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Flood Exposure and Child Health in Bangladesh

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INTRODUCTION

In the summer of 1998, Bangladesh was inundated by significant flooding that covered two-thirds of the country and affected more than 30 million people. Although annual flooding is normal and expected in Bangladesh, the 1998 floods caused extraordinary devastation and were considered a "century" flood. Homestead flooding, crop loss, and infrastructure damage compromised household food security and increased disease prevalence in a population with already high rates of poverty and malnutrition.

Unfortunately, this type of scenario has become increasingly common around the world: a significant crisis—whether environmental, economic, or political—devastates a large population of densely-settled households who are already trapped in chronic poverty. How do households anticipate and respond to such crises in the context of ongoing livelihood struggles? Do shocks affect investments in human capital? More specifically, what happens to children in the wake of such shocks? In this paper I use longitudinal data from the post-flood period in rural Bangladesh to examine how children's human capital, as measured by nutritional status, responds to severe flooding and its aftermath. I emphasize the importance of analyzing these responses in a dynamic context, linking exposure to shocks and nutritional outcomes to longer-term measures of household vulnerability and resilience.

I pose two related research questions. First, did flood exposure in 1998 cause marginal growth faltering in children? To isolate the effects of the flood and address the endogeneity of flood exposure, I use a difference-in-difference estimator and village fixed effects. I also exploit the fact that younger children are more vulnerable than older children to nutrition shocks. I next ask whether the effects of flooding on child growth faltering were mediated by household resources, hypothesizing that households with lower levels of pre-flood resources are less able to protect children from nutrition shocks.

These analysis help to answer several important policy questions related to crisis and recovery in vulnerable populations. The results reveal the extent to which children were nutritionally compromised by the flood, and which children fared worst. The analyses also contribute specifically to the design and implementation of livelihood interventions, and relief and recovery efforts. Can households use physical, financial and human capital to protect children's nutritional status from significant shocks to income and food security? If so, is it more effective to focus on long-term asset-building strategies in vulnerable populations, or to facilitate asset recovery post-shock through access to credit and other forms of relief? Given the increasing exposure to shocks and the quantity of resources allocated post-disaster to relief and recovery, these questions are not trivial.

CHILD GROWTH AND GROWTH FALTERING

Infants and young children grow rapidly from birth through age three or four. Nutrition and disease during this stage largely determines the proportion of genetic growth potential that will be achieved by age three (Martorell, 1995, 1999; Martorell & Ho, 1984). Stature achieved by age three is in turn associated with important human capital outcomes, including physical and mental development, school performance, labor productivity, and wages (Alderman, Hoddinott, & Kinsey, 2002; Behrman, 1996;

Grantham-McGregor, Fernald, & Sethurman, 1999; Grantham-McGregor, Walker, Chang, & Powell, 1997; Thomas & Strauss, 1997). This suggests that nutritional insults suffered before age three can have lasting consequences throughout life.

Nutritional vulnerability in the preschool years results from several factors. First, children require more food relative to their weight than older children and adults in order to maintain rapid growth at this stage (Martorell & Habicht, 1986). Immature immune systems leave children vulnerable to infections that can both lead to and exacerbate inadequate dietary intake (Chen, 1983; Scrimshaw, Taylor, & Gordon, 1968). The transition from breastmilk to table foods, usually occurring between 12 and 24 months, also makes toddlers vulnerable to malnutrition at a time when they are still wholly dependent on caregivers for feeding (UNICEF, 1990).

Nutrition shocks, therefore, most often take the form of inadequate dietary intake, severe or prolonged episodes of diarrheal and other diseases, or both. Abrupt reductions in dietary intake may be the result of child-specific factors such as the arrival of a new sibling or the death of a caregiver; of household-specific factors such as crop failure, business loss, eviction, or illness of a primary wage earner; or more macro factors like drought or conflict that may create widespread disruptions in food security. Similarly, diarrheal and infectious disease episodes that contribute to nutrition shocks can be child- , household-, or community-level events.

Growth Trajectories in Bangladesh

Unfortunately, both chronic and acute spells of inadequate dietary intake and infectious diseases are regular occurrences for many children in the developing world, resulting in substantial growth faltering. Growth faltering can be measured by tracking standardized anthropometric measures over time. For example, standardized height-for-age z-scores reflect the deviation from the growth standard for well-nourished children of the same age and sex (Kuczmarski, Ogden, & Guo, 2002).

Across Asia, children are born near the growth reference standard for height-for-age. However, height-for-age z-scores quickly fall from birth to 24 months, and then plateau or continue to fall more slowly (Shrimpton et al., 2001). The situation is particularly dire in Bangladesh where more than half of all of rural children aged two to six are stunted (Bangladesh Nutritional Surveillance Project, 2002). Stunting, defined as a height-for-age z-score of -2.0 or lower, is a pronounced slowing of skeletal growth and stature. Stunting typically results from chronic undernutrition but acute nutrition shocks can also permanently affect growth trajectories.

Estimated stunting rates for rural children in Bangladesh by age and over time are shown in figure Figure 1. These estimates (results not shown) are calculated from pooled observations from 32 different region and national anthropometry surveys in Bangladesh from 1982 through 2003 (World Health Organization, 2005). Figure 1 captures two important features of height-for-age trajectories in Bangladesh. First, the steep increase in stunting rates from birth through age two is clear, after which stunting rates improve a bit and then level off. Second, stunting rates in Bangladesh have improved considerably over time. In these estimates, stunting rates improve .74 percentage points each year. This implies a small but significant positive time trend in height-for-age z-scores.

Shocks, Resilience, and Household Coping Responses

How do households respond to a shock large enough to potentially compromise the nutritional status of household members? A rich literature on risk in developing countries (Alderman & Paxson, 1992; Besley, 1995; Cain, 1981; Cox & Jimenez, 1998; Morduch, 1995; Rosenzweig, 1988; Rosenzweig & Wolpin, 1993; Townsend, 1994, 1995; Udry, 1993) suggests that some households attempt to smooth either consumption or assets (or more generally, smooth utility or welfare) across time and across space in the wake of shocks to income. *Ex ante*, households can adopt risk management strategies including income smoothing and diversification, investment in formal and informal insurance arrangements, and asset accumulation, a key form of self-insurance (Alderman & Paxson, 1992; Deaton, 1991; Dercon, 2005). *Ex post*, coping strategies to smooth consumption can include spending down savings, selling assets, borrowing money, and relying on transfers from familial networks or governments (Dercon, 2005; Siegel & Alwang, 1999). Households can also forgo or delay consumption of some items to maintain spending on essential staples like food and shelter (Frankenberg, Smith, & Thomas, 2003).

