058)	(0.052)	(0.054)
Observations	70003	70003	70003	70003	70003
Panel C:					
District-cohort					
School-age exposure	-0.16**	-0.0093	-0.046	0.016	0.16***
	(0.066)	(0.060)	(0.054)	(0.044)	(0.054)
Observations	70003	70003	70003	70003	70003
Individual controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes	Yes

Notes: The table presents results on the employment status of young adults, with alternative clustering methods. The dependent variable is a dummy variable that takes the value of 1 if the individual's main activity is regular work (column 1), casual labor (column 2), self-employment (column 3), unpaid family work (column 4), or domestic duties (column 5). In Panel A, baseline estimates are shown with state clustering. Panel B uses district clustering, and in Panel C, standard errors are clustered at the district-cohort level. Individual controls include dummy variables indicating if the individual is female, the first-born child, and Hindu, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.7: Controlling for Early-life Exposure (Type of Activities)

	Regular work	Casual labor	Self-employed	Unpaid family work	Domestic duties
	(1)	(2)	(3)	(4)	(5)
Panel A:					
Baseline					
School-age exposure	-0.16**	-0.0093	-0.046	0.016	0.16***
	(0.076)	(0.075)	(0.059)	(0.067)	(0.056)
Observations	70003	70003	70003	70003	70003
Panel B:					
Sub-sample					
School-age exposure	0.000098	-0.10	-0.11***	-0.020	0.18**
	(0.032)	(0.11)	(0.029)	(0.045)	(0.069)
Observations	41892	41892	41892	41892	41892
Panel C:					
Early life					
Early-life exposure	-0.024	-0.051	0.068	-0.058*	-0.21***
	(0.067)	(0.040)	(0.054)	(0.030)	(0.070)
Observations	41892	41892	41892	41892	41892
Panel D:					
School & early life					
School-age exposure	0.00028	-0.100	-0.11***	-0.020	0.19***
	(0.032)	(0.11)	(0.029)	(0.046)	(0.067)
Early-life exposure	-0.024	-0.051	0.069	-0.058*	-0.21***
	(0.067)	(0.039)	(0.055)	(0.031)	(0.071)
Observations	41892	41892	41892	41892	41892
Individual controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes	Yes

Notes: The table presents results on the employment status of young adults, controlling for early-life exposure to storms. The dependent variable is a binary variable taking the 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), unpaid family work (column 4) or domestic duties (column 5). Panel A presents the baseline estimates, while Panel B reports the results for the sub-sample of individuals born after 1989. In Panels C and D, the analysis focuses on the same sub-sample. Panel C replaces the school-age exposure measure with the early-life exposure index, and Panel D includes both measures simultaneously. Individual controls include dummy variables indicating if the individual is female, the first-born child, and Hindu, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level.

Table D.8: Controlling for After-school Exposure (Type of Activities)

	Regular work	Casual labor	Self-employed	Unpaid family work	Domestic duties
	(1)	(2)	(3)	(4)	(5)
Panel A:					
Baseline					
School-age exposure	-0.16**	-0.0093	-0.046	0.016	0.16***
	(0.076)	(0.075)	(0.059)	(0.067)	(0.056)
Observations	70003	70003	70003	70003	70003
Panel B:					
School & after-school					
School-age exposure	-0.16**	-0.010	-0.046	0.016	0.16***
	(0.076)	(0.075)	(0.059)	(0.067)	(0.056)
After-school exposure	0.64***	-0.50***	0.072	0.22*	-0.44***
	(0.19)	(0.15)	(0.11)	(0.12)	(0.13)
Observations	70003	70003	70003	70003	70003
Individual controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes	Yes

Notes: The table presents results on the employment status of young adults, controlling for after-school exposure to storms. The dependent variable is a dummy variable that takes the value of 1 if the individual's main activity is regular work (column 1), casual labor (column 2), self-employment (column 3), unpaid family work (column 4), or domestic duties (column 5). Panel A shows the baseline estimates, while Panel B includes a control for after-school storm exposure, computed by summing yearly exposures over the after-school period up to 2018. Individual controls include dummy variables indicating if the individual is female, the first-born child, and Hindu, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level.

