Smooth or Lose: Natural Disasters and Child Outcomes in Vietnam

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Abstract

Using 4 rounds of data for 2000 children in Vietnam between 2006-2016, I investigate the relationship between natural disasters and child health and education outcomes. Two econometric models are presented, Fixed Effects (FE) and First-Differences (FD), with the FD model preferable for its greater efficiency. I conclude that child educational outcomes are affected by the advent of a natural disaster, with the FD model predicting an average 2.6 pp. decrease in a child's math percentage score due to a flood, and a 2.14 pp. decrease in a child's PPVT percentage score due to an erosion. There are no statistically significant impacts on health outcomes. I attempt to explain these findings by exploring theoretical frameworks relating to the consumption function. Further heterogeneity analyses demonstrate that the effects are largely homogenous for floods, but not so for erosions. Positive and negative sensitivity analyses are also undertaken. The latter supports the internal validity of the FD results, but the failure to corroborate my findings with the alternative sampling methodologies of the former suggest that earlier findings pertaining to education were sensitive to model specification.

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1. Introduction:

The threat of climate change has been increasingly salient in recent years. Aside from causing the annihilation of numerous wildlife ecosystems, climate change has exacerbated many weather-related events, increasing their frequency, intensity, and extent of impact (IPCC 2014). As a consequence, devastating natural disasters have seen greater prominence, and have become part and parcel of our daily lives (Thomas & Lopez 2015). The economic impact of these disasters cannot be understated. In 2018, the U.S. suffered more than $91 billion in losses from natural disasters alone, at almost 20 times that of the 1950s (Chappell 2019). At the micro-level, there is consensus that these disasters directly result in the loss of lives, property, and household income (Arouri et al. 2015). Still, there is considerable variation across communities based on localized exposures and vulnerabilities (Clark et al. 1998). Additionally, conditional on survival, supplementary indirect effects diverge as household preferences and domestic conditions play into decision-making processes. With child welfare often compromised in times of crisis (Chen & Corak 2008), it is important to investigate how these broader environmental events play into the circumstances of the younger generation. Specifically, I look at Vietnam, whose tropical and coastal geography results in a country with one of the highest disaster exposure rates in the world today (UNISDR 2009).

Economic growth in Vietnam has been phenomenal over the past two decades. A recent report from Pricewaterhouse Coopers (2017) indicates that Vietnam is likely to be the fastest-growing of the world's economies, with long-term annual GDP growth in excess of 5%. Jim O'Neill, who infamously coined the term BRICS, also contends that Vietnam is 1 of 11 countries that will complement the new powerhouses as the largest economies of the 21st century (Martin 2012). Fortunately, Vietnam's growth has been accompanied by an outstanding record of inclusivity, with a consistent reduction in the national poverty rate, and an annual growth of 9% in the average income of the bottom 40% between 1992-2012 (Tran 2013). Still, 9.8% of the country lives under the national poverty line, and more than 30% classified as economically insecure by international standards. In addition, concerns about inequalities are rife, with 45% of ethnic minorities living in poverty, despite comprising only 15% of the total population (Pimhidzai 2018). To aggravate matters, the poorest households tend to congregate in mountains and highlands, where suboptimal production, a lack of agricultural dynamism, and minimal access to important resources leave them especially susceptible to weather-related shocks (Le et al. 2014). While the Vietnamese government has enacted a number of anti-poverty programs aimed at elevating the functional knowledge of the poor (Quan 2009), there has been a conspicuous absence of an integrated disaster management framework (World Bank 2017). This is concerning because Vietnam is known to be prone to natural disasters, being ranked fourth in the world in terms of absolute number of people exposed to floods, tenth for cyclones, and fourteenth for droughts (UNISDR 2009). By failing to mitigate the risks for a vulnerable population, the Vietnamese government jeopardizes their ability to defeat chronic poverty, and forgoes a potentially productive labor force needed to sustain economic growth (Breu et al. 2012). Therefore, there is a critical necessity to provide policymakers with relevant information in this domain, so as to support overall human capital accumulation within Vietnam, and ensure that ethnic tensions arising from inequalities do not clog the country's future economic agenda.

Using data from the Young Lives (YL) Study at Oxford University, I collate a panel of 2,000 children across 4 rounds between 2006-2016. The choice of focus is informed by the prime candidacy of children for human capital investment, and their being integral to Vietnam's future growth (HKTDC 2017). Based on data availability and geographic relevance, I elect to consider two forms of natural disasters – floods and erosion, and four child outcomes that reflect educational and health attainment. The working assumption is that said disasters hinder income generation, which correspondingly affects child outcomes via alterations in household spending patterns. This is supported by a wide base of literature (e.g. Dercon 2004, Samphantharak 2014, Bui et al. 2014).

I first explore the two prominent theoretical frameworks by which natural disasters might affect child outcomes, namely Keynes' (1936) Absolute Income Hypothesis (AIH), and Friedman's (1957) Permanent Income Hypothesis (PIH), which posit contrasting consumption behavior based on income shocks. I then identify suitable controls via a simple OLS specification, before moving on to Fixed-Effects (FE) and First-Differences (FD) models that isolate causality based on the strict exogeneity of the independent variables. The two are utilized for better comparison, though the latter's efficiency is favored based on serial correlation between the error terms. Following which, I undertake heterogeneity analyses based on geographic and entity-specific characteristics, before testing the robustness of my results with a variety of positive and negative sensitivity analyses. I find that the results from the FD model are internally valid and preferred, with coefficients almost always negative. There is no statistically significant impact by natural disasters on child health outcomes, best explained by consumption smoothing akin to the PIH. Conversely, educational outcomes are affected, ostensibly via the AIH, with a 2.6 percentage point (pp.) decrease in a child's math percentage score due to floods, and a 2.14 pp. decrease in PPVT percentage score due to erosions. These are largely homogenous for floods, but less so for erosions. The lack of positive verification from the sensitivity analyses suggests that the findings are responsive to model specification. Section 2 overviews the related literature, Section 3 investigates the theoretical framework, Section 4 presents the data, Section 5 elaborates the empirical strategy, Section 6 discusses key findings, and Section 7 concludes.

2. Literature Review:

The relationship between household income shocks and child outcomes has received substantial interest over the past two decades. Capitalizing on natural experiments, many studies exploit the resulting variation in incomes to disentangle its relationship with child investment and outcomes. For instance, Miller & Urdinola (2010) utilize coffee price fluctuations in Colombia as a proxy for income shocks; Jensen (2000) and Macinni & Yang (2009) are two popular studies that look at fluctuations in agricultural yield; Paxson & Schady (2005) and Pongou et al. (2006) both study the effects of macroeconomic crises, with the former in Peru, and the latter in Cameroon; others like Duflo (2000) investigate policy-driven shocks a la the Old Age Pension Program in South Africa. Another strand of literature focuses on randomized experiments. Clark-Kauffman et al. (2003) and Duncan et al. (2011) provide two useful overviews. Both pool data from work-welfare programs in North America to conclude that positive income shocks improve school achievement and cognitive development. A thinner strand of literature looks specifically at Conditional Cash Transfer (CCTs) programs. Prominent papers include Fernald et al.'s (2008) evaluation of *Progresa-Oportunidades* in Mexico, and Riccio et al.'s (2010) study of the NYC Family Rewards program. However, these are more policy-specific due to the conditionalities attached. Regardless, the methods seen in the above studies have all helped to resolve the dominant endogeneity issue that has plagued earlier research.

Relating to this paper, the premise of employing natural disasters as a proxy for negative income shocks is found on three main causes. First, natural disasters are known to cause a reduction in household capital (see Dercon 2004, de Janvry et al. 2006, Carter et al. 2007), particularly through the loss of physical assets (Samphantharak 2014, Sahin & Sag 2015). This is especially salient in developing countries where limited resources are channeled to asset protection. Second, natural disasters impede the production of goods, especially agricultural, thereby hindering income generation (Xu et al. 2011, Lin & Chou 2014). Income from households in the manufacturing sector are also affected, as evidence suggests that procurement patterns might change (Hayakwa et al. 2015) or plants made redundant (Matsuki & Manugi 2016). Finally, natural disasters destroy surrounding infrastructure. This might hinder a household's ability to engage productively with the economy (Baez et al. 2010, Deshmukh et al. 2011), and/or cause dampened job prospects from a depressed business environment (Cashin & Sosa 2013). In developing countries, these effects are exacerbated due to the prevalence of low quality infrastructure (Escaleras & Register 2016).

At the macro-level, there appears to be a broad consensus on the effects of natural disasters on human capital development. Cuaresma (2010) and McDermott (2012) both look at cross-country data of more than 170 countries over a 20-year timeframe in the late 1900s and conclude that natural disasters have significantly negative effects on school enrollment rates and child health. This is backed by Toya et al. (2010), who look at a smaller subset of 89 countries, but over a longer period between 1960-1990. While their focus is on the relationship between human capital and economic growth, they provide evidence that per-land climatic and geologic disasters are strong instruments for a country's total years of schooling. Others have demonstrated that poorer/developing countries generally suffer more when compared to their richer counterparts (Jaharudin et al. 2018). Though sparse, some studies have also looked specifically at country-specific national data. For example, Rush (2018) concludes that natural disasters negatively affect primary and secondary school enrollment in Indonesia, while Tseng (2016) posits that in-utero exposure to disasters cause fetal mortality rates to rise from 3.2% to 4.4% in Taiwan. Beyond disasters, Flug et al. (1998) looks generally at income volatility in Latin American countries from 1970-72, and notes a countercyclical relationship between income and average secondary enrollment. Hitherto, the macro perspective suggests that investment in child development is often compromised when negative income shocks abound.

Unfortunately, the results are less conclusive at the household level. In this case, two prominent strands of literature exist. The first subscribes to Friedman's (1957) PIH, where households smooth consumption such that it is consistent with their long-run average income. Thus, natural disasters, or income shocks more generally, have no discernible effects on child outcomes. Friedman's hypothesis is supported by household data across a number of countries. Jacoby & Skoufias (1997), Schady (2004), and Strauss et al. (2004) all find that crises in India, Peru, and Indonesia respectively have little to no effect on overall schooling outcomes, while Miller & Urdinola (2010) document a procyclical relationship between coffee price and child mortality in Colombia. Pertaining to natural disasters, Guin (2015) maintains that conditional on survival, child health in Indonesia remains unaffected. Likewise, Baez & Santos (2007) find no significant impact of tropical storms in Nicaragua on overall school enrollment. Additionally, some studies directly address mechanisms of consumption smoothing, such as the sale of assets and buffer stock in Tanzania (Beegle et al. 2006), or the use of credit in Bangladesh (Alvi & Dendir 2011) that offset transitory shocks so that households do not turn to child labor – and consequently affect child outcomes – as a coping mechanism. Conversely, the second strand of literature affirms the intuitive Keynesian AIH (1936), which contends that contemporaneous disposable income is the decisive factor for household expenditure. Thus, to name a few, natural disasters as seen in France (Banerjee et al. 2010), Cote d'Ivoire (Jensen 2000, Cogneau & Jedwab 2012), El Salvador (Santos 2007), Mexico (Aguilar & Vicarelli 2011), and the Philippines (Deuchert & Felfe 2015) all had adverse effects on child education and/or health. In contrast to the macro view, literature in this domain is contentious, and the variance in results can be attributed to a variety of conditions, including spatial & temporal conditions, as well as the local population's general disaster resilience. Additionally, the specific type of shock is likely to matter, as results from a recession vis-à-vis a natural disaster may differ, or even between disasters such as tornadoes and floods, even if all serve as valid proxies for negative income shocks.