For many households in developing countries, however, the menu of risk management and coping strategies is constrained by access to well-functioning markets, including commodity, credit and insurance markets. Urban households may lack strong kin or social network ties that facilitate informal insurance arrangement. This leaves assets as a critical and flexible tool for welfare smoothing and risk management. The use of assets to manage risk and smooth welfare is context-dependent and varies by wealth levels (Carter, Little, Mogues, & Negatu, 2004). For example, distress sales of jewelry or land will depend on liquidity and prices (Frankenberg et al., 2003). Similarly, livestock may function as a consumption asset to be sold (Rosenzweig & Wolpin, 1993), but may also be an important (and lumpy) productive asset that a poor household will protect at the expense of other assets or consumption (Carter, 1997; Carter et al., 2004; Deaton, 1991).

Several studies in the past decade have documented the effects of shocks on children's human capital. Foster (1995) provides evidence that children were vulnerable to weight loss after the 1988 floods in Bangladesh, particularly in landless households that had no access to credit. Hoddinott and Kinsey (2001) show that children who were 12-24 months old during a severe drought in Zimbabwe were shorter four years later than children of the same age who had not been exposed to the drought. Deolalikar (2004) finds a significant association between the likelihood of being stunted and recent village-level flooding among children in Bangladesh. The effect of village-level flooding on the odds of being stunted is twice as large for children in the lowest wealth quintile relative to the full sample.

THE BANGLADESH CONTEXT

Bangladesh is a small, densely-populated country situated on the Bay of Bengal in South Asia. Three large river systems run from India, China, Nepal and Bhutan through Bangladesh to empty into the Bay of Bengal: the Ganges, the Brahmaputra, and the Meghna. During the monsoon season in July, August, and September, the rivers reach their peak flows and normally overflow their banks, inundating large parts of the country. This annual flooding cycle is an expected and important event. The floods irrigate the main *aman* monsoon rice crop and improve soil fertility in flood plain areas.

Certain factors, both natural and anthropogenic, can contribute to more severe flooding. Tectonic shifts, heavy snowfalls in the mountains, and cyclones can amplify the annual flooding. When the three rivers reach their peak flows at the same time (which happened five times between 1954 and 1998) flooding is considerably more severe. Changes in land use and land cover have reduced the absorptive capacity of the flood plains, while major flood control structures like embankments and levies can direct massive amounts of flood water towards vulnerable areas. While the evidence is still inconclusive, it does appear that global climate change may be contributing to more frequent and severe floods in the Bay of Bengal and elsewhere (Few, Ahern, Matthiers, & Kovats, 2004). Meanwhile, rapid population growth in Bangladesh in the last half-century has meant that floods affect more people and damage more property. Unstable *char* (floodplain) lands are now settled by an ever-changing population of poor landless households. Industry and agriculture also compete for limited land area.

The health impacts of severe flooding can be enormous (Few et al., 2004). Immediate risks include drowning and injuries from water-borne debris. In the aftermath of flooding, contaminated water and other vectors contribute to elevated rates of diarrheal diseases, respiratory infections, and skin infections. Access to food can be interrupted due to crop loss, road damage, and price swings. Households can suffer damage to housing and other productive assets, disrupting livelihoods. Access to health services may be compromised if health infrastructure is damaged.

The 1998 Floods

The 1998 flood season in Bangladesh was extraordinary in many ways. Several excellent and detailed accounts of the flooding are available elsewhere (see, for example, Beck, 2005; Del Ninno, Dorosh, Smith, & Roy, 2001; Few et al., 2004) but a few key points are worth highlighting here. The flood waters starting rising in early July as usual, but by late July heavy flows in all three river basins led to inundation of 30 percent of the country. By the end of the August, this figure was 41 percent. Flooding peaked in September with 51 percent of the country inundated. Both the coverage and duration of the 1998 floods exceeded the most recent severe flooding of 1988 by considerable margins: 100,250 square kilometers inundated in 1998 vs. 89,970 in 1988; and an average of 59 days of water above danger level in the river basins in 1998 vs. 34 in 1988. Peak flood levels in the two years were similar at over 11 meters (Del Ninno et al., 2001).

Flood damage in 1998 was commensurately severe as well. Almost 1,000 people were killed, 980,000 homes were affected, and more than one million people were displaced. In all, 30 million people were directly affected by the floods. More than two million tons of rice crops were lost, 15,000 kilometers of roads were damaged, and 26,000 cattle were lost.

Del Ninno and colleagues have produced a thorough account of the 1998 flood at the household level based on the same dataset used in this study (Del Ninno et al., 2001). Cross-sectional results from the first survey round indicate that more than half of households lost assets in the flood and almost half suffered housing damage. Employment for day laborers declined abruptly after the floods. A significant portion of households became food insecure after the floods, and diarrheal and respiratory diseases were common. To cope with the floods many household took on debt or purchased food on credit, and many households relied on food aid and cash transfers from the government and from local and national NGOs.

In a separate study, Del Ninno and Lundberg (2005) address similar questions to the ones I pose here. Specifically, they seek to demonstrate that the flooding caused growth faltering and that children who faltered post-flood experienced no catch-up growth. Using a sample of children less than five years old at the end of 1999, they find no evidence of catch-up growth by estimating the coefficient on a lagged height-for-age term in a model predicting the change in height-for-age across survey rounds. In other words, flood exposed children do not grow more quickly after the flood than unexposed children, suggesting that they do not recover their pre-flood growth trajecorties. Del Ninno and Lundberg also investigate the role that food aid played in preserving child nutritional status, determining that post-flood interventions helped little if at all.

