Destruction, Disinvestment, and Death: Economic and Human Losses Following Environmental Disaster*

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Abstract

The direct physical damage caused by environmental disasters is straightforward to document and often the focus of media and government attention, but addressing disasters' indirect effects remains difficult because they are challenging to observe. We exploit annual variation in the incidence of typhoons (West-Pacific hurricanes) to identify the effect of environmental disaster on economic and health outcomes in Filipino households. We find that the Philippines' typhoon climate causes large losses to households' economic well being, destroying durable assets and depressing incomes in the wake of storms. Household income losses translate directly into expenditure reductions, which are achieved in part through disinvestments in health and human capital. Examining infant mortality rates, we observe substantially increased female infant mortality in the years following storm exposure. Striking similarities in the structure of these mortality and economic responses, along multiple dimensions, implicates the deterioration of economic conditions and subsequent disinvestments as the cause of mortality among female infants. Bolstering this hypothesis, we find that mortality is highest in households where infant daughters face the greatest competition with other children for resources, particularly older brothers. We estimate that these delayed deaths among female infants outnumber officially reported typhoon deaths in the general populace by a factor of fifteen.

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1 Introduction

It is obvious that natural disasters cause immediate destruction and death. In theory, documenting the direct physical damages caused by hurricanes, earthquakes, and other catastrophes is straightforward, although the logisitics of doing so are often difficult. At the same time, even our theoretical understanding of disasters' aftereffects, particularly on economic outcomes, remains limited by a paucity of empirical observations. The few facts we have about post-disaster economics come primarily from studies that link macroeconomic data with country-level estimates of disaster impacts (see Strömberg (2007) and Cavallo and Noy (2009) for reviews of the literature). Thus even fairly basic questions about disasters' economic effects, such as whether household incomes rise or fall in a disaster's wake, remain unsettled (Albala-Bertrand (1993); Benson and Clay (2004); Caselli and Malhotra (2004); Hallegatte and Ghil (2008); Horchrainer (2009); Loayza et al. (2009); Dercon and Outes (2009); Noy (2009); Fomby, Ikeda and Loayza (2009); Hsiang (2010); Strobl (2011); Deryugina (2011)).

Improving our understanding of post-disaster economic outcomes is important for several reasons. Designing effective disaster management policies and institutions requires that we understand the full cost of disasters (Kunreuther et al. (2009); United Nations (2009)); if a sizeable portion of a disaster's costs manifest after the event itself, then models of humanitarian intervention which focus on immediate damages may need to be reassessed or expanded. Secondly, the wealth of evidence suggesting that disasters' immediate death and destruction is most acute in low-income countries (Kahn (2005), Mutter (2005), Yang (2008), Hsiang and Narita (forthcoming)) indicates that disasters might plausibly influence economic development. Of particular concern is disasters' potential to alter long-run outcomes due to short-run losses: if poor households have a limited ability to mitigate disaster-induced losses, disaster incidence may cause them to sacrifice valuable investment (Udry (1994); Jacoby and Skoufias (1997);

Duflo (2000); Maccini and Yang (2009); Banerjee and Mullainathan (2010)) for short-run needs. Lastly, recent evidence suggests that global climate change is expected to increase the frequency of certain types of environmental disaster, (IPCC 2007; Knutson et al. (2010)). This implies that any improvement in estimates of disasters' costs will necessarily inform estimates of climate change's anticipated damages (Narita, Tol and Anthoff (2009); Mendelsohn, Emanuel and Chonobayashi (2010)), and in turn the formulation of climate change policy in general (Stern (2006); Nordhaus (2008); Tol (2009); Weitzman (2009); Pindyck (2011)).

In this paper we measure the post-disaster economic and health effects of a specific type of environmental disaster: typhoons. Typhoons, or tropical cyclones[1], are large, fast-moving storms which form over the oceans and cause physical damage via intense winds, heavy rainfall, and ocean surges. We focus on typhoons both because they are one of the most common and costly types of natural disaster (Bevere, Rogers and Grollimund (2011)) and because their variation in timing and spatial distribution allow us to identify their effects using quasi-experimental techniques (Holland (1986)). Typhoons are relatively brief, usually affecting a given location for at most 1- 2 days. They are also sharply defined in space, being 100-200 kilometers across and traveling distances ranging from a few hundred to a few thousand kilometers in length. The intensity of a location's typhoon exposure is also variable, both because the storms themselves vary in frequency and intensity and because different locations are exposed to different parts of the same storm, another feature that we exploit in our econometric analysis.

The Philippines is situated in one of the most intense typhoon climatologies on the planet (see Figure 1), a fact that both improves our identification strategy and differentiates this study from analyses of one-off or infrequent natural disasters. In order to capture spatial and temporal variations in typhoon exposure within the Philippines

[1] "Typhoon" is the name for a tropical cyclone that occurs in the western Pacific Ocean. The same storms are called "hurricanes" in the Atlantic Ocean and simply "cyclones" in the Indian Ocean.

we use a physical model of typhoon winds developed in Hsiang (2010) to create a unique panel dataset of province-level incidence. This dataset allows us to adopt a difference-in-differences approach which takes advantage of each province's year-to-year variation in typhoon exposure.

We combine physical storm data with two household survey files: the Family Income and Expenditure Survey (FIES), a repeated cross sectional survey of household economic outcomes conducted by the Filipino government every three years; and the Demographic and Health Survey (DHS), a suite of cross sectional household-level health and fertility surveys. The FIES data allow us to identify the impact of storms on households' physical assets, income, and consumption[2], while the DHS's retrospective data on mothers' fertility allow us to reconstruct a mother-by-year panel dataset of infant births and mortality. Infant mortality constitutes a sensitive[3] measure of health itself as well as an indicator of general household well being. When linked together, these three datasets allow us to characterize the multidimensional response of households to typhoons.

We begin by demonstrating that our empirical model indeed captures typhoons' direct destructive impact. We verify that our measure of typhoon exposure, spatially-weighted maximum typhoon wind speed (henceforth "wind speed"), is a good predictor of damages and deaths at the national level. We demonstrate that these nationally-aggregated losses are also apparent at the household-level in the form of lost capital assets, such as televisions, toilets, and walls.

Turning next to household income, we find that typhoons reduce average income the year after they strike, presumably due to storms' direct physical damages as well

[2] We note that expenditures alone do not infer quantity of consumption in the absence of prices, and thus perform a variety of checks on storms' impact on prices, which we find to be negligible; see Section 5 for details.

[3] As Chay and Greenstone (2003) point out, infant mortality minimizes problems of cumulative exposure and a host of other potentially confounding identification concerns that emerge when examining other human capital measures.

as their more indirect disruption of economic activity. We find that household income drops linearly by 0.39% per meter per second of wind speed exposure. Given the average annual exposure at the province level of 16.9 m/s during our sample period, we estimate that the average short-run effect of the country's typhoon climate is to depress incomes by 6.7%. This effect occurs across a variety of income sources, affects both richer and poorer households, and is net of public and private transfers.

The income losses we measure translate nearly one-for-one into a reduction of household expenditures, which decrease 7.1% for the average household in the average year. These expenditure reductions track total income losses closely when they are examined across years (relative to the storm), across space, across typhoon intensities, and across income groups. This tight relationship suggests that households do not mitigate storm-induced losses via consumption-smoothing strategies, such as in-kind transfers, savings, or borrowing. Instead, we observe that households make large adjustments to their *relative* spending on different types of consumption and investment. In general, households reduce their spending the most on expenditures that most closely resemble human capital investments, such as medicine, education and high nutrient foods that include meat, dairy, eggs and fruit. In contrast, expenditures decline much less on pure consumption goods, such as recreation, alcohol and tobacco.