The aforementioned inconclusiveness points to the heterogeneity of effects across countries. This makes it difficult to draw conclusions on any specific country without a dedicated in-depth study. Regrettably, literature with a Vietnamese focus is sparse. As aforementioned, this is concerning because Vietnam is known to be prone to disaster shocks, being ranked fourth in the world in terms of absolute number of people exposed to floods, tenth for cyclones, and fourteenth for droughts (UNISDR 2009). Current literature typically addresses household welfare, leveraging the National Household Living Standards Surveys between 2000-2010 (Thomas et al. 2010, Bui et al. 2014, Arouri et al. 2015). There is consensus that natural disasters lead to a decline in per capita income and welfare of between 6.9-7.1% (Thomas et al. 2010, Bui et al. 2014), although frequent exposure has led to better coping mechanisms such that many households do not experience these as negative events (Thomas et al. 2010), and there is variance amongst floods, droughts, and storms (Arouri et al. 2015). Some work has also been done relating natural disasters to the macroeconomy (Noy & Vu 2010), underscoring the positive effects that "creative destruction" have in the short-run by forcing the replacement of archaic plants, and with firms (Vu & Noy 2018), where disasters depress retail sales but lead to an increase in firm investment of the same magnitude, though these are unrelated to changes in surrounding household incomes. To my knowledge, there is limited to no work on child outcomes. This is troubling given the Vietnamese population boom, its bottom-heavy population pyramid, and the need for human capital development to supplement its meteoric growth. In tandem with the frequency of local disasters, this is a gap that needs to be bridged.

The closest study to this paper is a recent article by Cuong & Nguyet (2018). Using data from the Young Lives dataset, they provide comparative evidence linking natural disasters to child outcomes in Ethiopia, India, Peru, and Vietnam, and conclude that there are significant and negative effects. Perhaps because the authors adopt a broad geographic representation, there is much room for improvement in terms of methodological depth. First, notwithstanding the presence of five rounds of data, the authors selectively employ data from the 2nd (2006) and 3rd round (2009). This defeats the purpose of an extensive longitudinal survey like the Young Lives dataset, and obscures important data points, particularly from the most recent 4th and 5th rounds, which are relevant for contemporary interests. Additionally, with only 2 rounds of data, no meaningful comparison can be made between the FE and FD models, since the results will be identical. Second, the authors surprisingly present only one econometric model based on child and time fixed effects. This is despite them noting that the impacts likely differ for "regions [and] communities." In the case of disasters, which tend to hit at a community, rather than at the individual level, this is worrisome. It would be useful to have a comparison with commune-level fixed effects, and interacted geographic-time fixed effects. This is exacerbated by the limited choice of controls, which only cover household composition and parental education. Important controls like wealth, access to credit, and community-level indicators are excluded. All in, there is reason to question the reported significance levels. Third, no attempt at further sensitivity analysis is made, which again begets the issue that the results are predicated on a singular model specification, especially since most of the survey is self-reported. Finally, in context of differing disaster resilience, the paper could benefit from some form of heterogeneity analysis.

Synthesizing the above, I contribute to the literature in a few main strands:

(i) Geographically, I seek to relieve the paucity of studies that engage with Vietnam. As aforementioned, there seems to be a lack of overall consensus relating to child outcomes, much less one with external validity applicable to Vietnam. The bridging of this gap is imperative given Vietnam's disaster exposure, the advent of climate change, the immense potential of its economy, and the pre-dominance of low income households who are most susceptible to weather-related income shocks.

(ii) I contribute to a growing strand of literature that uses micro-evidence to disentangle complex relationships by looking at the vessels that drive overall change. Household-level data facilitates greater insight by allowing me to delve into heterogenous effects that are potentially responsible for the inconclusiveness of earlier studies. Moreover, I am able to look at metrics previously unmeasurable by aggregate data. For example, individual test scores, which capture the cognitive development of a child.

(iii) Many studies to date opt to pursue educational outcomes. Studies relating to health outcomes are noticeably absent in Vietnam, while those present abroad tend to reflect long-term results. The Young Lives dataset provides necessary information on short to medium term health effects, which are equally important in context of income shocks.

(iv) Empirically, I assimilate two newer rounds of data, and adopt a more comprehensive approach, including the use of interacted fixed effects and the extension of controls, which provides for comparison amongst a variety of methodologies. Also, I explore a FD model, which helps control for unobserved time-invariant components, and is more efficient than the FE model due to serial correlation between the error terms.

(v) Finally, I incorporate multiple sensitivity analyses. This includes re-sampling at the commune level, which distinguishes between the effect of idiosyncratic versus commune-level shocks, helps negate any self-reporting bias, and explores differences that might arise due to community pooling of risks (Hyder et al. 2015). I also conduct falsification tests, as well as a difference-in-difference approach that looks at a subset of children whose only differentiating factor is a singular shock in the interim.

3. Theoretical Framework

Fundamentally, it is important to consider the mechanism by which natural disasters may affect child outcomes. Taking inspiration from Chandrasekharan (2016), I first propose a basic model of household income relevant to the households in our dataset.

(A) $$Y_{icpt} = Y(K_{icpt}, A_{icpt}, N_{cpt})$$

Here, the income Y of a household i in commune c of province p at time t is a function of three independent variables. These are a household's capital K, the amount of agricultural or manufactured goods produced A, and the infrastructure surrounding its locality N. As seen from the literature (e.g. Dercon 2004, Samphantharak 2014, Matsuki & Manugi 2016), household income is invariably affected by the advent of a natural disaster. Such disasters damage physical capital, which are often unprotected and comprise the majority of holdings in poverty-stricken households, impede income from the production of goods in both agricultural and manufacturing sectors, and harm surrounding infrastructure, hindering a household's ability to engage productively with the economy and dampening its job prospects from a depressed business environment. Thus, I take the onset of a natural disaster as a proxy for negative income shocks.

Two potential and contrasting hypotheses exist, which date back to regular debates surrounding the appropriate consumption function. Keynes (1936) in his critically acclaimed *General Theory*, proposes the Absolute Income Hypothesis.

(B) $$C_t = \alpha + \lambda Y_t$$

Keynes asserts that real consumption C_t is a stable and direct function of real disposable income Y_t. It is assumed to vary directly with income, although the Marginal Propensity to Consume (MPC) λ is less than unity. As such, when income rises/drops, consumption correspondingly increases/decreases, although this rate of increase/decrease is less than the rate of increase/decrease in income. Autonomous spending α is assumed to be positive, as agents with no income might still have to fund debt obligations and necessities. Since the Average Propensity to Consume (APC) $= \frac{C}{Y}$ is greater than MPC $= \frac{\Delta C}{\Delta Y}$, effects are more salient in the long run as MPC tends toward APC, and the income elasticity of consumption tends toward unity. Nonetheless, according to Keynes, income shocks should still produce noticeable effects on consumption in the short-run, even if the effects are not wholly equal. Following Keynes' hypothesis, a natural disaster that affects household income via equation (A) should lead to a negative shock to real disposable income Y_t, and hence, C_t at the rate of the MPC λ. With households reducing consumption, there should be visible effects on both child education and health outcomes.

Conversely, Friedman (1957) proposes a Permanent Income Hypothesis, where agents partake in consumption smoothing, such that consumption at any point is consistent with their expected long-term average income. Central to Friedman's argument is the delineation between permanent income Y^p and transitory income Y^t.

(C) $$Y = Y^p + Y^t$$

Permanent income is an agent's expected long-term average income, while transitory income may be interpreted as unanticipated income, that may be either positive or negative. For example, a windfall from a lottery results in positive transitory income, while a locust attack that destroys crops is seen as negative transitory income. Hence, in the case of a disaster in the interim period, $Y_2 < Y_1$ because $Y_2^t < Y_1^t = 0$. However, according to Friedman, permanent income remains at $Y_1^p = Y_2^p$, especially since households that live in tropical areas with high disaster exposure are likely to internalize the costs of potential natural disasters in their expected long-term average income.

In the case of consumption, the same demarcation between permanent consumption C^p and transitory consumption C^t is observed.

(D) $$C = C^p + C^t$$

Permanent consumption is what agents consume regularly based off their expected permanent income Y^p. Similarly, transitory consumption can be either positive or negative. For example, C^t might be positive due to abnormally high electricity bills during a cold spell, or negative in the event of a temporary tax relief. Adopting a modification of the two-period model by Dillon (2013),

(E) $$U = u(a_1, a_2) + ß\ddot{u}(c_1, y_2)$$

I assume that parents are rational and seek to maximize utility U. All parents invest in their children as household utility comprises adult consumption in period 1 (a_1) and 2 (a_2), child consumption in period 1 (c_1), and child's future income in period 2 (y_2). Child utility is discounted by ß. Hence, within Friedman's framework, educational and health costs for a child are components of a household's permanent consumption, where regular consumption has been smoothed upon their birth.

Returning to Friedman, it is thus established that C^p is a direct function of Y^p, and that this is independent on the size of Y^p. Rather, consumption depends on the MPC from Y^p, k, which comprises rate of interest (i), the ratio of nonhuman wealth to income (w), and other household-specific factors symbolized by the portmanteau variable (u) such as its taste and preferences.

(F) $$C^p = k(i, w, u)Y^p$$

Therefore, regardless of any shocks to income, $C_1^p = C_2^p$, assuming $k(i, w, u)$ is held constant.

Finally, (C), (D), and (F) must be viewed in context of Friedman's underlying assumption,

(G) $$\rho Y^t Y^p = \rho C^t C^p = \rho Y^t C^t = 0$$

where ρ represents the correlation coefficient between the variables. Importantly, Friedman believes that transitory income and transitory consumption have no direct correlation. This implies that the MPC from Y^t is zero. Put simply, a household fortunate enough to receive positive transitory income will not alter its consumption, while those unlucky enough to receive negative transitory income do not reduce consumption. Instead, they reduce savings, or look for alternative forms of credit. Thus, in line with Friedman's hypothesis, negative income shocks from a natural disaster should not cause any discernible effects on a child's education/health outcomes, which should be relatively stable across time as a constituent of a household's permanent spending.

A preliminary assessment of the contrasting hypotheses suggests that households in my context, which are mainly poverty-stricken, are more likely to subscribe to the Keynesian AIH. This is because they are unlikely to have large reserves of savings or easy access to credit that aid in consumption smoothing during the presence of negative income shocks. If so, there should be visible effects on both child education and health outcomes.

4. Data

The dataset that I will be using originates from the Young Lives (YL) study conducted by the Department of International Development at the University of Oxford. This is a longitudinal research project that tracks 3000 children in Vietnam over five rounds from 2002 to 2016, with the time between each round spanning 3-4 years. Two cohorts of children are followed simultaneously; the younger cohort (YC) comprises 2000 children born between 2001-02, while the older cohort is made up of 1000 children born between 1994-95. For the purposes of this paper, I will be examining data for the 2000 children in the YC from Round 2 to Round 5 of the study. This is because health and educational outcomes are unreported for the OC in the later rounds of the study, as it shifts toward observing job outcomes, as well as anthropomorphic characteristics of their new families and children. Round 1 is excluded because the YC was still in the infant stage, with little to no measurable outcomes.

As the initiative primarily aims to tackle childhood poverty, a multistage sentinel site sampling approach is undertaken. After a process of iterative consultation, 5 (out of 9) representative regions in Vietnam were selected, followed by a province within each region; these selections were based on regional and urban/rural differences in Vietnam. Following which, the communes in each province were ranked by poverty level and an over-poor sampling strategy is adopted, where four communes (two from the poor group, one from the average, and one from the above average) were ultimately selected in each province. Finally, within each commune, 100 YC children and 50 OC children were randomly sampled. If a selected commune had insufficient numbers of YC or OC children, children from a neighboring commune with similar socioeconomic conditions furnished the sample, leading to a total of 31 communes in the study. No household is allowed more than one YL child; this means that for the purposes of this paper, household and child attributes are referred to interchangeably.

Importantly, the over-poor bias implies that the findings of this paper might not be externally valid for all children in Vietnam. However, it is to date the only cohort study available for under-privileged children in Vietnam. Additionally, by comparing the characteristics of YL households in Round 1 with the nationally representative Living Standard Survey (VLSS 2002), and Demographic and Health Survey (DHS 2002), Nguyen (2008) concludes that while the YL households are poorer than the average Vietnamese household, the sample covers the diversity of children in Vietnam through a wide variety of attributes and experiences.