DATA

The data for this study are drawn from the Coping Strategies in Bangladesh survey, a longitudinal panel survey fielded by the International Food Policy Research Institute (IFPRI) in partnership with the Food Management and Research Support Project of the Bangladesh Ministry of Food. The goal of the survey was to assess household and community responses to the severe flooding in Bangladesh in summer 1998. Complete details of the survey design and sampling are provided in Del Ninno et al. (2001). The sampling design sought to represent portions of the country affected by the floods. In the first stage, seven flood-affected *thanas* (subdistricts) were selected to provide a range of both flood exposure and poverty levels: two nonpoor severely-affected thanas, two poor severely-affected thanas, one nonpoor moderately-affected thana, and two poor moderately-affected thanas. Within these categories, specific *thanas* were chosen that had already been included in other IFPRI studies and that would provide adequate coverage across the country's administrative regions.

Within six of the seven selected thanas, three unions were randomly selected. Unions are intermediate administrative structures between thanas and villages, with an average population of around 27,000 (Commonwealth Local Government Forum, 2005). Within each union, six villages were randomly selected with probability proportional to village size. Two clusters were then randomly selected from each villages (using preassigned random numbers), and three households within each cluster were chosen from a complete cluster census. In Saturia thana, a random sample drawn from another IFPRI study was used. The resulting sample includes 757 households in 126 villages, from 21 unions in seven thanas, drawn without replacement. In the second round of the survey seven households refused to be interviewed or were absent at the time of interview. In the third round, 23 households refused to be interviewed or were absent.

The survey was fielded in three waves: Round 1 in November-December 1998, round 2 in March-April 1999, and round 3 in November 1999. The household questionnaire included modules on flood exposure, individual employment and other income sources, individual borrowing, household expenditures and assets, receipt of transfers, and allocation of food to individuals in the day prior to the survey. Morbidity for all household members in the past two weeks was reported, and anthropometric data for all women and for children under 12 years old were recorded. Community information was collected at three levels: village, union, and thana. Village data for rounds 1 and 3 include local wages, prices, cropping, and NGO and food distribution activity. Union data (collected in all rounds) includes demographics, flood exposure, infrastructure, prices, and program interventions at the union level. Thana data includes historical agricultural production and thana-level program interventions.

For this study I define two analytic subsamples. The first subsample consists of children born in 1990 or later for whom I have complete age, height, household flood exposure, and household vulnerability measures (described below) for both the first and third survey rounds. This sample includes 757 children from 438 households[1]. Descriptive statistics for this sample are provided in Table 1. More than three-quarters of the children live in households that were directly exposed to flooding. The average height-for-age z-score in the sample is -2.20 in round 1 and -2.25 in round 3. More than

[1] Note that the number of children in this subsample (757) is only coincidentally the same as the number of households in the full survey sample.

half of the children live in households in which the household head has no formal education.

The second subsample consists of two cohorts of children: those born in 1997, and those born in 1998. I use data from survey round 1 (November-December 1998) for the 1997 cohort, and data from survey round 3 for the 1998 cohort (November 1999), creating a pooled sample of children measured at ages 12-24 months. This sample includes 140 children; descriptive statistics are provided in Table 2.

The outcome variable of interest in these analyses is child nutritional status. I operationalize nutritional status using height-for-age, a well-established anthropometric indicator of nutritional well-being. Height-for-age can be used both to track an individual's linear growth trajectory and as an index of the nutritional status of a population (Gibson, 1990). Height-for-age measures can be easily compared across children of different ages and across populations by use of standardized z-scores, which use a well-nourished population of children as the reference (Kuczmarski et al., 2002). The height-for-age z-score indicates by how much a child deviates from this reference population. A height-for-age z-score of -2.0 implies that the child is two standard deviations below the median of the reference population. Children with z-scores of -2.0 or lower are considered stunted, suggesting chronic malnutrition. In the Coping Strategies of Bangladesh dataset, z-scores were calculated by IFPRI from reported height and age data. Due to a large number of missing and inconsistent values, I recalculated z-scores using the "zanthro" command in Stata (Version 8.0) and I use these recalculated z-scored in this analysis.

The predictor variables in these analyses are household flood exposure and five measures of household vulnerability. IFPRI constructed two measures of household flood exposure (Del Ninno et al., 2001). The first is a summed index of three measures of flood exposure: depth of water on the homestead, depth of water in the home, and number of days of water in the home. This yields a score ranging from 0 to 16. This index was also aggregated into a categorical measure of flood exposure, defined as no exposure (index score of zero), moderate exposure (index score 1-5), severe exposure (6-10), and very severe exposure (11-16). Following Del Ninno and Lundberg (2005), I use a dichotomous measure of flood exposure, equal to 0 if the household had no exposure and equal to 1 if the household was moderately, severely, or very severely exposed.

I hypothesize in this study that the effect of flood exposure on child nutritional status depends on the household's pre-flood level of vulnerability. Here I operationalize vulnerability in five dichotomous variables, chosen both for their prominence in the literature on vulnerability and for their availability as pre-flood measures in the Coping Strategies in Bangladesh dataset. Households are considered more vulnerable if the household head lacks formal education, the household owns no farmland, the household head is younger than 30 years old, the spouse of the household head (or female household head) is shorter than 146.6 centimeters (the 25^{th} percentile for this population), and the walls of the house are made of earth, bamboo, or leaves (rather than concrete, tin or jute straw). These five measures are summed (unweighted) into a vulnerability index, and households with a total score of two or more are deemed vulnerable. In the first subsample of children, this cutoff divides the sample in half, with 49 percent of children living in households with vulnerability scores greater than one.

The third important control variable is age/birth cohort. Age in years and months is reported in all three survey rounds. However, the months measure is missing for many children, particularly at older ages. This has implications for height-for-age z-scores which are calculated based on the child's age in months. In addition, consistency in age reporting across survey rounds is only moderately high (see Bairagi, Aziz, Chowdhury, & Edmonston, 1982; Bairagi, Edmonston, & Hye, 1991; and Bairagi, Edmonston, & Khan, 1987 for helpful discussions of age misstatement problems in Bangladesh). To address these problems, I chose to add six months to the age in completed years for children missing age in months.[2] Where ages across survey rounds were inconsistent, I assumed that age in round 1 was the most accurate data point. Month of birth was then calculated by subtracting age in months from the month of the survey, and birth cohorts were then assigned based on month of birth.

