Investing in the Future After a Disaster: Human Capital Accumulation after the 2004 Indian Ocean Tsunami

Tereza Varejkova *

March 19, 2025

Abstract

Natural disasters have devastating immediate impacts but their long-term consequences remain underexplored. Through the disruption of schools in the affected areas they can negatively affect enrollment and school completion rates. I study the impact of the 2004 Indian Ocean tsunami on human capital accumulation among the affected children and adolescents. Comparing older and younger cohorts within provinces hit by the tsunami and in the rest of the country in a cross-cohort difference-in-differences framework, I find that the tsunami shock increased primary school completion by 1.8 percentage points, while the effect on lower secondary school completion was negative and insignificant and the effect on upper secondary school completion was positive and insignificant. The young adults affected by the tsunami during lower secondary school age or younger were also more likely to perform unpaid family work. A preliminary exploration of mechanisms suggests that the positive effects on human capital accumulation were concentrated among households who did not migrate away from the affected provinces, thus benefiting from relief aid.

Keywords: Human Capital, Natural Disaster, Labor Supply, Informal Labor
JEL codes: O15, Q54, J22, J24

1 Introduction

Natural disasters are expected to increase in frequency and intensity in the near future due to climate change. Even if global warming is limited to 1.5°C, cohorts born after 2010 are expected to see a fourfold increase in extreme events experienced during their lifetime relative to older cohorts (Thiery et al., 2021). Disasters often result in substantial damage to infrastructure and loss of life. Some disasters, such as floods or tropical cyclones, can be predicted with reasonable accuracy and their negative impacts can be mitigated by early

*PhD Candidate, University of Maryland, College Park, Department of Economics. I am extremely grateful to Sebastian Galiani, Guido Kuersteiner, and Maureen Cropper for their guidance and support. I would also like to thank Eva Dziadula and Brianna Halladay for useful comments during the Committee on the Status of the Women in the Economics Profession (CSWEP) Mentoring Workshop and to the seminar participants at the 2025 University of Maryland Graduate Research Appreciation Day (GRAD), Maryland Population Research Center (MPRC), the Population Association of America (PAA) 2025 conference, and the Southern Economic Association (SEA) 2025 95^{th} Annual Meeting. I acknowledge Statistics Indonesia (Badan Pusat Statistik, BPS) as the original source of the census data used in this study. The data were obtained through IPUMS International, Minnesota Population Center, University of Minnesota.

warning of affected populations. Other disasters, most notably earthquakes, tsunamis, or wildfires, are difficult to forecast and their negative impacts can only be mitigated by disaster relief and recovery efforts. To better target these efforts, it is therefore important to understand the nature of the negative effects of natural disasters, as well as the mechanisms through which these effects propagate.

Natural disasters not only cause severe immediate damage to economic activity and infrastructure but can also have significant long-term consequences. Children are particularly vulnerable to these lasting effects. A vast body of literature shows that adverse shocks experienced early in life can have repercussions well into adulthood. Weather shocks or conflicts can cause undernourishment and lower height-for-age (Hoddinott and Kinsey, 2001; Maccini and Yang, 2009; Bozzoli, Deaton and Quintana-Domeque, 2009; Alderman, Hoddinott and Kinsey, 2006; Akresh, Verwimp and Bundervoet, 2011; Akresh, Lucchetti and Thirumurthy, 2012; Weldeegzie, 2017), which can further lead to lower income in adulthood (Dercon and Porter, 2014). Disasters can also have long-term effects through their impact on human capital accumulation since the level of education affects future productivity and earnings (Card, 1999).

This paper investigates the long-term effects of natural disasters on human capital accumulation and the mechanisms through which these effects occur in the context of the 2004 Indian Ocean tsunami. This event was extraordinary in terms of the extent of damage caused but also in the extent of mobilized international aid which allowed for one of the largest humanitarian relief efforts in history.[1]

The impact of natural disasters on human capital is *a priori* ambiguous since disasters can have both direct and indirect effects. Direct effects are likely to have an unequivocal negative impact on children's schooling. Emotional distress and destruction of infrastructure lead to higher marginal costs of human capital production. In other words, it becomes more costly for children to stay enrolled in school in the aftermath of a natural disaster if schools are destroyed and their relatives and teachers die or suffer from injuries. Even a temporary disruption can possibly lead to a decrease in educational attainment.

Indirect effects have an ambiguous direction and operate through four channels: household income, parental time, migration, and preferences. First, changes in household income can affect investment in human capital through the household budget constraint. Higher household income is in theory associated with higher educational attainment of

[1]The 2004 Indian Ocean tsunami caused deaths of approximately 230,000 people across 15 countries with Indonesia, Sri Lanka, India, and Thailand suffering the worst damage. The total estimated damage was USD 13 billion and the global relief aid amounted to USD 13.5 billion. This is equivalent to USD 7,100 per capita if the aid were distributed equally among the 1.9 million affected (Banerjee, Bevere and Shah, 2024; Telford and Cosgrave, 2006).

children but the direction of the disaster effect on household income is ambiguous.[2] Although most empirical evidence points to a fall in household income in the aftermath of a disaster, this relationship could go the other way. For instance, reconstruction efforts might offer households employment opportunities and thus a source of income. There could also be direct cash transfers intended as a compensation for the victims.[3]

Second, parental time is a key ingredient in children's human capital accumulation and it is not clear whether its marginal cost increases or decreases after a disaster. On one hand, destruction might lead to a loss of employment and recreational activities for parents, thus making time devoted to children's human capital less costly. On the other hand, post-disaster reconstruction efforts could increase hours worked by parents, which would make them less likely to invest time in their children and help them improve their educational outcomes.

Third, migration can be a way to deal with negative shocks implied by natural disasters (Belasen and Polachek, 2013; Berlemann and Steinhardt, 2017). However, it is unclear how migration affects educational attainment. On one hand, migration away from the disaster can improve employment opportunities of parents and increase household income that can help keep children enrolled in school. On the other hand, migration can lead to a disruption of schooling for children which could have permanent effect on their human capital stocks. Furthermore, out-migrating households might miss out on cash transfers and other governmental programs aimed at supporting victims of natural disasters.

Finally, a natural disaster might lead to a change in household preferences. Affected households might find education more valuable as the destruction of physical assets makes the importance of intangible assets, such as human capital, more salient. Given all these potential mechanisms, it is unclear what direction should the effect of natural disasters on human capital accumulation take, and also, which mechanisms are actually at play.

To estimate the impact of the 2004 Indian Ocean tsunami, I exploit the plausibly exogenous variation in exposure to the resulting destruction using a cross-cohort difference-in-differences framework. I compare individuals who were of school age when the disaster struck to those who were older, across both tsunami-affected provinces (Aceh and North Sumatra) and unaffected provinces. The key identifying assumption is that in the absence of the tsunami educational attainment would have followed a parallel trajectory in affected and unaffected districts. I estimate this impact in the context of Indonesia — the country

[2] A higher household income implies that the household is more likely to afford school fees and other educational expenses and less likely to rely on child labor and, therefore, less inclined to pull children out of school.

[3] For instance, affected households received financial compensation in the amount of 43% of average annual expenditure after the 2005 Pakistan earthquake (Andrabi, Daniels and Das, 2023).

most heavily hit — and use data from the 10% sample of the 2010 Population Census of Indonesia obtained from the Integrated Public Use Microdata Series (IPUMS).

I find that individuals who were of primary school age at the time of the tsunami were actually more likely to complete primary school compared to their unaffected counterparts. This positive effect, an increase of 1.8 percentage points in primary school completion rates, suggests that the large-scale humanitarian response played a crucial role in mitigating the potential negative consequences of the disaster. For upper secondary school completion, I find a positive but statistically insignificant effect of 2.2 percentage points. In contrast, those who were in lower secondary school at the time of the tsunami were less likely to complete their education and were more likely to enter the labor force, often in informal or unpaid work.