Table 1 presents the 2002 distribution of YL observations by province, with supplementary data adapted from the General Statistics Office of Vietnam (GSO 2002) and Nguyen (2008). As observed, at the commencement of the study, despite the pro-poor bias, there is diversity in the choice of province. Geographically, the five selections span both urban and rural communities, covering coastal, mountainous, and river settlements, which are the three main forms of Vietnamese terrain. Economically, Da Nang appears to be the richest and most developed, followed by Hung Yen and Ben Tre, which are rather average, and finally, Phu Yen and Lao Cai, representative of the poorer provinces. The variation is also observed in other social indicators, though Da Nang has a significantly lower number of schools, perhaps due to it being a city with a greater population density that facilitates larger per-school student population. This stratification in provincial choice is further supported by the random sampling of households within. Thus, though the pro-poor bias suggests that it would be a stretch to draw conclusions for the Vietnamese population as a whole, for the purposes of this paper, the YL dataset is a valuable tool through which the impact of natural disasters on underprivileged children in Vietnam can be analyzed.

Table 2 reports the descriptive statistics for our variables of interest. Panel A captures our two independent variables, believed to have the largest explanatory power in terms of natural disasters for child outcomes in the Vietnamese context. The two are dummy variables, indicating whether a child/household reports experiencing a flood and/or erosion in between the previous and the current round. Notably, floods occur considerably more than erosions, at 7.1% of all observations compared to that of 1.4%. The fact that the percentage occurrence is still relatively low allows for a substantial basis of comparison. Panel B looks at our four dependent variables, including a child's scores in the Young Lives Mathematics Test (YLMT), and the Peabody Picture Vocabulary Test (PPVT) to determine educational achievement. Results are mediocre, with the average child barely achieving a passing grade. Health outcomes are seen in a child's BMI and weight. By comparing the mean BMI of the sample in each round with peers of the same age group globally, the YC children are seen to borderline on being underweight, suggesting malnourishment. This is likely due to the low mean monthly expenditure on food, which stands at approximately $13 USD per household. Panel C and D exemplify key child and commune characteristics, that are likely to influence our stipulated outcomes. The use of these will be further elaborated below.

5. Empirical Strategy

I first run a simple OLS specification; this reports the initial correlation between the variables and identifies suitable controls.

(H) $$Y_{icpt} = \alpha + ßD_{icpt-\theta} + C_{icpt} + X_{cpt} + V_{icp} + \epsilon_{icpt}$$

Here, Y_{icpt} represents either one of the four educational or health outcomes of a child *i*, in commune *c* of province *p*, at time *t*. Pertaining to educational outcomes, I focus on metrics previously unobserved in aggregate data. These are a child's test scores in the PPVT and the YLMT. The former is a popular international assessment which screens a child's receptive vocabulary, while the latter is a specialized amalgamation of national and international tests that defines quantitative performance. In tandem, they demonstrate a child's cognitive performance, and are likely connected to a household's education expenditure. Relating to health, I adopt two commonly used anthropometric indicators for nutritional status, BMI and weight, which are responsive to nutrition in the short and medium term. $D_{icpt-\theta}$ is the independent binary variable that indicates whether the child (or guardian) reports having experienced a natural disaster in the time since the past round, with θ noting the time lapse, and ß capturing the relationship of interest. Based on data availability and geographic relevance, the two types of natural disasters selected are floods and erosions. A large number of earlier studies have documented determinants of education for children (Filho 2008, Edmonds 2008, Krutikova 2009). C_{icpt} is a vector of child-specific variables that reflects these controls, including household wealth, location, composition, food expenditure, access to sanitation, current and potential access to credit, and the amount of time a child spends at work and school, all of which are seen in Panel C of Table 2. Additionally, I include X_{cpt}, a vector of commune-specific control variables, seen in Panel D of Table 2, comprising commune population size, poverty level, and access to health facilities, which are relevant to the outcome variables. V_{icp} is a vector of time-invariant controls, including a child's gender, and whether or not the child's household site is rural.

Following the confirmation of the necessary control variables, I proceed with three separate fixed effects (FE) regressions. The assumption here is that the onset of natural disasters are strictly exogenous. Hence, by controlling for observed and unobserved variation between entities, I am able to isolate the causal impact of natural disasters on child outcomes. The regressions follow:

(I) $$Y_{icpt} = \alpha + ßD_{icpt-\theta} + C_{icpt} + X_{cpt} + V_{icp} + \theta_t + \gamma_c + \epsilon_{icpt}$$

(J) $$Y_{icpt} = \alpha + ßD_{icpt-\theta} + C_{icpt} + X_{cpt} + \theta_t + \sigma_i + \epsilon_{icpt}$$

(K) $$Y_{icpt} = \alpha + ßD_{icpt-\theta} + C_{icpt} + X_{cpt} + \sigma_i + \delta_{pt} + \epsilon_{icpt}$$

The first regression (I) employs θ_t and γ_c, round and community fixed effects. θ_t is used to control for unobserved entity-invariant variables that shift across time. For instance, the overall quality of education in Vietnam might have increased between Round 2 and Round 3; this effect is captured by the round dummies in vector θ_t as opposed to being inaccurately attributed to the presence/absence of natural disasters. Conversely, γ_c encompasses unobserved time-invariant commune-level attributes, such as commune geography or practices, that impact child outcomes. The controls from (H) remain. In (J), I replace γ_c with σ_i, child fixed effects. By further reducing the level of the entity at which I employ said fixed effects, I am able to control for additional time-invariant differences, such as a child's natural cognitive ability, or innate health. V_{icp} is removed to avoid collinearity with σ_i. Finally, regression (K) replaces θ_t from (J) with δ_{pt}, an interacted province-round fixed effect. I elect to interact round with province rather than commune as floods and erosion tend to extend over a sizeable geographic area, possibly at or larger than a commune. In this manner, commune-round fixed effects are less functional, as a whole commune might be affected by a natural disaster between rounds. Rather, province-round fixed effects account for regional differences across time that are unlikely to be endogenous with the advent of a disaster. These include changes in wealth, composition, legislation, or anti-poverty programs of a province.

I also proceed with a first-differences (FD) model for comparison. The benefits are similar to a child FE model, as the FD estimator avoids bias due to unobserved time-invariant attributes. However, because the data spans more than 2 rounds, the FD estimator is more efficient since the error terms ϵ_{icpt} are likely to be serially correlated.

(L) $$\Delta Y_{icpt} = \alpha + ß\Delta D_{icpt-\theta} + \Delta C_{icpt} + \Delta X_{cpt} + \theta_t + \Delta\epsilon_{icpt}$$

(M) $$\Delta Y_{icpt} = \alpha + ß\Delta D_{icpt-\theta} + \Delta C_{icpt} + \Delta X_{cpt} + \delta_{pt} + \Delta\epsilon_{icpt}$$

The FD model is similar to the FE model, except that differences are computed for all observed variables, i.e. $\Delta Y_{icpt} = Y_{icpt} - Y_{icpt-1}$. The same goes for the independent variable, the two vectors of control variables, and the error term. Akin to the FE model, two separate regressions are run, with (L) and (M) employing round and province-round fixed effects respectively. No time-invariant fixed effects are employed as they are naturally incorporated in the FD model. The interpretations of the predicted coefficients remain the same.

While the above provides a causal average treatment effect (ATE) of natural disasters on child outcomes, part of the reason studies to date have been inconclusive is likely due to the inconsistency of experiences across entities. The YL dataset, being documented at the child level, provides a unique opportunity to delve into heterogenous treatment effects (HTE) such that policymakers might derive richer information for better targeting. As such, leveraging any previously discovered ATE with the preferred FD model, I build on my findings with independent HTE regressions of the form:

(N) $$\Delta Y_{icpt} = \alpha + ß\Delta D_{icpt-\theta} + \lambda(\Delta D_{icpt-\theta} \times H_{icp}) + \Delta C_{icpt} + \Delta X_{cpt} + \delta_{pt} + \Delta\epsilon_{icpt}$$

(O) $$\Delta Y_{icpt} = \alpha + ß\Delta D_{icpt-\theta} + \lambda(\Delta D_{icpt-\theta} \times \Delta W_{icpt-1})$$
$$+\Delta W_{icpt-1} + \Delta C_{icpt} + \Delta X_{cpt} + \delta_{pt} + \Delta\epsilon_{icpt}$$

Regression (N) is utilized for analyzing heterogeneity pertaining to time-invariant variables, of which no FD is taken. H_{icp} is a dummy variable that takes the value of 1 if a child is male or separately, if the child's household is located in a rural area. Therefore, ß now captures the effect of a natural disaster for a female child or urban household, and λ represents the additional difference in impact that a male child or rural household is predicted to face on average. I employ (O) in the case of three time-variant variables. The first is a household's wealth, rated increasingly on an index [0, 1]. The second is a dummy variable, indicating whether or not a household is able to access emergency credit. The third is the amount of paid work hours a child engages in a day. However, since these are likely to be contemporaneous variables that are in turn affected by natural disasters, I choose to lag said variable by 1 round, resulting in W_{icpt-1}. To elaborate an example, a household's wealth is likely to be an indicator of its disaster resilience and its subsequent reaction to an income shock, but this wealth is concurrently affected by the occurrence of a natural disaster. By lagging said variable, I observe how households react to shocks based on pre-existing, rather than endogenous characteristics. Like (N), λ denotes the additional effect based on the variable, with interpretation dependent on particular units. The FD model a la equation (M) is employed as the base for the greatest efficiency and specificity.

6. Results & Discussion

The results for the initial OLS regression (H) are reported in Table 3. At first glance, the relationship between floods and the outcome variables are substantially higher, at values approximately 4 to 5 times that of erosion. The predicted coefficients are also statistically significant across the board, unlike those of erosion. For the most part, the direction of effect for the statistically significant coefficients seem to adhere to intuitive predictions. However, there are two main anomalies. The first lies in conflicting results for the flood and rural dummy variables. In the case of the former, floods appear to share a positive correlation with a child's math results, unlike the negative impact it has on all other outcomes. For the latter, rural households have contradictory and significant results within both educational and health outcomes. Second, the correlation between two variables – a household's access to emergency redit and the presence of a public hospital in the locality – and the outcome variables are counter-intuitive, with negative and significant effects. Additionally, like that of floods, the predicted coefficients for the three other outcomes are inconsistent with that of a child's math scores. This is possibly due to the presence of 3 rounds of data for math results, as opposed to 4 for the rest. Nonetheless, little weightage should be given to the interpretation of these results, as I have yet to account for unobserved spatial and temporal differences, which are important considering the diversity arising from stratification. Neither did I cluster errors at the commune-level, a practice critical in the case of natural disasters which tend to hit minimally at said level. Returning to the OLS' primary aim of identifying suitable controls, the prevalent statistical significance of the predicted coefficients suggest vital relationships with the outcome variables that might be inaccurately attributed to our independent variables if obscured. Therefore, I utilize this same set of controls for all regressions that follow in this paper; this minimizes any selection and/or omitted variable bias.

The FE results from equations (I) to (K) are presented in Tables 4-7; all variations are run twice, with and without controls. Standard errors are clustered at the commune-level, as with the remainder of all regressions in the paper. In terms of education, the occurrence of a flood appears to have significant and negative effects on a child's math scores. Referring to Table 4, controlling for variance across communes and rounds, (2) predicts that the occurrence of a flood leads to a 3.65 pp. decrease in a child's math percentage score, ceteris paribus. This is statistically significant at a 5% level. Unfortunately, the effects taper off in both statistical and economic significance with the introduction of more specific FEs. Regressions (3) and (4) demonstrate no statistically significant effects if child, rather than commune FEs are employed. This is also the case for (5) and (6) that replace round FEs with interacted province-round FEs which account for province-specific time trends as opposed to general time trends. Similarly, as evidenced by (7), erosions are initially predicted to have an economically and statistically significant impact of a 5.23 pp. decrease in a child's math percentage score, but this is quickly diminished when more variation is accounted for. The above suggests that by ignoring heterogeneity in time trends, and/or at the child-level, such as innate child psychology or household preferences, the effects of a flood might be severely overestimated.

