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The tempest: Short- and long-term consequences of a natural disaster for children's development[☆]

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ABSTRACT

This paper analyzes the short- and long-run consequences of a natural disaster on children's education and health. The particular focus lies on variation in idiosyncratic shocks to households using housing damages caused by a super typhoon as a proxy. Relying on individual panel data and a setting in which typhoons are a relatively rare event, we find negative and persistent effects on children's education but no effects on children's health. Effects on education are likely driven by a shift in parental investments made to cope with the economic consequences of typhoon damages. Subgroup analysis suggests that results are stronger for girls, children with no older siblings, children from poor families, and families with no strong family or social network.

1. Introduction

Climate change comes with an increased frequency and intensity of extreme weather events (such as droughts, floods, and tropical storms). The focus of politicians and researchers lies mostly on the immediate consequences of such events. Yet, a better understanding of the long-term consequences is crucial for designing strategies to protect the most vulnerable populations. This paper is about the short- and long-term consequences of a super typhoon for children's development. The particular focus lies on the intensity of idiosyncratic shocks to households using housing damages caused by a super typhoon as a proxy. Housing damages serve as a measure of how badly individuals are affected by the disaster. This makes it possible to study the emergence and persistence of inequalities among the affected population.

The natural disaster under study is super typhoon Mike, which hit Cebu Island in the Philippines in 1990. This setting, combined with the availability of individual panel data, is ideal for analyzing the question under study. First, the Cebu

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Longitudinal Health and Nutrition Survey (CLHNS) provides the rare opportunity to observe health and education outcomes of affected children for up to 15 years after the occurrence of the disaster. Second, the data contains information on damages to families' homes, which serves as a proxy for variation in the intensity of the idiosyncratic shock. Third, the data is very informative about pre-disaster characteristics, which is crucial for our identification strategy. Housing damages are the result of two components: local differences in the severity of the typhoon (i.e., gusts or mudslides) and the quality of the house. The richness of our data and random differences in local typhoon intensity allow us to balance any observable

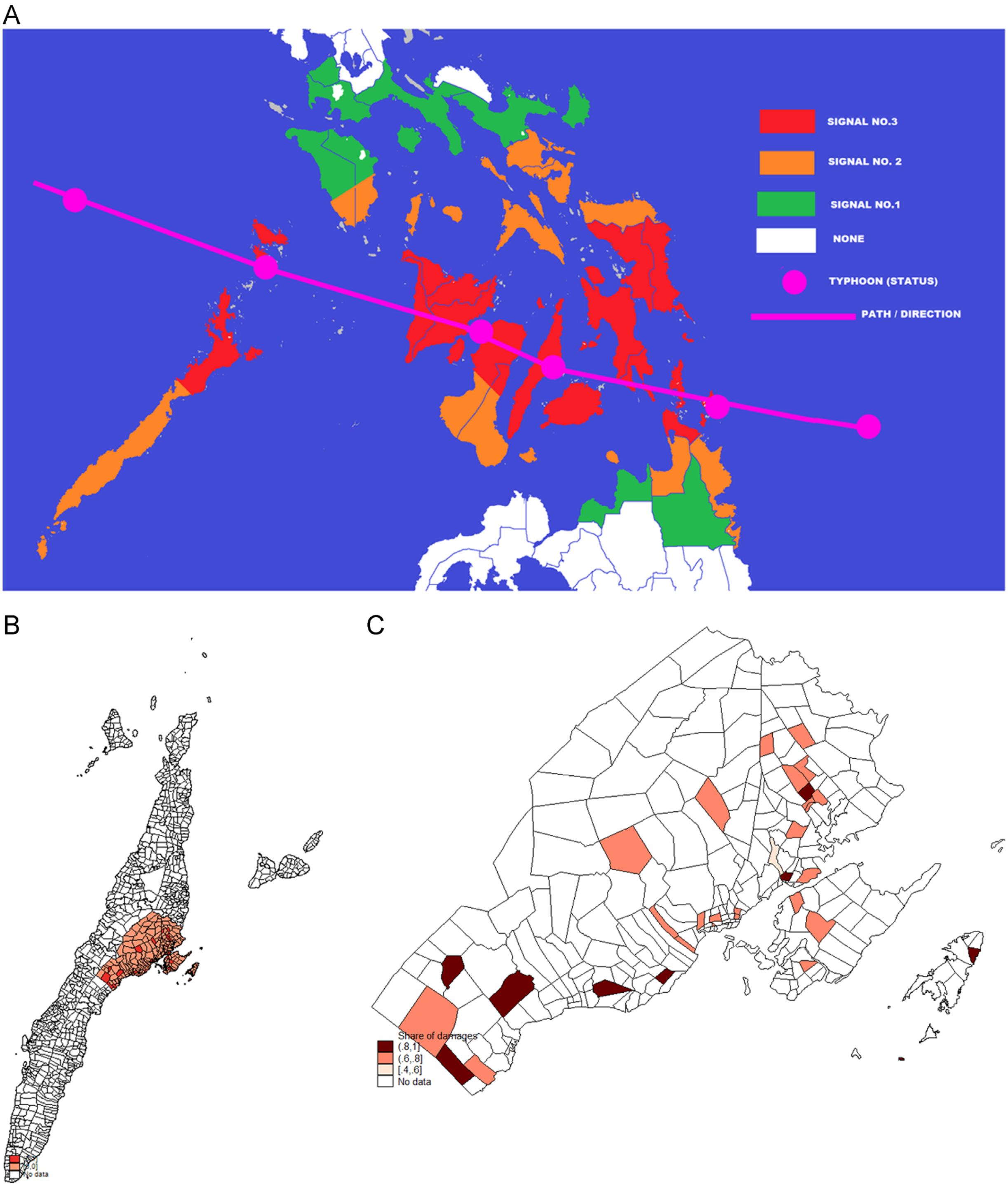

Fig. 1. Track and intensity of typhoon Mike. Panel A: tracking of typhoon Mike and warning signal, Panel B: Cebu Island, Panel C: damages in Cebu Metropolitan Area.

differences between those that suffer housing damages and those who do not. After controlling for proxies of housing quality and relevant determinants of child development, the remaining differences in housing damages should be random. A series of sensitivity checks, including omitted variables and balancing (placebo) tests in important pre-disaster outcomes, provides empirical evidence that our results are not driven by omitted confounding factors. Finally, super typhoons are relatively rare in this area, which makes it possible to investigate the long-term consequences without being confounded by similar earlier or later disasters.

Our results indicate a direct pathway from typhoon damages to children's education but not to health. In addition to short-term effects, we observe widening educational gaps as children grow older. This is expressed in lower test scores, an increasing prevalence of grade retentions and a reduction in overall schooling in the long run. Analysis of a broad set of observed potential channels provides evidence that the driving underlying mechanism is a severe and permanent reduction in families' wealth. A reduction in school expenditures and lower school enrollment rates indicate a shift of parental investments away from children's education. Subgroup analysis suggests that results are driven by girls, children with no older siblings, children from the bottom half of the wealth distribution, and children from families with no family or social network in the location of residence.

Our study contributes to a growing literature on the consequences of climate change and resulting natural disasters. Besides empirical evidence on macroeconomic consequences (see, for instance, Hsiang and Jina, 2014; Ferreira and Schady, 2009), previous literature has provided empirical evidence on the overall consequences of natural disasters on children's short- and medium-run development. This literature either compares children who are born before the disaster occurrence with children born after it (e.g., Currie and Rossin-Slater, 2013; Fuller, 2014; Simeonova, 2011) or compares children who live in a disaster area with children who do not live in the disaster area (for example, Antilla-Hughes and Hsiang, 2013; Aguilar and Vicarelli, 2011; Baez and Santos, 2007; Frankenberg, et al., 2011; Maccini and Yang, 2009; Poertner, 2009). This literature usually finds negative short- and medium-run effects on children's health and education.