Preliminary exploration of potential mechanisms highlights the role of post-disaster relief efforts in shaping long-term educational outcomes. In response to the tsunami, governments, NGOs, and international organizations directed significant resources toward ensuring educational continuity in affected regions. Temporary schools were established, school fees were reduced, and scholarships were widely provided to support children's education (Iwo et al., 2024). I estimate heterogeneous treatment effects by migration status. The suggest that the relief interventions were particularly effective for primary school-aged children who remained in their communities after the disaster. In contrast, children from households that migrated away from affected areas were less likely to complete primary school, indicating that displacement may have disrupted their education and reduced their access to relief-driven educational support.

This paper contributes to the literature on the impact of natural disaster on educational outcomes. The existing evidence shows that slow-onset natural disasters, such as excessive or insufficient rainfall, have immediate impacts on human capital flows. For instance, Jensen (2000) finds that school enrollment rates drop in places affected by adverse rainfall shocks in Côte d'Ivoire. Similarly, Jacoby and Skoufias (1997) find a negative link between rainfall shocks and school attendance among agrarian households in India. However, temporary schooling disruption does not necessarily imply lower overall educational attainment as children can catch up on missed school.

The long-term effects on human capital stock, as proxied by the number of years of education or school completion rates, tend to be relatively understudied (Baez, de la Fuente and Santos, 2010). Although there have been some recent advances in this literature, the evidence remains mixed. Some studies find negative long-term effects. For instance, Caruso (2017) finds that different types of natural disasters in Latin America decrease the number of years of education for cohorts who were exposed in utero or at a young age.

Andrabi, Daniels and Das (2023) also document a negative long-term impact on human capital as measured by test scores despite the affected communities having caught up in terms of assets, consumption, and infrastructure. Similarly, **?** find that droughts in Zimbabwe led to a decrease in grade completion rates. However, other studies point to null or even positive long-term effects. In the context of the 2004 Indian Ocean tsunami, Iwo et al. (2024) find that in the affected districts, individuals from communities that sustained a lot of damage were no less likely to complete high school than those from communities without damage. Some evidence even points to a positive effect of the frequency of natural disasters on human capital accumulation as returns to human capital increase relative to the returns to physical capital (Zhang and Ruan, 2020). Similarly, in the context of Hurricane Katrina, improvements in long-term outcomes were documented for income (Deryugina, Kawano and Levitt, 2018) and student test scores (Sacerdote, 2012).

This paper contributes to the literature on the impacts of natural disasters on human capital accumulation in two ways. First, my identification strategy improves on the existing work by relying on a weaker assumption. Previous studies argued that the unexpected nature of natural disasters generated a plausibly exogenous variation in affected and unaffected areas and they considered these areas comparable (Andrabi, Daniels and Das, 2023), sometimes relying on propensity score weighting to improve comparability (Deryugina, Kawano and Levitt, 2018). The underlying assumption is that in the absence of the disaster, the *levels* of the outcome would have been the same in the treated and the control areas. In contrast, the cross-cohort difference-in-differences design used in this paper relies on a weaker parallel trends assumption. In the absence of the tsunami, the outcomes in the treated and control areas would have followed the same *trend*. The treatment effect is identified by comparing school-age cohorts to older cohorts in both affected and unaffected districts. To ensure a more meaningful comparison, I restrict the age difference between cohorts so that it is not too large. In contrast, Caruso (2017) compares much younger to much older cohorts, which can be problematic, particularly in low-income countries where primary and secondary school enrollment rates increased rapidly over the last fifty years, thus making school completion rates mechanically higher among younger cohorts. Finally, this paper makes a contribution by exploring the mechanisms through which natural disasters affect educational outcomes. While Caruso (2017) leverages some cross-country comparisons and finds a non-linear trend between the level of development and the size of the impact, I focus on household-level mechanisms.

This paper also contributes to the literature on migration as a coping mechanism for negative shocks (Nakamura, Sigurdsson and Steinsson, 2021; Bryan, Chowdhury and Mobarak, 2014) and as a means of escaping poor neighborhoods (Chetty, Hendren and Katz, 2016; Chyn, 2018). Unlike previous studies, I find that migration away from the affected provinces negatively impacts children's human capital accumulation. This is most likely due to the unique context where the natural disaster was accompanied by a substantial flow of relief funds and significant governmental efforts to promote the importance of schooling.

The paper is organized as follows: Section 2 provides background information on the tsunami shock and on the educational system in Indonesia. Section 3 describes the data and Section 4 details the empirical strategy. Section 4 presents the main effects on educational attainment, Section 5 explores the possible mechanisms behind these effects and Section 6 concludes.

2 Background: The 2004 Tsunami and Education System in Indonesia

2.1 The 2004 Indian Ocean Tsunami

The 2004 Indian Ocean tsunami struck on December 26, 2004. It was triggered by a magnitude 9.1 earthquake with the epicenter near the coast of Sumatra. As the world's deadliest tsunami, it claimed approximately 230,000 lives and made close to 2 million people homeless. Material damages were the worst in Indonesia, specifically in the Aceh and North Sumatra provinces, Sri Lanka, Thailand, and India. The total losses were estimated to USD 13 billion (Banerjee, Bevere and Shah, 2024).

On the positive side, the areas affected by the tsunami experienced one of the most remarkable recoveries. In the aftermath of the disaster, a total of USD 13.5 billion in international relief aid was pledged by both governments and private sources, exceeding the estimated total losses (Telford and Cosgrave, 2006). Indonesia received the largest portion of the relief funds (USD 1 billion).

The tsunami had an immediate and devastating impact. In the most heavily damaged communities, an average of 25% of individuals perished, with mortality rates exceeding 80% in some of the worst-hit areas (Frankenberg et al., 2011). Destruction of housing led to two thirds of the population being displaced (Gray et al., 2014). In terms of mental health impacts, the rates of post-traumatic stress reactivity (PTSR) disorder increased in the affected province both in heavily damaged areas and in areas without direct damage (Frankenberg et al., 2008). However, the PTSR rates dropped sharply in the next five months (Frankenberg, Nobles and Sumantri, 2012).

There is also evidence of the tsunami's impact on fertility and children's human capital. Fertility increased in affected regions due to two main factors. First, mothers who lost a child were significantly more likely to give birth post-tsunami. Second, childless women initiated childbearing earlier in more damaged areas compared to unaffected ones. Regarding human capital, Cas et al. (2014) find that children who lost one or both parents had gender-specific outcomes: boys were more likely to leave school and enter the workforce, while girls tended to drop out due to early marriage. In contrast, the tsunami's impact on younger children's health appears limited. Those exposed in utero even exhibited higher height-for-age relative to older counterparts.

2.2 Education System in Indonesia

The education system in Indonesia consists of four levels: preschool, basic education, secondary education, and higher education. Preschool education comprises two years of schooling during ages five and six. Basic education is subdivided into primary and lower secondary school. Primary school comprises grades 1 through 6 (ages seven to twelve) and lower secondary school consists of grades 7 through 9 (ages thirteen to fifteen). While the official primary school start age is seven, many children begin one year earlier or one year later. Secondary education spans additional three years and comprises grades 10 through 12 (ages sixteen to eighteen). It offers a general or a vocational (technical) track with the majority of the students choosing the general track. Finally, enrollment in higher education starts at age nineteen and several diplomas of varying lengths of study time are offered, from one-year diplomas to doctoral degrees (Mullis et al., 2016). Education is administered in public, private, or Islamic schools. The school year operates from July through June of the following calendar year.