Surprisingly, a contrasting pattern is observed when considering the relationship between floods and PPVT scores. Like the above, the effects of floods as predicted from (1) to (6) in Table 5 are negative. However, they increase in economic and statistical significance with the introduction of more precise FEs, possibly due to a reduction in the relative standard errors that might occur from unobserved variation. At a 10% significance level, (6) predicts that controlling for variance across children and province-specific time trends, floods on average cause a 1.24 pp. decrease in a child's PPVT percentage score, ceteris paribus. Nonetheless, this again underscores the importance of controlling for variation, where greater weightage should be placed on the results in columns (6), (12), and (18) of both tables, or the conclusions might otherwise be biased. Concerning erosions, it appears that there are consistently negative and statistically significant effects between the occurrence of erosions and a child's PPVT scores. The effects of erosion on PPVT scores are also higher than that of floods, with (12) in Table 5 predicting a 2.01, as opposed to 1.24 pp. decrease in (6). Regrettably, for both educational outcomes, there is substantial loss in statistical significance when running both floods and erosion together. A drop in economic significance is also noticeable for erosions, suggesting that erosions might have absorbed much of the effects of a concurrent flood when run alone. Still, there is value in considering erosion independently, as wind and climate change are other prevalent causes. Thus, the FE results suggest that natural disasters have a negative impact on child educational outcomes, with consumption behavior likely influenced by the Keynesian AIH. With lower overall income, households reduce spending, leading to poorer child achievement. The economic significance is large, given that the mean percentage score in both cases are about 55%.

Transitioning to child health, the FE results in Table 6 and 7 mostly document negative effects that arise from the advent of a natural disaster. Regrettably, the results for child weight are almost always statistically insignificant. The exception to this is with the use of less specific fixed effects, and with the exclusion of controls, seen in regressions (1) and (13) of Table 7. Similar to the results for math, discounting of controls and more precise fixed effects lead to large economic and statistical overestimations for the effects of floods, and also that of erosions, albeit in the opposite direction. Regarding a child's BMI, Table 6 shows relatively consistent and significant negative effects of floods at an approximate 0.25 reduction, up until province-round FEs are introduced. Considering the low mean and median BMI of the sample at 16.5 and 15.8 respectively, and that the average child borderlines on being underweight, this is economically significant. The onset of a natural disaster could easily push a victim over the 'unhealthy' threshold. With province-round FEs, these effects are reduced drastically and also relegated to statistical insignificance. Specific to erosions, there again appears to be economically large negative effects similar in size to that of floods, but no conclusions can be drawn due to statistical insignificance. These observations hold for floods and erosion regardless of whether they are run independently or concurrently (see (13) to (18)).

The overall inconclusive results support the earlier supposition that model specification is integral. As aforementioned, the YL team has endeavored via stratified sampling to ensure broad representation across geography, experience, and conditions. Hence, I elect to be strict in choice of specification. In particular, the use of interacted province-round alongside child FEs controls for the most variation, and allows accurate isolation of the relationship of interest. In context of this sample, the choice is practical given that recent developments in Vietnam have been less than uniform, and highly contingent on geography and experiences (Bui & Imai 2018). Referring to the results seen in (6), (12), and (18) of Tables 6 and 7, it is then apparent that there is no statistically significant effect relating natural disasters to child health outcomes. In contrast to educational outcomes, the FE results for child health reflect consumption behavior in alignment with Friedman's PIH hypothesis. This suggests that households are more likely to adopt consumption smoothing for child nutrition, which is unsurprising, as the immediate returns from health tend to take precedence over the long-term returns from education for most low-income households in developing countries.

Due to the advent of natural disasters fulfilling the assumption of strict exogeneity, the FE models provide an internally valid causal estimate. However, many studies have emphasized a preference for the usage of FD models in panel data, as the estimators are more efficient due to serial correlation amongst the error terms. This is likely to be the case for the YL dataset. Since I utilize more than 2 rounds of data, the FD and FE estimators yield differing coefficients, and it is useful to compare them here. Tables 8 and 9 present the results from specifications (L) and (M), the FD estimators for child educational outcomes. The former demonstrates that floods now have consistent and statistically significant negative effects on a child's math scores. From (4), controlling for unobserved time-invariant attributes and province-specific time trends, experiencing a flood is predicted to cause a 2.6 pp. decrease in a child's math percentage score, ceteris paribus. This result is comparable to the FE model, (6) in Table 4, where we control for similar attributes, except the FD estimator retains statistical significance at 10%. The impacts of erosion are less salient, with economically large negative effects, but a loss in statistical significance when controlling for province-specific time trends. Running both floods and erosion together does not change these findings.

Pertaining to a child's PPVT scores, the FD estimator affirms the earlier finding that erosions maintain more substantial and significant effects than floods. Now, (8) in Table 9 predicts that an erosion causes on average a 2.14 pp. decrease in a child's PPVT score, and this is statistically significant at a 1% level. Floods have little economic and statistical significance. Unlike the FE estimator, running both floods and erosions together in a FD model does not reduce the statistical significance of the findings, though there is a minor decline in the absolute value of said effects, resulting in the predicted 1.93 pp. drop in (12). Hitherto, the FD estimator is noticeably more efficient than the FE estimator. Focusing on the model that accounts for the most variation (i.e. (4), (8), or (12) in Tables 8 and 9), it is apparent that floods have a negative and significant effect on a child's math scores, while PPVT scores are mainly affected by erosion. The FD estimators, with their efficiency and greater statistical significance, validate the previous conjecture that the Keynesian AIH is applicable to child educational outcomes, which are sensitive to the experience of a natural disaster, presumably due to its effect on household income. The findings are also economically significant. Given that the mean performance of a child in both tests is 55 pp, such disasters cause an approximate 4% drop in a child's achievement.

Similarly, the FD findings on child health outcomes in Tables 10 and 11 affirm the statistical insignificance found in the FE model. Additionally, any projected economic significance is further reduced. As observed, the majority of predicted coefficients are negative. Unfortunately, there is little statistical significance except in (1) and (2) of Table 10, which emerge to be large overestimates both statistically and economically when dummies for province-specific time trends are introduced. The overall loss in economic significance is also visibly pervasive. Comparing (12) in Table 10 to (18) in Table 6, the impact of floods on a child's BMI is reduced by more than half, while the predicted impact of erosions drops by almost 4 times. Between floods and erosions, (12) in Table 10 predicts a less than 0.06 drop in BMI, which is almost negligible. This recurs in the case of weight. (12) in Table 11 suggests that floods and erosions on average lead to an approximate 100g reduction in a child's weight. Compared to (18) in Table 7, which predicts up to a 350g loss, the difference is striking. Nonetheless, no valid conclusion can be drawn about a causal relationship between natural disasters and child health outcomes with the use of the FD estimators. Rather, the insignificant results mirror the initial speculation that Friedman's PIH applies to child health outcomes, where consumption smoothing assures stable spending in this domain. Like the case for educational outcomes, the FD estimators merely rehash earlier findings but with better reliability and also to a greater effect.

Building on these discoveries, I undertake heterogeneity analyses via regressions (N) and (O). Specifically, I look at the preferred FD model, and where there are established ATEs between the independent and dependent variables. Thus, two relationships are scrutinized, namely the impact of floods on a child's math scores, and the impact of erosions on a child's PPVT scores. The results are seen in Table 12. Regressions (1), (3), (6), and (8) note no significant additional effects of natural disasters on educational outcomes that can be attributed to a child's gender, or a household's wealth. The former is surprising because it defies a popular strand of literature, which contends that gender gaps prevail in the event of household income shocks (Foster 1995, Bjorkman-Nyqvist 2013). Though the latter might appear counter-intuitive, a possible explanation might lie in the YL's computing of the wealth index. The index is an aggregate of a household's housing quality, access to services, and ownership of consumer durables. Though durables may be liquidated to facilitate consumption smoothing (Beegle et al. 2006), the wealth index ignores other more immediate and fundamental coping mechanisms, including a household's existing savings, or agricultural livestock/assets, which may be of equal or larger influence. This might cause the large standard errors and consequent statistical insignificance of the interacted variables.

Comparing (2) and (7), the statistical insignificance in (2) suggests that the effect of floods are largely homogenous across rural and urban households. In contrast, (7) shows that erosion is especially harmful to rural households, almost doubling the negative impact on a child's PPVT scores from a 3.52 pp. drop to a 6.3 pp. drop, at a 1% significance level. The divergence in results is understandable, as floods have equally devastating effects on both rural and urban areas, while erosion mainly hampers agricultural activities, which are a common means of subsistence for rural households. This dissimilarity is repeated in terms of a household's access to emergency credit. Seen in (9), the additional effects for a household that faces erosion but with such financial reinforcement at its disposal is positively large and significant, so much so that it more than offsets the ATE of erosion on child PPVT scores.. Unfortunately, as (4) illustrates, the same cannot be said for floods and math scores, with the predicted coefficient of the interacted term having little economic and statistical significance. Perhaps this is because erosions alone are less physically destructive than floods, and a small emergency sum allows appreciably more alleviation.

Finally, regressions (5) and (10) illustrate at a 1% significance level that children with longer working hours are hurt less, or may even benefit from the onset of a disaster. The results suggest that the negative effects of a natural disaster on educational outcomes are actually eroded by every lagged additional hour of work a child used to engage in per day, at an approximate 8 pp. increase in test scores. This result is extremely startling, since the mean performance of a child sits at about 55%, and that our earlier FD results suggest negative effects chiefly in the domain of 2 pp.. One possible implication could be that child laborers actually benefit from physical destruction, as there is less arable land and productive work requiring their input, which in turn allows them to dedicate more effort toward their studies. However, I am cautious about drawing this conclusion, as the sample size of children who engage in paid labor is small, with only about 11 out of the 2000 children in the sample doing so. The abnormality of this finding might lie in the atypical experiences of these few children.

Nonetheless, to summarize the findings of the above section, the empirical results from the FE and FD models both reveal that natural disasters are particularly harmful to child educational outcomes and not much so for child health outcomes, though the predicted coefficients across all 4 outcome variables are mostly negative. These findings allude to Keynesian-inspired spending behavior for educational outcomes, and the relevance of consumption smoothing a la the Friedman PIH in terms of health outcomes. The direct effects of natural disasters on child educational outcomes manifest in a flood's impact on math scores, and erosion's impact on PPVT scores. Model specification is pertinent, although the FD model, with its added efficiency, is more consistent in results. Relating to heterogeneous effects, child gender and household wealth are not differential factors. Erosions tend to exacerbate the rural-urban divide, but are largely counteracted by a household's readiness for financial emergencies. Children who work more seemingly benefit from both floods and erosions, though the empirical results are suspect due to a small sample size.

7. Sensitivity Analyses

Returning to the main interests of this paper, I proceed with a variety of sensitivity analyses to further validate the conclusions made. First, I run a leads test, to ensure that there is no correlation between a household's characteristics and its reported experience with natural disasters. I do so to discount possible biases that might arise from the self-reporting of the independent variable.

$$D_{icpt+\theta} = \alpha + Y_{icpt} + C_{icpt} + \sigma_i + \delta_{pt} + \epsilon_{icpt} \quad \text{(P)}$$

The equation (P) is a simple OLS formulation with child and province-round FEs. Here, $D_{icpt+\theta}$ represents whether or not a child/household reports having experienced a natural disaster in the next interim $t + \theta$, with the information recorded in the next round of the survey, $t + 1$. The outcome variables and controls are all derived from the current round in the survey, t. In order for the strict exogeneity assumption to hold, the respective coefficients should be statistically insignificant, such that there is no correlation between any specific child outcome/characteristic and the later reporting of a natural disaster. This would suggest that no particular type of child or household tends to over/under-report experiences with natural disasters that may bias the results. The results for (P) are seen in columns (1) and (2) of Table 13, with regressions for floods and erosions run independently. Notably, none of the predicted coefficients carry any statistical significance; this is an encouraging outcome, which suggests that most households are responsible in their reporting. Taken in tandem with the fact these disasters are induced by nature, I contend that the exogeneity assumption for the independent variables within this study holds.