We contribute to this literature in two ways: first, we provide empirical evidence on long-term consequences of a natural disaster for children's health and education. In particular, we provide evidence of the consequences of a disaster that occurred when children were six years old, and thus at the onset of investment into formal education, up to age 21/22, when formal education is mostly complete. We are aware of only two other papers providing long-term impacts: Sotomayor (2013) studies the consequences of exposure to tropical storms, either in the womb or during early infancy, for long-term health and schooling outcomes in adulthood; and Eskander and Barbier (2014) study the consequences of exposure to natural and political disasters such as tropical storms, wars, or famines (all in the early 1970s in Bangladesh) during early infancy for long-term health and schooling outcomes. Second, most other studies provide evidence for a combined effect, combining the impact of common threats (such as destruction of local public infrastructure, epidemics, or food shortages) and idiosyncratic threats (such as the destruction of individual property, family member death, or displacement). In contrast, we focus on variation in the intensity of idiosyncratic damages to study inequalities that arise among the population affected by a disaster. A better understanding of the consequences of specific threats is relevant for tailoring disaster aid to the most vulnerable population. To the best of our knowledge, there are only two other studies that analyze the consequences of idiosyncratic threats caused by natural disasters: Cas et al. (2014) study the impact of parental death caused by the Tsunami in Indonesia in 2004, and Sacerdote (2012) studies the consequences of forced migration due to Hurricanes Katrina and Rita for children's educational achievements.

The reminder of the paper is structured as follows: the next section provides background information on the specific setting under study as well as the conceptual framework for understanding any short- and long-term consequences of a natural disaster on children's development. Section 3 discusses the empirical strategy with a focus on the underlying identifying assumptions. Section 4 describes the data. Section 5 presents the results, provides a series of sensitivity checks, and discusses the nature of the shock and heterogeneity in the effects across several subgroups. Section 6 offers concluding remarks.

2. Background and conceptual framework

2.1. Cebu Metropolitan Area and typhoon Mike

The Cebu Metropolitan Area is located along the central eastern region of Cebu Island (see Fig. 1, Panel B). The island is long and narrow, stretching 196 km from north to south and 32 km across at its widest point. The island's area is 4468 km^2 and thus only slightly larger than Rhode Island, the smallest US state. The Cebu Metropolitan Area accounts for 20% of the land area and 60% of the population of Cebu Island. It stretches over 65 km, comprises five major cities, and counts around 2.5 million inhabitants. The Cebu Metropolitan Area is one of the most developed and most populated areas in the Philippines, with Cebu City as its leading commercial and financial hub.

In contrast to other parts of the Philippines, which are frequently exposed to typhoons, Cebu Island enjoys a particularly beneficial geographical location. It lies in the center of an archipelago and is thus surrounded by larger landmasses that serve as a natural barrier to typhoons (see Fig. 1, Panel A). Moreover, Cebu Island lies at the southern limit of the typhoon belt. As a result, Cebu Island is rarely hit by typhoons. This is particularly true for the Cebu Metropolitan Area, which lies on the central eastern coast of Cebu Island. Super typhoon Mike, which occurred in 1990, is one of the rare exceptions. Prior to super typhoon Mike, the Cebu Metropolitan Area experienced its last super typhoon in 1951, and no other super typhoon has occurred since then. We are also not aware of any further disaster occurring during our study period that would have been strong enough to have severe consequences for the citizens of the Cebu Metropolitan Area. Obviously, the area under study is frequently exposed to smaller tropical storms. However, these climatic conditions are typical for the area, and thus should not confound our results.

Super typhoon Mike started forming over the Pacific Ocean on November 6, 1990. It headed westward and was first designated a typhoon on November 9. On November 10, the typhoon rapidly intensified, reaching peak winds of 265 km/h and thus qualifying as a super typhoon (an especially destructive typhoon with exceptionally high wind speeds – around 250 km/h or greater). Initially, Mike was forecasted to move northwest and make landfall over the Manila Metropolitan Area, but it instead slammed over the eastern Philippines and hit Cebu Metropolitan Area on November 12. Fig. 1, Panel A displays the course of the typhoon.

The overall damages were severe: 748 people were killed, 3.2 million people were forced into temporary shelters in schoolhouses and evacuation centers, more than 100,000 houses were destroyed and almost 300,000 houses damaged, and the majority of the disaster area was left without electricity and potable water (Williams, et al., 1993).

Damages that result from a typhoon are not uniform but are rather most severe right next to the eye of the typhoon. Variation in damages occurs due to strong gusts (i.e., wind peaks of a few seconds) or mudslides. As a consequence, damages are likely to vary even within the disaster area. Fig. 1, Panel C displays the average damages (i.e., the share of families that report damages on their house) across different barangays (the smallest administrative division in the Philippines) of the Cebu Metropolitan area.[1] While the overall share of damages is high (75% of all families report some housing damages), variation across barangays is substantial and ranges from 45% of all families reporting some housing damages to as much as 100%.

2.2. Natural disasters, idiosyncratic shocks and expected effects on children's development

Natural disasters are associated with several threats to children's development: general equilibrium effects, common shocks, and idiosyncratic shocks. General equilibrium effects may negatively affect individuals living in the disaster area as well as individuals not living in the disaster area, due to spillover effects. Common threats include destruction of public infrastructure, epidemics, and food shortages, which affect all individuals living in the disaster area. Idiosyncratic shocks affect only a subset of individuals living in the disaster area and include damages to individual property, family member death, and displacement. This paper focuses on idiosyncratic shocks to households proxied by damages to families' homes (a marker of how badly an individual was affected by the typhoon).

Real estate represents an important component of families' wealth, particularly in a developing country such as the Philippines (in our sample, real estate wealth corresponds to 40% of total wealth). Real estate is characterized by two important features, which distinguish it from pure financial assets: first, real estate serves as collateral. A shock on real estate wealth may therefore lead to binding credit market constraints (Chaney, et al., 2012). Second, people live in their homes. In other words, real estate serves not only as an investment good but also as a consumption good. In a situation of an underdeveloped rental housing market, which was the case for the Philippines in the early 1990s (Ballesteros, 2001), people are thus forced to invest in reconstruction of their houses.

One underlying mechanism by which damages to real estate affect children's development is a shift of investments into reconstruction of houses and away from children's health and education. In the Philippines in the early 1990s, primary school attendance was compulsory for six years, but attendance was not perfectly enforced. Besides attending school, many children engaged in market or home production activities and thus were likely to contribute to household income (DeGraff and Bilsborrow, 2003). Given the costs of schooling, which are either direct (costs for material and janitorial fees as well as tuition fees in the case of private schools, which in the 1990s constituted the minority of schools in the Philippines) or indirect (opportunity costs in time), investments in education were likely to decline following the occurrence of housing damages. This was particularly likely if children were helping to rebuild the family home. The consequences for health investments are a priori unclear. On the one hand, we might expect them to go down, particularly given the rather low health insurance penetration in the Philippines in the early 1990s (in our sample 33%); on the other hand, some children might already live at the subsistence level, and thus parents might try to avoid a further reduction in health investments.

In addition to the immediate consequences driven by a shift of investments, there may be important intertemporal consequences. Human capital is commonly modeled as the outcome of a cumulative production process (Cunha, et al., 2006). Dynamic complementarities and self-productivity are the key features of this production process; that is, capacities produced at earlier stages enhance the productivity of later investments and thus the attainment of capacities at later stages. As a result, investments or adverse shocks during early childhood are not only expected to have an immediate effect on children's human capital, but any effects are likely to widen over time. This is likely in our case, since the shock under study occurred when children were 6 or 7 years old and thus at the onset of investments into formal education. Thus, if the typhoon and arising damages to families' homes indeed led to a reduction of investments into children's human capital, we expect not only that children will be worse off in the short run but also that any gap arising in the short run will widen in the long run.

As mentioned above, housing damages are only a proxy for the intensity of the idiosyncratic shock to the household. Thus, besides the obvious shift in investments, housing damages may proxy further threats to children's development. Section 5 sheds some light on the nature of the shock proxied by housing damages.

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[1] Data is from the Cebu Longitudinal Health and Nutrition Survey. This survey only covers 33 random barangays of the Cebu Metropolitan Area. Detailed information is provided in Section 4.