We next examine whether typhoons impact household health outcomes by examining infant mortality rates. We find no evidence that infant mortality rises during or immediately following typhoon exposure, implying that deaths from physical exposure to the storm itself, which we term 'trauma deaths', are few. However, we find that typhoons cause infant mortality to rise the calendar year *after* the storm itself has passed. This is illustrated in Figure 2, which shows the cumulative monthly mortality impact of typhoons. The vast majority of infant female deaths (grey line) manifest well after the typhoon event; moreover, many of the infants who die in the aftermath of the storm were not even conceived until after the storms are gone (dotted black line), implying that the direct mortality impact of the storm is minimal. We estimate that in our sample of mothers who never migrate, these typhoon-associated deaths amount to an annual average of 1,130 female infant deaths per million households, corresponding to 55% of the baseline infant female mortality rate.

Multiple aspects of our findings suggest that deteriorating economic conditions and disinvestments in human capital are responsible for these female infant deaths. We "fingerprint" patterns of economic contraction and disinvestment across many dimensions by looking at their timing relative to storms, their nonlinear response to storm intensity, their short and medium-run lag structure at different locations in the income distribution, and their spatial patterns. We then demonstrate that patterns of female infant mortality exhibit an almost identical "fingerprint" across these same dimensions. Furthermore, we find that mortality is highest in households where infant daughters face competition from other children over resources, particularly if those siblings are male. These findings together suggest that female infant deaths following typhoon events are 'economic deaths' resulting from economic losses and the resulting household decisions regarding human capital investments and within-household resource allocation. This conclusion that female infants bear a differentially large share of the burden from income loss is consistent with findings from a variety of other contexts (Rosenzweig and Schultz (1982); Rose (1999); Duflo (2000); Duflo (2005); Bhalotra (2010)). Extrapolating these estimates to the entire non-migrant population suggests that approximately 11,000 female infants suffer 'economic deaths' in the Philippines every year due to the previous year's storm season. In contrast, there was an average of 743 'trauma deaths' per year according to official reports for the same period (OFDA/CRED 2009). This suggests that mortality attributable to Filipino typhoons is roughly 1500% of previous estimates.

The remainder of the paper is structured as follows. Section 2 presents background on typhoons and disaster impacts. Section 3 presents the data and Section 4 presents the identification strategy. Section 5 presents our results. We conclude in Section 6, which discusses our findings and some of their implications for policy.

2 Background

The Typhoon Climate of the Philippines

Figure 1 shows a map of the Philippines' annual expected typhoon exposure, or its typhoon climate. The Philippines possesses one of the most active cyclone climatologies in the world, on average experiencing over ten typhoons each year ranging in intensity from mild to severe. Because the Philippines is large compared to typhoons[4], different regions within the country may experience entirely different levels of storm exposure in the same year.

The Philippines' active typhoon climate provides additional benefits for analysis compared to other idiosyncratic destructive events such as earthquakes[5] or wars, in that typhoons in the Philippines are a regular and expected occurrence. This can be seen in Figure 4, which shows the distribution of typhoon exposure for each Filipino province; we note that median exposure (white bars at the center of each box) is non-zero for all but a handful of provinces. While destructive and unpredictable, typhoon exposure itself is thus not surprising, and households almost certainly incorporate typhoon risk into their economic decisions. We can thus plausibly infer that any impacts that we observe occur in spite of all the adaptive responses that households employ to mitigate

[4] See, for example, Appendix Figure C.1 showing a satellite photo of Typhoon Nanmadol.

[5] Note that while earthquakes certainly have a spatial and temporal incidence structure similar to typhoons the interarrival time of destructive earthquakes is orders of magnitude longer than for destructive typhoons. See, for example, Triep and Sykes (1997).

typhoon impacts.

Our estimate of typhoons' costs adds to the rapidly growing literature on the natural environment's impact on health outcomes (Deschênes and Moretti (2009); Maccini and Yang (2009); Deschênes and Greenstone (2011)), as well as the literature on the economic and health impacts of disasters (Toya and Skidmore (2007); Strömberg (2007); Cavallo and Noy (2009); Simeonova (2011); Hsiang (2010); Deryugina (2011)) as well as the more general literature exploring climatic influence on economic outcomes (Gallup, Sachs and Mellinger (1999); Acemoglu, Johnson and Robinson (2002); Bloom, Canning and Sevilla (2003); Easterly and Levine (2003); Miguel, Satyanath, and Sergenti (2004); Nordhaus (2006b); Schlenker and Roberts (2009); Dell, Jones, and Olken (2009); Hsiang (2010); Graff Zivin and Neidell (2010)). It also augments the literature on the economic consequences of physically destructive shocks (Davis and Weinstein (2002); Vigdor (2008); Miguel and Roland (2011)) though it differs from much of that literature in that typhoons, rather than being idiosyncratic events like bombings, are a persistent and common state of the climate.

Household Adjustments to Income Loss

Reductions in household income have the obvious potential to cause deleterious effects: consumption of goods and services, investments in health and education, and savings for future use are all potential margins of adjustment which may suffer following income loss. There are a variety of means by which households seek to minimize these costs. Firstly, households may attempt to smooth their income or consumption over time, thereby spreading costs out and attenuating the immediate impact of income loss. This smoothing can come in the form of within-household adjustments such as accumulating precautionary savings (Paxson (1992); Kazarosian (1997)), directly supplanting income through adaptive labor market activity (Kochar (1995); Jacoby and Skoufias (1998);

Kochar (1999)), or selling assets during times of duress (Rosenzweig and Wolpin (1993). It may also come in the form of extra-household adjustments such as accessing credit markets (Rosenzweig(1988); Cochrane (1991); Morduch (1995)) or relying on transfers (Foster and Rosenzweig (2001); Fafchamps and Lund (2003); Yang and Choi (2007)). It is important to note that the income and consumption smoothing literature differentiates between relatively easily insured-against idiosyncratic shocks (i.e., income losses affecting different households at different times) and less easily mitigated aggregate shocks affecting many houses (Cochrane (1991); Townsend (1995)). One might thus expect that income losses due to typhoons, which are particularly large and common aggregate shocks, might be particularly difficult to smooth over time.

If income losses cannot be smoothed then households may adjust by altering their expenditure patterns. This adjustment may manifest in altered consumption, e.g., via changes in eating habits (Subramanian and Deaton (1996); Jensen and Miller (2008)), or it may manifest as a reduction in investments, such as to human capital (Mincer (1958); Jensen (2000); Banerjee and Mullainathan (2010))[6]. If losses to income result in disinvestment in human capital, particularly among children, then the potential costs of a shock may far exceed its immediately observable effects in the long run via worsened later-life outcomes (Strauss and Thomas (1998); Maccini and Yang (2009); Banerjee et al. (2010)). Moreover these losses may become compounded if households differentially disinvest in children by type, for example due to gender biases (Sen (1990); Duflo (2005)).

This paper expands upon the literature documenting household disinvestments in children's human capital following income loss (Jacoby and Skoufias (1998); Strauss and Thomas (1998); Jensen (2000)), particularly disinvestments in girls' human capital

[6]Note that in many instances consumption and investment cannot be disentangled; expenditures on nutritious food, for example, can be equally viewed as consumption as well as investment in future human capital.

(Rose (1999); Bhalotra and Heady (2003); Maccini and Yang (2009); Chen (2011)). More broadly, this paper adds to the growing body of research documenting the excess risk burden born by female household members in developing contexts (Horton (1986); Sen (1990); Duflo (2005); Qian (2008); Robinson and Yeh (2011)).

3 Data

Our analysis requires data describing household assets, income, expenditures, health outcomes, and typhoon exposure. Summary statistics of these data are presented in Tables 1, 2, and 3. For reference, Appendix Figure C.2 displays an administrative map of the 82 provinces (smaller units) and 17 regions (larger units) we include in our data.