Basic and secondary education up to grade 12 (age eighteen) is compulsory. Primary school completion is required since the implementation of the National Compulsory Program in 1984 and lower secondary education was mandated in 1994. While the 1984 policy was deemed successful in terms of primary school enrollment and attainment, the 1994 policy failed to yield the desired effect (Lewis and Nguyen, 2020). Compulsory schooling was extended to the upper secondary school in 2015. Primary school education is universal in the sense of everybody attending primary school rather than completing it. Lower secondary education completion rates are even farther from 100% as can be seen in Table 1.

3 Data and Summary Statistics

To estimate the long-run impacts of a natural disaster on human capital accumulation, I use the ten percent sample of the 2010 Population Census of Indonesia obtained through the Integrated Public Use Microdata Series (IPUMS) International (Ruggles et al., 2025).[4] IPUMS International is a nationally representative survey and provides information on all the individuals in the households, their educational attainment, their employment status, as well as the characteristics of the household dwelling. The 2010 census surveyed 23,603,049 individuals.

The data was collected in May 2010, five and a half years after the tsunami. This allows me to estimate the long-run effects of the disaster on human capital accumulation of those individuals who were of primary or secondary school age in December 2004 and who should have completed their respective level of education by 2010. Additionally, the census data is well-suited for examining the plausibility of the identifying assumption—– that in the absence of the tsunami, educational attainment in affected and unaffected areas would have followed similar trends. By analyzing the educational attainment of individuals who were beyond school age before the disaster, I can assess whether there were any pre-existing trends in education.

Summary statistics for individuals aged 6–30 are reported in Table 2. Column 2 shows the raw means for given outcomes in Aceh and North Sumatra — the two provinces heavily damaged by the tsunami — while column 1 reports the raw means for all the other provinces in Indonesia. Column 3 shows the results of a simple t-test of the equality of the means. The affected and unaffected provinces differ significantly in almost all of the variables.

The second panel of Table 2 reports means for migration status and parental characteristics, which are variables relevant to the preliminary exploration of mechanisms behind the main results. The young individuals in the tsunami provinces are more likely to have completed primary school, lower secondary, and upper secondary school. They are less likely to be employed, and if employed, they are more likely to perform unpaid work rather than receive monetary compensation for their work. The young individuals in the affected provinces were also more likely to migrate in the past five years, both outside and within their province. Their parents are less likely to be employed and they are more likely to live in a single-parent household.

4 Empirical Strategy

I exploit the variation in exposure to the destruction caused by the 2004 Indian Ocean tsunami in Indonesia in a difference-in-differences framework. I compare individuals who

[4]Unfortunately, the 2010 Population Census is the latest census currently made available by IPUMS International. The most recently collected 2020 census is not yet publicly available.

were of school age when the disaster struck to individuals who were older, in provinces struck by the tsunami (Aceh and North Sumatra) and in the unaffected ones. This approach is similar to other cohort studies. For instance, the difference-in-differences framework with year of birth as the time variable is employed by Duflo (2001) who explores the long-term effects of childhood exposure to newly built schools in Indonesia; by Bailey, Sun and Timpe (2021) who study the impacts of exposure during childhood to the Head Start program in the United States; or by Carrillo, Charris and Iglesias (2023) who estimate the long-run impact on educational attainment and employment outcomes of individuals affected by forced removals of black population to segregated areas during apartheid.

To estimate the following cross-cohort difference-in-differences specification:

$$s_{ipc} = \beta\, tsunami_p \times age_c + \mu_p + \delta_c + \mathbf{X}'_{ipc}\Gamma + \varepsilon_{ipc} \tag{1}$$

where s_{ipc} is an indicator variable equal to one if individual i born in province p and belonging to cohort c completed a given educational level. In this paper, I examine primary, lower secondary, and upper secondary school completion as the three main outcome variables. I also examine the probability of being employed and the probability of performing unpaid family work as opposed to paid employment. Note that the information on the exact number of years of schooling is not available in the 2010 Indonesia Census.

$tsunami_p \times school\_age_c$ is an interaction term between a province-level indicator for the two provinces struck by the disaster (Aceh and North Sumatra) and a cohort-level indicator for being of relevant age at the time of the disaster. For the primary school completion, I consider the relevant age to be 11 years or younger since 12 is the age by which most Indonesian children complete primary school. For the secondary school completion, I consider the relevant age to be 18 or younger since 19 is the age by which most Indonesian adolescents complete secondary school.

μ_p and δ_c are province-of-birth and cohort fixed effects. $\mathbf{X}_{ipc}$ denotes individual-level characteristics. These include sex and dummies for parental education.

Finally, ε_{ipc} is an idiosyncratic error term. The sample is restricted to individuals aged 12–30 at the time of the census (6–24 at the time of the tsunami) for the primary school completion outcome, to individuals aged 15–33 at the time of the census (9–27 at the time of the tsunami) for lower secondary school completion, and to individuals aged 18–36 at the time of the census (12–30 at the time of the tsunami) for the upper secondary school completion. Standard errors are clustered at the province-of-birth level.

5 Main Results

5.1 Potential Threats to Internal Validity

The validity of cross-cohort difference-in-differences estimates relies on parallel trends assumption. This requires that in the absence of the tsunami the change in educational attainment of the school-age and the beyond-school-age cohorts would have followed the same trend. While this assumption cannot be tested, we can still judge its plausibility by considering the evolution of the primary and secondary school completion between the affected and unaffected provinces among the beyond-school-age cohorts. To do that, I estimate an event-study version of Equation 1 where I estimate differences between the tsunami and non-tsunami provinces separately for each cohort relative to the last cohort that is not affected by the disaster:

$$s_{ipc} = \sum_{c \neq c'} \beta^c \, \mathbb{1}[age = c] \times tsunami_p + \mu_p + \delta_c + \mathbf{X}'_{ipc}\Gamma + \eta_{ipc} \qquad (2)$$

where $\mathbb{1}[\cdot]$ is an indicator function which is equal to one if the individual belongs to cohort c and zero otherwise. The excluded cohort c' refers to individuals aged 12 at the time of the tsunami for primary school completion, to individuals aged 15 at the time of the tsunami for lower secondary school completion, and to individuals aged 18 at the time of the tsunami for upper secondary school completion.

The event-study plots for educational outcomes are shown in Figure 1. Cohort-specific treatment effects and the associated 95% confidence intervals are displayed. Treated cohorts are those who were younger than a certain cutoff age, and thus the estimated coefficients for the treated cohorts appear on the left-hand side of the plots. Control cohorts are older and appear on the right-hand side. All three graphs point to an absence of significant pre-trends. For primary school completion, the coefficients for the control cohorts are close to zero. For lower secondary completion, there seems to be a positive pre-trend, however, the confidence intervals overlap with zero for all control cohorts. For upper secondary school completion, there is a negative pre-trend, however, the coefficients are again statistically insignificant.

The event study plots for the probability of being employed are shown in Figure 2. Each plot shows the cohort-specific treatment effects for different definitions of treated cohorts. First, individuals of primary school age at the time of the tsunami are considered as treated and the event study plot shows an increase in the probability of being employed relative to the cohorts who were thirteen years or older in 2004. There is a slight negative pre-trend that is statistically significant for the control cohorts aged thirteen to sixteen. This suggests that being born in provinces affected by the tsunami has an effect on older children and that the relevant cutoff age should be higher.

The second plot in Figure 2 shows the effect on the probability of being employed fifteen-year-olds are the excluded category. The treated cohorts are the individuals of lower secondary school age or younger at the time of the tsunami. The treatment effects for the treated cohorts are larger in magnitude relative to the first plot and the pre-trend coefficients are statistically insignificant, except for cohort sixteen.

Finally, the bottom plot in Figure 2 considers the individuals of upper secondary school age or younger as treated with eighteen-year-olds as the reference category. The effect on the likelihood of employment is negative for most treated cohorts but none of the coefficients are statistically significant. There are also no significant pre-trends.