3. Empirical strategy

The objective of this paper is to identify whether variation in the intensity of idiosyncratic damages caused by a natural disaster leads to inequalities in children's development in the short and long run. Damages to families' homes serve as a measure of how badly an individual was affected by a typhoon. We compare children whose houses were damaged due to the typhoon with children who live in the same area, and are thus exposed to the same general equilibrium effects and common threats, but did not experience this particular idiosyncratic shock.

One natural question to ask is whether housing damages happened at random. Random factors, particularly local wind speed and the prevalence of severe gusts, are important determinants of damages (Imamura and Van To, 1997; Nordhaus, 2006). Yet, individual factors, such as the location of residence and the quality of the house, are likely to be key in explaining the severity of damages to private property and may confound our results (Fronstin and Holtmann, 1994). Our baseline specification thus controls for a vast array of potential determinants of housing damages and important determinants of child development. The equation underlying all our estimates (estimated by ordinary least squares and clustered at the barangay level) is as follows:

$$Y_{i,t+s} = \alpha + \beta D_{i,t} + \gamma H_{i,0} + \delta F_{i,0} + \theta C_{i,0} + e_{i,t+s}$$

where $Y_{i,t+s}$ represents child i's health or education outcome measured in period s after the occurrence of the typhoon, and $D_{i,t}$ represents damages to child i's home due to the typhoon. The core set of controls can be grouped into the following three blocks of characteristics: $H_{i,0}$ stands for characteristics of the house (construction material, underlying soil formation and soil depth, available hygienic infrastructure, size, value, and home ownership), $F_{i,0}$ for characteristics of the family (household type, number of siblings, absenteeism of family members, parents' education and employment status, income, wealth), and $C_{i,0}$ for characteristics of the child (gender, weight and height at birth, complications at birth, place of delivery). Note that all control variables stem from surveys before the typhoon and are thus not plagued by reverse causality.

The interpretation of the coefficient β as an estimate for inequalities due to variation in the intensity of idiosyncratic damages relies on several assumptions: first, we assume that the remaining variation (net of the core set of control variables) in housing damages is not confounded with any further factors correlated with parental investments in children's human capital – the so-called conditional independence assumption. Second, outcome variables are taken from surveys in subsequent years that are subject to attrition. We rely on the assumption of random attrition conditional on our set of control variables. And finally, we rely on the stable unit treatment value assumption; that is, we assume that our control group of children who did not experience housing damages was not affected by the idiosyncratic shock experienced by their neighbors.

To challenge the underlying assumptions, we pursue the following sensitivity checks (results are shown in Section 5.2): we carefully discuss the relevance of the included control variables and conduct a series of omitted variable tests. These omitted variable tests are performed from two angles, excluding core blocks of control variables and adding further control variables. We also employ several balancing tests with respect to pre-disaster child development outcomes (such as initial health outcomes, preschool and primary school enrollment of the index child, and educational achievement of older siblings prior to the disaster). These are results of parental investments but cannot have been influenced by the disaster. Violations of the conditional independence assumption should therefore be detected by imbalances in pre-treatment outcomes after controlling for the core set of control variables (Pischke and Schwandt, 2015). We furthermore use these balancing tests and apply them to the subsamples remaining in subsequent surveys to test for violations of the random attrition assumption.

While these sensitivity tests allow us to assess empirically the conditional independence and the random attrition assumption, they are silent about potential violations of the stable unit treatment valuation assumption. Living in an area were many residential houses are damaged may have negative spillover effects on the children of the control group. Yet, if this is the case, the estimate of β is likely to represent a lower bound for the impact of housing damages on child development outcomes.

4. Data

4.1. Sample description

The dataset used in this study is the Cebu Longitudinal Health and Nutrition Survey (CLHNS), which is a 12-month birth cohort study (May 1983–April 1984) from 33 randomly selected barangays in the Cebu Metropolitan Area. Initial interviews were held with all pregnant women in the sample area. Follow-up interviews took place immediately after birth, at bimonthly intervals for 24 months after birth, and in 1991, 1994, 1998, 2002, and 2005 (see http://www.cpc.unc.edu/projects/cebu for more information). We restrict the dataset to children who survived until 1990 (i.e., did not die before typhoon Mike) whose mothers answered the last interview prior to super typhoon Mike and for whom we have complete and consistent background information.[2] As a result, our baseline sample consists of 2391 children.

The first survey after the typhoon (1991) provides retrospectively reported information on housing damages. 76% of all households experienced some damages. For those who reported damages, the survey additionally asked about

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[2] In 1983/84 there were 3'327 (officially) pregnant women in the survey area. 3'122 participated in the baseline interview. 2'631 women still participated in the last interview available prior to typhoon Mike which took place in 1985/86 when children where 2 years old. 116 out of these 2'631 children died until 1991. Furthermore, there are data inconsistencies or missing background information for 124 children.

Table 1
Descriptive statistics (control variables at baseline).

Variables	*Total*	*No damages*	*Damages*
Characteristics of the house			
Material of house (omitted category: mixed materials)			
___: nipa	0.43 (0.50)	0.34 (0.47)	0.46 (0.50)[***]
___: cement, wood	0.17 (0.38)	0.28 (0.45)	0.14 (0.35)[**]
Soil formation (omitted category: weathered limestone)			
___: unconsolidated	0.39 (0.49)	0.40 (0.49)	0.39 (0.49)
___: core basalt rocks	0.10 (0.30)	0.08 (0.28)	0.10 (0.30)
Average soil (omitted category: > 3 m)			
___: 1–3 m	0.17 (0.38)	0.19 (0.40)	0.17 (0.37)
___: 0.3–1 m	0.32 (0.47)	0.28 (0.45)	0.33 (0.47)
___: < 0.3 m	0.14 (0.34)	0.13 (0.34)	0.14 (0.34)
House ownership	0.72 (0.45)	0.68 (0.47)	0.73 (0.45)[*]
Log of house value	5.00 (2.64)	5.12 (3.00)	4.96 (2.51)
Nr of rooms	2.61 (1.34)	2.95 (1.61)	2.49 (1.22)[***]
GaGarbage disposal (omitted category: other)			
___: collected	0.13 (0.33)	0.19 (0.39)	0.11 (0.31)[***]
___: burning	0.42 (0.49)	0.37 (0.48)	0.43 (0.50)
___: dumping	0.14 (0.34)	0.13 (0.34)	0.14 (0.34)
Characteristics of the child			
Female	0.47 (0.50)	0.51 (0.50)	0.46 (0.50)[**]
Size at birth (omitted category: normal)			
___: smaller than normal	0.18 (0.39)	0.17 (0.38)	0.19 (0.39)
___: bigger than normal	0.27 (0.44)	0.25 (0.43)	0.28 (0.45)
Place of delivery: hospital	0.36 (0.48)	0.47 (0.50)	0.32 (0.47)[***]
Birth complication	0.14 (0.34)	0.13 (0.34)	0.14 (0.35)
Height for age (first interview after birth)	−0.71 (1.10)	−0.64 (1.04)	−0.73 (1.11)[*]
Weight for age (first interview after birth)	−1.18 (1.15)	−1.19 (1.12)	−1.18 (1.16)
Characteristics of the household			
Type of family (omitted category: one nuclear)			
___: multi-nuclear	0.20 (0.40)	0.25 (0.43)	0.19 (0.39)[***]
___: other	0.16 (0.36)	0.15 (0.36)	0.16 (0.36)
Nr of siblings	2.23 (2.02)	1.95 (1.90)	2.32 (2.05)[***]
Spouse lives in HH	0.95 (0.23)	0.93 (0.26)	0.95 (0.21)[**]
Spouse temporary absent	0.04 (0.19)	0.03 (0.18)	0.04 (0.19)
Father's highest grade	6.65 (4.30)	7.32 (4.67)	6.42 (4.15)[**]
Father voc. Training	0.12 (0.32)	0.15 (0.36)	0.11 (0.31)[**]
Mother's highest grade	7.35 (3.70)	8.54 (4.05)	6.96 (3.49)[***]
Mother voc. Training	0.16 (0.37)	0.17 (0.37)	0.16 (0.36)
Father employment	0.86 (0.35)	0.84 (0.37)	0.86 (0.34)
Mother employment	0.41 (0.49)	0.41 (0.49)	0.41 (0.49)
Log of total income	8.37 (1.28)	8.60 (1.25)	8.29 (1.28)[***]
Log wealth	7.31 (1.68)	7.75 (1.88)	7.16 (1.58)[***]

Note: Standard deviations are shown in parentheses.