Typhoon data

A central innovation of our analysis is the development of a comprehensive data file describing a physical measure of typhoon incidence over time. We develop this measure to ensure that our typhoon data are sufficiently precise to describe meaningful variations in typhoon exposure in a climate where typhoons are common. We begin by reconstructing the wind field for every West Pacific cyclone in the International Best Track Archive for Climate Stewardship (IBTrACS) database (Knapp (2009)) using the Limited Information Cyclone Reconstruction and Integration for Climate and Economics (LICRICE) model (see Hsiang (2010) for a detailed description of the model[7]). LICRICE only reconstructs wind fields and does not explicitly account for rains, flooding, or storm surges because wind fields are less influenced by topography and are thus more generalizable. However, our wind field measures describes these other typhoon

[7]Since Hsiang (2010), version 2 of LICRICE was built (used in this study), substantially improving upon the model's original accuracy. However these improvements were focused on numerical methods and the heuristic description in Hsiang (2010) remains accurate.

impacts to the extent that they are correlated with wind speed.

We use LICRICE to reconstruct the wind field as a translating vortex for all 2,246 storms recorded in the West Pacific Basin between 1950-2008 by interpolating among 72,901 6-hour observations over every $1/34° \times 1/34°$ pixel of the Philippines ($1/34° \approx 0.0294° \approx 2.02$ miles ≈ 3.26 kilometers). Figure 3 illustrates a snapshot of a storm's wind field for an example storm, with the height of the surface depicting the speed of the surface winds. Using this approach, we find that 837 storms affected the Philippines[8] between 1950-2008 (13.72 storms per year). Of these storms, 411 occurred during 1979-2008 (13.70 storms per year), the period for which we have overlapping economic and health data.

To match typhoon exposure with annualized socioeconomic data files, our continuous physical measure of typhoons must be summarized to form a single observation for each location in every year. We summarize annual typhoon exposure for provinces and regions by computing the maximum wind speed achieved at each pixel and then taking the average across pixels within an administrative unit. We opt for this measure because it allows us to capture storm intensity while controlling for variations in the physical size of regions and provinces[9]; a storm that passes over the entirety of a geographic area would thus register as stronger treatment than one that merely passed over a small portion of it. For succinctness, we refer to this statistic as 'wind speed'

[8]That is, 837 storms registered non-zero wind speeds over at least one $1/34° \times 1/34°$ pixel.

[9]Hsiang and Narita (forthcoming) discuss the variety of tropical cyclone measures that have been employed in previous econometric studies, such as windspeed at landfall, minimum central pressure and total energy dissipated. As Hsiang and Narita demonstrate, the spatially-weighted maximum wind speed measure that is employed in this study is well-supported by theory and outperforms alternative measures in a country-by-year panel analysis. Briefly, the theoretical basis for this measure rests on two observations. First, the stress-strain relationship for most materials is highly non-linear, with catastrophic failure occurring at a critical level of stress (Nordhaus (2006a)). Thus, for a given material, only the maximum level of stress that the material is exposed to, i.e. the maximum wind speed, is relevant for determining whether failure is expected. Secondly, people and capital are distributed across space within a province, making it necessary to construct some sort of spatial average for wind exposure. We follow Hsiang (2010) and Hsiang and Narita (forthcoming) and adopt area-weights for our averages because they cannot be endogenous in the same ways that population, capital or income weights might be. For further discussion see section A in the appendix.

and it is presented in the units of meters per second (1 m/s = 3.6 km per hour ≈ 2.24 miles per hour)[10].

Figure 4 displays medians, inter-quartile ranges and extreme values of typhoon exposure for each of the 82 provinces in our sample during 1950-2008. The figure illustrates that there is strong variation in typhoon exposure between provinces as well as strong year-to-year variations in exposure at the level of an individual province. Note that no province completely escapes typhoon exposure in the period of observation and there are many provinces that are exposed to typhoons every year. Approximately half of the provinces have median annual exposures in excess of 20 m/s and many provinces are exposed to events exceeding 50 m/s. As shown in Table 1, the average province was exposed to wind speeds of 17.6 m/s (s.d. = 12.0 m/s) between 1950 and 2008, or 16.9 m/s (s.d. = 11.6 m/s) between 1979 and 2008.

Household Asset, Income and Expenditure Data

Information on household assets, income and consumption are obtained from the cross-sectional Family Income and Expenditure Survey (FIES) conducted by the National Statistics Office (NSO) of the Philippines (Ericta and Fabian (2009)). In 1957, the government of the Philippines began conducting the FIES irregularly (approximately every five years) to understand the distribution of income, spending patterns and the prevalence of poverty, as well as to benchmark consumer price indices. In 1985, the survey was completely restructured and the NSO began conducting it at regular three year intervals. In this analysis, we obtain and use FIES Public Use Files for the years 1985, 1988, 1991, 1994, 1997, 2000, 2003 and 2006.

The FIES provide us with data on each household's assets across several different

[10] It is important to note that because reported wind speed values are area-averages, actual wind speeds at the center of storms are substantially greater than the values we report and cannot be directly compared.

categories, household income by source, total income *net* of any transfers and subsidies, and household expenditures on different goods and services.

We note that there are important timing issues to contend with in analyzing the FIES data that arise from the manner in which the survey is administered. FIES data are collected twice for each household, just after the middle of the year (July) and just following the end of the year (the following January), with responses for each survey reflecting economic behaviors over the preceding six months. Responses for each household are then averaged between the two surveys to construct annual estimates; however, if a household cannot be found in either round of the survey they are dropped from the sample[11]. Figure 5 shows the FIES survey timeline overlaid with mean monthly typhoon strikes to indicate why this is a potential concern. Typhoon activity in the Philippines is concentrated late in the year, so estimates of typhoon impacts during the year of exposure may be somewhat attenuated because first phase responses are recorded prior to the bulk of typhoon events. This motivates us to focus on capital losses the year following typhoon exposure, since it seems unlikely that capital can be replaced immediately following a storm[12].

Also concerning is the NSO's policy of dropping second round non-respondents, since typhoons may cause households to migrate, the obliteration of participants' physical homes, or villages becoming inaccessible due to flooding or infrastructure damage[13]. This results in observations being dropped from our sample based on extreme values of the treatment effect we are interested in, a fact that probably biases our estimate of

[11]Surveyors attempt to revisit households two additional times if the household head cannot be located in the first visit. Only after three unsuccessful interview attempts is a household dropped.

[12]We note that a typhoon's contemporaneous effect on capital is roughly 53% of its effect in the following year. This is consistent with our concern that contemporaneous effects will be smaller because phase 1 responses occur before roughly 65% of storm events.

[13]The NSO explicitly states that a major cause of second phase survey attrition is the inability to locate households when the physical structure they inhabited during the first phase interview is destroyed by a typhoon before the second phase. When areas become inaccessible due to flooding or infrastructure damage, the NSO generally tries to postpone surveys within the affected region. Unfortunately, the NSO does not provide statistics on these types of attrition.

treatment effects towards zero. We attempt to minimize this attenuation by including a vector of observable household covariates in all our models. In addition, we explicitly test for balance on treatment in Section 4.

Lastly, we note that the lowest unit of geographic designation in the FIES surveys changed between the 2000 and 2003 waves; early years include province level identifiers (more detailed) while later years only report regional identifiers (less detailed). Thus, our baseline models analyze outcomes at the province level but omit 2003 and 2006. We then reintroduce these years in region level estimates as a check on our main results. For province level models we are able to match 142,789 household observations contained in the period 1985-2000, whereas our sample expands to 174,896 observations in regional level models that span the period 1985-2006.