Some discussion of attrition and missingness is warranted here. The full sample includes 1,168 children born in 1990 or later. This means that only 65 percent of the children are represented the analytic first subsample of 757. Forty-one children are excluded because their household either refused reinterview in round 3 or could not be located. Eighty-seven of the children are in the 1999 birth cohort and so are not yet born in 1998. Only three children interviewed in round 1 are reported to have died by round 3,

[2] Other options considered but not yet tested include using age in completed years for all children, using age in completed years only for children with missing age in months, and adding a random number of months to age in completed years drawn from a distribution with, e.g., a mean of zero months and a standard deviation of two months. Evidence from Bairagi (Bairagi et al., 1982; Bairagi et al., 1991) suggests that the most appropriate solution may vary by age of child.

although it could be the case that other children whose data are missing by round 3 have also died. (In addition, the sample may be biased by the absence of children who died between the floods and round 1 of the survey).

The majority of excluded cases are due, therefore, to missing height measurement in one or both survey rounds. The anthropometry module recorded the reason why children were not measured, and these reasons vary substantially by age. Many of the school-aged children are recorded "absent." For example, 54 of the 626 children born 1990-1994 were marked absent during anthropometry in round 1. In the younger cohorts, children are more likely to be reported as sick or refusing measurement. A small proportion of children who were measured fell below the height-for-age z-score cutoff of -6, and another small group had weight but not height data. In multivariate analysis stratified by survey round (results not shown), a missing height-for-age z-score is significantly associated with age and with being a student, but not with household flood exposure nor with any of the household vulnerability measures. Both household-level attrition and missingess of height-for-age z-score are clustered within Derai thana, suggesting perhaps less rigorous survey work in this area (Derai is a poor but only moderately flood-affected thana).

METHODS

In this paper I seek to identify a causal link between 1998 flood exposure and growth faltering among children. I also test the hypothesis that children in households with fewer pre-flood resources faltered more than children in more resilient households.

My measure of growth faltering is a decline in height-for-age z-score that exceeds the typical age-specific height-for-age z-score decline for rural children in Bangladesh.

My analysis must account for the fact that household flood exposure is likely to be correlated with unobserved household characteristics that are also associated with poor nutritional status. For example, if poorer households are more likely to live on marginal lands that are vulnerable to flooding, and also more likely to have stunted children, then estimates of the effects of flood exposure on nutritional status will be biased if household wealth is not observed or is measured with considerable error. While I have attempted to control for some of these characteristics through the household vulnerability measures, it is likely that I have not controlled for all of them. In their analysis Del Ninno and colleagues contend that flood exposure can be considered an exogenous shock, correlated neither with community wealth nor with household landholdings (Del Ninno et al., 2001; Del Ninno & Lundberg, 2005). I take a different approach here.

Within-Cohort Effects

To assess the impact of the floods on growth trajectories I exploit the age difference in vulnerability to nutrition shocks with a difference-in-difference approach. I compare the difference in height-for-age z-scores (HAZ) between exposed and unexposed children in November-December 1998 to the difference in HAZ between exposed and unexposed children in November 1999 for each of four birth cohorts of children: those born in 1990-4, 1995-6, 1997, and 1998. Table 3 identifies the cohorts and shows the age of each cohort at the onset of the 1998 flood period, at survey round 1, and at survey round 3. The difference equation is:

$$\text{Difference-in-Difference}_{ij} = [HAZ_{ij}{}^{E}{}_{3} - HAZ_{ij}{}^{U}{}_{3}] - [HAZ_{ij}{}^{E}{}_{1} - HAZ_{ij}{}^{U}{}_{1}]$$

where *i* indexes child, *j* indexes cohort, E and U denote exposed and unexposed children respectively, 3 indicates survey round 3 and 1 indicates survey round 1.

For the oldest birth cohort (children born prior to 1995), I expect a minimal difference-in-difference. That is, even if flood-exposed children are shorter than unexposed children at both points in time (due to observed and unobserved differences in resources), the *gap* in HAZ between exposed and unexposed children should not grow much wider between 1998 and 1999 because children at this age are less vulnerable to a nutrition shock. For the 1995-6 and particularly for the 1997 cohorts, however, who experienced the 1998 floods during a more nutritionally vulnerable time, I expect the difference-in-difference to be larger and significant if the flood itself negatively affected linear growth.

The effect of flooding on the 1998 cohort could go in either direction. If the floods caused only short-term declines in food availability and increases in disease prevalence, then this cohort should have been reasonably well-protected *in utero* and as small infants who were most likely exclusively breastfed. If, on the other hand, households were slow to recover their health and food security, then the exposed children in this cohort may show significantly more faltering by November 1999 relative to unexposed children.

To test the hypothesis that the effects of flood vary by pre-flood resources, I then stratify the analysis by the dichotomous measure of household vulnerability. Again, I expect to see little difference at older ages, but I expect that for the 1997 and 1998 cohorts, household vulnerability will amplify the effects of flooding on growth faltering.

Note that this analysis rests on a crucial assumption: I use height-for-age z-score from round 1 (measured in November-December 1998) as a *pre-flood* measure of nutritional status. That is, I use the difference in height-for-age z-scores from 1998 to 1999 identify the impact of the flood, even though both height-for-age z-scores are measured post-flood. Here again I follow Del Ninno and Lundberg (2005) in assuming that height-for-age is slow enough to respond to acute nutrition shocks that I can use the height-for-age z-score as a pre-flood measure. For the older cohorts, this appears to be a reasonable assumption. For example, a six-year-old boy with a constant z-score of -2.0 (typical in this sample) would only be expected to grow 2.9 centimeters in six months (the maximum lag time between the onset of flooding and the round 1 measurement) (Kuczmarski et al., 2002). However, a six-month-old girl with a constant z-score of -1.0 would be expected to grow 8.2 centimeters in the same six months. Even if the child faltered from a z-score -1.0 to -1.5 over those six months (as would be typical in this population), there would still be an expected height gain of 6.7 centimeters. This suggests that a different analysis is warranted for the youngest cohort, because the November-December 1998 z-score is likely to already incorporate the direct effects of the flood period.