[*] denote statistically significant differences at the 10 level of significance, respectively, between the means of children with and without damages. The underlying *P*-values are calculated using wild bootstrap (999 replications) clustered at the barangay level.

[**] denote statistically significant differences at the 5% level of significance, respectively, between the means of children with and without damages. The underlying *P*-values are calculated using wild bootstrap (999 replications) clustered at the barangay level.

[***] denote statistically significant differences at the 1% level of significance, respectively, between the means of children with and without damages. The underlying *P*-values are calculated using wild bootstrap (999 replications) clustered at the barangay level.

estimated reparation costs. Average reported costs amounted to 3,972 Philippine Pesos (approximately 260 constant 1990 international dollars). This value may, however, vastly underestimate the shock: it includes neither the loss if the house was irreparably destroyed nor any losses of other items, such as furniture or household appliances. Moreover, it does not consider opportunity costs if the reparation was done by a household member. For these reasons, our main analysis focuses on the binary indicator of reported damages. However, we draw on the reparation costs as a proxy for how badly families were affected by the typhoon to test for sensitivity of the underlying functional form (results are shown in Section 5.2).

Table 1 describes our sample in terms of background characteristics taken from pre-typhoon surveys. The table displays background variables separately for children who experienced damages and children who were not exposed. As expected, housing damages are correlated with housing and family background characteristics. As such we cannot assume that damages occur at random and will control for such confounders in all our estimations.

Table 2
Attrition rates.

	Sample		*No damages*		*Damages*		*Significance*
Year	*Obs.*	*Attrition since baseline*	*Obs.*	*Attrition since 1991*	*Obs.*	*Attrition since 1991*	*p-value*
Baseline	2391	–	–	–	–	–	–
1991	2095	0.124	521	–	1'574	–	–
1994	1952	0.184	485	0.069	1'464	0.070	0.973
1998	1903	0.204	472	0.094	1'423	0.096	0.897
2002	1935	0.191	471	0.096	1'458	0.074	0.114
2005	1891	0.210	453	0.131	1'430	0.091	0.010

Note: Damages are only retrospectively reported in 1991. We therefore cannot calculate attrition rates from baseline to 1991 separately for treated and control children, but only attrition with respect to 1991. *P*-values indicating the significance of the unconditional correlation between attrition and housing damages are calculated using wild bootstraps (999 replications) clustered at the barangay level.

4.2. Attrition

Like any longitudinal survey, our sample is plagued by attrition. Initial attrition from our baseline sample in 1985/86 to the first post-disaster survey in 1991 amounts to 12% (which corresponds to an annual attrition rate of roughly 2%) and total attrition from the baseline to 2005 amounts to 21%. In comparison to common attrition rates of 10% after one year and 50% after 20 years (Lee, 2003), attrition in our sample is thus remarkably low. Table 2 displays attrition rates for the pooled sample as well as separately for affected and unaffected children.

The major reason for attrition in our sample is outmigration (Adair, et al., 2011). One threat to our analysis is that outmigration might be systematically related to housing damages. Unfortunately, we cannot test for such attrition bias, as damages were retrospectively reported in 1991 and thus are not available for households dropping out of the sample. Yet, a comparison of the sample that dropped out before 1991 with the remaining sample does not provide any evidence that attrition is systematically correlated with a higher probability to suffer from housing damages (see Table I.1 in the Internet Appendix). In fact, those dropping out of the sample enjoy at baseline a house of better quality and a higher socio-economic status, suggesting that attrition is more likely to be driven by economic factors (such as work opportunities of the household members) than by damages due to the typhoon. We thus expect a bias towards zero, if any (children from advantaged families are not only more likely to drop out, as shown in Table I.1, but also more likely to be spared from housing damages, as shown in Table 1).

There is also no evidence that cumulative attrition rates from 1991 onwards are significantly higher among children who experience housing damages. In fact, the unconditional correlation between cumulative attrition and housing damages is mostly insignificant and meaningless in size (less than 1 percentage point, henceforth pp). If anything, their attrition rates are slightly lower once they grow older: attrition among affected children amounts to 9% and among unaffected children to 13%. We provide empirical evidence that these differences in cumulated attrition do not unbalance our sample and thus do not invalidate our results (results are shown in Section 5.2).

4.3. Outcome variables

Child development outcomes are taken from surveys subsequent to the disaster (1991–2005). The CLHNS provides comprehensive information on children's development in terms of education and health. To keep this paper within reasonable limits, we restrict our main analysis to a subset of outcome variables. Health outcomes that are provided in all surveys and thus can be compared across years include anthropometric measures, such as body weight and height (which we standardize with respect to the age- and gender-specific mean, so-called *z*-scores). Regarding education outcomes, we concentrate on two measures for children's cognitive development: grade progression and standard IQ tests. Unfortunately, information on grade progression is only reported from 1994 onwards, and IQ tests were only administered in 1991 and 1994. Importantly, IQ tests are part of the interview and are thus not restricted to children attending schools; in other words, test scores do not suffer from any additional attrition. Proxies for parental investments into children's education and health that are available in all post-disaster surveys are expenditure data on education, food, and medical purposes, as well as data on children's school enrollment, school attendance, paid child work, immunization, and nutritional supplements. Table I.2 displays the descriptive statistics for all child outcomes.

Finally, the data provides a vast set of variables that makes it possible to study the underlying nature of the shock. Our prior assumption is that housing damages mainly affect the financial wellbeing of the family, measured by household wealth.[3] We also analyze further potential mechanisms, such parental economic activity, parental health, and household composition. Descriptive statistics for all outcome variables are provided in the Internet Appendix (Table I.3 –I.5).

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[3] Data includes information on ownership of different wealth items for all years and the value of different asset groups for the year 1991. Information on ownership of different assets is used to construct an asset index employing a principal components analysis (Filmer and Pritchett, 2001). The disadvantage of this procedure is that the resulting asset index has no economic meaning other than a higher index being a sign of greater wealth. We therefore use available information in 1991 to regress the total value of different asset groups on a set of indicators for ownership of the mentioned asset groups. The estimated regression coefficients are then used to construct a linear wealth index for asset ownership indicators in later surveys.

Table 3
Effects of damages on child development outcomes.

Dependent variables	*Survey year*				
	1991	1994	1998	2002	2005
Education outcomes					
Highest grade completed	–	−0.130**	−0.273**	−0.515***	−0.667***
	–	(0.051)	(0.095)	(0.100)	(0.179)
	–	[0.028]	[0.010]	[0.002]	[0.008]
Mean control group	–	3.76	8.08	10.61	11.83
IQ score (std.)	−0.115***	−0.111**	–	–	–
	(0.039)	(0.051)	–	–	–
	[0.004]	[0.048]	–	–	–
Mean control group	0.23	0.22	–	–	–
Health outcomes					
Weight for age (std.)	−0.055	−0.027	−0.011	0.020	0.025
	(0.069)	(0.079)	(0.071)	(0.077)	(0.093)
	[0.390]	[0.679]	[0.841]	[0.751]	[0.835]
Mean control group	−2.00	−1.69	−1.71	−1.96	−1.97
Height for age (std.)	0.031	−0.003	−0.021	0.031	0.034
	(0.052)	(0.054)	(0.047)	(0.056)	(0.058)
	[0.581]	[0.923]	[0.631]	[0.627]	[0.585]
Mean control group	−2.13	−1.66	−1.93	−2.06	−2.07

Note: Each cell corresponds to the regression coefficient of the binary damage indicator resulting from a separate estimation using equation (1) for each outcome variable. Control variables are the set of variables displayed in Table 1. The results for the full specification are available upon request. Standard errors (in parentheses) are clustered by barangay.