Table D.9: Falsification Test (Type of Activities)

	Placebo Share of estimations with statistical significance at:		
	1%	5%	10 %
	(1)	(2)	(3)
Panel A:			
Regular work			
School-age exposure	0.023	0.066	0.119
Panel B:			
Casual work			
School-age exposure	0.027	0.077	0.138
Panel C:			
Self-employed			
School-age exposure	0.022	0.06	0.127
Panel D:			
Unpaid family work			
School-age exposure	0.022	0.085	0.133
Panel E:			
Domestic duties			
School-age exposure	0.018	0.076	0.125
Individual controls	Yes	Yes	Yes
District FE	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes
District trends	Yes	Yes	Yes
Observations	70003	70003	70003

Notes: The table reports the results of falsification tests for the regression on the employment status of young adults. In Panels A-E, the dependent variable is a binary variable taking the value of one if an individual's primary activity is regular work, casual work, self-employment, unpaid family work, or domestic duties, respectively. Columns (1)-(3) show the share of statistically significant results over 1000 randomizations, where the school-age exposure measure is randomized over the entire sample. Individual controls include dummy variables indicating if the individual is female, the first-born child, and Hindu, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level.

Table D.10: Removing Extreme Exposures and Controlling for Climate Variables (Type of Activities)

	Regular work	Casual labor	Self-employed	Unpaid family work	Domestic duties
	(1)	(2)	(3)	(4)	(5)
Panel A:					
Baseline					
School-age exposure	-0.16**	-0.0093	-0.046	0.016	0.16***
	(0.076)	(0.075)	(0.059)	(0.067)	(0.056)
Observations	70003	70003	70003	70003	70003
Panel B:					
Excl. Orissa					
School-age exposure	-0.26***	-0.069	0.026	0.066	0.19**
	(0.083)	(0.11)	(0.051)	(0.12)	(0.088)
Observations	67770	67770	67770	67770	67770
Panel C:					
Excl. extremes					
School-age exposure	-0.15***	-0.022	-0.0056	0.026	0.089*
	(0.051)	(0.065)	(0.052)	(0.058)	(0.046)
Observations	70003	70003	70003	70003	70003
Panel D:					
Sub-sample					
School-age exposure	-0.15*	-0.011	-0.066	0.031	0.16**
	(0.076)	(0.079)	(0.048)	(0.072)	(0.060)
Observations	66702	66702	66702	66702	66702
Panel E:					
Climate controls					
School-age exposure	-0.13*	-0.053	-0.070	0.026	0.17**
	(0.074)	(0.085)	(0.048)	(0.072)	(0.064)
Observations	66702	66702	66702	66702	66702
Individual controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes	Yes

Notes: The table reports results on the employment status of young adults, after removing extreme exposures or controlling for climate variables. The dependent variable is a dummy equal to 1 if the main activity of the individual is to perform regular work (column 1), casual labor (column 2), be self-employed (column 3), work as an unpaid family worker (column 4), and perform domestic duties (column 5). Panel A presents baseline estimates, while Panel B presents results for the baseline specification estimated on a subsample that excludes individuals located in Orissa. In Panel C, we recompute the exposure index by removing all the winds with values falling above the 95th percentile of the wind distribution. Panel D replicates the baseline specification on the subsample for which climate variables are available. In Panel E, we include climate controls, such as a district-specific measure capturing the average yearly precipitation (in millimeters) between ages 5-15. We also 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. Individual controls include dummy variables indicating if the individual is female, the first-born child, and Hindu, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level.