Being fourteen years old or younger at the time of the tsunami is the most relevant age cutoff, and therefore, I will focus on this treatment definition when I report the effects on employment in the next section. Figure 3 further confirms that lower secondary school age is the relevant treatment group. The three plots show cohort-specific treatment effects on the probability of performing unpaid family work with different definition of treatment and different omitted age categories. In the census, being employed amounts to both paid employment and unpaid family work. The first plot considers the primary school aged cohort as treated. The probability of unpaid work increases for all treated cohorts relative to the reference category. However, there is a significant and large negative pre-trend, suggesting that the treated and control cohorts did not follow the same trend in the incidence of unpaid work in tsunami relative to non-tsunami provinces.

The second plot displays the cohort-specific treatment effects using individuals of lower secondary school age or younger as the treatment group. The probability of unpaid family work increases for cohorts who were in lower secondary school or younger during the tsunami. There is still evidence of a negative pre-trend, however, the coefficients are much smaller in magnitude than for the primary school age treatment and are not statistically significant at the 1% level.

Finally, the third plot considers the individuals of upper secondary school age or younger as treated. Similarly to the previous case, there is a significant increase in the probability of unpaid family work for younger cohorts relative to the omitted category of eighteen-year-olds. However, the coefficients are smaller in magnitude than the treatment effects defined using the lower secondary age cutoff. The plot also displays a marginally significant and small pre-trend.

5.2 The Tsunami Effects on Educational Attainment and Employment Outcomes

Table 3 reports the estimated effects of the 2004 Indian Ocean tsunami on three main outcomes related to school completion and three labor market outcomes. In column 1, I report the effect of being exposed to the tsunami shock while being of primary school age. Those who were twelve or older at the time of the tsunami serve as a control group since their ability to complete primary school is less likely to be affected as they should have already graduated from primary school. The coefficient of interest is 0.018 and is statistically significant at the 5% level. This implies that children living in provinces affected by the tsunami and belonging to primary school age cohorts were 1.8 percentage points (pp) more likely to complete primary school. This represents a moderate increase of 2.5% relative to the mean primary school completion rate among the 18–30-year-old adults in the sample (72%).

Column 2 shows the analogous effect on lower secondary school completion, that is, completion of grade 9. The main difference with the previous analysis is the definition of the treated cohort. Those who were fourteen or younger at the time of the tsunami are considered to be the most likely to be impacted in terms of their ability to complete lower secondary school since they were of lower secondary school age at the time of the disaster. The coefficient of interest is -0.027 (standard error = 0.017) and statistically indistinguishable from zero. Lower secondary school completion rate is smaller than primary school completion rate among individuals aged 18–30 in the sample (40.4%) and so the 2.7 pp decrease in the probability of completion of grade 9 represents a 6.7% decrease relative to the mean of the control group.

In Column 3, the effect on completion of upper secondary school (grade 12) is reported. The estimated coefficient is positive but not statistically significant (0.022; standard error = 0.018).

In Columns 4–6, results for the employment outcomes are shown. It is important to examine these effects since the decision to invest in children's human capital could be affected by the opportunity cost of their time. If wages increase, for instance, as a result of higher demand for labor due to post-disaster reconstruction efforts, the opportunity cost of going to school becomes higher, which decreases the incentive to enroll children in school. Column 4 reports the effect of being exposed to the tsunami during the lower secondary school age (fourteen or younger) on the probability of being employed relative to being either unemployed or not participating in the labor market at all. As discussed in the previous section, I chose the lower secondary school age as the relevant cutoff for defining the treatment group because data shows that the parallel trend assumption is more likely to hold in this case. Also, lower secondary school completion rate seems to be the only educational outcome negatively impacted by the tsunami. The estimated coefficient is 0.038 (standard error = 0.024). It is not statistically significant but it is large in magnitude as it represents a 36% increase relative to the mean in the control group (10.6% of the 9-to-27-year-olds are employed).

The results in Columns 2 and 4 suggest that there is some substitution between investment in human capital and participating in the labor market for cohorts of lower secondary school age or younger during the tsunami. However, it is still not clear whether the lower educational attainment in the tsunami-affected provinces could be explained by the children's higher opportunity cost of time, or whether the children work because their families cannot afford to send them to school. In Columns 5 and 6, I the effects on the probability of paid employment as opposed to performing unpaid family work. The treated cohorts are 24.1 pp less likely to be paid for their work, which represents a 27% drop relative to the mean of the control group (89.2% among the 9-to-27-year-olds who are employed work for pay).

In contrast, the treated cohorts are more likely to perform unpaid family work. The effect reported in column 6 mirrors exactly the effect on the probability of paid employment in column 5 because by construction, an employed individual must perform either paid or unpaid work. These results suggest that the decision to withdraw children from compulsory schooling is not necessarily be related to the children's opportunity cost of time. However, this could still be related to the parent's opportunity cost of time as they could be choosing to work longer hours, and therefore might need more help with chores and other activities around the house.

6 Preliminary exploration of mechanisms

In this section, I explore some possible mechanisms behind the tsunami effects on educational attainment and employment outcomes. In particular, I am interested in three potential mechanisms: whether the household migrated after the disaster, whether parents are employed, and whether both parents are present in the household.[5] To examine these mechanisms, I estimate heterogeneous treatment effects by interacting the treatment variable (the interaction between being born in an affected province and belonging to an affected cohort) with a dummy variable describing a given mechanism. Note that these mechanisms are defined after the shock happened, which means that they are potentially

[5]Parents are defined as the head of the household and spouse. In some instances, there are multiple household heads and/or multiple spouses per household. In this case, I define a father (mother) as working if at least one of the male (female) household heads or spouses is reported as working.

endogenous, and therefore, I consider this analysis as only suggestive evidence, rather than causal evidence.[6]

6.1 Mechanisms Behind the Increase in Educational Attainment

The exploration of potential mechanisms behind the increase in primary school completion is summarized in Table 4. First, households who migrated after the tsunami have lower treatment effects than households who stayed in place (column 1). This effect is particularly driven by the households who migrated away from the affected provinces (column 2) rather than by households who migrated within these provinces (column 3).

Treatment effects are slightly smaller for those individuals whose parents are employed (columns 4 and 5) but noticeably larger for households where both parents are present, which is a proxy for not losing a parent in the disaster.

6.2 Mechanisms Behind Higher Incidence of Unpaid Family Work

Table 5 reports on the analysis of potential mechanisms behind the higher probability of unpaid family work for cohorts who were exposed at the lower secondary school age. The positive treatment effect is almost mitigated for the households who migrated away in the aftermath of the tsunami (column 1), and more so for those households who migrated away from the affected provinces (column 2) than for households who changed their residence within the same province (column 3). This suggests that the increase in unpaid family work could be driven by reconstruction efforts where the households most affected in terms of housing destruction could be relying on their children to help them rebuild.

For the households where either both parents have a job (column 4) or where specifically the mother is employed (column 5), the differential treatment effect (the coefficient on the interaction term) cancels the main negative effect. This implies that children in the households were both parents do not work are in fact less likely to perform unpaid work, and instead take on paid employment opportunities to help contribute to the family budget. In contrast, children in households where both parents (or specifically mother) work are more likely to perform unpaid family work. This suggests that the main tsunami effect on the lower secondary school cohorts (a decrease in lower secondary school completion and an increase in the incidence of unpaid work) could be driven by some parents losing jobs in the aftermath of the disaster, and thus not being able to send their children to school anymore.

[6] Causal mediation analysis could be an appropriate method to shed light on mechanisms (Hicks and Tingley, 2011). However, implementing this type of analysis in a difference-in-differences framework is not straightforward.