[*] denote statistical significance at the 10% level of significance. *P*-values from wild clustered bootstraps (999 replications) are provided in square brackets. The “mean control group” refers to the mean of the respective outcome variable among the children not suffering from any housing damages.

[*] denote statistical significance at the 5% level of significance. *P*-values from wild clustered bootstraps (999 replications) are provided in square brackets. The “mean control group” refers to the mean of the respective outcome variable among the children not suffering from any housing damages.

[***] denote statistical significance at the 1% level of significance. *P*-values from wild clustered bootstraps (999 replications) are provided in square brackets. The “mean control group” refers to the mean of the respective outcome variable among the children not suffering from any housing damages.

5. Empirical results

5.1. Baseline outcomes

This section provides empirical evidence for the effect of housing damages on child development outcomes. Table 3 presents the effect of the binary damage indicator on different education and health outcomes. Standard errors are clustered on the barangay level. Given the relatively small number of clusters (33 barangays), one might be concerned about the correlation of errors within geographical units. We therefore also provide *P*-values from wild clustered bootstraps (Cameron, et al., 2008) in square brackets. In addition, we display the mean value of the respective outcome variable among the children not affected by housing damages (Mean Control group). This shall allow an easy assessment of the magnitude of the effect.

In line with the theoretical considerations discussed in Section 2.2, an adverse shock during early childhood translates into worse educational performance not only in the short run but also in the long run. We observe increased grade retention already four years after typhoon Mike occurred. In 1994, children who suffered from housing damages lagged on average 0.13 years behind in school. This gap remains significant and even widens when children grow older. The gap in completed grades amounts to 0.27 years in 1998, 0.52 years in 2002 and 0.67 years in 2005. The estimated gaps are statistically significant (the difference between 1994 and 1998 at the 5% significance level and the difference between 1994 and 2002, and 1994 and 2005 at the 1% significance level). Thus, our results are in line with the idea of the cumulative nature of the human capital production process.

Since cognitive skills are malleable during early childhood and are enhanced through investments by parents and the social environment (Cunha, et al., 2006), one would also expect a gap in IQ test scores. Indeed, in the immediate years after the natural disaster (1991 and 1994), children affected by housing damages scored on average lower on a general IQ test (by 0.12 sd and 0.11 sd, respectively). Additional estimates using results from tests in Cebuano, English, and Math confirm the negative impact of housing damages caused by a typhoon on children's education (see Internet Appendix, Table I.5). Since none of the tests was conducted in later years, we cannot test whether the gap widens in the long run.

In a similar vein to the findings for children's educational development, we expect detrimental consequences on children's health. Such negative effects may be driven by direct effects due to reduced investments in health and by indirect negative effects due to the complementary nature of health and education (Grossman, 2006). Our results, however, do not indicate a significant impact on children's health, either in the short run or in the long run (see Table 3). The average effects on children's z-scores for weight and height are insignificant. These results are consistent using a large battery of further physical and mental health outcomes, which are both objectively measured and self-reported (see Table I.5).

Table 4
Effects of damages on parental investments.

Dependent variables	*Survey year*				
	1991	1994	1998	2002	2005
Education investments					
School expenditures (log)	–	-0.104^{**}	-0.180^{***}	-0.235^{**}	-0.237^{**}
	–	0.048	0.051	0.086	0.098
	–	[0.023]	[0.002]	[0.010]	[0.032]
Mean control group	–	0.80	0.92	0.78	0.47
Health investments					
Medical Expenditure (log)	–	−0.031	−0.140	0.043	0.052
	–	0.074	0.111	0.076	0.108
	–	[0.659]	[0.198]	[0.545]	[0.663]
Mean control group	–	0.78	0.79	0.80	1.18
Food Expenditures (log)	–	-0.043^{*}	-0.061^{**}	-0.048^{*}	-0.068^{*}
	–	0.024	0.026	0.025	0.035
	–	[0.086]	[0.022]	[0.064]	[0.074]
Mean control group	–	3.55	3.11	2.90	2.85

Note: Each cell corresponds to the regression coefficient of the binary damage indicator resulting from a separate estimation using equation (1) for each outcome variable. Control variables are the set of variables displayed in Table 1. The results for the full specification are available upon request. Standard errors (in parentheses) are clustered by barangay.

[*] denote statistical significance at the 10% level of significance. *P*-values from wild clustered bootstraps (999 replications) are provided in square brackets. The "Mean Control Group" refers to the mean of the respective outcome variable among the children not suffering from any housing damages.

[**] denote statistical significance at the 5% level of significance. *P*-values from wild clustered bootstraps (999 replications) are provided in square brackets. The "Mean Control Group" refers to the mean of the respective outcome variable among the children not suffering from any housing damages.

[***] denote statistical significance at the 1% level of significance. *P*-values from wild clustered bootstraps (999 replications) are provided in square brackets. The "Mean Control Group" refers to the mean of the respective outcome variable among the children not suffering from any housing damages.

We expect the main mechanism underlying the negative impact of housing damages on children's educational outcomes to be a shift of parental investment activities away from investments into children's human capital. Table 4 provides evidence supporting this hypothesis. There is a significant reduction in parental expenditures on schooling in the short run: in 1994, parents spent 9.1 pp less on schooling. This is even more pronounced in the long run: in 2005, parents' expenditures on schooling decreased by 23.7 pp. In line with this reduction in expenditures, we observe a persistent reduction in school enrollment in both the short and the long run (3.7–7.8 pp, see Table I.6). Additional analysis using children's time use data indicates not only that children miss school more often (0.32 days per month in 1991, which, given the average absence in our sample of 1.17 days per month, is a non-negligible increase), but also that the reduction in schooling is most likely driven by an increase in children's time spent helping at home but not by an increase in work for pay (see Table I.6).

Consistent with our previous finding that housing damages have no impact on health outcomes, we observe relatively small reductions in health investments (if any). The impact on medical expenditures is insignificant, and the impact on expenditures for nutrition amounts to 4.3 pp in 1994 and 6.8 pp in 2005. There are several explanations for why this reduction in food expenditures does not translate into worse health outcomes. First, the estimated reduction in expenditures is relatively small and may thus not translate into a meaningful effect on health outcomes. Moreover, families may try to keep total nutritional intake constant by substituting different food items, for instance, replacing more expensive proteins, such as meat, with less expensive proteins, such as fish (see Table I.6). Finally, families who experienced housing damages were also more likely to receive disaster relief, which may have prevented the immediate shock from affecting health in the short run (see Table I.6).

5.2. Sensitivity analysis

Causal interpretation of the results presented in the previous section relies on the conditional independence and random attrition assumption. As such, the choice of control variables is crucial. Our core set of controls includes a vast array of quality indicators of the house and determinants of child development. However, the concern remains that relevant confounding variables are unobserved. Below, we provide empirical evidence to ease this concern.

In a first step, we empirically assess the relevance of our core set of control variables for explaining the occurrence of housing damages and test whether an array of potentially omitted variables significantly contributes to predicting housing damages. Table 5 demonstrates that quality indicators of the house matter most for the likelihood of reported damages (quality indicators of the house are jointly significant at the 1% significance level). The most important determinants of housing damages are the material and the size of the house. Family background characteristics matter jointly (at the 1% significance level) even if each individual estimated marginal effect is relatively small. Overall, however, a family's socio-economic status (proxied by parental education, employment, and wealth) is negatively correlated with housing damages. Initial child characteristics tend to matter the least. Marginal effects are small and insignificant in most cases. The only surprising result is related to gender: girls experience housing damages more often. Yet, it is unlikely that gender picks up

Table 5
Determinants of damages (dependent variable: binary indicator for damages).