Table D.11: Educational Controls (Type of Activities)

	Regular work	Casual labor	Self-employed	Unpaid family work	Domestic duties
	(1)	(2)	(3)	(4)	(5)
Panel A:					
Baseline					
School-age exposure	-0.16**	-0.0093	-0.046	0.016	0.16***
	(0.076)	(0.075)	(0.059)	(0.067)	(0.056)
Observations	70003	70003	70003	70003	70003
Panel B:					
Predicted educ. attainment					
School-age exposure	-0.16**	-0.0087	-0.047	0.015	0.16***
	(0.075)	(0.075)	(0.059)	(0.067)	(0.056)
Predicted educ. attainment	Yes	Yes	Yes	Yes	Yes
Observations	70003	70003	70003	70003	70003
Panel C:					
Sub-sample					
School-age exposure	-0.18*	0.0019	-0.11	0.036	0.15***
	(0.10)	(0.060)	(0.11)	(0.090)	(0.050)
Observations	31243	31243	31243	31243	31243
Panel D:					
Parental education					
School-age exposure	-0.18	0.00050	-0.11	0.036	0.15***
	(0.11)	(0.059)	(0.11)	(0.090)	(0.050)
Parental education	Yes	Yes	Yes	Yes	Yes
Observations	31243	31243	31243	31243	31243
Individual controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes	Yes

Notes: The table presents results on the employment status of young adults, controlling for education. The dependent variable is a dummy equal to 1 if the main activity of the individual is to perform regular work (column 1), casual labor (column 2), be self-employed (column 3), work as an unpaid family worker (column 4), or perform domestic duties (column 5). Panel A shows baseline estimates. In Panel B, we add a control for the individual's predicted probability of completing the reported level of education. Panel C replicates the baseline specification on a subsample for which parental education is available. In Panel D, we use the same subsample as in Panel C and additionally control for parental education. Individual controls include dummy variables indicating if the individual is female, the first-born child, and Hindu, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level.

Table D.12: Alternative Measures (Type of Activities)

	Regular work	Casual labor	Self-employed	Unpaid family work	Domestic duties
	(1)	(2)	(3)	(4)	(5)
Panel A:					
Baseline					
School-age exposure	-0.16**	-0.0093	-0.046	0.016	0.16***
	(0.076)	(0.075)	(0.059)	(0.067)	(0.056)
Panel B:					
Sum of squares					
School-age exposure	-0.072	0.029	-0.14***	-0.012	0.19**
	(0.065)	(0.037)	(0.020)	(0.032)	(0.087)
Panel C:					
50, cubic					
School-age exposure	-0.12	0.016	-0.11**	-0.0032	0.21**
	(0.096)	(0.060)	(0.046)	(0.052)	(0.096)
Panel D:					
64, quadratic					
School-age exposure	-0.12	0.012	-0.093*	-0.0025	0.21**
	(0.086)	(0.062)	(0.051)	(0.055)	(0.088)
Panel E:					
64, cubic					
School-age exposure	-0.069	0.034	-0.14***	-0.016	0.20*
	(0.063)	(0.032)	(0.019)	(0.028)	(0.100)
Panel F:					
All winds					
School-age exposure	-0.011	0.00010	-0.022	0.00032	0.013
	(0.011)	(0.015)	(0.019)	(0.0091)	(0.012)
Panel G:					
HURRECON					
School-age exposure	-0.13	0.016	-0.087*	0.0044	0.17**
	(0.083)	(0.073)	(0.051)	(0.060)	(0.065)
Observations	70003	70003	70003	70003	70003
Individual controls	Yes	Yes	Yes	Yes	Yes
District FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
District trends	Yes	Yes	Yes	Yes	Yes

Notes: The table presents results on the employment status of young adults, using alternative specifications of school-age exposure to storms. The dependent variable is a dummy equal to 1 if the main activity of the individual is to perform regular work (column 1), casual labor (column 2), be self-employed (column 3), work as an unpaid family worker (column 4) and perform domestic duties (column 5). Panel A shows baseline estimates, and Panel B to Panel G shows results using different specifications of the storm exposure measure. In Panel B, storm exposure is computed using the sum of the squares of yearly exposures. In Panel C, storm exposure is computed using a threshold of 50 knots and a cube function. In Panel D, exposure is computed using a threshold of 64 knots and a square function. In Panel E, exposure is computed using a threshold of 64 knots and a cube function. In Panel F, exposure is computed using all winds. Finally, in Panel G, exposure is computed using the HURRECON model, a threshold of 50 knots, and a square function. Individual controls include dummy variables indicating if the individual is female, the first-born child, and Hindu, respectively. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level.