Finally, column 6 shows that children in households where both parents are present are slightly less likely to perform unpaid work.

7 Conclusion

Natural disasters can have long-lasting consequences on the affected populations. Beyond the immediate impacts in terms of material damage and loss of life, there can also be persistent impacts on human capital accumulation and later labor market outcomes of individuals exposed during childhood. This paper documents the effects, six years after the natural disaster struck, on educational attainment and labor market outcomes of cohorts affected during the primary school, lower secondary school, and upper secondary school age.

First, primary school completion rates actually increased among children affected by the tsunami by 1.8 pp. This could be explained by the extraordinary humanitarian response following the 2004 Indian Ocean tsunami where extremely large amounts of relief funds were raised and sent to the affected regions. There was a special emphasis on education. To help prevent schooling disruptions, temporary schools were set up, fees were reduced, and scholarships were provided (Iwo et al., 2024). A positive, although insignificant effect of 2.2 pp is also observed for upper secondary school completion rates. These results suggest that even one of the largest natural disasters ever recorded does not necessarily lead to human capital losses if the relief efforts are well targeted.

Some preliminary explorations of mechanisms provides evidence that the relief efforts were key in achieving the positive impact on human capital accumulation. In fact, the effects on primary school completion were the largest among those households who did not migrate in the aftermath of the disaster. Furthermore, children in households that migrated away from the affected provinces were less likely to complete primary school relative to the control cohorts.

While primary school aged individuals benefited, the slightly older individuals who were of lower secondary school age during the tsunami seem to have been negatively impacted. They were less likely to complete lower secondary school by 2.7 pp, although the estimate is not statistically significant. Additionally, they were more likely to enter the workforce and be employed, but less likely to receive monetary compensation for their work.

The preliminary exploration of mechanisms suggests that these effects could be driven by the demand for assistance with chores and other household needs, which could be higher due to the post-disaster reconstruction efforts. In fact, children in the households who migrated away from the affected provinces, that is, to the places where there would not be any ongoing reconstruction efforts, were less likely to perform unpaid family work, while the individuals whose parents were employed were more likely to work without pay.

These findings highlight the heterogeneous effects of natural disasters on human capital accumulation. While younger cohorts benefited from well-targeted relief efforts that mitigated potential educational losses, older cohorts faced greater challenges in continuing their education, potentially due to increased household responsibilities. These results underscore the importance of timely policy interventions that ensure access to education for all affected individuals. Future research could further explore the long-term labor market trajectories of these cohorts and assess whether the initial human capital accumulation effects translate into persistent economic outcomes.

Tables

Table 1: School completion rates by cohort

Age	% complete primary school	% complete lower sec. school	% complete upper sec. school
10	0.%	0.%	0.%
11	13.22%	0.%	0.%
12	42.76%	0.%	0.%
13	78.22%	4.72%	0.%
14	90.68%	19.03%	0.%
15	93.95%	46.79%	0.%
16	95.25%	70.2%	3.41%
17	95.38%	75.79%	17.04%
18	95.28%	76.4%	37.52%
19	95.04%	75.3%	47.45%
20	94.64%	73.22%	48.45%
21	95.38%	74.09%	49.91%
22	95.41%	73.12%	48.79%

Note: Author's own calculations based on the 2010 Indonesia Census.

Table 2: Summary statistics, individuals aged 6–30 at tsunami

	All other provinces (1)	Aceh & North Sumatra (2)	Difference (2) - (1) (3)
Educational attainment & employment status			
Completed primary school	0.892	0.912	0.020***
	(0.000)	(0.000)	(0.000)
Completed lower secondary school	0.564	0.664	0.100***
	(0.000)	(0.001)	(0.001)
Completed upper secondary school	0.309	0.400	0.092***
	(0.000)	(0.001)	(0.001)
Male	0.503	0.502	-0.001
	(0.000)	(0.001)	(0.001)
Employed	0.826	0.817	-0.008***
	(0.000)	(0.001)	(0.001)
If employed, paid work	0.892	0.793	-0.098***
	(0.000)	(0.001)	(0.001)
If employed, unpaid work	0.108	0.207	0.098***
	(0.000)	(0.001)	(0.001)
Migration status & parental characteristics			
Migrated in the past 5 years	0.064	0.083	0.018***
	(0.000)	(0.000)	(0.000)
Migrated outside of province in the past 5 years	0.036	0.041	0.005***
	(0.000)	(0.000)	(0.000)
Migrated within province in the past 5 years	0.028	0.042	0.014***
	(0.000)	(0.000)	(0.000)
Both mother and father are employed	0.455	0.443	-0.012***
	(0.000)	(0.001)	(0.001)
Mother is employed	0.487	0.487	-0.000
	(0.000)	(0.001)	(0.001)
Both mother and father present in the household	0.839	0.823	-0.016***
	(0.000)	(0.000)	(0.000)
N	9,348,337	875,069	10,223,406

Note: Summary statistics are calculated for individuals aged 6–30 at the time of the tsunami using the 2010 Population Census of Indonesia. Column (1) reports the means for provinces not affected by the tsunami and Column(2) reports the means for provinces affected by the tsunami. Column (3) shows the results of a t-test of equality between the two means. Standard errors are in parentheses. *** significant at 1% level. ** significant at 5% level. * significant at 1% level.

Table 3: Tsunami impacts on educational and employment outcomes

	Completed:				If employed:	
	primary school (1)	lower second. school (2)	upper second. school (3)	Prob. of being employed (4)	Paid work (5)	Unpaid work (6)
Ts. × primary school age (6-11)	0.018**					
	(0.008)					
Ts. × lower secondary school age or younger (9-14)		-0.027		0.038	-0.241***	0.241***
		(0.017)		(0.024)	(0.022)	(0.022)
Ts. × upper secondary school age or younger (12-17)			0.022			
			(0.018)			
Observations	7,927,752	7,758,543	7,622,730	4,814,555	5,579,152	5,579,152
Ages included (at tsunami)	6–24	9–27	12–30	9–27	9–27	9–27
R-squared	0.37	0.223	0.399	0.106	0.157	0.157
Control mean	0.721	0.404	0.236	0.89	0.892	0.108

Note: This table reports results from the cross-cohort difference-in-differences regression specification formulated in 1. The reported coefficient is the coefficient on the dependent variable capturing the difference-in-differences treatment effect: the interaction term between a dummy representing the tsunami-affected provinces and a dummy representing the affected cohorts. The affected cohorts vary based on the main outcome variable and are described by their age at the time of the tsunami. Standard errors clustered at the birth-province level are in parentheses. *** significant at 1% level. ** significant at 5% level. * significant at 1% level.

Table 4: Potential mechanisms behind higher primary school completion rates

	Heterogeneous treatment effects by:					
	Migrant 1	Migrant 2	Migrant 3	Both parents employed	Mother employed	Both parents in the HH
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.022***	0.022***	0.022***	0.027***	0.026***	0.022**
	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.009)
Migrant 1	0.001					
	(0.002)					
Treatment × Migrant 1	-0.014***					
	(0.002)					
Migrant 2		0.002				
		(0.003)				
Treatment × Migrant 2		-0.027***				
		(0.003)				
Migrant 3			-0.001			
			(0.002)			
Treatment × Migrant 3			-0.004*			
			(0.002)			
Both parents employed				-0.007**		
				(0.003)		
Treatment × Both parents employed				-0.007		
				(0.007)		
Mother employed					-0.008***	
					(0.003)	
Treatment × Mother employed					-0.006	
					(0.007)	
Both parents in HH						0.024***
						(0.008)
Treatment × Both parents in HH						-0.000
						(0.005)
Observations	4,726,228	4,726,228	4,726,228	4,038,454	4,568,942	4,726,813
R-squared	0.392	0.392	0.392	0.403	0.394	0.392

Note: This table reports results from the cross-cohort difference-in-differences regression specification where the treatment dummy (interaction between tsunami provinces and affected cohort dummies) is interacted with dummies for potential mechanisms. The affected cohort are individuals of primary school age at the time of the tsunami (6–11). "Migrant 1" equals one for those who migrated in the past 5 years (between 2005 and 2010). "Migrant 2" equals one for those who migrated to a different province in the past 5 years. "Migrant 3" equals one for those who migrated to a different district or village within the same province in the past 5 years. Standard errors clustered at the birth-province level are in parentheses. *** significant at 1% level. ** significant at 5% level. * significant at 1% level.