Cross-Cohort Effects

In a second cross-cohort analysis, then, I attempt to parse the difference in growth trajectories between flood exposed and unexposed children and between vulnerable and less vulnerable households in a different way. I first compare HAZ scores for exposed vs. unexposed children in the 1998 birth cohort, those who were less than one year old in November-December 1998. Because of rapid growth at this age I assume that HAZ scores already incorporate some effect of the floods. While I expect to see a cross-sectional difference in z-scores between these two groups, I cannot consider this to be the correct estimate of the flood's impact due to the endogeneity of flood exposure with respect to child growth. In this case, the observed difference is likely to be greater than the true effect of the flood.

However, I have another available cohort for comparison: the 1999 birth cohort (or those children age 0-11 months in November 1999). Because these children were not directly exposed to the 1998 flood, none of the difference in HAZ scores (as measured in 1999) between exposed and unexposed children can be attributed to the immediate impact of the flood. Note also that the 1999 cohort is measured at the same age (in 1999) as the 1998 cohort is measured in 1999: between 0 and 11 months. Therefore, I subtract this difference in HAZ scores between exposed and unexposed children for the 1999 cohort from the difference in HAZ scores between exposed and unexposed children for the 1998 cohort (as measured in 1998), to obtain the immediate effect of the flood on the 1998 cohort (Frankenberg, Suriastini, & Thomas, 2004). As with the cross-cohort analysis above, I also stratify this analysis by household vulnerability to test the hypothesis that flood effects were greater in households with fewer pre-flood resources.

For both the within cohort and the cross-cohort analyses described above, I execute the difference-in-difference estimations in two ways. For the within-cohort analyses, I regress height-for-age z-scores on flood exposure, a time dummy for survey round 3, and an interaction term for flood exposure and time. I also adjust standard errors to reflect clustering at the individual level. The interaction term is the coefficient of interest, indicating the effect of flood exposure on the change in height-for-age z-score between survey rounds 1 and 3. The zero-order flood exposure term indicates the baseline (round 1) difference in z-scores between exposed and unexposed children. However, this model is still vulnerable to endogeneity concerns, as flood exposure remains a potentially endogenous variable.

To address this, I estimate a second set of models with individual fixed-effects, still controlling for flood exposure, the time dummy, and the exposure * time interaction term. The fixed-effects model is a difference estimator that sweeps out of the model any time-invariant characteristics (observed or unobserved) at the individual, household or community level. Because flood exposure is constant within individual across survey rounds, it cannot be estimated in a fixed-effects approach, and so this term is dropped from the fixed-effects models. The flood exposure * time interaction term is again the coefficient of interest. While the coefficients in these two approaches are identical, the standard errors are different. These two different models are estimated for each of four birth cohorts, and then for each birth cohort by household vulnerability status.

For the cross-cohort analysis, I take a slightly different approach. I first estimate two restricted models using the difference-in-difference estimator and the fixed-effects estimator as above, controlling for cohort, flood exposure, and then interaction of flood exposure and cohort. In this case the fixed effects are at the village level as the data are cross-sectional. I then estimate two full models (difference-in-difference and village fixed effects again) that include the household vulnerability indicator, and all two-way and three-way interactions between cohort, flood exposure and household vulnerability.

RESULTS

Within-cohort effects

Table 4 shows the unadjusted difference-in-difference in height-for-age z-scores from 1998 to 1999 for flood-exposed vs. unexposed children. Results are shown for all 757 children together, and separately for each birth cohort. For all children, the flood exposed children have an average height-for-age z-score (HAZ) of -2.28 in 1998, 0.37 standard deviations lower than the unexposed children. By the end of 1999, this gap has widened to 0.43. The negative sign of the difference-in-difference in the right column (-.06) indicates that the height-for-age gap between flood exposed and unexposed children has widened from 1998 to 1999.

The differences by cohort paint a more detailed picture. There is a sizable gap for the oldest cohort, children born before 1995, but the gap is similar across the survey rounds at around .50 standard deviations. This suggests that factors associated with flood exposure, rather than with the flooding itself, are responsible for the gap in HAZ scores for this age group. The next cohort, born in 1995-96, shows both a minimal cross-sectional gap and a small (and positive) difference-in-difference. The largest differences in exposed vs. unexposed children can be seen in the 1997 birth cohort. In 1998, when the cohort is 12-23 months old, the gap is .61 standard deviations. By 1999, the gap has widened to 0.90, almost a full standard deviation. The youngest children, the 1998 birth cohort, show almost no difference in 1998 height-for-age z-scores by flood exposure, but the flood exposed children drop behind by 1999.

Regression analysis can help assess whether these bivariate relationships are statistically significant. Table 5 summarizes the results of the estimates of the effect of the flood on the change in child height-for-age z-score from 1998 to 1999. In addition to estimates for each cohort (models 1 and 4), separate models are also estimated for vulnerable (models 2 and 5) vs. non-vulnerable household (models 3 and 6). Each reported coefficient is the flood exposure * time term from a separate difference-in-difference or fixed effects regression. In other words, each coefficient indicates the magnitude and sign of the change in the gap between flood exposed and unexposed children from 1998 to 1999. Note that the coefficients are the same for each corresponding pair of difference-in-difference and fixed effects regressions (e.g, -0.063 for all households, all children), but the standard errors are for the most part larger (and therefore t-statistics are smaller) in the fixed-effects models.

With vulnerable and non-vulnerable households pooled, the effect of flooding on the change in height-for-age z-score is not significant for any cohort. However, for vulnerable households, the gap widens significantly for the 1997 birth cohort and (marginally) significantly for the overall sample in the difference-in-difference model. Once unobserved individual, household and community characteristics are swept out of the model in the fixed-effects specification, it appears that the gap between exposed and unexposed children significantly widens in the 1995-1996 cohort of children in vulnerable households. In households that are not particularly vulnerable according to pre-flood measures, flood exposure appears to actually close the gap in height-for-age z-scores for 1995-1996 cohort (compared to no exposure) but also widens the gap in the youngest (1998) birth cohort. The results for the 1995-96 cohort remain significant in the fixed-effects specification.