Table D.13: Summary Statistics: Consumer Pyramid DX and DISE

Variable	Mean	Std. Dev.	Min.	Max.	N
I. Consumer Pyramid DX:					
Household income:					
Overall	12299	13111	0	2722678	9932098
From wages	8745	9690	0	1413483	9932098
From business profits	2328	9634	0	2719837	9932098
II. DISE:					
Avg. # of classrooms:					
In good conditions	4.36	2.795	0	94.5	153789
Share of schools:					
With electricity	0.688	0.334	0	1	153789
Without electricity	0.285	0.325	0	1	153789
With unreliable electricity	0.026	0.055	0	1	153789
Exiting	0.026	0.09	0	1	110099
Under construction	0.008	0.032	0	1	110099
Avg. # of kids:					
In primary school:					
C1	20.102	14.415	0	616.5	143853
C2	19.588	14.097	0	426	143853
C3	19.524	14.205	0	421	143853
C4	19.264	14.047	0	438.5	143853
C5	19.11	14.502	0	489	143853
In Middle school:					
C6	17.917	14.766	0	519.5	143853
C7	17.603	14.833	0	527.5	143853
C8	16.388	15.634	0	677.333	143853
Appearing at the exam:					
C5	8.140	13.006	0	923	126981
C7	0.976	4.839	0	280	65386
Passing the exam:					
C5	8.048	12.882	0	923	126981
C7	0.912	4.492	0	257	65386
Scoring above 60% at the exam:					
C5	5.665	9.867	0	827	126981
C7	0.473	2.714	0	205	65386

Note: Income, wages, and business profits are obtained from the Consumer Pyramid DX dataset and are expressed in lakhs of rupees in real terms using the CPI with a base year of 2010. The remaining variables in the table present summary statistics constructed from the DISE data. Averages and shares are computed at the postal code-year level. School ratios are expressed relative to the number of schools in 2010 for a given postal code-year, thus having a potential maximum value over 1. The maximum average number of kids appearing at/passing the exam at C5 (C7) may exceed the maximum average number of kids at C5 (C7) since not all schools offer exams, which means that some students may have to take their exams in other schools.