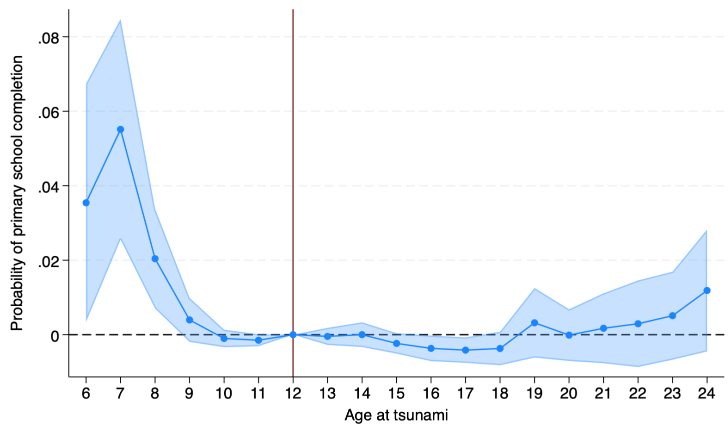
Table 5: Mechanisms behind higher probability of unpaid family work

	Heterogeneous treatment effects by:					
	Migrant 1	Migrant 2	Migrant 3	Both parents employed	Mother employed	Both parents in the HH
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.277***	0.274***	0.272***	-0.033**	-0.058***	0.268***
	(0.022)	(0.022)	(0.022)	(0.013)	(0.013)	(0.020)
Migrant 1	-0.016					
	(0.011)					
Treatment × Migrant 1	-0.245***					
	(0.015)					
Migrant 2		-0.006				
		(0.013)				
Treatment × Migrant 2		-0.281***				
		(0.019)				
Migrant 3			-0.029**			
			(0.011)			
Treatment × Migrant 3			-0.197***			
			(0.021)			
Both parents employed				0.129***		
				(0.024)		
Treatment × Both parents employed				0.423***		
				(0.031)		
Mother employed					0.128***	
					(0.025)	
Treatment × Mother employed					0.421***	
					(0.031)	
Both parents in HH						-0.031*
						(0.016)
Treatment × Both parents in HH						0.002
						(0.004)
Observations	2,473,823	2,473,823	2,473,823	2,020,737	2,374,191	2,473,899
R-squared	0.192	0.192	0.192	0.241	0.230	0.192

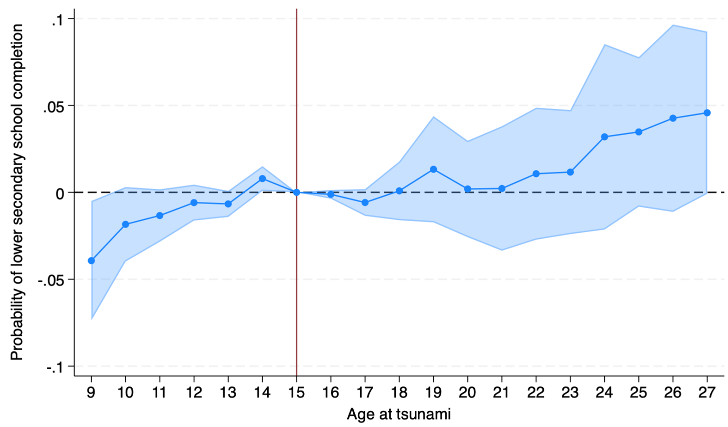
Note: This table reports results from the cross-cohort difference-in-differences regression specification where the treatment dummy (interaction between tsunami provinces and affected cohort dummies) is interacted with dummies for potential mechanisms. The affected cohort are individuals of lower secondary school age or younger at the time of the tsunami (9–14). "Migrant 1" equals one for those who migrated in the past 5 years (between 2005 and 2010). "Migrant 2" equals one for those who migrated to a different province in the past 5 years. "Migrant 3" equals one for those who migrated to a different district or village within the same province in the past 5 years. Standard errors clustered at the birth-province level are in parentheses. *** significant at 1% level. ** significant at 5% level. * significant at 1% level.

Figures

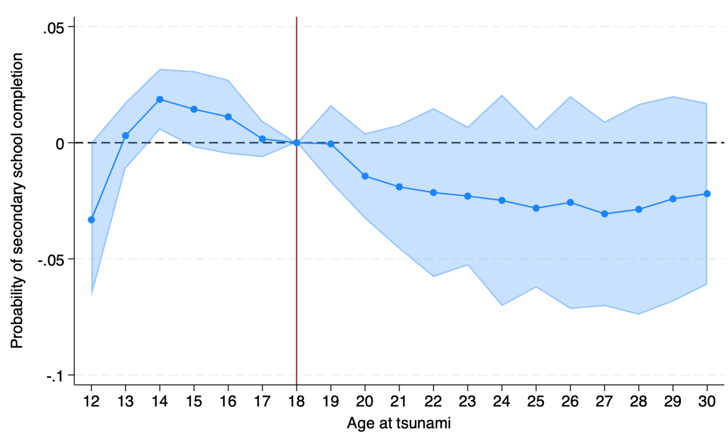
Figure 1: Estimated effects on schooling outcomes: event study



(a) Primary school completion



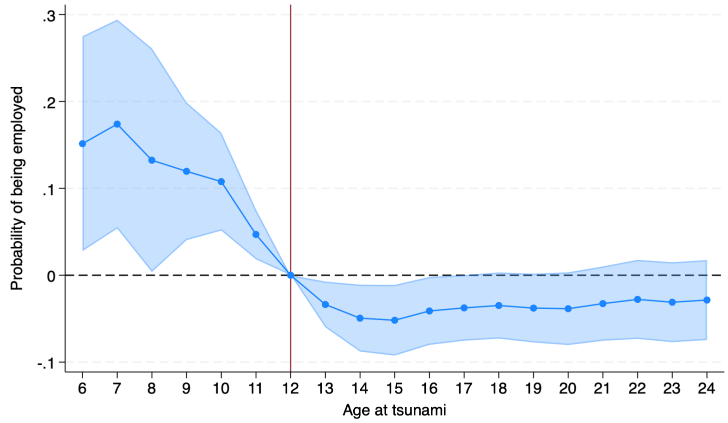
(b) Lower secondary school completion



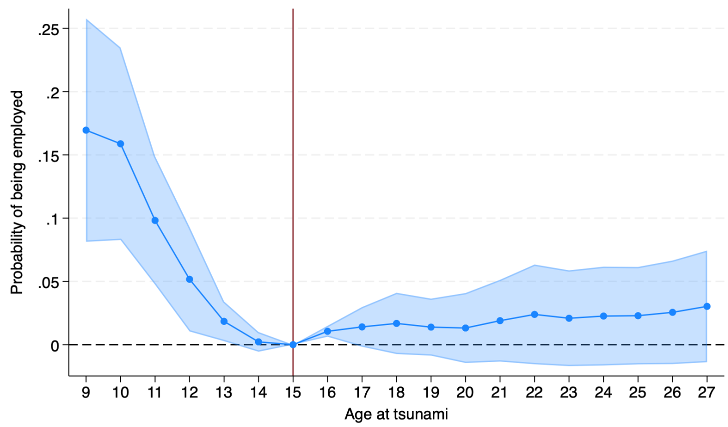
(c) Upper secondary school completion

Note: The graphs plot the β^c coefficients and their respective 95% confidence intervals obtained by estimating Equation 2 with completion of different levels of schooling as the main outcome variables. The excluded reference cohorts are marked by the red line. The excluded cohort is 12 for primary school completion, 15 for lower secondary school completion, and 18 for upper secondary school completion. Treated cohorts are on the left-hand side of the cutoff whereas the control cohorts are on the right-hand side.