Cross-cohort effects

Results for the comparisons of the 1998 and 1999 birth cohorts are shown in Table 6. The first two columns show difference-in-difference and village fixed-effects models that control for flood exposure, cohort (the reference cohort is 1998) and the flood * cohort interaction. No significant effects emerge. In the next two columns, I add the household vulnerability indicator and the two-way and three-way interactions between vulnerability, cohort, and flood exposure. The difference-in-difference model shows strong and significant effects for all terms except the three-way interaction. The household vulnerability indicator and the vulnerability * flood exposure interaction remain marginally significant in the village fixed-effects model, but a Hausman test rejects the fixed-effects model.

The signs of some of these coefficients are surprising. Flood exposure and household vulnerability are both negatively associated with height-for-age z-score, as would be expected. Being in the 1999 birth cohort is also associated with a lower height-for-age z-score, implying either that the effects of the flooding extended well beyond the July-September 1998 period, or that the 1999 birth cohort suffered *in utero* nutrition shocks that exceed the shock suffered by the 1998 cohort. Note also the three positive two-way interaction terms: The negative effects of household-level flooding are mitigated for the 1999 cohort, as are the negative effects of household vulnerability. In addition, flood exposure attenuates the negative effects of household vulnerability and vice versa.

These results are most easily seen in graphic form. In Figure 2 I show the adjusted mean height-for-age z-scores for the 1998 cohort (measured in 1998) and the 1999 cohort (measured in 1999), by household vulnerability and flood exposure status from the difference-in-difference model in the third column of Table 6. The results are striking, reflecting some of the counterintuitive interactions described above. Note that all means are negative, with taller (less negative) bars indicating taller (less negative height-for-age z-scores) children. The tallest children (controlling for age) are the 1998 birth cohort children in less vulnerable households with no flood exposure. This result is consistent with the hypothesis that both flooding and household vulnerability negatively affect height-for-age. Children in the same cohort with a similarly low level of household vulnerability who were exposed to the flood have average z-scores of -1.28, more than one standard deviation below the unexposed children.

The remaining comparisons reveal more surprising results. First, the 1999 cohort of children in less vulnerable households with no flood exposure (the "best case scenario" group), have an average height-for-age z-score of -1.33, again less than one standard deviation below the 1998 cohort in the same circumstances, and comparable to the flood-exposed 1998 cohort. This suggests that living conditions in the flood-exposed regions were bad enough in the aftermath of the flood to compromise the long-term nutritional status of children who otherwise would have fared reasonably well, i.e. children in unexposed, less vulnerable households. In flood-exposed non-vulnerable households, the the 1999 cohort also fared worse than the 1998 cohort, but by a much smaller margin (declining only .13 standard deviations).

What about children in vulnerable households? Here again the results are somewhat counterintuitive. The shortest children (for age) are not those in flood-exposed vulnerable households, but in vulnerable non-exposed households. In this subgroup, the 1999 cohort improves slightly over the 1998 cohort. Note, however, how much taller the flood-exposed children are (about 0.6 standard deviations taller), with the 1999 vulnerable flood-exposed children gaining almost 0.2 standard deviations over the 1998 vulnerable flood-exposed children. In fact, there is no vulnerability gap for flood-exposed children in the 1999 cohort (both groups have height-for-age z-scores of -1.41), whereas the vulnerable flood-exposed children in the 1998 cohort are .32 standard deviations shorter than the non-vulnerable flood-exposed children.

DISCUSSION

This analysis of the effects of the 1998 floods on child nutritional status suggests that the flooding caused marginal growth faltering for some children. Specifically, within-cohort analysis reveals that flooding had no significant effect on children's growth trajectories when children from more and less vulnerable households are pooled in the same analysis. When models are estimated separately for children by household vulnerability, a distinct pattern emerges. For vulnerable households, the flood appears to widen the gap in height-for-age z-scores between exposed and unexposed children in the three older cohorts. For the youngest cohort in this analysis, however, those born in 1998, the effect of the flood is positive. In non-vulnerable households, the flood narrows this gap.

These results prompt the cross-cohort analysis, in which height-for-age z-scores for the 1998 and 1999 cohorts measured at the same age are compared. Flood status, vulnerability status and cohort are fully interacted. Again, results suggest that household resilience moderates the effect of flood exposure on height-for-age. In vulnerable households, both the cross-cohort gap and the flood gap are positive: the 1999 cohort is taller than the 1998 cohort, and the flood-exposed children are taller in both cohorts than unexposed children. The gap is slightly wider (more positive) for the 1999 cohort. In non-vulnerable households, the significant difference in height-for-age z-scores between nonexposed and exposed children that is observed in the 1998 cohort is eroded by 1999, when the 1999 cohort is measured at the same age.

Taken together, these findings are consistent with several possible explanations for the observed effects of the floods on growth faltering. One explanation centers around the role of food aid and transfers. The within-cohort results suggest that flood exposure widened the gap in nutritional status between exposed and unexposed children in vulnerable households but closed it in non-vulnerable households, at least for the 1995-1996 birth cohort. If food aid was targeted at the level of the household based on flood exposure but not on household resources, then preschoolers in exposed households that were otherwise resilient may have been able to translate additional food into linear growth. In less resilient households, food aid may not have made enough of a difference to overcome the negative effects of flooding. This could be tested in future analysis that incorporates receipt of food aid at the household level.

However, if this explanation is true, then why do the 1998 and 1999 cohorts display the opposite effect? In vulnerable households, the positive height advantage that flood-exposed infants hold over exposed infants increases by 1999. In vulnerable households, the large negative height difference in flood exposed infants is eroded by 1999 as the non-vulnerable, non-exposed infants falter relative to the 1998 cohort. One reason may be that direct food aid would make little difference to this age group in any case. Another may be differential infant mortality. If, among vulnerable households, weaker infants in flood-exposed households died before the first survey round at a higher rate than did infants in nonexposed households, then the remaining infants may have had faster growth trajectories. A third explanation may be that, for this youngest and (normally) fastest growing cohort, the coefficient in the flood * time interaction term is actually capturing the degree of catch-up growth. As discussed above, the heights of the