E Appendix. Figures

Figure E.1: Oldest Cohort

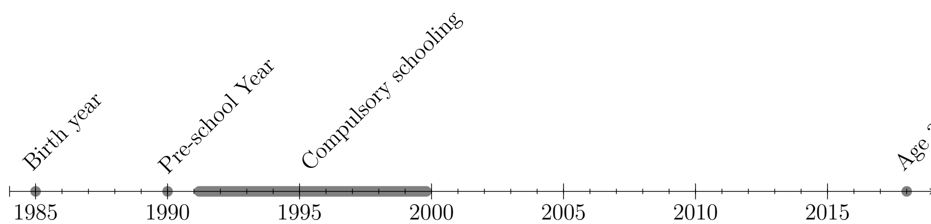
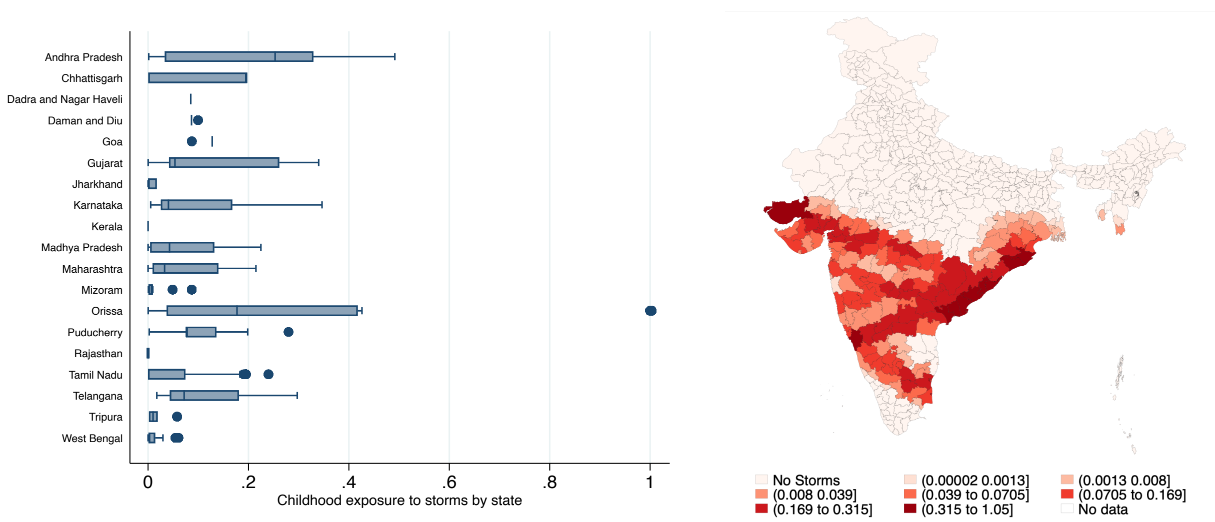
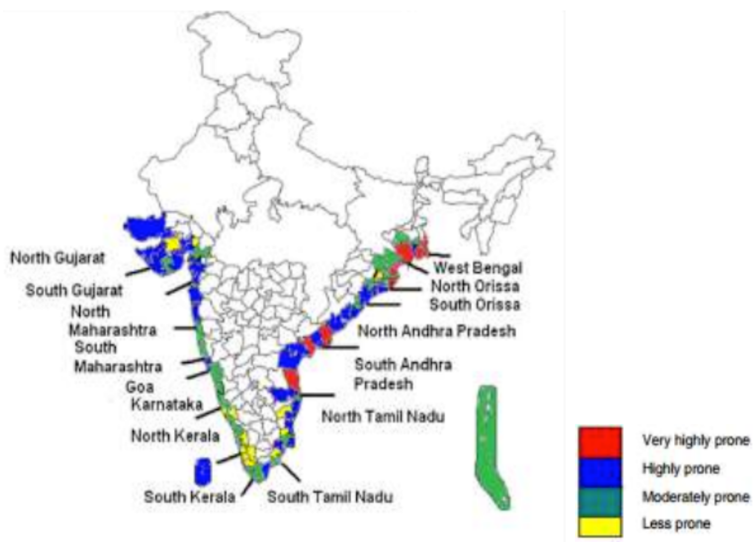


Figure E.2: School-age Exposure to Storms



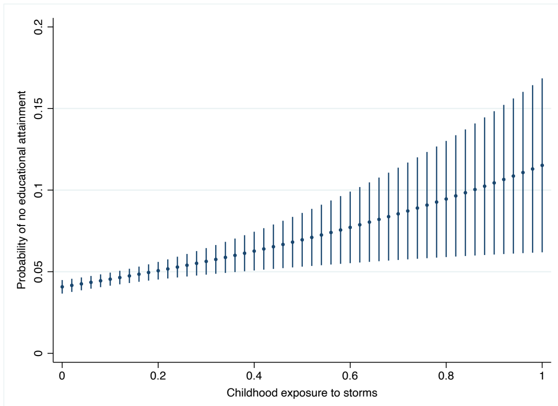
Notes: The left panel of the figure displays boxplots that describe the measure of school-age exposure to storms for individuals born between 1985 and 1995, with positive exposure ($C_{bd} > 0$), by state in alphabetical order. The figure only includes states with positive exposure. The blue line in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