Second, taking inspiration from Rothstein (2010), I run a falsification test to confirm that any effects on child outcomes that I previously derived were not random occurrences. Similar to the above, the falsification test exploits the use of leads, and substitutes them within the specifications that I have confirmed to be the most reliable in Section 6. These are the FD models relating floods to child math scores, and erosion to child PPVT scores. I generate two rounds of leads, $t+\theta$ and $t+1+\theta$ and run (M) in Section 5 except with the independent variable being determined in the future interim. The premise of this test is that future disasters cannot affect a child's past outcomes.

(Q) $$\Delta Y_{icpt} = \alpha + ß\Delta D_{icpt+\theta} + \Delta C_{icpt} + \Delta X_{cpt} + \delta_{pt} + \Delta\epsilon_{icpt}$$

(R) $$\Delta Y_{icpt} = \alpha + ß\Delta D_{icpt+1+\theta} + \Delta C_{icpt} + \Delta X_{cpt} + \delta_{pt} + \Delta\epsilon_{icpt}$$

Columns (3), (4), and (5) of Table 13 describe the results. Importantly, (R) is invalid for the math outcome, as there are only three rounds of data, with the YC children being too young in Round 2 for any applicable testing. With an FD model, generating a 2 round lead results in no testable data. Fortunately, the results for all three regressions illustrate no statistical significance for the predicted coefficients ß. This indicates that the FD model and its effects are only valid for when the use of the variables is logical, and not when they are carelessly applied. The above lends strength to the argument that the effects derived earlier were authentic and found statistical significance from their causal relationship with the outcome variables. Together, the results from the tests predicated on the use of leads are encouraging, though it must be underscored that these are negative rather than positive tests. What I have done merely shows that the FD model and its results are not invalid. Success in a positive test will provide far stronger validation for the effects that I seek to draw conclusions upon.

Third, I proceed with a quantitative sub-sample difference-in-differences technique previously used by Autor (2003). This alternative methodology is used as positive verification. If the new results commensurate with the effects established in the earlier FD model, it provides reasonable proof that my findings were appropriate. I first construct a new set of treatment and control. The former comprises children who never experienced a flood except between Rounds 4 and 5, with the information recorded in Round 5. The latter consists of children who never experienced a flood across all 4 rounds used in this study. I am unable to undertake similar analysis for erosions because of a much lower rate of occurrence, which leads to the treatment group comprising only 2 children. Controlling for all other observed and unobserved variables, causality toward the outcome variable should be cleaner, given that there remains only one exogenous difference between the two groups.

$$Y_{icpt} = \alpha + T_{icp} + \sum_{j=3}^{5} \eta_j (T_{icp} \times Round_t^j) + C_{icpt} + X_{cpt} + \sigma_i + \delta_{pt} + \epsilon_{icpt} \quad \text{(T)}$$

T_{icp} is now a dummy variable that adopts a value of 1 for the treatment group. $Round_t^j$ represent dummies for each round between 3 and 5. Since there are 4 rounds of data, I exclude data from Round 2 to avoid collinearity and also to utilize it as a baseline. For the limited math scores, data from Round 3 is discounted instead. Then, η_j captures the difference in the difference of the outcome variables for the treatment group versus the control group in a given round as opposed to the second round.

Prior to the onset of a flood, there should be no differences in change of outcomes (η_3 and η_4 should be insignificant), suggesting that both groups were observing parallel trends prior to the shock, and that the control group is a suitable counterfactual for the treatment group. If there is indeed a treatment effect, η_5 should be statistically significant and $\neq 0$, which indicates that the treatment group broke from the parallel pre-trend post-treatment, most likely due to the experience of floods between Rounds 4 and 5. Following the strict exogeneity assumption, this would be seen as the causal effect of a flood on child outcomes. Table 14 shows the empirical results for (T) with Fig. 1-4 providing visual representations of the confidence intervals. From columns (1) to (4) it is evident that there are no statistically significant effects. The corresponding figures also demonstrate that the predicted coefficients across rounds are largely stagnant and are not significantly different from 0 at a 5% level, regardless of the advent of a flood between rounds 4 and 5 as represented by the vertical red line. While this is promising for η_3 and η_4 as it suggests parallel pre-trends, the insignificance of η_5 indicates that the treatment group did not differ from the control group post-shock, and by extension implies that floods have no overall effect on child outcomes. This corresponds with my earlier position on health outcomes, but undermines the conclusions drawn for education, and in particular, math scores, which are seen to be directly affected by floods in the FD model. A possible reason for this discovery is the relatively small treatment sample size of 30. Thus, the differential effects of the 30 might be uncharacteristic of a larger population. In contrast, the control group comprises more than 1000 children. Nonetheless, the failure to provide validation here must be underscored, and the possible bias of concluding solely based on the FD estimators noted.

Fourth, adapting Hyder (2015), instead of relying on self-reported idiosyncratic shocks for the independent variables, I re-sample (K) with information derived from commune-level informants. Under the YL methodology, each commune typically provides 5-15 independent and disinterested informants to provide complementary commune-level data. The test is fairly straightforward, and simply replaces $D_{icpt+\theta}$ with $ID_{cpt-\theta}$. This new independent variable captures whether or not commune-informants collectively indicate that the commune experienced a natural disaster in the interim with the last round. Unfortunately, commune-level information was only recorded for floods, and not that of erosion. Hence, I am only able to enact the following two regressions for floods.

(S) $$Y_{icpt} = \alpha + ßID_{cpt-\theta} + C_{icpt} + X_{cpt} + \sigma_i + \delta_{pt} + \epsilon_{icpt}$$

(T) $$\Delta Y_{icpt} = \alpha + ß\Delta ID_{cpt-\theta} + \Delta C_{icpt} + \Delta X_{cpt} + \delta_{pt} + \Delta\epsilon_{icpt}$$

This test assumes that communes have relatively uniform experiences in terms of floods. There are a total of 11,112 communes in Vietnam, making each commune geographically small. In particular, an area is only required to have a primary school, health center, post office, and market to be considered a commune. Thus, this would be a reasonable assumption. Even if the actual physical impacts might vary slightly between households, spillovers from inter-dependencies within a commune invariably affect the children/households. The above test functions as a second positive verification. It is simultaneously a sign verification test to counteract biases from self-reporting, and one that provides further information that distinguishes between the effects of idiosyncratic and commune-level shocks.

By looking at the results of regression (S) and (T) in Table 15, three insights can be gleamed. First, if I follow the assumption that communes have relatively uniform experiences in terms of floods, the statistical insignificance of the coefficients are discouraging for educational outcomes but favorable for health outcomes. This insinuates that the earlier findings on education were again, particular to the FD child-level specification, and provides no real validation. Second, if I instead reject the assumption of relatively uniform experiences, but assume responsible self-reporting, effects between community and idiosyncratic shocks can be compared. A key finding from Hyder (2015) is that the former tends to hit harder than the latter, simply because poorer communities tend to pool risks, and come together to support individuals in times of need. The results here are mixed. By directly comparing the economic significance of (T) with (M), (i.e. (5) in Table 15 with (4) in Table 8), it seems that commune-level shocks hurt a child's math scores less but damage the rest of the outcomes more. The results here are partial toward Hyder's conclusion though the lack of statistical significance begets care in interpretation. Finally, despite a lack of statistical significance, the results evidently demonstrate the greater consistency of the FD versus FE model and ratifies my choice. The former demonstrates stable negative coefficients, similar, though not absolutely, in value to those of Section 6. The FE results are more contradictory. That the economic value of the FD coefficients do not vary drastically with before also suggest that individual self-reporting was largely accurate and responsible.

The above-mentioned sensitivity analyses fall into two distinct categories. Those based on the use of leads were principally employed as negative tests. Consequently, the results support my earlier suppositions, and indicate that there was little bias in self-reporting and no functional defect in the FD model. Unfortunately, the positive tests, which tend to hold more weightage in verification of effects, are more complex. For health outcomes, the lack of significant results are commensurate with my assertion that consumption smoothing a la Friedman's PIH hypothesis is dominant in child health spending. In terms of education, they challenge my earlier findings, implying that the negative effects of disasters on child education outcomes via reduced income and the Keynesian AIH were specific to the FD methodology and not broadly valid. The fourth test serendipitously confirms the superiority of the FD model. However, it is important to highlight that the positive sensitivity analyses were challenged by two fundamental issues. The first is a small sample size, whether in the 30 children classified as treatment from the 3rd test, or in the 31 communes that were used in the YL study, and accordingly the 4th test, which allowed for little comparable variance. Additionally, the positive tests lacked critical data on erosions, which I observed to have the most consistent results in its relationship with PPVT scores across both FE and FD models. Internalizing the above, the conclusions drawn from the positive sensitivity analyses should be provisional and taken cautiously.

8. Concluding Remarks

At the household level, the relationship between natural disasters and child outcomes has long been contentious. Using data from a unique longitudinal study of childhood poverty in Vietnam, I have provided novel insight into effects relevant to the local context. In doing so, I contribute to a growing stand of literature that seeks to disentangle complex relationships with micro-data, and also seek to bridge the geographic gap to provide accurate information for local policymakers, in order to facilitate better targeting. The key findings of this paper are as follows:

1) Controlling for observed and unobserved differences across entity and entity-specific temporality, the FD model predicts that the advent of a flood leads to an average **2.6 pp.** decrease in a child's math percentage score, ceteris paribus. This is statistically significant at a 10% level. Similarly,

erosions are predicted to decrease a child's PPVT percentage score by **2.14 pp.**, with 1% statistical significance. The results and their statistical significance are consistent, albeit with minor value changes, when both floods and erosion are run together. Economically speaking, the findings are significant, representing an approximate 4% reduction in child performance from a 55 pp. mean.

2) There are unfortunately no statistically significant effects within the FD model linking floods to PPVT, and erosions to math scores, although both relationships display negative coefficients.
3) The FD model also predicts no statistically significant effects linking both health outcomes to the two natural disasters. The economic significance is also minute, though all predicted coefficients carry negative signs. These suggest that consumption smoothing is salient for health spending.
4) The FD results affirm the trends demonstrated in the FE model, although with greater efficiency due to the serial correlation between error terms. Thus, the use of the FD model, and its comparison with the FE results allows us to conclude the above with greater certainty.
5) Leveraging the statistically significant ATEs as advanced in the first point, further heterogeneity analyses is undertaken. The results suggests no differential effects based on gender and household wealth. Erosions hurt rural households in particular, but are also easily offset by access to emergency finances, implying smaller economic effects. Children who have longer working hours surprisingly benefit from both floods and erosions, though the small sample size is suspect.
6) The results from the negative sensitivity analyses suggest that the strict exogeneity assumption which prefaces our results hold. There appears to be no bias in self-reporting and no ostensible deficiency in the FD model. Hence, it would be fair to assert the internal validity of the findings.
7) While the lack of statistical significance amongst the positive sensitivity analyses verifies earlier conclusions pertaining to health, they also undermine any earlier findings on education, implying sensitivity to model specification. Nonetheless, the small sample size and lack of data on erosions in these analyses must be taken into consideration, and the results interpreted prudently.

8) On balance, in relation to the theoretical framework, the results indicate that consumption smoothing a la the Friedman PIH best explains the lack of impact by natural disasters on child health outcomes, while the Keynesian AIH is relevant to child educational outcomes.