Infant Mortality Data

Our infant mortality data are taken from the 1993, 1998, 2003, and 2008 waves of the Demographic and Health Surveys (DHS)[14] for the Philippines. The DHS are cross-sectional surveys with questions related to population, health, and nutrition, particularly pertaining to maternal and child health. The DHS program is highly standardized with changes between surveys documented and propagated, allowing for comparison of surveys both across countries and within countries across time. Samples are designed to be representative at the national and regional levels. Within the Philippines, each household's location is identified according to its administrative region and provincial identifiers are not available.

The primary interview targets of the DHS are women between the ages of 15 and 49.

[14]The DHS are administered by the Measure DHS project (funded largely by USAID) and are available for free download online at `http://www.measuredhs.com/`. Started in 1984, the DHS program has collected survey data on 84 countries as of late 2011, with many of those countries having been subject to multiple survey waves.

A wide suite of questions are asked on topics ranging from HIV awareness to nutritional practices to each woman's full fertility history. The latter provides us with a source of our infant mortality data, as each woman is asked to provide detailed information about every child she has ever born, including any children who have died. We are thus able to construct a time series for each woman's fertility and mortality events over the duration of her life up until the survey, echoing recent research that uses the DHS in a similar way (Kudamatsu (2011); Kudamatsu, Persson, and Strömberg (2011); Chakravarty (2011)). We follow these authors in excluding migrant mothers from our sample, thereby minimizing the sorting and migration concerns that arise in the FIES. The 24,841 non-migrant mothers in our sample yield 265,430 mother-by-year observations, or nearly 11 years of longitudinal data per woman. Table 3 shows summary statistics.

The DHS data include several variables aside from infant mortality events which are particularly useful for this analysis. Of particular note are: a measure of each woman's prior migration history, captured by whether she has ever lived anywhere else and, if so, when she moved to her current location of residence; educational attainment of both the woman and her husband, if any; the geographical region in which the woman resides; and the woman's age at time of survey. While there is no direct questioning on each woman's or household's income, a variety of socioeconomic status (SES) indicators are collected, ranging from whether the household has electricity to whether anyone in the househole owns a car. We construct a proxy for socioeconomic status from these data for comparison of distributional impacts in a process detailed in Section B.

Other Data

Emergency Events Database (EM-DAT) Nationally aggregated data on economic losses and deaths from tropical cyclones are obtained from the Emergency Events

Database data file commonly referred to as "EM-DAT" (OFDA/CRED 2009). The EM-DAT data file contains information provided by national governments, international organizations, NGOs, and private companies (e.g., re-insurance companies) on a self-reporting basis[15]. EM-DAT data of economic losses are an estimate of negative economic impacts that may include lost consumption goods, lost productive capital or cost of business interruption, depending on the protocols of the reporting institution. EM-DAT is the database used in most previous cross-country studies of post-disaster economics, with some of its limitations discussed in the review by Cavallo and Noy (2009).

Temperature and Rainfall We control for mean annual temperature and rainfall in all of our analyses to minimize potentially confounding climate behaviors that might be correlated with typhoon incidence (Auffhammer et al. 2010). Temperature observations are extracted from the gridded reanalysis of the Climate Data Assimilation System I (CDAS1) produced by the National Center for Environmental Prediction (NCEP) and the National Center for Atmospheric Research (NCAR) (Kalnay et al. (1996). Rainfall estimates are obtained from the Climate Prediction Center (CPC) Merged Analysis of Precipitation (CMAP) which merges station readings on the ground with available satellite data (Xie and Arkin 1996). Both temperature and precipitation data are spatially averaged over each region or province.

Crop Prices and Wage Rates Province-level data on annual farm worker wage rates and commodity retail prices are obtained from the Bureau of Agricultural in the Philippines[16]. Data are available for the period 1985-2008.

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[15]EM-DAT is provided for free by the Centre for Research on the Epidemiology of Disasters (CRED) at `www.emdat.be`, Universite Catholique de Louvain, Belgium.

[16]Details and data are available at `http://countrystat.bas.gov.ph`

4 Identification

To empirically identify the impact of typhoons on household outcomes we use a difference-in-differences approach that exploits random variations in each location's typhoon incidence. Identifying the treatment effect of typhoons requires that we must only utilize variations in typhoon exposure that are randomly assigned to households (Holland (1986); Freedman (1991)). Because the formation of typhoons and their trajectories have strong spatial patterns, some locations have relatively higher or lower levels of average typhoon exposure (recall Figure 1). However, these cross-sectional variations in mean exposure might be correlated with cross-sectional differences in the unobservable characteristics of different locations, for example culture. For this reason, we do not utilize the cross-sectional variation in average exposure and instead rely only on random year-to-year variations in exposure at each specific location. To achieve this, we include province (or region) fixed-effects in all of our regressions to absorb any cross-sectional variation in typhoons exposure or losses. If there are unobservable reasons why some locations have higher (or lower) incomes or infant mortality on average, these fixed-effects will non-parametrically account for this difference and it will not contaminate our estimates of the typhoon treatment effect (Greene (2003)).

Randomness in typhoon exposure arises because both the location and timing of storm formation as well as storm trajectories themselves are stochastic. One might be concerned that annual variations in storm exposure might not be entirely random because households could make location choices based on seasonal typhoon forecasts. Yet, while it is now possible to predict average storm frequencies for each storm season in a given basin with moderate skill (Heming and Goerss (2010); Smith et al. (2010)), these forecasts have almost no predictive power if one were to try forecasting *location-specific* seasonal risk. Thus, it is reasonable to assume that annually varying risk differentials are imperceptible for individuals on the ground, since these differentials still cannot be predicted by scientists. In contrast to seasonal prediction, it is possible to forecast typhoon exposure a few days before a storm strikes[17] (Heming and Goerss (2010)), a fact that often allows individuals to evacuate and protect some of their assets. This is important for interpreting our results, because the treatment effect that we estimate is the effect of typhoons after households have employed the full range of adaptive behaviors available to them, such as evacuation. But it does not seem plausible that short-term evacuations based on short-term forecasts lead to the reorganization of populations on annual time-scales, so it is unlikely that forecast-based sorting affects our annualized estimates.

We wish to avoid spurious correlations, so we must avoid correlating trends in typhoon incidence and our outcomes of interest. To do this, we flexibly account for common trend behaviors by including year fixed-effects in all of our models (Greene (2003)). These fixed effects also account for any unobservable common climatic shocks, such as the El-Niño-Southern Oscillation, which could be correlated with typhoon exposure (Camargo and Sobel (2005)) as well as socio-economic outcomes (Hsiang, Meng and Cane (2011)).

The primary threat to the validity of our study is the potential for household sorting in the wake of typhoon exposure. As we explained, sorting due to typhoon *risk* should not be a major concern since we include location fixed-effects and annual changes in risk are imperceptible to households. Sorting on typhoon *incidence*, however, could be problematic if the passage of a storm causes families to migrate away for long periods, altering the household composition of different locations. This is of particular concern for the FIES data given their survey methodology (discussed in Section 3).

To address this concern, we test for balance in the FIES data by regressing ob-

[17] For example, Willoughby et al. (2007) note: "In the past, a forecast was considered successful if it predicted the hurricanes position and intensity 12 - 72 h into the future. By the 1990s, forecast users came to expect more specific details such as spatial distributions of rainfall, winds, flooding, and high seas. In the early 21st century, forecasters extended their time horizons to 120 h."

servable household characteristics on typhoon exposure, presenting results in Table 4. This approach checks whether observable household characteristics vary with the intensity of the previous years' typhoon exposure. We allow household composition to vary nonlinearly in response to typhoon exposure by including indicator variables for prior year's maximum wind speed[18]. In support of our approach, we find almost no evidence of sorting. Out of 49 parameters estimated, six are statistically significant at the 10% level and one is significant at the 5% level; this is very close to what we would expect if household composition were random (five and two respectively). If one interprets these coefficients literally, they might provide suggestive evidence that typhoon exposure is positively associated with total family size and negatively with the probability that the household head has finished primary school. However, in neither case does the intensity of cyclone exposure matter in a systematic way, suggesting that these correlations are probably random[19]. Nonetheless, to be certain that bias from sorting along these covariates is minimized, we control for all of these all of them in our main regression models.