Figure E.3: Cyclone Hazard-prone Districts

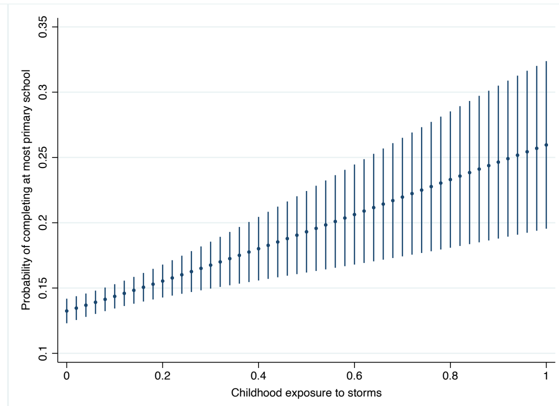


Note: Districts are classified into hazard-prone districts depending on the frequency of total cyclones and measures of the total severity of cyclones for the period 1981-2008. Source: India Meteorological Department, Ministry of Earth Sciences, Government of India.

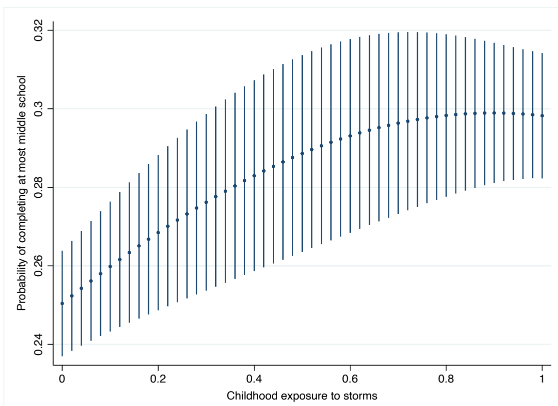
Figure E.4: Effect of School-age Exposure on the Probability of a Given Educational Attainment