Taken together, it appears that the YL data displays rather tempered linkages between natural disasters and children outcomes. This might be because of the frequent exposure to high rainfall in tropical countries like Vietnam, which invariably builds greater resilience and encourages the development of sustainable coping mechanisms among its population. Thus, consumption smoothing and innate disaster resilience work in tandem to counteract potentially larger effects. Still, it must be underscored that almost all of the predicted coefficients across the FE and FD models are negative in value, which is an important finding in itself, though large standard errors may challenge attempts at statistical interpretation. In terms of the effects found hitherto, it is unmistakable that policymakers might be better served looking toward child educational rather than health outcomes in times of crisis, of which the former, with a long-term return horizon, is seemingly sacrificed before the latter. Doing so via an integrated disaster management framework will help Vietnam elevate many of its poorest and prevent further loss to human capital accumulation, which is especially important as it grows into a regional and global economic power.

Alongside the above, there are four key avenues for further research. First, my study looks primarily at floods and erosions, which are selected based on the availability of data and their high frequency of occurrence in Vietnam. Notably, the YL data does not distinguish between standalone floods, and floods that occur together with, or as a consequence of larger disasters, such as typhoons which are pertinent to the Vietnamese context. The omission of this information might bias the predicted coefficients, though the threat is low, given the relatively low rate of incidence and that the provinces under consideration in the YL data are comparatively unaffected (Mah 2018). Nevertheless, there is clear value for future research to collate data which distinguishes between the two, as diminishing their conflation might lead to more pronounced and accurate effects. Second, methodological improvements can be made with the incorporation of GIS data. In particular, the matching of rainfall data at a commune grid level and reflecting deviations from the mean will enable far greater accuracy in determining the severity of a flood. This allows for more precise variation than the current dummy variable, and also counteracts any potential bias from reporting. Unfortunately, the YL team has rejected the release of geo-specific data due to privacy concerns, and together with time constraints, has rendered this approach unfeasible for the current study. If future data permits, this would be a welcome improvement. Third, while the stratification in sample selection supports the external validity of my results for *poor* households in Vietnam, these might be less applicable in context for all households across the wealth strata of the country. Additionally, the over-poor selection bias might have caused the largely homogenous results that I have observed. Perhaps further research might look to the general Vietnam Household Living Standards Surveys, which would improve the within-country external validity of said findings, though it must be highlighted that these are less specific to children, and accordingly have limited information on child outcomes. Finally, based on an overwhelming consensus amongst past literature, I have attempted to explain the relationship between natural disasters and child outcomes via theoretical frameworks underlying consumption behavior. To draw an even cleaner causal link with the respective income hypotheses, it might be worthwhile to affirm the relationship with the interim mechanisms, including household income, and specific consumption expenditure, data on which is lacking here. Categorically, the results of this study are foundational. Accounting for both time and data constraints, I have endeavored to bridge the geographic and information gap in Vietnam, and provide localized evidence that addresses the often disputed relationship between natural disasters and corollary household outcomes. Yet as evidenced, there are a number of ways to build on these findings, and only in doing so will there be greater internal and external validity most appropriate to the needs of the respective stakeholders.

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Appendix

Table 1: Distribution of YC Observations by Province (2002)

Province	Freq. (%)	Geography	Provincial GDP Per Capita	Population Under Poverty Line (%)	HDI Rank	Population ('000s)	Number of General Education Schools	Access to Electricity (%)
Ben Tre	20	Coastal	258	22	27	1289	374	73.9
Da Nang	20	City	409	12	4	740	157	99.7
Hung Yen	20	River Delta	209	13	14	1092	359	99.0
Lao Cai	20	Mountain	144	22	55	626	374	52.0
Phu Yen	20	Coastal	202	9	49	817	263	91.0

Notes: HDI Ranking out of 61 provinces in descending order.
12.9% of the entire Vietnamese population was below the poverty line in 2002.

Source: GSO Vietnam (2002) & Nguyen (2008)

Table 2: Descriptive Statistics

	(1) N	(2) mean	(3) median	(4) min	(5) max
Panel A: Independent Variables					
Flood (=1 if Yes in Interim)	7634	0.0711292	0	0	1
Erosion (=1 if Yes in Interim)	7627	0.0144224	0	0	1
Panel B: Dependent Variables					
Math (% Correct)	5676	0.5567536	0.533334	0	1
PPVT (% Correct)	7437	0.5570299	0.627451	0	0.9868421
BMI	7745	16.58536	15.80277	7.944597	34.67779
Weight (Kilograms)	7757	30.74772	27.1	10	99
Panel C: Key Child Characteristics					
Ability to Raise Money (=1 if Yes)	7771	0.9124952	1	0	1
Access to Sanitation (=1 if Yes)	7792	0.6923768	1	0	1
Current Loan (=1 if Yes)	7794	0.455222	0	0	1
Hours at School/Day	7606	5.214844	5	0	12
Hours at Work/Day	7605	0.183169	0	0	15
Household Size	7801	4.544674	4	1	16
Male (=1 if Yes)	7880	0.5142132	1	0	1
Monthly Food Expenditure ('0000 VND)	7683	30.66186	24.64561	0	4055.529
Rural (=1 if Yes)	7773	0.7872121	1	0	1
Wealth (Increasing Index [0, 1])	7749	0.6242575	0.6574074	0.0061728	1
Panel D: Key Community Characteristics					
Population Size ('000)	7121	9.952991	9.547	1.717	21.787
Poverty (% Households)	7121	0.158824	0.115	0.0024631	0.802589
Presence of Hospital (=1 if Yes)	7121	0.6403595	1	0	1

Except for math scores which are recorded for only 3 rounds, all other observations span 4 rounds, with 2000 children in 31 communes and 5 provinces. Administrative units in Vietnam run from communes to districts to provinces.

Table 3: OLS

	(1)	(2)	(3)	(4)
VARIABLES	% Math	% PPVT	BMI	Weight
Flood	0.0597***	-0.0905***	-0.995***	-5.554***
	(0.0115)	(0.0107)	(0.132)	(0.563)
Erosion	-0.00841	0.0202	-0.135	-0.497
	(0.0260)	(0.0228)	(0.285)	(1.207)
Male	-0.0186***	0.00204	0.164**	1.191***
	(0.00552)	(0.00533)	(0.0671)	(0.287)
Rural	-0.0484***	0.0939***	-0.486***	2.680***
	(0.0106)	(0.0100)	(0.125)	(0.532)
Wealth Index	0.0857***	0.615***	4.112***	30.33***
	(0.0286)	(0.0253)	(0.317)	(1.353)
HH Size	0.000614	-0.000830	-0.0420*	-0.244**
	(0.00205)	(0.00195)	(0.0244)	(0.104)
Current Loan	0.00714	-0.0306***	-0.280***	-1.681***
	(0.00558)	(0.00539)	(0.0678)	(0.290)
Monthly Food Exp.	1.80e-05	0.000129***	0.00115**	0.00512**
	(3.95e-05)	(4.37e-05)	(0.000560)	(0.00239)
Access to Credit	0.0418***	-0.0544***	-0.0281	-1.957***
	(0.00586)	(0.00561)	(0.0708)	(0.302)
Hours at Work/Day	-0.0185***	-0.0125***	0.249***	1.420***
	(0.00223)	(0.00230)	(0.0293)	(0.125)
Hours at School/Day	0.0106***	0.00246*	-0.00787	-0.197***
	(0.00157)	(0.00134)	(0.0167)	(0.0712)
Access to Sanitation	-0.00252	0.0394***	0.287***	1.308***
	(0.00865)	(0.00808)	(0.102)	(0.436)
Commune Pop. Size	0.00225***	-0.000446	0.0135	0.146***
	(0.000861)	(0.000836)	(0.0105)	(0.0450)
Commune Hospital	0.0604***	-0.151***	-1.031***	-7.924***
	(0.00598)	(0.00576)	(0.0730)	(0.312)
Commune %Poverty	-0.332***	-0.460***	-0.523*	-9.995***
	(0.0283)	(0.0243)	(0.305)	(1.303)
Constant	0.451***	0.267***	15.06***	17.04***
	(0.0291)	(0.0267)	(0.332)	(1.418)
Observations	4,871	6,396	6,639	6,650
R-squared	0.190	0.386	0.159	0.313

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 4: FE Math (% Correct)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
VARIABLES	% Math	% Math	% Math	% Math	% Math	% Math	% Math	% Math	% Math	% Math	% Math	% Math	% Math	% Math	% Math	% Math	% Math	% Math
Flood	-0.0418***	-0.0365**	-0.0367	-0.0378	-0.0262	-0.0278							-0.0404***	-0.0363**	-0.0354	-0.0366	-0.0258	-0.0278
	(0.0150)	(0.0154)	(0.0264)	(0.0254)	(0.0215)	(0.0218)							(0.0147)	(0.0151)	(0.0261)	(0.0249)	(0.0210)	(0.0213)
Erosion							-0.0523**	-0.0386	-0.0467	-0.0494	-0.0309	-0.0299	-0.0354	-0.0234	-0.0312	-0.0326	-0.0204	-0.0181
							(0.0255)	(0.0283)	(0.0281)	(0.0306)	(0.0303)	(0.0317)	(0.0234)	(0.0272)	(0.0255)	(0.0279)	(0.0284)	(0.0298)
Constant	0.650***	0.547***	0.652***	0.721***	0.659***	0.691***	0.477***	0.355***	0.648***	0.721***	0.651***	0.691***	0.651***	0.356***	0.653***	0.722***	0.658***	0.691***
	(0.00783)	(0.0628)	(0.0108)	(0.0778)	(0.0197)	(0.0745)	(0.00539)	(0.0695)	(0.0107)	(0.0779)	(0.0222)	(0.0751)	(0.00794)	(0.0695)	(0.0110)	(0.0772)	(0.0194)	(0.0746)
Controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Fixed Effects	Commune, Round	Commune, Round	Child, Round	Child, Round	Child, Province-Round	Child, Province-Round	Commune, Round	Commune, Round	Child, Round	Child, Round	Child, Province-Round	Child, Province-Round	Commune, Round	Commune, Round	Child, Round	Child, Round	Child, Province-Round	Child, Province-Round
Observations	4,986	4,896	4,986	4,896	4,986	4,896	4,980	4,890	4,980	4,890	4,980	4,890	4,979	4,889	4,979	4,889	4,979	4,889
R-squared	0.318	0.331	0.722	0.724	0.733	0.734	0.316	0.329	0.722	0.723	0.732	0.734	0.318	0.331	0.723	0.724	0.733	0.734
No. of Children	1,850	1,843	1,850	1,843	1,850	1,843	1,850	1,843	1,850	1,843	1,850	1,843	1,850	1,843	1,850	1,843	1,850	1,843