We are less concerned about sorting in the DHS data for two reasons. First, households in the DHS are asked whether they have ever lived anywhere other than their current location. This allows us to directly avoid sorting behavior by restricting our sample to non-migrant mothers. The second reason is that, unlike FIES data, the DHS data are a true panel that allows us to follow specific women over time. Thus, there are no compositional changes in the DHS panel that can be driven by typhoon exposure during the mother's adult life.

[18]This exact model is used throughout the paper to identify the effect of typhoons on time-varying outcomes. It is explained in greater detail in the next section.

[19]See Appendix Section A for additional discussion.

5 Results

We structure the presentation of our results as follows: We first demonstrate that our measure of typhoon incidence accurately predicts physical damage at both the macro and micro level in Section 5.1. We then demonstrate in Section 5.2 that the legacy of this physical destruction leads to losses to income the year following storms, which are closely matched by expenditure and consumption losses as detailed in Section 5.3. We then demonstrate the infant mortality impacts of typhoons in Section 5.4 and provide evidence supporting the argument that they stem from economic losses in Section 5.4.2. Lastly we explore cross-sectional evidence of adaptation in Section 5.5.

5.1 Physical damages

It may seem obvious that typhoons are physically destructive, but measuring the economic importance of this destruction is not trivial. The first studies that used aggregate measures of tropical cyclone (including typhoon) losses were unable to detect any effect of storm intensity on losses (Kahn (2005); Noy (2009)). If this result were accepted at face value, it would imply that variations in the intensity of cyclone climates have no effect on economies. In this section we show evidence that our measure of typhoon incidence predicts physical damages using both national data from EM-DAT as well as asset loss data from FIES.

5.1.1 *Prima Facie* Evidence from National Data

We begin by presenting *prima facie* evidence that aggregate losses scale with typhoon intensity in the Philippines. Using “standard” EM-DAT estimates for all the economic losses and deaths attributed to typhoons in each year, we estimate whether national losses increase with wind speed exposure, averaged over the entire country. National losses and their bivariate dependance on wind speed are shown in Figure 6. In Table 5 we present several ordinary least-squares estimates for the time-series regression

$$Z_t = \alpha W_t + \mu + \theta_1 t + \theta_2 t^2 + \epsilon_t \tag{1}$$

where Z is the log of total deaths or total economic losses, W is typhoon wind speed, μ is a constant, θ_1 and θ_2 are trend terms and ϵ is variation that we do not explain. Following Pielke et al. (2003) and Hsiang and Narita (2011), we also present models where the dependent variable Z is normalized by the size of the economy (GDP) or the country's population.

We find that national average typhoon exposure explains about a third of the variation in EM-DAT's estimates for both total typhoon damages and total typhoon deaths. In all models, the intensity of wind exposure is highly significant, with an increase in wind exposure by one meter per second increasing losses roughly 22%. We note that the economic damages estimated by EM-DAT include capital losses, lost revenue and any other "economic cost" that is associated with a storm, but it is impossible with these data to uncover finely-grained structure that might indicate the mechanism by which either damages or, for that matter, deaths occur.

5.1.2 Household asset losses

To estimate typhoons' impact on household assets, we use ordinary least-squares regression to estimate the linear probability that a household has each of several different types of physical capital recorded in the FIES data. We control for unobserved household attributes common across households in a given year or province by including province and year fixed effects. We further augment the model with controls for households' observable characteristics, namely: the total number of household members; the number of members above fourteen years old; and age, gender and education level of the household head. Finally, we control for the annual mean temperature and rainfall observed in each province in each year, since these variables are known to affect economic conditions (Miguel, Satyanath, and Sergenti (2004); Nordhaus (2006b); Schlenker and Roberts (2009); Dell, Jones, and Olken (2009); Hsiang (2010)) and they are driven by many of the same climatological factors that affect typhoon incidence[20]. Thus our complete regression model is

$$Z_{hprt} = \sum_{L=0}^{5} \left[\alpha_L W_{p,t-L} + \beta_L T_{p,t-L} + \gamma_L R_{p,t-L}\right] + \tau_t + \mu_p + \zeta X_h + \epsilon_{rt} + \epsilon_{ht} \tag{2}$$

where h indexes households, p indexes provinces, r indexes regions and t indexes years. Z is a one if a household has an asset and zero otherwise while W is typhoon wind speed, T is temperature, R is rainfall, τ is a year fixed-effect, μ is a province fixed-effect, X is the vector of observable household characteristics, ϵ_{rt} is a shock affecting all households in a region and ϵ_{ht} is a household level disturbance. We employ a distributed lag model to examine the effect of typhoon exposure for the five years prior to the survey, with lags indexed by L. In addition, because region-level shocks may exhibit unknown patterns of serial correlation and household-level shocks may exhibit spatial correlations at a sub-regional but supra-provincial scale, we cluster our estimated standard errors at the region level following Bertrand, Duflo, and Mullainathan (2004)[21] and Conley (1999).

Table 6 presents estimates of α for four of the most general and widely owned household assets: a closed toilet (eg. not a pail or open pit), a television, walls constructed with primarily strong materials (compared to light or salvaged materials) and access

[20]For example, typhoon activity in the West Pacific is affected by the El Niño-Southern Oscillation (Camargo and Sobel (2005)), which also influences temperatures and rainfalls in the Philippines .

[21]Because regions are aggregations of provinces and provinces are the level of treatment, clustering by region means we are also clustering at the level of treatment. Clustering at the province level does not appreciably change our results.

to electricity. We find that for all these assets response to previous year's typhoon treatment is negative and significant, varying between a 0.11% and 0.16% probability of loss per m/s of typhoon treatment, or 1.9 - 2.7 % given the average annual provincial treatment of 16.9 m/s. Cars, which we also show, do not respond at all, possibly because they are a valuable asset that can easily be moved quickly when typhoon forecasters warn populations about an impending storm[22]. We show additional results of typhoon treatment for an array of other household assets in appendix table C.2. We note that the average coefficient across all 14 assets in year zero is -0.036, versus an average coefficient in the first year lag of -.069. We estimate that the asset response in year 0 is thus 52.5% of the response in year 1, consistent with our observation that year-of typhoon impacts estimated using FIES data will be biased downwards due to averaging across the two waves of the survey and possible attrition.

Non-Linear Estimates of Asset Losses We now relax, and test, the assumption that physical damages, and hence asset losses, are linear in windspeed. It is plausible that damage is highly non-linear in windspeed; for example, Nordhaus (2010) and Mendelsohn et al. (2010) argue that losses are a power function of windspeed at landfall. However, these papers examine aggregate storm damages, similar to Equation 1, so it is not obvious whether estimates using our micro-data should have similar functional forms. Thus, we estimate the losses to wind speed non-parametrically, allowing the response function to have an arbitrary functional form, and examine whether it is approximately linear or not. To do this, we construct dummy variables that are one if exposure falls within a five meter per second range and zero otherwise

$$\tilde{W}_{p,t-1}^{[x,x+5)} = \mathbf{1}\left[W_{p,t-1} \in [x, x+5)\right]$$

[22]It is also possible that the coefficients for cars is small because there are a limited number of households in our sample that ever possess a car.