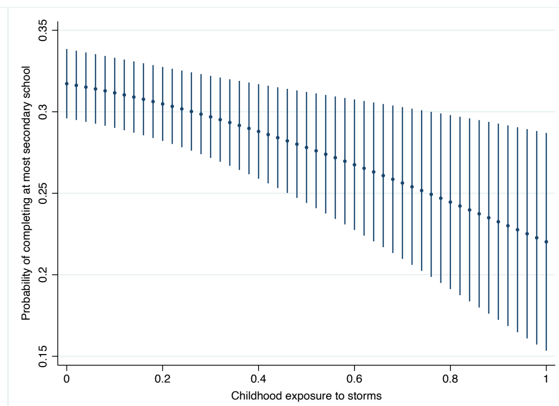
(a) Below primary



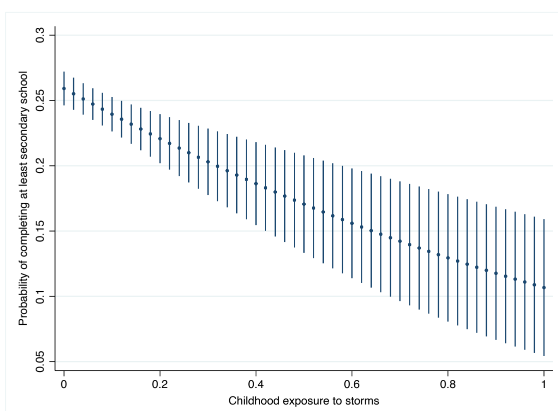
(b) At most primary school



(c) At most middle school



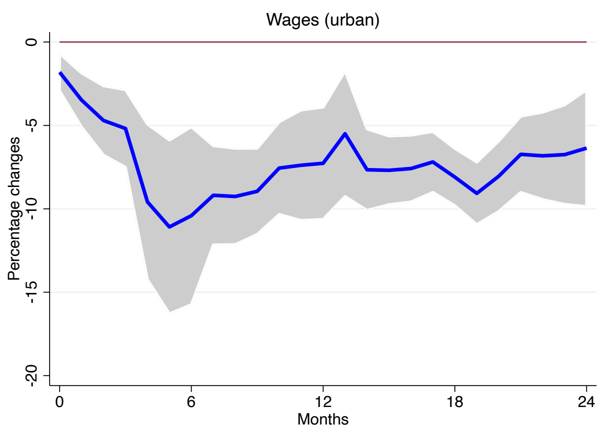
(d) At most secondary school



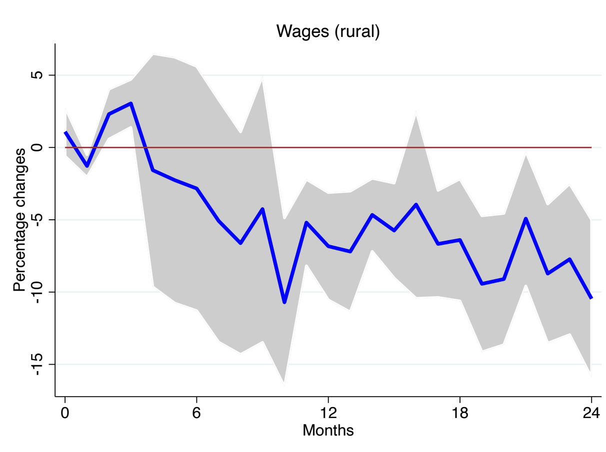
(e) At least secondary school

Note: The figure displays the predicted probabilities (and their corresponding 95% confidence intervals) of not completing primary school (Panel a), completing at most primary school (Panel b), completing at most middle school (Panel c), completing at most secondary school (Panel d), and completing at least secondary school (Panel e), across the range of storm exposures. The estimates are obtained from Panel C in Table 1, which presents an ordered logit regression of educational attainment. The range of exposures is [0,1].

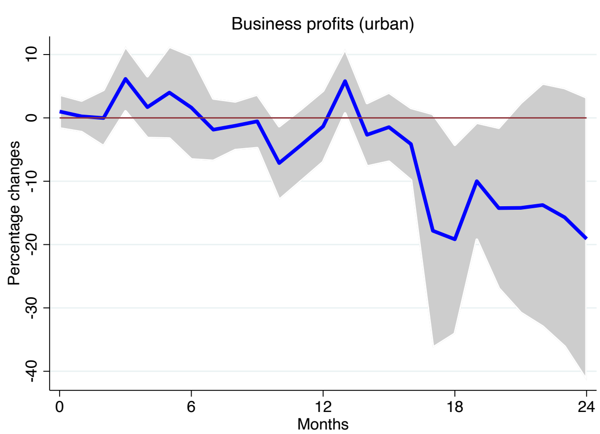
Figure E.5: Local Projections on Household Income, by Source



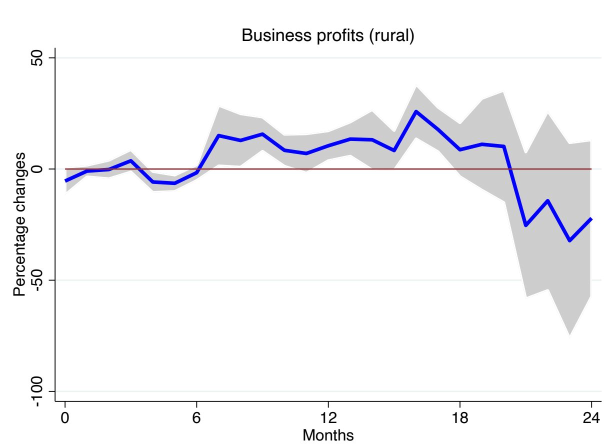
(a) Household wage, urban areas



(b) Household wage, rural areas



(c) Household business profits, urban areas



(d) Household business profits, rural areas

Note: The figure shows the results of local projections (direct effect) on household income by source (wage and business profits) and area (urban and rural), 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.

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