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 5: FE PPVT (% Correct)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
VARIABLES	% PPVT	% PPVT	% PPVT	% PPVT	% PPVT	% PPVT	% PPVT	% PPVT	% PPVT	% PPVT	% PPVT	% PPVT	% PPVT	% PPVT	% PPVT	% PPVT	% PPVT	% PPVT
Flood	-0.00696	-0.00240	-0.00854	-0.00750	-0.0124*	-0.0124*							-0.00618	-0.00230	-0.00797	-0.00723	-0.0117	-0.0118
	(0.00752)	(0.00723)	(0.0105)	(0.0104)	(0.00682)	(0.00689)							(0.00777)	(0.00739)	(0.0108)	(0.0106)	(0.00726)	(0.00736)
Erosion							-0.0213**	-0.0109	-0.0179*	-0.0140	-0.0219**	-0.0201*	-0.0193**	-0.0102	-0.0151	-0.0114	-0.0181*	-0.0164
							(0.00913)	(0.00894)	(0.0104)	(0.0116)	(0.00955)	(0.0112)	(0.00915)	(0.00896)	(0.0107)	(0.0116)	(0.0100)	(0.0114)
Constant	0.180***	0.210***	0.180***	0.217***	0.174***	0.219**	0.180***	0.212***	0.180***	0.220***	0.172***	0.241***	0.180***	0.212***	0.180***	0.219***	0.175***	0.223**
	(0.00636)	(0.0550)	(0.00768)	(0.0527)	(0.00956)	(0.0841)	(0.00627)	(0.0556)	(0.00754)	(0.0535)	(0.00916)	(0.0855)	(0.00638)	(0.0556)	(0.00769)	(0.0534)	(0.00939)	(0.0842)
Controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Fixed Effects	Commune, Round	Commune, Round	Child, Round	Child, Round	Child, Province-Round	Child, Province-Round	Commune, Round	Commune, Round	Child, Round	Child, Round	Child, Province-Round	Child, Province-Round	Commune, Round	Commune, Round	Child, Round	Child, Round	Child, Province-Round	Child, Province-Round
Observations	6,655	6,549	6,655	6,549	6,655	6,549	6,648	6,542	6,648	6,542	6,648	6,542	6,647	6,541	6,647	6,541	6,647	6,541
R-squared	0.858	0.867	0.917	0.919	0.923	0.924	0.858	0.867	0.917	0.919	0.923	0.924	0.858	0.867	0.917	0.919	0.923	0.924
No. of Children	1,897	1,894	1,897	1,894	1,897	1,894	1,897	1,894	1,897	1,894	1,897	1,894	1,897	1,894	1,897	1,894	1,897	1,894

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 6: FE BMI

VARIABLES	(1) BMI	(2) BMI	(3) BMI	(4) BMI	(5) BMI	(6) BMI	(7) BMI	(8) BMI	(9) BMI	(10) BMI	(11) BMI	(12) BMI	(13) BMI	(14) BMI	(15) BMI	(16) BMI	(17) BMI	(18) BMI
Flood	-0.308***	-0.258**	-0.226**	-0.224*	-0.0801	-0.0686							-0.291***	-0.248**	-0.216*	-0.216*	-0.0613	-0.0523
	(0.107)	(0.116)	(0.110)	(0.114)	(0.112)	(0.117)							(0.106)	(0.117)	(0.108)	(0.112)	(0.109)	(0.115)
Erosion							-0.151	-0.0359	-0.213	-0.187	-0.255	-0.230	-0.0564	0.0441	-0.141	-0.114	-0.236	-0.214
							(0.102)	(0.0935)	(0.181)	(0.196)	(0.202)	(0.196)	(0.0954)	(0.0852)	(0.163)	(0.173)	(0.195)	(0.190)
Constant	14.96***	13.65***	14.94***	14.86***	14.82***	15.23***	14.93***	13.66***	14.92***	14.87***	14.97***	15.24***	14.95***	13.66***	14.94***	14.86***	14.84***	15.23***
	(0.0645)	(0.481)	(0.0820)	(0.528)	(0.222)	(0.527)	(0.0679)	(0.483)	(0.0808)	(0.540)	(0.230)	(0.529)	(0.0649)	(0.481)	(0.0816)	(0.530)	(0.215)	(0.529)
Controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Fixed Effects	Commune, Round	Commune, Round	Child, Round	Child, Round	Child, Province-Round	Child, Province-Round	Commune, Round	Commune, Round	Child, Round	Child, Round	Child, Province-Round	Child, Province-Round	Commune, Round	Commune, Round	Child, Round	Child, Round	Child, Province-Round	Child, Province-Round
Observations	6,941	6,829	6,941	6,829	6,941	6,829	6,934	6,822	6,934	6,822	6,934	6,822	6,933	6,821	6,933	6,821	6,933	6,821
R-squared	0.405	0.415	0.819	0.821	0.824	0.825	0.404	0.415	0.819	0.821	0.824	0.825	0.405	0.415	0.819	0.821	0.824	0.825
No. of Children	1,903	1,901	1,903	1,901	1,903	1,901	1,903	1,901	1,903	1,901	1,903	1,901	1,903	1,901	1,903	1,901	1,903	1,901

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 7: FE Weight

VARIABLES	(1) Weight	(2) Weight	(3) Weight	(4) Weight	(5) Weight	(6) Weight	(7) Weight	(8) Weight	(9) Weight	(10) Weight	(11) Weight	(12) Weight	(13) Weight	(14) Weight	(15) Weight	(16) Weight	(17) Weight	(18) Weight
Flood	-0.544**	-0.376	-0.289	-0.303	-0.161	-0.111							-0.546**	-0.398	-0.312	-0.328	-0.133	-0.0927
	(0.234)	(0.260)	(0.260)	(0.289)	(0.308)	(0.313)							(0.231)	(0.260)	(0.261)	(0.290)	(0.306)	(0.315)
Erosion							0.208	0.605	0.110	0.160	-0.503	-0.377	0.387*	0.734	0.214	0.271	-0.462	-0.348
							(0.205)	(0.586)	(0.525)	(0.662)	(0.492)	(0.511)	(0.193)	(0.693)	(0.497)	(0.625)	(0.490)	(0.515)
Constant	16.51***	11.45***	16.48***	16.69***	19.86***	19.25***	16.45***	11.45***	16.45***	16.67***	18.84***	19.19***	16.50***	11.44***	16.48***	16.66***	19.89***	19.25***
	(0.213)	(1.830)	(0.273)	(1.955)	(0.868)	(1.742)	(0.223)	(1.830)	(0.265)	(1.968)	(0.734)	(1.722)	(0.215)	(1.827)	(0.273)	(1.961)	(0.852)	(1.751)
Controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Fixed Effects	Commune, Round	Commune, Round	Child, Round	Child, Round	Child, Province-Round	Child, Province-Round	Commune, Round	Commune, Round	Child, Round	Child, Round	Child, Province-Round	Child, Province-Round	Commune, Round	Commune, Round	Child, Round	Child, Round	Child, Province-Round	Child, Province-Round
Observations	6,953	6,840	6,953	6,840	6,953	6,840	6,946	6,833	6,946	6,833	6,946	6,833	6,945	6,832	6,945	6,832	6,945	6,832
R-squared	0.798	0.803	0.932	0.933	0.935	0.935	0.798	0.803	0.931	0.933	0.935	0.935	0.798	0.803	0.931	0.933	0.935	0.935
No. of Children	1,904	1,902	1,904	1,902	1,904	1,902	1,904	1,902	1,904	1,902	1,904	1,902	1,904	1,902	1,904	1,902	1,904	1,902

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 8: FD Math (% Correct)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Δ% Math	Δ% Math	Δ% Math	Δ% Math	Δ% Math	Δ% Math	Δ% Math	Δ% Math	Δ% Math	Δ% Math	Δ% Math	Δ% Math
ΔFlood	-0.0438**	-0.0444**	-0.0250*	-0.0260*					-0.0424**	-0.0424**	-0.0252*	-0.0259*
	(0.0184)	(0.0169)	(0.0141)	(0.0134)					(0.0181)	(0.0162)	(0.0138)	(0.0131)
ΔErosion					-0.0520*	-0.0614**	-0.0241	-0.0300	-0.0341	-0.0436*	-0.0142	-0.0198
					(0.0271)	(0.0281)	(0.0264)	(0.0277)	(0.0249)	(0.0254)	(0.0251)	(0.0265)
Constant	-0.0892***	-0.0892***	-0.0882***	-0.0875***	-0.0874***	-0.0870***	-0.0869***	-0.0861***	-0.0894***	-0.0894***	-0.0882***	-0.0875***
	(0.00552)	(0.00726)	(0.00521)	(0.00621)	(0.00582)	(0.00696)	(0.00536)	(0.00615)	(0.00561)	(0.00720)	(0.00528)	(0.00622)
Controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Fixed Effects	Round	Round	Province-Round	Province-Round	Round	Round	Province-Round	Province-Round	Round	Round	Province-Round	Province-Round
Observations	3,130	2,982	3,130	2,982	3,124	2,976	3,124	2,976	3,123	2,975	3,123	2,975
R-squared	0.017	0.023	0.063	0.067	0.014	0.020	0.062	0.066	0.018	0.024	0.063	0.067
No. of Children	1,850	1,843	1,850	1,843	1,850	1,843	1,850	1,843	1,850	1,843	1,850	1,843

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 9: FD PPVT (% Correct)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Δ% PPVT	Δ% PPVT	Δ% PPVT	Δ% PPVT	Δ% PPVT	Δ% PPVT	Δ% PPVT	Δ% PPVT	Δ% PPVT	Δ% PPVT	Δ% PPVT	Δ% PPVT
ΔFlood	0.00317	0.00341	-0.00562	-0.00723					0.00431	0.00454	-0.00493	-0.00653
	(0.0110)	(0.0112)	(0.00623)	(0.00675)					(0.0112)	(0.0113)	(0.00650)	(0.00695)
ΔErosion					-0.0222**	-0.0227**	-0.0206***	-0.0214***	-0.0237***	-0.0244***	-0.0189***	-0.0193***
					(0.00920)	(0.0108)	(0.00456)	(0.00491)	(0.00827)	(0.00883)	(0.00553)	(0.00555)
Constant	0.194***	0.191***	0.193***	0.188***	0.193***	0.191***	0.193***	0.188***	0.193***	0.191***	0.193***	0.188***
	(0.00434)	(0.00632)	(0.00326)	(0.00469)	(0.00432)	(0.00628)	(0.00326)	(0.00470)	(0.00437)	(0.00631)	(0.00327)	(0.00469)
Controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Fixed Effects	Round	Round	Province-Round	Province-Round	Round	Round	Province-Round	Province-Round	Round	Round	Province-Round	Province-Round
Observations	4,540	4,329	4,540	4,329	4,528	4,317	4,528	4,317	4,526	4,315	4,526	4,315
R-squared	0.501	0.504	0.527	0.533	0.501	0.504	0.527	0.533	0.501	0.504	0.527	0.534
No. of Children	1,897	1,894	1,897	1,894	1,897	1,894	1,897	1,894	1,897	1,894	1,897	1,894

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 10: FD BMI

VARIABLES	(1) ΔBMI	(2) ΔBMI	(3) ΔBMI	(4) ΔBMI	(5) ΔBMI	(6) ΔBMI	(7) ΔBMI	(8) ΔBMI	(9) ΔBMI	(10) ΔBMI	(11) ΔBMI	(12) ΔBMI
ΔFlood	-0.224*	-0.196*	-0.0481	-0.0326					-0.216*	-0.193*	-0.0386	-0.0258
	(0.111)	(0.112)	(0.0818)	(0.0803)					(0.110)	(0.114)	(0.0821)	(0.0823)
ΔErosion					-0.184	-0.114	-0.0926	-0.0617	-0.110	-0.0485	-0.0805	-0.0535
					(0.132)	(0.111)	(0.128)	(0.0977)	(0.126)	(0.107)	(0.127)	(0.102)
Constant	1.429***	1.522***	1.433***	1.498***	1.434***	1.530***	1.434***	1.500***	1.430***	1.525***	1.434***	1.500***
	(0.0382)	(0.0723)	(0.0332)	(0.0595)	(0.0385)	(0.0721)	(0.0330)	(0.0592)	(0.0383)	(0.0724)	(0.0331)	(0.0599)
Controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Fixed Effects	Round	Round	Province-Round	Province-Round	Round	Round	Province-Round	Province-Round	Round	Round	Province-Round	Province-Round
Observations	4,872	4,645	4,872	4,645	4,860	4,633	4,860	4,633	4,858	4,631	4,858	4,631
R-squared	0.139	0.141	0.157	0.159	0.138	0.141	0.157	0.159	0.139	0.141	0.157	0.158
No. of Children	1,903	1,901	1,903	1,901	1,903	1,901	1,903	1,901	1,903	1,901	1,903	1,901