leaving events with 0-5 meters per second as the dropped bin. We then run the regression from Equation 2 where the inner product of these dummy variables and their coefficients replace the linear term $\alpha_1 W_{p,t-1}$. To limit the number of estimated parameters, we keep the remaining terms unchanged and focus our attention on the coefficients of these dummy variables. The full nonlinear model that we estimate is

$$\begin{aligned} Z_{hprt} =& \alpha_1^{[5,10)} \tilde{W}_{p,t-1}^{[5,10)} + \alpha_1^{[10,15)} \tilde{W}_{p,t-1}^{[10,15)} + \alpha_1^{[15,20)} \tilde{W}_{p,t-1}^{[15,20)} + \alpha_1^{[20,25)} \tilde{W}_{p,t-1}^{[20,25)} + \\ & \alpha_1^{[25,30)} \tilde{W}_{p,t-1}^{[25,30)} + \alpha_1^{[30,35)} \tilde{W}_{p,t-1}^{[30,35)} + \alpha_1^{[35,\infty)} \tilde{W}_{p,t-1}^{[35,\infty)} + \\ & \sum_{L \in \{0,[2,5]\}} \alpha_L W_{p,t-L} + \sum_{L=0}^{5} [\beta_L T_{p,t-L} + \gamma_L P_{p,t-L}] + \\ & \tau_t + \mu_p + \zeta X_h + \epsilon_{rt} + \epsilon_{ht} \end{aligned} \tag{3}$$

and it is estimated using the same method and sample as Equation 2. Panel A of Figure 7 displays these coefficients for six of the main asset types. The probability that households lose electricity, a closed toilet, walls made of strong materials, their television or their refrigerator increase approximately linearly with typhoon wind speed exposure. In contrast, the probability that a household loses a car to typhoon exposure remains near zero.

The linearity of these response functions indicates that our earlier estimate for the average number of households missing an asset due to typhoons was a good approximation. Furthermore, the coefficient for the 15-20 meter per second bin is generally in the range of 1.5-3%, matching our earlier linearized estimates. Finally, it is worth noting that exposures exceeding 35 meters per second (spatially-averaged) are not uncommon, recall Figure 4, and these stronger events cause 4-7% of households to lose their immobile assets.

5.2 Income Losses

Our approach to estimating income losses mirrors our approach to estimating physical damages. We focus our attention on income earned by households the year following storm exposure, partly to minimize the aforementioned attenuation risk in the FIES data and partly because that is where the result manifests most strongly. We again note that the measures of total household income collected by FIES include all reported transfers from other households and the government. Prior work by Yang (2008) demonstrated that tropical cyclone strikes increased remittances to some countries, suggesting that transfers provided a mechanism for income insurance. In addition, Fafchamps (2003) used Filipino micro data to show that some income shocks lead to inter-household transfers that partially compensate for losses[23]. These previous studies suggest that transfers might be important for mitigating household income losses, so it is fortunate that our measures of total income account for them.

5.2.1 Household Income Losses

To estimate the effect of typhoons on income, we estimate Equation 2 replacing Z with the natural logarithm of household income. Table 7 presents these results[24] in in columns 1-4. Including all all our control variables, we find that household income falls by 0.39% for each additional meter per second of windspeed exposure the year prior. This implies that under average exposure levels (16.9 m/s), average household income is depressed 6.6%. In column 5 we estimate the same model except we match households to the average exposure of its region (the larger administrative unit) rather than its province. Doing this allows us to include the 2000 and 2006 waves of the

[23]Deryugina (2011) finds similar results for Federal transfers in response to hurricanes in the United States.

[24]Similar to our results for capital losses, province fixed-effects are the most important control for limiting bias.

FIES which we could not do otherwise because households in these waves lack province identifiers. Using this longer sample with coarser measures of exposure, we continue to find a large effect of typhoon exposure on household income. In a final specification check presented in column 6, we collapse our household data to the province level, dramatically reducing our number of observations from 142,779 to 367. This allows us to conduct two additional checks: (1) whether we are over-estimating our true number of independent observations (Bertrand, Duflo, and Mullainathan (2004)) and (2) whether spatial correlations in ϵ cause us to underestimate our standard errors[25] (Conley (2008)). Estimating an analog[26] of Equation 2 using this collapsed data set and estimating spatially-robust standard errors[27] we find our coefficient of interest unchanged and that our standard errors increase only slightly.

In Table 8, we examine whether wage or entrepreneurial income responds more strongly to cyclone exposure. Entrepreneurial income is income from self-employed activities, including own agricultural cultivation, whereas wage income is income earned by selling labor to firms or other households. We find that self-employed entrepreneurial income responds strongly and negatively in the year following storms, falling -0.28% per m/s or 4.7% for average treatment. Non-agricultural wages also fall by an average of 3.2% per year, although the effect is not significant, and it is worth noting that both coefficients are negative, but not significant, in the second lagged year as well. This is in stark contrast to agricultural wages, which do not respond negatively in the first lag and exhibit little systematic variation in response to typhoon exposure.

Table 9 presents the estimated value of α_1 for different categories of entrepreneurial

[25]In theory, we could explicitly account for spatial correlation in errors using our micro-data, however it is not computationally cost effective.

[26]Using our collapsed data set requires that we introduce a lagged dependent variable into Equation 2 because aggregated output measures are highly correlated over time. We do not do this in the model with household data because that data set is not a true panel, so we do not know what household incomes were in the last period of observation.

[27]For technical reference, see Conley (1999).

income ranked by the number of respondents claiming any income from that source. None of these estimates are statistically different from zero, probably because the sample size declines rapidly for each subcategory. However, we find that the point estimates for lost income are consistently negative with only two exceptions: earnings from gambling and income in the transport and storage industry. We thus are confident in stating that income losses seem to not be driven by losses confined to a single sector.

Non-Linear Estimates of Income Losses We verify that our linear approximation of the income response is reasonable by estimating Equation 3 for household income. Panel B in Figure 8 plots the coefficient for each wind speed bin along with its confidence interval, and panel B of Figure 7 presents coefficients for the entrepreneurial and non-agricultural wage components of income. All of theses measures of household income loss are approximately linear in wind speed exposure. This linearity suggests that Equation 2 is a good approximation of the response function and it agrees with previously measured GDP responses to tropical cyclone exposure (Hsiang (2010)).

5.2.2 Income Losses at Different Locations of the Income Distribution

Kahn (2005), Skidmore and Toya (2007), Noy (2009), Hsiang (2010), Hsiang and Narita (2011), the United Nations (2009) and the World Bank (2010) have all suggested that poor populations experience larger relative losses to natural disasters, including tropical cyclones. Though compelling, these analyses have been based on country-level comparisons of income which may be tainted by an array of confounding factors as well as subject to omitted variable bias. We explore whether this relationship is plausible within-country by comparing losses for high and low-income households that inhabit the same Filipino province and are subject to the same institutional environment.

The FIES are not a true panel, so we cannot condition household income (or capital) losses on income in the previous period. We overcome this limitation by comparing how the *income distribution* in each province responds to typhoon exposure. To do this, we first collapse the data by province-year, retaining estimates of Y_{pt}^{q}, the household income at the q-quantile of the income distribution for province p in year t. Thus, for each value of q we have a panel of province by year observations that we use to estimate the model

$$Y_{pt}^{q} = \rho^{q} Y_{p,t-1}^{q} + \sum_{L=0}^{5} [\alpha_{L}^{q} W_{p,t-L} + \beta_{L}^{q} T_{p,t-L} + \gamma_{L}^{q} R_{p,t-L}] + \tau_{t}^{q} + \mu_{p}^{q} + \epsilon_{pt}^{q} \qquad (4)$$

where all coefficients are q-quantile specific versions of those described in Equation 2 and ρ^{q} is a q-quantile specific autocorrelation coefficient that we introduce because our collapse of the dataset generates substantial autocorrelation (recall column 6 of Table 7). Similar to before, our variable of interest is α_{1}^{q}, the relative shift of income observed at the q-quantile following the previous year's typhoon exposure.