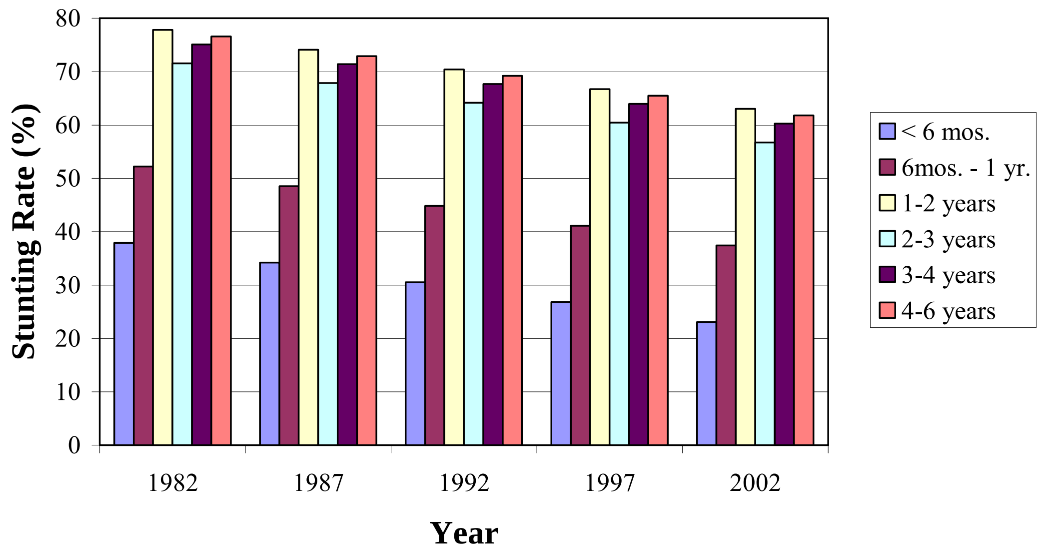
1998 cohort of children may as measured in November-December 1999 may already have incorporated the nutrition shock of the summer's floods.

Given the challenges of this dataset and the nature of the dichotomous variables employed in the models, sensitivity analyses are warranted here. I did estimate all of the models with a different age specification, using age in completed years where no months were reported instead of adding six months as described above. Results were substantively similar. In addition, I estimated all of the models using a different cut-off point for flood exposure. Instead of no exposure vs. any exposure, I divided the sample into households with no or moderate exposure and those with severe or very severe exposure. Again, results were substantively very similar. Other sensitivity analyses that remain to be done include different specifications of the household vulnerability measure and outlier analysis for the height-for-age z-scores. I have also chosen not to use the data from the second survey round (April 1999), nor to analyze changes in weight-for-age, both of which deserve future exploration.

Finally, I hope to expand this research project in several ways. Given the results of the cross-cohort analysis, I would like to pursue to role that food aid and other coping strategies played in child growth trajectories. I also plan to construct more detailed measures of household assets, looking at pre-flood holdings, the degree of asset loss, asset sales post-flood, and the pace of asset recovery over the post-flood year. This analysis should expand understanding of the asset- and consumption-smoothing strategies that were available and attractive to households with differing levels of resources. Another human capital asset that could be analyzed is the change in maternal BMI.

Poor, densely-settled populations in developing countries will continue to experience devastating environmental disasters and other shocks, perhaps with increasing frequency and intensity. Considerable aid monies and development projects are focused on protecting these vulnerable populations before such events, and assisting with their relief and recovery in the wake of major catastrophes. It is important to understand exactly which groups and individuals are most at risk of experiencing permanent negative effects from weather shocks in order to craft effective and well-targeted interventions. This study suggests that pre-shock household resources play an important role in moderating nutrition shocks for small children. Results also suggest that environmental disasters combined with targeted post-disaster food aid may attenuate pre-flood disparities in nutritional status. More work is needed to pinpoint the exact role of food aid and other coping strategies, and to determine which resources contribute most to resilience from shocks.

Figure 1: Estimated stunting rates by age and year, rural children in Bangladesh, 1982-2002



Source: WHO Global Database on Child Growth and Malnutrition

Figure 2: Adjusted height-for-age z-scores by flood exposure, household vulnerability and birth cohort, children born in 1998 or 1999 measured at ages 0-11 months, in flood-affected *thanas* in rural Bangladesh, 1998-1999 [N=140].

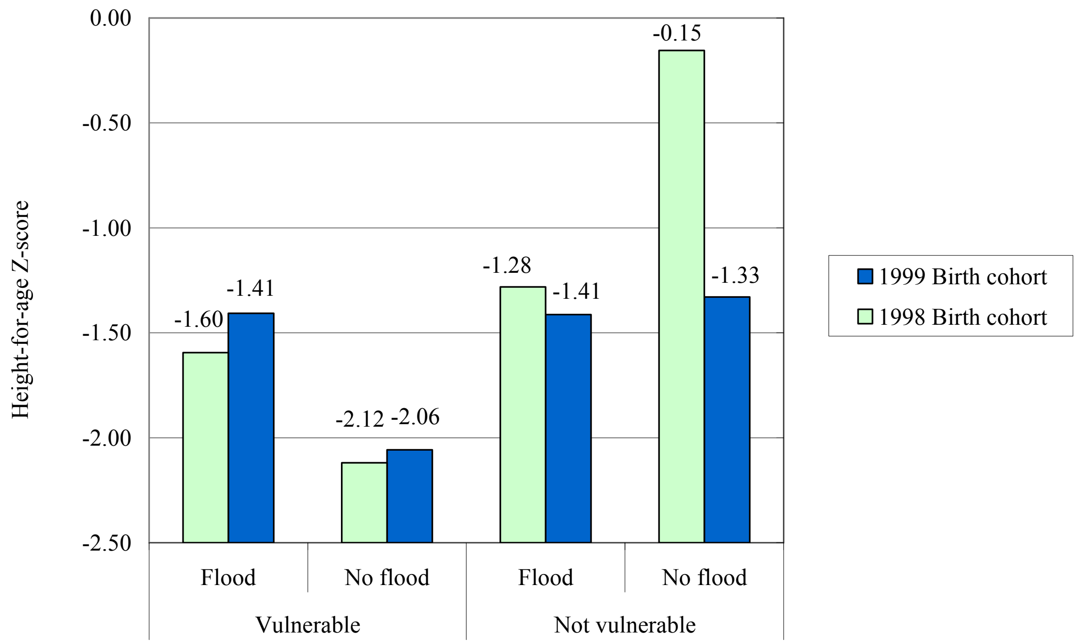


Table 1: Descriptive statistics, Bangladesh children born 1990-1998 in rural flood-affected thanas, 1998-1999 [N=757].