Independent variables	*dy/dx*	*Std. Err.*	*P-value*	*Independent variables*	*dy/dx*	*Std. Err.*	*P-value*
Characteristics of the house (*p-value*=0.000):				Birth complication	0.039	(0.028)	[0.180]
Material of house (omitted category: mixed materials)				Height for age	−0.006	(0.008)	[0.432]
___: nipa	−0.011	(0.018)	[0.478]	Weight for age	0.014	(0.016)	[0.230]
___: cement, wood	−0.073**	(0.025)	[0.010]	*Characteristics of the household* (*P-value*=0.000):			
Soil formation (omitted category: weathered limestone)				Type of family (omitted category: one nuclear)			
___: unconsolidated	0.019	(0.034)	[0.695]	___: multi-nuclear	0.058**	(0.023)	[0.032]
___: core basalt rocks	−0.010	(0.045)	[0.905]	___: other	0.034	(0.028)	[0.250]
Average soil depth (omitted category: > 3 m)				Nr of siblings	0.010	(0.006	[0.120]
___: 1–3 m	−0.023	(0.025)	[0.364]	Spouse lives in HH	0.063	(0.061)	[0.338]
___: 0.3–1 m	−0.009	(0.041)	[0.861]	Spouse temp. absent	0.091	(0.064)	[0.208]
___: < 0.3 m	−0.003	(0.042)	[0.917]	Father's highest grade	0.002	(0.003)	[0.621]
Own house	0.035	(0.033)	[0.268]	Father voc. Training	−0.038	(0.0300)	[0.182]
Log of house value	0.005	(0.005)	[0.304]	Mother's highest grade	−0.011**	(0.003)	[0.008]
Nr of rooms	−0.028***	(0.007)	[0.002]	Mother voc. Training	0.024	(0.027)	[0.396]]
Garbage disposal (omitted category: other)				Father employment	−0.046*	(0.023)	[0.096]
___: collected	−0.067	(0.046)	[0.186]	Mother employment	−0.000	(0.020)	[0.967]
___: burning	0.027	(0.040)	[0.557]	Log of total income	−0.007	(0.010)	[0.515]
___: dumping at house	−0.038	(0.047)	[0.454]	Log wealth	−0.023**	(0.010)	[0.040]
___: dumping away	−0.014	(0.035)	[0.647]				
Characteristics of the child (*P-value*=0.019):				*Omitted variable tests*		*P-value*	
Female	−0.045**	(0.0175)	[0.014]	Barangay Fixed Effects		0.000	
Size at birth (omitted category: normal)				Birth months dummies		0.004	
___: smaller than normal	0.024	(0.029)	[0.416]	House characteristics		0.414	
___: bigger than normal	0.025	(0.023)	[0.370]	Child characteristics		0.257	
Place of delivery: hospital	−0.037	(0.028)	[0.260]	Household characteristics		0.509	

Note: Above coefficients stem from an OLS regression where standard errors are clustered by barangay.

* denote statistical significance at the 10% level of significance. *P*-values are calculated using t-wild bootstrap (999 replications) clustering at the barangay level are provide in brackets. The *P*-values for the omitted variable tests refer to a test of joint significance of the barangay dummies, birth month dummies, house characteristics (type of water supply and distance from the next vehicular road), child characteristics (health conditions at age 2, such as fever, cough and measles), and family characteristics (parental health such as skinfold, weight and height) obtained from separate regressions augmenting the core set of controls by the respective of the respective set of variables.

** denote statistical significance at the 5% level of significance. *P*-values are calculated using t-wild bootstrap (999 replications) clustering at the barangay level are provide in brackets. The *P*-values for the omitted variable tests refer to a test of joint significance of the barangay dummies, birth month dummies, house characteristics (type of water supply and distance from the next vehicular road), child characteristics (health conditions at age 2, such as fever, cough and measles), and family characteristics (parental health such as skinfold, weight and height) obtained from separate regressions augmenting the core set of controls by the respective of the respective set of variables.

*** denote statistical significance at the 1% level of significance. *P*-values are calculated using t-wild bootstrap (999 replications) clustering at the barangay level are provide in brackets. The *P*-values for the omitted variable tests refer to a test of joint significance of the barangay dummies, birth month dummies, house characteristics (type of water supply and distance from the next vehicular road), child characteristics (health conditions at age 2, such as fever, cough and measles), and family characteristics (parental health such as skinfold, weight and height) obtained from separate regressions augmenting the core set of controls by the respective of the respective set of variables.

any unobserved housing quality. Gender preferences are uncommon and abortions are illegal in the Philippines (Almond, et al., 2013). Moreover, gender is not significantly correlated with features of socio-economic status at baseline. Thus, it is likely that the significant coefficient of gender is a statistical artifact driven by the relatively low number of barangays and the initial sampling procedure, which relied on local barangay reporters, who may have underreported female births.

We also test the explanatory power of further potential confounders, such as the location of the house (proxied by a set of barangay dummies) to capture endogenous location choice, birth months dummies to capture the importance of socio-economic differences across mothers giving birth in different seasons (Buckles and Hungermann, 2013), additional quality indicators of the house (the distance to the nearest road and the type of water provision), and information on detailed pre-disaster child health (illnesses such as fever, cough or measles) and parental health (proxied by skinfold, weight and height). *P*-values associated with the respective joint significance tests for the different blocks of additional control variables coming from separate regressions are shown at the bottom of Table 5. The location of residence is a significant predictor of individual housing damages. The joint significance test for birth month dummies is also significant, but with the exception of one birth month, none of the birth month dummies has a statistically significant impact on its own. None of the additional tests point to a relevant variable being omitted.

In a second step, we test whether our baseline results are sensitive to excluding or adding blocks of control variables. We focus on “highest completed grade” as a dependent variable; results for the remaining outcome variables can be obtained from the authors on request. Results are shown in Table 6. The first three columns display the results when the main blocks of core control variables are excluded, and columns four and five display the results when barangay and birth month dummies (i.e., the set of additional confounders found to significantly predict damages in the previous analysis) or the above mentioned housing, child and household characteristics are added to the set of control variables. The last column uses a

Table 6
Specification checks using “highest completed grade” as outcome variable.

	– House characteristics	*– Child characteristics*	*– Family characteristics*	*+ Barangay dummies*	*+ Birth month dummies*	*+ Additional controls*	*Continuous treatment*
1994	– 0.114*	– 0.151**	– 0.190***	– 0.117**	– 0.143**	– 0.142***	– 0.400**
	(0.054)	(0.054)	(0.051)	(0.051)	(0.053)	(0.049)	(0.156)
	[0.430]	[0.085]	[0.006]	[0.600]	[0.348]	[0.464]	–
1998	– 0.259**	– 0.283***	– 0.401***	– 0.270**	– 0.265**	– 0.297***	– 1.033***
	(0.096)	(0.096)	(0.094)	(0.103)	(0.107)	(0.108)	(0.341)
	[0.675]	[0.549]	[0.003]	[0.908]	[0.569]	[0.451]	–
2002	– 0.526***	– 0.604***	– 0.677***	– 0.509***	– 0.506***	– 0.516***	– 1.497***
	(0.103)	(0.093)	(0.103)	(0.112)	(0.136)	(0.112)	(0.407)
	[0.767]	[0.043]	[0.005]	[0.840]	[0.594]	[0.984]	–
2005	– 0.695***	– 0.757***	– 0.881***	– 0.650***	– 0.635***	– 0.644***	– 1.784***
	(0.162)	(0.174)	(0.215)	(0.180)	(0.186)	(0.202)	(0.484)
	[0.485]	[0.060]	[0.002]	[0.713]	[0.113]	[0.560]	–

Note: Each cell corresponds to the regression coefficient of the binary damage indicator resulting from a separate estimation using equation (1) for each outcome variable but leaving blocks of control variables out of the estimation (blocks are defined as in Table 1). The full set of control variables corresponds to the set of variables displayed in Table 1. The additional controls are house characteristics (type of water supply and distance from the next vehicular road), child characteristics (health conditions at age 2, such as fever, cough and measles), and family characteristics (parental health such as skinfold, weight and height). The results for the full specification are available upon request. Standard errors (in parentheses) are clustered by barangay.

* denote statistical significance at the 10% level of significance. Square brackets denote the *P*-values of the test for the equality of the regression coefficients for damages of the baseline model and the restricted model.

** denote statistical significance at the 5% level of significance. Square brackets denote the *P*-values of the test for the equality of the regression coefficients for damages of the baseline model and the restricted model.