Panel A of Figure 9 presents OLS estimates of α_{1}^{q} for $q \in \{10, 20, ...90\}$ along with confidence intervals. Strikingly, the semi-elasticity of income to wind speed is practically constant at all points along the income distribution, with the response always near the average household response (horizontal line). These results seem at odds with earlier cross-country studies that found different short-run responses for high and low-income populations. Yet, a completely different picture emerges when we examine the cumulative impact of typhoons on the shape of the income distribution. Panel B shows estimates for the cumulative effect of one additional meter per second in wind: $\sum_{L=0}^{5} \alpha_{L}^{q}$. When we sum coefficients for all the years following a storm, we see that incomes below the median suffer much larger cumulative losses than income above the median, which actually exhibit no cumulative losses. This occurs because losses at low ends of the income distribution persist for several years after the storm, whereas incomes at the high end of the distribution actually rise slightly above average a few years after the storm, allowing these groups to recover previously unearned income. Thus, when we look at typhoon-induced income losses beyond the first year we find strong evidence that the income distribution widens, with low-income households suffering differentially larger cumulative losses when compared to high-income households.

5.3 Consumption and Investment Effects

Having found strong evidence that household incomes respond negatively to typhoon-induced economic losses, we examine whether consumption and investment expenditures also adjust. To determine whether expenditures fall, we implement the same analysis that we conducted for income, but instead examine the consumption and investment variables available in FIES. Broadly, these variables are all "expenditures," although it is not always possible to clearly distinguish whether a specific variable represents "consumption" or "investment," since many expenditures represent a combination of the two. For example, "recreation" is clearly a consumption good, while "education" is mostly an investment in human capital, but "food" is probably some combination since food consumption increases immediate utility but also augments health, a specific dimension of human capital.

We note that a change in expenditures absent information about a potential change in prices does not allow one to infer changes in consumption. We check for and find little evidence that typhoons affect regional prices for food in Section B of the Appendix.

5.3.1 Household Losses to Consumption and Investment

We estimate Equation 2, replacing Z with the logarithm of household expenditures, and present the results in Table 10. In column 1 we show that household expenditures fall 0.42% for each additional meter per second in the prior year's typhoon wind speed.

This implies that under the average level of exposure (16.9 m/s), total expenditures are 7.1% lower than they would otherwise be due to the transient impact of the typhoon climate, mirroring the average income loss of 6.6%. In columns 2-11, we estimate Equation 2 for different expenditure subcategories. For eight out of ten subcategories we observe similar patterns of losses with the exceptions being recreational expenditures, which declines insignificantly, and repairs to the household's capital assets, which rise slightly. Notably, some of the largest relative declines in spending occur in categories related to human capital investments: personal care (-0.74% per m/s), medical services (-0.85% per m/s), and education (-0.79% per m/s). Food expenditures, another type of investment in human capital, do not decline as strongly. However, clear reductions in food expenditure seem to extend over a longer period of time, beginning immediately in the year of storm exposure and continuing for three years afterwards.

In Table 11 we decompose the response of food expenditures into its different subcategories. The strongest declines are clearly in the purchase of meat, with strong responses also appearing in the fish, dairy & eggs and fruit categories. Purchases of cereal also decline, but much less than the more nutritious foods. The overall structure of this response is consistent with previous observations by Subramainian and Deaton (1996) and Jensen and Miller (2008) that real income losses lead to a shift in food consumption that protects overall calorie intake at the expense of nutrients.

The last three columns of Table 11 present the expenditure response for nonalcoholic beverages, alcoholic beverages and tobacco. All three types of purchases decline in the year of typhoon exposure, however their responses after that year diverge: nonalcoholic purchase remain low for up to three more years, alcoholic purchases mostly recover but also have long but statistically insignificant declines, while tobacco purchases become insignificantly positive in the year following exposure but then return to their original level. This relatively lower income elasticity of alcohol and tobacco, pure consumption goods, relative to more nutritious foods, partially human-capital investments, agrees with our earlier observation that other non-food varieties of human capital investments decline more rapidly than expenditures that more closely resemble pure consumption goods (Table 10). This finding is consistent with the hypothesis that wealth shocks directly alter the utility function of household members by increasing the marginal utility of immediate consumption (e.g., Banerjee and Mullainathan (2010)).

Non-Linear Estimates of Consumption and Investment Losses Following our earlier estimates, we verify that our linear model is a good approximation of the expenditure response by non-parametrically estimating the impact to typhoon exposure. We estimate Equation 3 for total expenditures and present our coefficients in Panel D of Figure 8. We find that the response of expenditures is almost exactly linear and mirrors the response to income, which we illustrate by overlaying the two responses in Panel C of Figure 7. When we examine the various subcategories of expenditures, which we show in Panel D of Figure 7, we continue to observe responses that are linear in typhoon wind speed. Inspecting subcategories of food purchases, displayed in Panel E, we see the same linear structure.

5.3.2 Expenditure Losses at Different Locations of the Expenditure Distribution

We next examine how the expenditure distribution responds to typhoon incidence, demonstrating that it mirrors the response of in the income distribution. We estimate Equation 4 for both total expenditures and food expenditures, displaying our results in panels C-F of Figure 9. Identical to our results for the income distribution, the total expenditure and food expenditure distributions shift coherently the year after typhoon exposure. This shift persists for the following years at q-quantiles below the median, generating large cumulative impacts. In contrast, total expenditure actually rises in later years for households above median expenditure, leading to a cumulative impact near zero. Households above median food expenditure do not consume extra food in later years, however, so cumulative effects are observable throughout the distribution. It is plausible that this occurs because the benefits or utility from food consumption are not substitutable over time periods longer than a year.

5.4 Infant deaths

Our final analysis of household outcomes centers around estimating infant mortality. We first show that the entirety of the child and infant mortality response is driven by infant female deaths, and then provide evidence suggesting that these deaths are attributable to typhoon-induced economic losses and the resulting household decisions.

5.4.1 The Response of Infant Mortality

We estimate the effect of typhoons on child mortality by altering Equation 2 to reflect the structure of the DHS data. We arrive at the model

$$Z_{wrt} = \sum_{L=0}^{5} [\alpha_L W_{r,t-L} + \beta_L T_{r,t-L} + \gamma_L R_{r,t-L}] + \tau_t + \mu_w + \zeta X_{wt} + \epsilon_{rt} + \epsilon_{wt} \qquad (5)$$

where w indexes a woman. Here, Z is one if a woman reports that a child of the relevant demographic category died in year t and zero otherwise. X_{wt} are the time-varying traits of a woman's age and age-squared. μ_w is a woman-specific fixed-effect that controls for any time-invariant woman-specific traits. Estimates with this model benefit from the fact that our reconstructed panel contains more years (24) than the FIES data (6); they suffer, however, in that DHS reports only the region a woman lives in and not her province, substantially shrinking the number of distinct treatment groups that we have in any given year (13). To account for the fact that all women in a region are coded as receiving the same typhoon exposure, as well as to account for any serial correlation within or between women that in same region, we cluster our standard errors at the region-level.

We present estimates for our parameter of interest, α, in Table 12. The coefficients report the number of additional women, out of one-million, who report the death of a child in association with an increase in wind speed of one meter per second. The first column shows that there is a detectable increase in child mortality the year following a typhoon, with roughly 80 additional deaths (per one-million women) for an increase in exposure by one meter per second. For the period of observation, regional mean exposure was 15.3 meters per second. This suggests that in an average year roughly 1,220 women out of one-million would report a child dying due to typhoon exposure the prior year. Columns 2 and 3 decompose this response into deaths of male and female children, revealing that the bulk of these deaths are among females. Columns 4 and 5 examines whether theses female deaths are from young children and infants, and we see that almost all of the additional deaths are infant females: of the 80 child deaths per m/s, 73 of them are female infants. In contrast, examination of infant males in column 6 reveals that they do not contribute to the observed mortality response in the first lagged year at all.