Variable	Mean	Std. Dev.
Age in months, Round 1 (Fall 1998)	58.06	30.49
Age in months, Round 3 (Fall 1999)	69.04	30.49
Height-for-age z-score, Round 1 (Fall 1998)	-2.20	1.39
Height-for-age z-score, Round 3 (Fall 1999)	-2.25	1.34
Household exposed to flood = 1	0.76	0.43
Household vulnerability score >= 2	0.49	0.50
Household head < 30 years old	0.06	0.23
Spouse of household head < 145 cm	0.13	0.34
Household owns no farmland	0.38	0.49
Household head has no formal education	0.54	0.50
Walls of primary dwelling not permanent	0.41	0.49
Number of children	757	
Number of households	438	

Table 2: Descriptive statistics, Bangladesh children born 1998-1999 in rural flood-affected thanas, 1998-1999 [N=140].

	1998 Cohort in 1998		1999 Cohort in 1999	
	Mean	Std. Dev.	Mean	Std. Dev.
Age in months	6.19	2.96	5.62	3.18
Height-for-age z-score	-1.49	1.30	-1.47	1.30
Household exposed to flood = 1	0.80	0.40	0.74	0.44
Household vulnerability score > 1	0.58	0.50	0.48	0.50
Household head < 30 years old	0.15	0.36	0.08	0.28
Spouse of household head < 145 cm	0.19	0.39	0.08	0.28
Household owns no farmland	0.46	0.50	0.39	0.49
Household head has no formal education	0.56	0.50	0.48	0.50
Walls of primary dwelling not permanent	0.42	0.50	0.49	0.50
N	79		61	

Table 3: Age at flood onset and at survey rounds 1 and 3 by birth cohort, rural children in flood-affected thanas in Bangladesh, 1998-1999 [N=757].

Year Born	Age at Flood Onset (July 1998)	Age at Round 1 (Nov-Dec 1998)	Age at Round 3 (Dec 1999)
1990-94	43 mos. +	4 yrs +	5 yrs +
1995-6	19-42 mos.	24-47 mos.	36-59 mos.
1997	6-18 mos.	12-23 mos.	24-35 mos.
1998	prenatal-6 mos.	0-11 mos.	12-23 mos.
1999	---	---	0-11 mos.

Table 4: Height-for-age z-scores by birth cohort and flood exposure, rural children in flood-affected thanas in Bangladesh, 1998-1999 [N=757].

	Age at onset of 1998 flooding	Fall 1998			Fall 1999			Unadjusted Difference-in-Difference
		No Flood	Flood	Δ	No Flood	Flood	Δ	
All children		-1.92	-2.28	-0.37	-1.92	-2.36	-0.43	-0.06
Born 1990-1994	> 42 mos.	-1.90	-2.42	-0.51	-1.86	-2.39	-0.54	-0.02
Born 1995-1996	19-42 mos.	-2.23	-2.30	-0.06	-2.17	-2.20	-0.03	0.04
Born 1997	6-18 mos.	-1.53	-2.15	-0.61	-1.68	-2.58	-0.90	-0.29
Born 1998	< 6 mos.	-1.49	-1.47	0.02	-2.09	-2.21	-0.13	-0.15

Table 5: Estimates of the effect of flood exposure on change in height-for-age z-scores from 1998 to 1999, by birth cohort and household vulnerability, children in rural flood-affected thanas in Bangladesh [N=757].

	N	Age at peak flooding in months	Difference-in-Difference			Individual Fixed Effects		
			(1)	(2)	(3)	(4)	(5)	(6)
All children	757		-0.063	-0.165	0.052	-0.063	-0.165	0.052
			[1.00]	[1.68]*	[0.71]	[0.93]	[1.61]	[0.60]
Born 1990-4	472	> 42	-0.023	-0.056	0.02	-0.023	-0.056	0.02
			[0.42]	[0.62]	[0.31]	[0.36]	[0.57]	[0.24]
Born 1995-6	156	19-42	0.035	-0.428	0.498	0.035	-0.428	0.498
			[0.18]	[1.44]	[2.34]**	[0.22]	[1.88]*	[2.48]**
Born 1997	66	6-18	-0.287	-0.689	0.267	-0.287	-0.689	0.267
			[1.19]	[2.40]**	[0.71]	[0.94]	[1.52]	[0.73]
Born 1998	63	< 6.	-0.146	0.294	-0.622	-0.146	0.294	-0.622
			[0.45]	[0.62]	[1.78]*	[0.38]	[0.54]	[1.26]

Each reported coefficient is from a separate regression
Absolute value of t-statistics in brackets
*significant at 10% , **5% , ***1 %

Table 6: Estimates of the effect of cohort, flood exposure and household vulnerability on height-for-age z-scores for children born in 1998 or 1999, rural children in flood affected thanas in Bangladesh.

	Restricted models		Full models	
	Difference-in-Difference	Village fixed effects	Difference-in-Difference	Village fixed effects
Exposed to flood	0.178	0.975	-1.127	-0.638
	[0.53]	[1.30]	[3.02]***	[0.58]
Born 1999	-0.111	0.511	-1.175	-0.477
	[0.28]	[0.61]	[2.66]***	[0.42]
Exposed to flood* Born 1999	0.184	-0.442	1.321	0.911
	[0.38]	[0.48]	[2.12]**	[0.69]
Household vulnerable			-1.965	-1.794
			[3.66]***	[2.01]*
Household vulnerable * Born 1999			1.237	0.502
			[2.22]**	[0.53]
Household vulnerable * Exposed to flood			1.652	1.823
			[2.84]***	[1.96]*
Household vulnerable * Exposed to flood * Born 1999			-0.445	-0.374
			[1.60]	[0.85]
Constant	-1.629	-2.313	-0.155	-0.743
	[5.82]***	[3.81]***	[0.49]	[0.75]
F- test of significance of all vulnerability variables			3.51	1.17
			[.011]	[.355]
Hausman test of fixed effects model		1.57		2.4
		[.663]		[.935]
Number of observations	140	140	140	140
Number of villages			86	86

* significant at 10%; ** 5%; ***1%

Robust t statistics in brackets

P-values in brackets for F-tests and Hausman tests

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