*** denote statistical significance at the 1% level of significance. Square brackets denote the *P*-values of the test for the equality of the regression coefficients for damages of the baseline model and the restricted model.

Table 7
Balancing tests.

	Sample 1991 (raw)	Sample 1991	Sample 1994	Sample 1998	Sample 2002	Sample 2005
Preschool (binary)	– 0.137***	– 0.019	– 0.017	– 0.017	– 0.022	– 0.029
	(0.027)	(0.021)	(0.021)	(0.021)	(0.022)	(0.023)
	–	–	[1.000]	[0.603]	[0.499]	[0.033]
Enrolled in school (binary)	– 0.042*	0.010	0.010	0.011	0.013	0.015
	(0.024)	(0.021)	(0.021)	(0.022)	(0.022)	(0.023)
	–	–	[0.578]	[0.812]	[0.672]	[0.436]
Highest education (years) Sibling	– 0.286**	– 0.070	– 0.117	– 0.079	– 0.093	0.029
	(0.122)	(0.222)	(0.252)	(0.256)	(0.229)	(0.226)
	–	–	[0.462]	[0.901]	[0.736]	[0.183]
Height for age (month 2)	– 0.249***	– 0.026	– 0.057	– 0.053	– 0.021	– 0.026
	(0.049)	(0.044)	(0.047)	(0.050)	(0.053)	(0.052)
	–	–	[0.141]	[0.106]	[0.737]	[0.989]
Weight for age (month 2)	– 0.199***	– 0.027	– 0.043	– 0.038	– 0.033	– 0.034
	(0.062)	(0.053)	(0.059)	(0.061)	(0.057)	(0.058)
	–	–	[0.387]	[0.498]	[0.686]	[0.690]

Note: Each cell corresponds to the regression coefficient of the binary damage indicator resulting from a separate estimation using equation (1) for each outcome variable, controlling for the set of control variables shown in Table I.1 (with the exception of column I, which corresponds to the unconditional estimate). Standard errors (in parentheses) are clustered by barangay.

* denote statistical significance at the 10% level of significance. Square brackets denote the *P*-values of the test for the equality of the regression coefficients for damages of the baseline model and the restricted model.

** denote statistical significance at the 5% level of significance. Square brackets denote the *P*-values of the test for the equality of the regression coefficients for damages of the baseline model and the restricted model.

*** denote statistical significance at the 1% level of significance. Square brackets denote the *P*-values of the test for the equality of the regression coefficients for damages of the baseline model and the restricted model.

continuous indicator for damages (i.e., the ratio of self-reported repair costs to total initial wealth). Results generally maintain their sign and significance. Moreover, with a few exceptions (when dropping the family background characteristics) the results are not statistically different from the baseline results (the *P*-value from the respective test of equality between the two estimates is shown in brackets). In models where child characteristics or family background characteristics are excluded, estimated effects are somewhat higher than in the core specification (the increase amounts to at most 52% when family characteristics are excluded). Adding control variables does not significantly alter our baseline results. Using the continuous rather than the binary indicator of housing damages confirms our previous findings: housing damages have detrimental effects on children's education, both in the short run and in the long run: at age 14/15, children who experienced damages that corresponded to a reduction in wealth by 10% already lag behind by 0.04 years, and at age 21/22, when schooling should be completed, this gap amounts to 0.18 years. In the case of full wealth destruction, the final gap corresponds to almost two years.

We also perform several balancing (placebo) tests with respect to pre-disaster outcomes (Table 7). Pre-disaster outcomes inherently cannot be influenced by later damages; any significant impact on pre-disaster outcomes thus indicates a violation of the conditional independence assumption. Note that we observe substantial raw differences between affected and unaffected children with respect to pre-disaster outcomes (see Table 7, Column I). These differences, however, vanish once the core set of control variables is taken into account (Table 7, Column II). Hence, our control variables help to balance our sample.

Table 7 provides furthermore evidence regarding the random attrition assumption. Balancing tests in the series of pre-disaster outcomes using the subsamples available in the different post-disaster survey years and thus using children still included in the surveys from 1994, 1998, 2002, and 2005, respectively, helps us to assess whether attrition unbalances the sample when the core set of control variables is taken into account. *P*-values of the test of equality between the estimate and the estimate from the respective baseline regression are shown in brackets. As we can see in Table 7, Columns three through five, the resulting coefficients do not significantly alter across the subsamples remaining over the different survey years. Therefore, cumulative attrition over the survey years does not seem to invalidate our baseline results.

Table 8
Nature of the shock.

	Survey year				
Dependent variables	1991	1994	1998	2002	2005
Financial shock					
Asset index	-0.510^{***}	-0.531^{***}	-0.364^{***}	-0.545^{***}	-0.472^{***}
	0.074	0.098	0.105	0.126	0.158
	[0.002]	[0.002]	[0.002]	[0.002]	[0.004]
Wealth (log)	-0.103^{*}	-0.132^{*}	-0.147^{**}	-0.201^{***}	-0.136^{*}
	0.053	0.073	0.058	0.059	0.068
	[0.080]	[0.098]	[0.010]	[0.002]	[0.044]
Household income (log)	0.013	0.004	−0.035	−0.070	−0.083
	0.050	0.060	0.025	0.070	0.071
	[0.865]	[0.951]	[0.156]	[0.306]	[0.258]
Parental activity					
Employment father (binary)	0.016	0.015	0.003	0.040^{*}	0.020
	0.015	0.015	0.012	0.021	0.028
	[0.330]	[0.378]	[0.779]	[0.080]	[0.448]
Employment mother (binary)	−0.016	0.018	0.005	−0.007	0.029
	0.019	0.032	0.023	0.025	0.022
	[0.412]	[0.509]	[0.813]	[0.817]	[0.168]
Parental health					
Mother ill (binary)	−0.021	0.011	0.004	−0.015	−0.006
	0.017	0.021	0.018	0.026	0.006
	[0.228]	[0.623]	[0.847]	[0.549]	[0.314]
Mother died (binary)	-0.006^{*}	−0.012	−0.020	−0.019	−0.015
	0.006	0.017	0.018	0.020	0.010
	[0.328]	[0.428]	[0.259]	[0.371]	[0.144]
Household composition					
Change of residence (binary)	-0.019^{**}	−0.017	0.011	−0.023	0.002
	0.007	0.018	0.013	0.024	0.022
	[0.014]	[0.364]	[0.376]	[0.366]	[0.961]
Family separation (binary)	0.009	0.008	−0.002	0.022	0.010
	0.011	0.013	0.012	0.016	0.020
	[0.418]	[0.637]	[0.853]	[0.132]	[0.583]
Household member absent (binary)	0.013	0.032^{**}	0.015	0.015	−0.026
	0.014	0.011	0.016	0.018	0.018
	[0.350]	[0.006]	[0.290]	[0.382]	[0.166]

Note: Each cell corresponds to the regression coefficient for the binary damage indicator using equation (1) and controlling for the set of variables displayed in Table A.4. The results for the full specification are available upon request. Standard errors (in parentheses) are clustered by barangay.

* denote statistical significance zero at the 10% level of significance. *P*-values from wild clustered bootstraps (999 replications) are provided in square brackets.

** denote statistical significance zero at the 5% level of significance. *P*-values from wild clustered bootstraps (999 replications) are provided in square brackets.

** denote statistical significance zero at the 1% level of significance. *P*-values from wild clustered bootstraps (999 replications) are provided in square brackets.

5.3. *Nature of the shock*

One obvious question to ask is what kind of shock housing damages due to a super typhoon represent. Do they only represent the visible consequences for families' real estate, or do they trigger any further channels that may harm children's development directly?

Our prior assumption is that housing damages mainly represent a wealth shock. And indeed, there is an immediate and significant drop in reported wealth among families whose homes were damaged by typhoon Mike (see Table 8). On average, the wealth loss amounts to 10.1 pp. The estimates suggest that the wealth gap is persistent, or, if anything, widens over the course of the following years – in 2002, the estimated gap amounted to 19.6 pp. Results based on the asset index (a more stable measure, because weights are chosen to minimize projection residuals) support the finding that housing damages have a persistent effect on wealth.