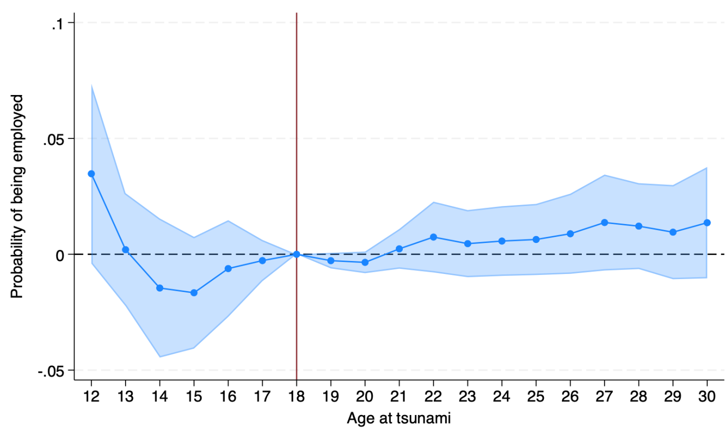
Figure 2: Estimated effects on employment probability by treated cohorts



(a) Probability of being employed, primary school age treatment



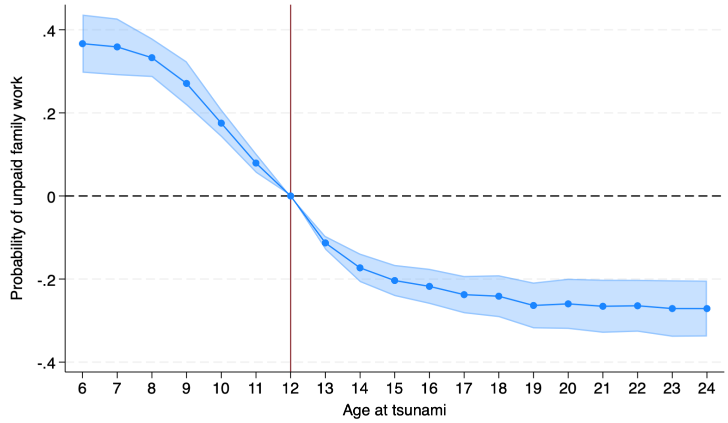
(b) Probability of being employed, lower secondary school age treatment



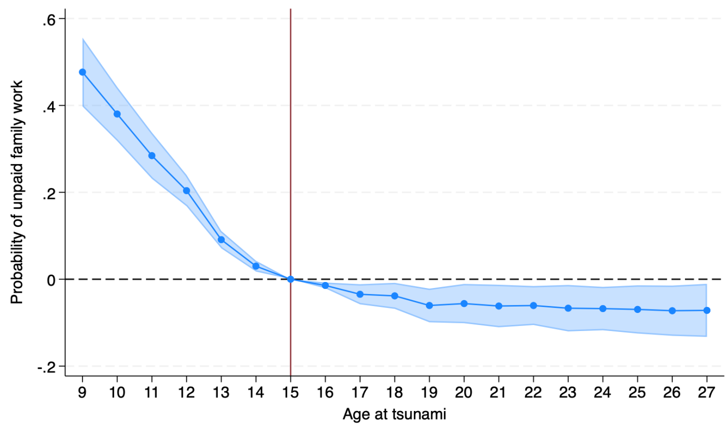
(c) Probability of being employed, upper secondary school age treatment

Note: The graphs plot the β^c coefficients and their respective 95% confidence intervals obtained by estimating Equation 2 with probability of being employed (relative to unemployed or inactive) as the main outcome variable. The excluded reference cohorts are marked by the red line. The excluded cohort is 12 for primary school age treatment, 15 for lower secondary school age treatment, and 18 for upper secondary school age treatment. Treated cohorts are on the left-hand side of the cutoff whereas the control cohorts are on the right-hand side.

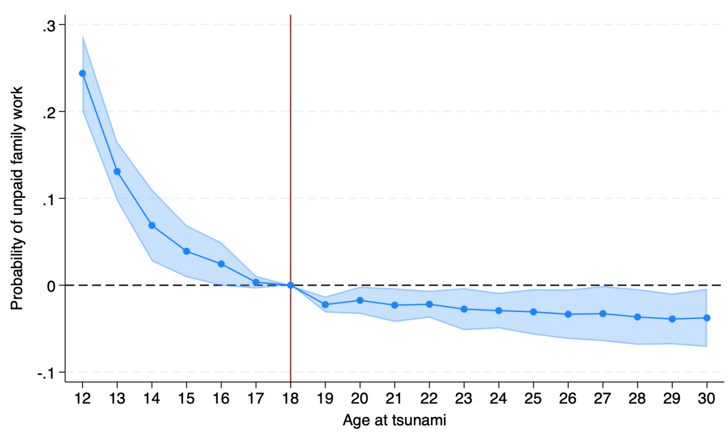
Figure 3: Estimated effects on unpaid family work probability by treated cohorts



(a) Probability of unpaid family work, primary school



(b) Probability of unpaid family work, lower secondary school age treatment



(c) Probability of unpaid family work, upper secondary school age treatment

Note: The graphs plot the β^c coefficients and their respective 95% confidence intervals obtained by estimating Equation 2 with probability of unpaid family work (relative to paid employment) as the main outcome variable. The excluded reference cohorts are marked by the red line. The excluded cohort is 12 for primary school age treatment, 15 for lower secondary school age treatment, and 18 for upper secondary school age treatment. Treated cohorts are on the left-hand side of the cutoff whereas the control cohorts are on the right-hand side.

References

Akresh, Richard, Leonardo Lucchetti, and Harsha Thirumurthy. 2012. “Wars and child health: Evidence from the Eritrean–Ethiopian conflict.” *Journal of Development Economics*, 99(2): 330–340.

Akresh, Richard, Philip Verwimp, and Tom Bundervoet. 2011. “Civil War, Crop Failure, and Child Stunting in Rwanda.” *Economic Development and Cultural Change*, 59(4): 777–810.

Alderman, Harold, John Hoddinott, and Bill Kinsey. 2006. “Long term consequences of early childhood malnutrition.” *Oxford Economic Papers*, 58(3): 450–474.

Andrabi, Tahir, Benjamin Daniels, and Jishnu Das. 2023. “Human Capital Accumulation and Disasters.” *Journal of Human Resources*, 58(4): 1057–1096.

Baez, Javier, Alejandro de la Fuente, and Indhira Santos. 2010. “Do Natural Disasters Affect Human Capital? An Assessment Based on Existing Empirical Evidence.” *IZA Discussion Paper No. 5164*.

Bailey, Martha J., Shuqiao Sun, and Brenden Timpe. 2021. “Prep School for Poor Kids: The Long-Run Impacts of Head Start on Human Capital and Economic Self-Sufficiency.” *American Economic Review*, 111(12): 3963–4001.

Banerjee, Chandan, Lucia Bevere, and Drashti Dilip Shah. 2024. “The Indian Ocean tsunami: 20 years on.” Swiss Re Institute Economic insights 20/2024. www.swissre.com/institute/.

Belasen, A. R., and S. W. Polachek. 2013. “Natural disasters and migration.” In *International Handbook on the Economics of Migration*. , ed. A. F. Constant and K. F. Zimmermann, Chapter 17, 309–330. Cheltenham, UK; Northampton, MA, USA:Edward Elgar.