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 11: FD Weight

VARIABLES	(1) ΔWeight	(2) ΔWeight	(3) ΔWeight	(4) ΔWeight	(5) ΔWeight	(6) ΔWeight	(7) ΔWeight	(8) ΔWeight	(9) ΔWeight	(10) ΔWeight	(11) ΔWeight	(12) ΔWeight
ΔFlood	-0.147	-0.113	-0.125	-0.0793					-0.162	-0.139	-0.114	-0.0769
	(0.203)	(0.201)	(0.159)	(0.149)					(0.201)	(0.203)	(0.160)	(0.153)
ΔErosion					0.0334	0.224	-0.261	-0.141	0.0893	0.271	-0.224	-0.117
					(0.339)	(0.235)	(0.319)	(0.217)	(0.327)	(0.225)	(0.325)	(0.228)
Constant	10.52***	10.69***	10.52***	10.63***	10.52***	10.69***	10.52***	10.64***	10.52***	10.69***	10.52***	10.64***
	(0.122)	(0.175)	(0.0955)	(0.139)	(0.123)	(0.176)	(0.0956)	(0.138)	(0.123)	(0.176)	(0.0959)	(0.139)
Controls	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Fixed Effects	Round	Round	Province-Round	Province-Round	Round	Round	Province-Round	Province-Round	Round	Round	Province-Round	Province-Round
Observations	4,889	4,660	4,889	4,660	4,877	4,648	4,877	4,648	4,875	4,646	4,875	4,646
R-squared	0.357	0.364	0.389	0.392	0.357	0.363	0.388	0.392	0.357	0.363	0.388	0.392
No. of Children	1,904	1,902	1,904	1,902	1,904	1,902	1,904	1,902	1,904	1,902	1,904	1,902

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 12: Heterogeneity Analyses

VARIABLES	(1) Δ% Math	(2) Δ% Math	(3) Δ% Math	(4) Δ% Math	(5) Δ% Math	(6) Δ% PPVT	(7) Δ% PPVT	(8) Δ% PPVT	(9) Δ% PPVT	(10) Δ% PPVT
ΔFlood	-0.0382**	-0.0158	-0.0222	-0.0266*	-0.0305**					
	(0.0172)	(0.0394)	(0.0167)	(0.0134)	(0.0131)					
ΔFlood*Male	0.0234									
	(0.0205)									
ΔFlood*Rural		-0.0108								
		(0.0418)								
ΔFlood*ΔWealth_Lag			-0.0480							
			(0.116)							
ΔWealth_Lag			-0.0190					0.0317		
			(0.0370)					(0.0255)		
ΔFlood*ΔEmergencyCredit_Lag				-0.00202						
				(0.0120)						
ΔEmergencyCredit_Lag				-0.000677					0.00703	
				(0.00817)					(0.00490)	
ΔFlood*ΔHwork_Lag					0.0741***					
					(0.0120)					
ΔHwork_Lag					0.00594					-0.0032
					(0.0216)					(0.0112)
ΔErosion						-0.0177	-0.0252***	-0.0167	-0.0257	-0.0159
						(0.0163)	(0.0072)	(0.0252)	(0.0170)	(0.0185)
ΔErosion*Male						-0.00589				
						(0.0289)				
ΔErosion*Rural							-0.0213***			
							(0.0070)			
ΔErosion*ΔWealth_Lag								-0.0359		
								(0.184)		
ΔErosion*ΔEmergencyCredit_Lag									0.0804***	
									(0.0209)	
ΔErosion*ΔHwork_Lag										0.0896***
										(0.0201)
Constant	-0.0875***	-0.0874***	-0.0863***	-0.0871***	-0.0899***	0.188***	0.188***	0.148***	0.150***	0.142***
	(0.00620)	(0.00621)	(0.00703)	(0.00615)	(0.00588)	(0.00470)	(0.00471)	(0.00645)	(0.00634)	(0.00665)
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Fixed Effects	Province-Round	Province-Round	Province-Round	Province-Round	Province-Round	Province-Round	Province-Round	Province-Round	Province-Round	Province-Round
Observations	2,978	2,982	2,943	2,974	2,836	4,313	4,317	2,948	2,978	2,834
R-squared	0.068	0.067	0.068	0.070	0.073	0.533	0.533	0.555	0.556	0.552
No. of Children	1,618	1,621	1,595	1,615	1,599	1,760	1,763	1,630	1,650	1,631

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 13: Leads & Falsification Tests

VARIABLES	(1) Flood_Lead	(2) Erosion_Lead	(3) Δ% Math	(4) Δ% PPVT	(5) Δ% PPVT
% PPVT	0.0819	0.0301			
	(0.0939)	(0.0303)			
% Math	0.00276	0.0175			
	(0.0401)	(0.0223)			
BMI	-0.00508	0.00213			
	(0.00519)	(0.00261)			
Weight	0.00209	-0.000489			
	(0.00255)	(0.000648)			
Wealth Index	-0.0479	-0.000408			
	(0.0917)	(0.0187)			
HH Size	0.00291	0.000320			
	(0.00562)	(0.00407)			
Current Loan	0.00275	0.00263			
	(0.00919)	(0.00608)			
Monthly Food Exp.	-3.28e-05	3.65e-06			
	(3.18e-05)	(5.88e-06)			
Access to Credit	-0.0238	-0.00857			
	(0.0260)	(0.0102)			
Hours at Work/Day	0.000551	0.00115			
	(0.00725)	(0.00202)			
Hours at School/Day	0.00580	-0.00129			
	(0.00566)	(0.00369)			
Access to Sanitation	0.0157	0.000157			
	(0.0156)	(0.00880)			
ΔFlood_Lead			0.0228		
			(0.0217)		
ΔErosion_Lead				0.0258	
				(0.0208)	
ΔErosion_Lead2					-0.0353
					(0.0319)
Constant	-0.00119	-0.0304	-0.0966***	0.285***	0.254***
	(0.160)	(0.0327)	(0.0153)	(0.00793)	(0.00971)
Controls	N/A	N/A	YES	YES	YES
Fixed Effects	Province-Round	Province-Round	Province-Round	Province-Round	Province-Round
Observations	3,211	3,211	1,558	2,835	1,436
R-squared	0.628	0.605	0.061	0.094	0.114
No. of Children	1,768	1,768	1,558	1,625	1,436

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 14: Sub-Sample Difference-in-Differences Test (Floods)

	(1)	(2)	(3)	(4)
VARIABLES	% Math	% PPVT	BMI	Weight
Treatment*R3		-0.0468	0.00997	-0.272
		(0.0410)	(0.338)	(0.418)
Treatment*R4	0.0297	0.0122	-0.404	-0.863
	(0.0517)	(0.0257)	(0.429)	(1.221)
Treatment*R5	-0.00738	-0.00268	-0.467	-0.653
	(0.0649)	(0.0308)	(0.580)	(1.423)
Constant	0.495***	0.179*	14.93***	20.44***
	(0.0792)	(0.0901)	(0.641)	(2.204)
Controls	YES	YES	YES	YES
Fixed Effects	Child, Province-Round	Child, Province-Round	Child, Province-Round	Child, Province-Round
Observations	3,258	4,290	4,475	4,480
R-squared	0.729	0.919	0.827	0.935
No. of Children	1,205	1,184	1,208	1,208

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

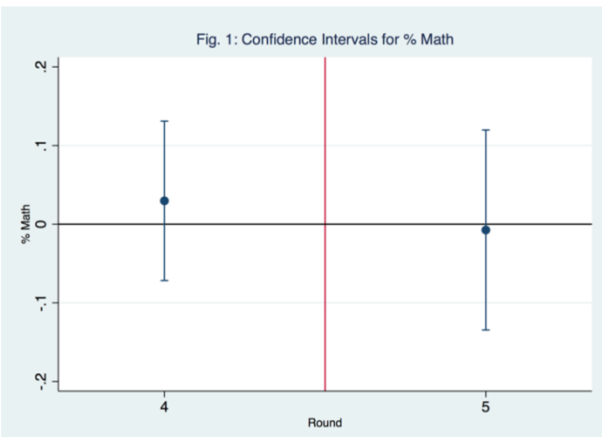

Fig. 1: Confidence Intervals for % Math

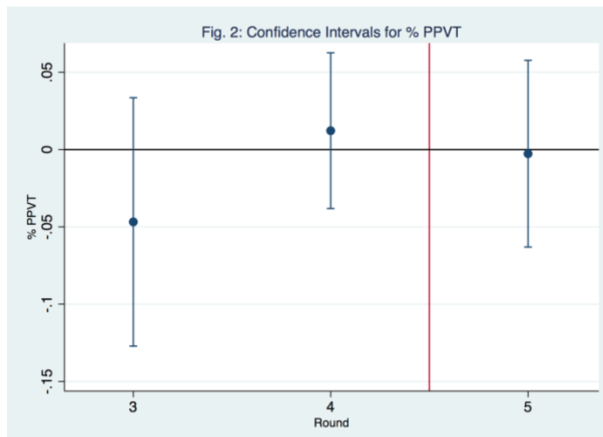

Fig. 2: Confidence Intervals for % PPVT

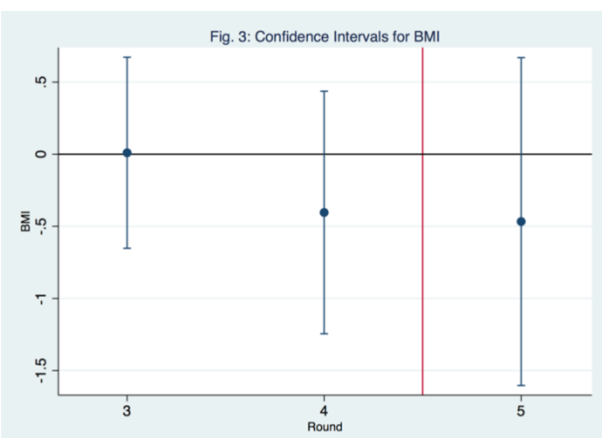

Fig. 3: Confidence Intervals for BMI

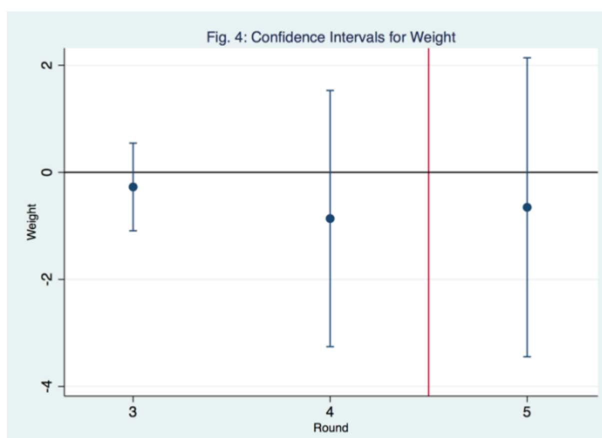

Fig. 4: Confidence Intervals for Weight

Table 15: FE & FD (Re-sampled with Commune-Level Shocks)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	% Math	% PPVT	BMI	Weight	Δ% Math	Δ% PPVT	ΔBMI	ΔWeight
Flood_Comm	0.00833	-0.0100	0.0846	0.161				
	(0.0260)	(0.0174)	(0.0855)	(0.370)				
ΔFlood_Comm					-0.0135	-0.0172	-0.106	-0.309
					(0.0219)	(0.0154)	(0.0832)	(0.246)
Constant	0.663***	0.222**	15.15***	20.29***	-0.0842***	0.194***	1.474***	10.58***
	(0.0793)	(0.0823)	(0.484)	(1.634)	(0.00593)	(0.00485)	(0.0572)	(0.139)
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Fixed Effects	Child, Province-Round	Child, Province-Round	Child, Province-Round	Child, Province-Round	Province-Round	Province-Round	Province-Round	Province-Round
Observations	5,048	6,700	6,983	6,994	3,115	4,601	4,936	4,951
R-squared	0.730	0.922	0.826	0.935	0.061	0.525	0.166	0.404
No. of Children	1,851	1,896	1,903	1,903	1,631	1,785	1,814	1,816

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1