Besides the impact on families' wealth, there is little evidence that other idiosyncratic shocks (such as income loss, maternal health, maternal mortality, migration, family separation, or temporary absence of a family member) are associated

Table 9
Subgroup analysis using "highest grade completed" as outcome variable.

	Survey year			
Strata	1994	1998	2002	2005
Panel A: Gender				
Male	−0.076	−0.202	−0.505**	−0.729**
	0.077	0.148	0.184	0.309
Female	−0.195**	−0.358**	−0.557***	−0.618***
	0.081	0.144	0.141	0.204
	[0.294]	[0.465]	[0.842]	[0.750]
Panel B: Nr. of elderly siblings				
No or one sibling	−0.189***	−0.304**	−0.570***	−0.757**
	0.069	0.122	0.140	0.303
More than one sibling	−0.075	−0.234	−0.404**	−0.509*
	0.094	0.151	0.173	0.253
	[0.377]	[0.729]	[0.498]	[0.513]
Panel C: Family wealth				
Poor (total wealth < 862 $)	−0.147**	−0.421*	−0.802***	−0.885***
	0.063	0.198	0.237	0.301
Rich (total wealth > 862 $)	−0.126	−0.218*	−0.379**	−0.583*
	0.097	0.108	0.139	0.313
	[0.867]	[0.390]	[0.174]	[0.533]
Panel D: Home ownership				
No homeowner	−0.084	0.029	−0.244	−0.639
	0.095	0.206	0.228	0.403
Homeowner	−0.149**	−0.343***	−0.571***	−0.690***
	0.055	0.110	0.118	0.207
	[0.342]	[0.273]	[0.369]	[0.806]
Panel E: Household type				
One-nuclear HH	−0.153*	−0.368**	−0.536***	−0.497*
	0.070	0.136	0.136	0.219
Multi-nuclear HH	−0.118	−0.053	−0.404**	−0.704*
	0.092	0.196	0.198	0.384
	[0.756]	[0.218]	[0.603]	[0.638]
Panel F: Origin of the family				
Parents born in barangay	−0.053	−0.221**	−0.476***	−0.505***
	0.062	0.095	0.091	0.159
No parent born in barang.	−0.280***	−0.247	−0.518**	−0.911**
	0.086	0.202	0.248	0.426
	[0.026]	[0.944]	[0.902]	[0.334]

Note: The table reports effects of the binary damage indicator on highest grade completed for each group separately. Control variables are the variables shown in Table I.5. The results for the full specification are available upon request. Standard errors (in parentheses) are clustered by barangay.

* denote statistical significance at the 10% level and are based on *P*-values resulting from wild bootstraps (999 replications) clustered at the barangay level. *P*-values for the test of equality of coefficients between the respective strata are provided in square brackets.

** denote statistical significance at the 5% levels and are based on *P*-values resulting from wild bootstraps (999 replications) clustered at the barangay level. *P*-values for the test of equality of coefficients between the respective strata are provided in square brackets.

*** denote statistical significance at the 1% level and are based on *P*-values resulting from wild bootstraps (999 replications) clustered at the barangay level. *P*-values for the test of equality of coefficients between the respective strata are provided in square brackets.

with housing damages. Yet, despite the richness of our data, we cannot exclude the possibility that housing damages relate to other unobserved channels that may be detrimental for children.

Our prime concern is related to evacuations. Unfortunately, our dataset does not contain any detailed information about the duration of people's stay in emergency lodgings, which are mostly located in schoolhouses or evacuation centers. However, evacuation camps usually operate for only a few weeks, and thus we expect their long-term impact to be small. Moreover, we do not find any empirical evidence for relocation. If anything, our results suggest that affected families are more likely to stay in the same area.

In addition, there is the possibility that children who already suffer from idiosyncratic shocks are more vulnerable to the consequences of common threats. The negative impact of housing damages on child development outcomes may thus capture a direct effect running through damages to individual property and an indirect effect driven by a decreased ability to buffer the negative consequences of common threats. Therefore, our results should not be interpreted as consequences of a pure shock to individual property (in which case one could have used these damages as an instrumental variable for individual wealth) but rather as predictors of inequalities that arise due to variation in intensity of exposure to a natural disaster.

5.4. *Effect heterogeneities*

To better identify the most vulnerable populations, we stratify our sample according to different background characteristics (individual characteristics such as gender and number of older siblings as well as family characteristics such as wealth, home ownership, type of household, and origin of the family, all taken from pre-disaster surveys) and test whether estimated effects differ among these strata. The *P*-value for the respective hypothesis test is shown in brackets. For the purpose of illustration, Table 9 presents results for the main education outcome (highest grade completed). Estimated results for all remaining outcomes can be obtained from the authors upon request. Overall, the differences in the effects across strata are mostly insignificant on any conventional significance level. The differences are thus of a suggestive nature only.

Our results indicate that the short- and medium-run effects are more pronounced for girls. In the long run, however, initial gender differences vanish, and effects are more significant for boys. Those with no or only one older sibling experience the greatest consequences. This suggests that older siblings are more likely to leave school in order to support the family. Notice that these findings are in line with the findings by Antilla-Hughes and Hsiang (2013), who show that increased infant mortality in the immediate years after natural disasters is concentrated among girls and families with numerous children.

Stratification with respect to families' initial wealth reveals that negative effects on education tend to be stronger for children living in poor households (with initial wealth below the median of $862): at the age of 22, the gap in educational attainment amounts to almost one year, an effect which is almost double that among children in rich families. This evidence points to the role of credit constraints in buffering the impacts of a shock to families' wealth on investments into children's human capital (see also Burgess et al. (2014) for arguments along these lines). Not surprisingly, the effect is stronger for children whose families own their home. In the case of families who rent their home, the majority of damages do not occur to their own property (except their furniture and household appliances). Hence, the liability for reconstructing the building is not their responsibility.

Finally, we look at the importance of informal insurance devices due to social networks. (Foster and Rosenzweig, 2001; Rosenzweig, 1989; Zylberberg, 2012). We distinguish between families who prior to the disaster lived in the barangay where at least one of the parents was born and families where none of the parents lived in their barangay of origin as well as between single-nuclear and multi-nuclear households. Highlighting the importance of social (family) ties, effects on educational achievement tend to be most severe among children who live in single-nuclear households as well as among children who live in a neighborhood where none of their parents was born.

6. Conclusion

This study analyzes the short- and long-term effects of a typhoon on children's health and education. The particular question of this study is whether variation in the intensity of idiosyncratic damages (proxied by damages to families' homes) leads to persistent inequalities among the affected population.

Our results reveal negative effects on children's education, which are exacerbated over time. Effects are most severe for girls, children with no older siblings, children from poor families, and families with no strong family or social network. However, we do not find any effect on children's health. The underlying mechanism is probably a shift of parental investments away from children's education. The relatively small reduction of expenditures for nutrition, the possibility of substituting cheaper proteins for more expensive ones to keep nutritional intake constant, and the provision of disaster relief probably explain the absence of any negative effects on children's health.

Our results are in line with the human capital production theory, which postulates that an adverse shock during early life causes an immediate gap in human capital that widens over time. However, the absence of any effect on children's health outcomes contrasts with the prediction of common theories postulating that inequalities in one type of human capital translate into inequalities in other types of human capital.

Our results have important policy implications: first of all, one preventive measure to avoid housing damages due to natural disasters is to strengthen regulations on construction and the geographical development of residential areas. Second, in addition to the usual provision of disaster aid, there should be further financial help targeting the most affected and most vulnerable populations. Particularly in developing countries where formal insurance coverage for natural disasters does not exist and credit constraints prevent access to capital markets, it is crucial to go beyond the common public health interventions. Financial disaster relief may be crucial, potentially even tied to children's continuous participation in education, to prevent negative consequences for children's intellectual development and aggravation of existing inequalities over time. The evidence for more severe effects among children from families without access to informal networks of insurance points towards the need that disaster aid should be targeted primarily to these families.

Appendix A. Supplementary material

Supplementary data associated with this article can be found in the online version at http://dx.doi.org/10.1016/j.euroecorev.2015.09.004.

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