Berlemann, Michael, and Max Friedrich Steinhardt. 2017. “Climate Change, Natural Disasters, and Migration—a Survey of the Empirical Evidence.” *CESifo Economic Studies*, 63(4): 353–385.

Bozzoli, Carlos, Angus Deaton, and Climent Quintana-Domeque. 2009. “Adult height and childhood disease.” *Demography*, 46(4): 647–669.

Bryan, Gharad, Shyamal Chowdhury, and Ahmed Mushfiq Mobarak. 2014. “Underinvestment in a Profitable Technology: The Case of Seasonal Migration in Bangladesh.” *Econometrica*, 82(5): 1671–1748.

Card, David. 1999. “Chapter 30 - The Causal Effect of Education on Earnings.” In . Vol. 3 of *Handbook of Labor Economics*, , ed. Orley C. Ashenfelter and David Card, 1801–1863. Elsevier.

Carrillo, Bladimir, Carlos Charris, and Wilman Iglesias. 2023. "Moved to Poverty? A Legacy of the Apartheid Experiment in South Africa." *American Economic Journal: Economic Policy*, 15(4): 183–221.

Caruso, Germán Daniel. 2017. "The legacy of natural disasters: The intergenerational impact of 100 years of disasters in Latin America." *Journal of Development Economics*, 127: 209–233.

Cas, Ava Gail, Elizabeth Frankenberg, Wayan Suriastini, and Duncan Thomas. 2014. "The Impact of Parental Death on Child Well-being: Evidence From the Indian Ocean Tsunami." *Demography*, 51: 437–457.

Chetty, Raj, Nathaniel Hendren, and Lawrence F. Katz. 2016. "The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment." *American Economic Review*, 106(4): 855–902.

Chyn, Eric. 2018. "Moved to Opportunity: The Long-Run Effects of Public Housing Demolition on Children." *American Economic Review*, 108(10): 3028–56.

Dercon, Stefan, and Catherine Porter. 2014. "LIVE AID REVISITED: LONG-TERM IMPACTS OF THE 1984 ETHIOPIAN FAMINE ON CHILDREN." *Journal of the European Economic Association*, 12(4): 927–948.

Deryugina, Tatyana, Laura Kawano, and Steven Levitt. 2018. "The Economic Impact of Hurricane Katrina on Its Victims: Evidence from Individual Tax Returns." *American Economic Journal: Applied Economics*, 10(2): 202–33.

Duflo, Esther. 2001. "Schooling and Labor Market Consequences of School Construction in Indonesia: Evidence from an Unusual Policy Experiment." *American Economic Review*, 91(4): 795–813.

Frankenberg, Elizabeth, Jed Friedman, Thomas Gillespie, Nicholas Ingwersen, Robert Pynoos, Iip Umar Rifai, Bondan Sikoki, Alan Steinberg, Cecep Sumantri, Wayan Suriastini, and Duncan Thomas. 2008. "Mental health in Sumatra after the tsunami." *American jJournal of Public Health*, 98(9): 1671–1677.

Frankenberg, Elizabeth, Jenna Nobles, and Cecep Sumantri. 2012. "Community destruction and traumatic stress in post-tsunami Indonesia." *Journal of Health and Social Behavior*, 53(4): 498–514.

Frankenberg, Elizabeth, Thomas Gillespie, Samuel Preston, Bondan Sikoki, and Duncan Thomas. 2011. "Mortality, The Family and The Indian Ocean Tsunami." *The Economic Journal*, 121(554): F162–F182.

Gray, Clark, Elizabeth Frankenberg, Thomas Gillespie, Cecep Sumantri, and Duncan Thomas and. 2014. "Studying Displacement After a Disaster Using Large-Scale Survey Methods: Sumatra After the 2004 Tsunami." *Annals of the Association of American Geographers*, 104(3): 594–612.

Hicks, Raymond, and Dustin Tingley. 2011. "Causal Mediation Analysis." *The Stata Journal*, 11(4): 605–619.

Hoddinott, John, and Bill Kinsey. 2001. "Child Growth in the Time of Drought." *Oxford Bulletin of Economics and Statistics*, 63(4): 409–436.

Iwo, René, Elizabeth Frankenberg, Cecep Sumantri, and Duncan Thomas. 2024. "Extreme events, educational aspirations, and long-term outcomes." *Population and Environment*, 46(20).

Jacoby, Hanan G., and Emmanuel Skoufias. 1997. "Risk, Financial Markets, and Human Capital in a Developing Country." *The Review of Economic Studies*, 64(3): 311–335.

Jensen, Robert. 2000. "Agricultural Volatility and Investments in Children." *The American Economic Review*, 90(2): 399–404.

Lewis, Blane D., and Hieu T.M. Nguyen. 2020. "Assessing the causal impact of compulsory schooling policy in Indonesia." *International Journal of Educational Research*, 104: 101693.

Maccini, Sharon, and Dean Yang. 2009. "Under the Weather: Health, Schooling, and Economic Consequences of Early-Life Rainfall." *American Economic Review*, 99(3): 1006–26.

Mullis, I. V. S., M. O. Martin, S. Goh, and K. Cotter, ed. 2016. *TIMSS 2015 Encyclopedia: Education Policy and Curriculum in Mathematics and Science.* TIMSS & PIRLS International Study Center, Boston College.

Nakamura, Emi, Jósef Sigurdsson, and Jón Steinsson. 2021. "The Gift of Moving: Intergenerational Consequences of a Mobility Shock." *The Review of Economic Studies*, 89(3): 1557–1592.

Ruggles, Steven, Lara L. Cleveland, Rodrigo Lovatón Dávila, Sula Sarkar, Matthew Sobek, Derek Burk, Dan E. Ehrlich, Jane Lee, and Nate Merrill. 2025. "IPUMS International: Version 7.7."

Sacerdote, Bruce. 2012. "When the Saints Go Marching Out: Long-Term Outcomes for Student Evacuees from Hurricanes Katrina and Rita." *American Economic Journal: Applied Economics*, 4(1): 109–35.

Telford, John, and John Cosgrave. 2006. "Joint Evaluation of the International Response to the Indian Ocean Tsunami. Synthesis Report." Tsunami Evaluation Coalition 1.

Thiery, Wim, Stefan Lange, Joeri Rogelj, Carl-Friedrich Schleussner, Lukas Gudmundsson, Sonia I. Seneviratne, Marina Andrijevic, Katja Frieler, Kerry Emanuel, Tobias Geiger, David N. Bresch, Fang Zhao, Sven N. Willner, Matthias Büchner, Jan Volkholz, Nico Bauer, Jinfeng Chang, Philippe Ciais, Marie Dury, Louis François, Manolis Grillakis, Simon N. Gosling, Naota Hanasaki, Thomas Hickler, Veronika Huber, Akihiko Ito, Jonas Jägermeyr, Nikolay Khabarov, Aristeidis Koutroulis, Wenfeng Liu, Wolfgang Lutz, Matthias Mengel, Christoph Müller, Sebastian Ostberg,

Christopher P. O. Reyer, Tobias Stacke, and Yoshihide Wada. 2021. "Intergenerational inequities in exposure to climate extremes." *Science*, 374(6564): 158–160.

Weldeegzie, Samuel G. 2017. "Growing-up Unfortunate: War and Human Capital in Ethiopia." *World Development*, 96: 474–489.

Zhang, Zhidi, and Jianqing Ruan. 2020. "Do Long-Run Disasters Promote Human Capital in China? -The Impact of 500 Years of Natural Disasters on County-Level Human-Capital Accumulation." *International Journal of Environmental Research and Public Health*, 17(20): 7422.