5.4.2 Attributing Infant Mortality to Economic Conditions and Household Decisions

We chose to examine child mortality partly because we claimed that they reflect human capital stocks, and we interpret these female infant deaths as evidence that households are disinvesting in the health of their female infant children. However, it is possible that these deaths are not a result of disinvestment, but instead result from the physical trauma of exposure to the typhoon itself or the typhoon's aftereffects on the ambient environment, e.g., disease ecology. We lack data on the proximate cause of death, so we cannot completely rule out these hypotheses. However, we are able to demonstrate that this mortality response mirrors the economic response across multiple dimensions, strongly suggesting that economic conditions are causing these infant deaths. Further, we are able to demonstrate that the patterns of infant mortality are consistent with our understanding of how households reallocate resources in response to a wealth shock.

Temporal Structure The timing of female infant deaths does not suggest that they are a result of direct exposure to typhoons. We illustrate this point in Figure 2, where we estimate a version of Equation 5 at the region-month, rather than woman-year, level. The black line shows cumulative monthly infant female mortality impact of typhoon exposure, normalized such that the cumulative effect evaluated at the month preceeding typhoon impact is 0. We see that the coefficient of typhoon impact on month-of mortality is near zero, and the increase in mortality rates does not manifest until nearly a year after the storm has hit. Deaths continue to accumulate past the 12 month mark, beyond which all infants in the sample have been born after typhoon exposure. We can thus conclude that the bulk of deaths occur significantly later than any immediate traumatic impact of the storm could plausibly be acting.

It is reasonable to question whether fetal exposure to typhoons may be partly driving our results. The recent explosion of literature in fetal origins[28], including natural disasters' impacts on them (Simeonova (2011)), suggests that exposure to shocks while in utero can have serious deleterious effects on later health. One might thus posit that in utero weakening contributes to or perhaps even drives the increase in death rates. We explore this claim in Figure 2 where the dotted grey line shows the number of female

[28]For a detailed overview see Almond and Currie (2011).

births *resulting in an infant deaths* per million households. We see that in utero effects may be contributing somewhat to the increase in deaths, as evinced by the increase in births ending in infant death immediately after typhoon impact. We nonetheless note that a large portion of the total cumulative births resulting in deaths occur 9 or more months after typhoon exposure, when in utero effects are strictly impossible. We conclude that while in utero impacts may be accelerating the increase in infant deaths, they can at most be an auxiliary. This observation is further supported by the lack of a similar death pattern among infant males as detailed in Appendix Section B.

Returning to the annual data, we note that there is a striking agreement between the timing of depressed economic conditions and female infant mortality. The left panels of Figure 8 display the timing of income losses, expenditure reductions and female infant mortality. The spike in mortality coincides with the sharp reductions in income and expenditure described earlier. Both mortality and economic conditions remain abnormal two years after a typhoon, although effects are smaller and are only marginally significant. Differences only arise three and four years after the storm, when mortality remains slightly elevated but average income and expenditure return to their baseline values.

Non-linear Structure We look for nonlinear structure of the mortality response by altering Equation 5 so that α_1 is decomposed following Equation 3. Panel F of Figure 7 displays the response function for all children, all infants and female infants. The responses of the larger samples are noisy but approximately linear, but when we isolate the infant female deaths that are driving the pooled response we see an almost exactly linear response. Presenting the income, expenditure and female infant mortality responses in the right panels of Figure 8, we see that all three all three match in their linear responses to typhoon wind speed exposure.

Distributional Structure The DHS data lack income information, so to examine distributional aspects of our results we instead examine the mortality response at different locations in the *wealth* distribution. We outline our method for inferring household wealth in Appendix Section B. In Table 13 we present the response of female infant mortality for women above and below the median for assets, as well as for the bottom and top deciles. We find that in the year immediately following storm exposure, mortality is slightly higher in the lowest wealth groups, and moreover remains elevated for several years. This pattern of relative uniformity in the year after the storm, with a slower recovery for poor households, matches the response of income, expenditures and human capital investments, as we illustrate in Figure 4.

Spatial Structure Up to this point, we have only estimated response functions that pool all Filipino observations, however it is possible that some regions are more or less susceptible to typhoon-induced economic losses. If this is true, and if economic losses are the mechanism through which typhoons increase female infant mortality, then regions suffering larger typhoon-induced economic losses should also exhibit larger typhoon-induced infant mortality. To examine whether this is the case, we estimate region-specific versions of the coefficient α_1. We do this by modifying Equations 2 and 5 so that α_1 is interacted with a vector of region dummies. In the top panel of Figure 10, we plot $-\alpha_1^{food\ expenditure}$ against $-\alpha_1^{income}$ for each region. The strong positive correlation verifies that locations with larger typhoon-induced income losses are also the regions with larger reductions in typhoon-induced food purchases, one of the most important inputs to human capital. In the bottom panel, we plot $\alpha_1^{infant\ mortality}$ against the coefficient for income, finding that the regions with stronger economic responses to typhoons are also the regions with stronger mortality responses.

Gender Bias A striking feature of the response is that it is completely restricted to female infants, with no similar response in male infants, as can be seen in Table 12. Differentially worse health outcomes for female children in times of economic duress are a common result in the development literature, see e.g., Rose (1999) for a specific case and Duflo (2005) for an overview. This pattern is generally thought to arise because parents give less weight to girls' outcomes when making decisions about intrahousehold resource allocations. Maccini and Yang (2009) and Senauer, Garcia, and Jacinto (1988) both provide evidence that this dismal situation applies to the Philippines as well. Thus, our finding that typhoon-induced infant mortality is a strictly female phenomenon is consistent with previous work on the within-household allocation of resources following income shocks.

We note that it is possible that the gender differential in mortality could be partly driven, at least shortly after impact, by the commonly documented tendency of males to die in utero at higher rates than females (Almond and Currie (2011); Sanders and Stoecker (2011)). We examine this claim in section B in the appendix and note that while it may be occurring in our data, it cannot explain more than a portion of unobserved male deaths in the first year.

Resource Competition Among Siblings If the female infant mortality that we observe occurs because of disinvestment in female children, it is plausible that this disinvestment will be larger if the female infant faces greater competition for resources via older siblings, particularly older brothers[29]. We look for evidence that female children who must compete for resources with other children are more likely to die in the year following a typhoon by estimating the mortality response of four subsamples of female infants: those who are the first born to their mother, those who have only older sisters,

[29] See, for example, Butcher and Case (1994).

those who have only older brothers, and those who have both older sisters and brothers. Table 14 presents our results. We find that mortality among first born females is moderate, but it doubles when infants have older sisters and nearly doubles again if there are any older brothers. We interpret these findings as strong evidence that female infant mortality is driven by resource scarcity within households and not by physical exposure to typhoons themselves.

5.4.3 "Economic Deaths" Exceed "Trauma Deaths"

Our finding that infant mortality mirrors the structure of economic losses (in time, space, income/wealth, and storm intensity) combined with our finding that it is both gendered and enhanced by sibling competition, strongly suggests that these infant deaths are caused by economic conditions that deteriorate in the wake of typhoons and not by physical exposure to the typhoons themselves. We thus term the lagged mortality we observe in our data "economic deaths" to distinguish it from the "trauma deaths" resulting from direct physical exposure to the storm itself, e.g., via drowning or blunt injuries. We estimate that in the average year, the prior year's typhoon climate causes 1,130 additional female infant deaths in every one-million households, roughly 55% of female infant mortality in our sample. A back-of-the-envelope calculation[30] suggests that across the entire country this amounts to approximately 11,300 "economic deaths" annually. This number exceeds 721, the annual average[31] number of "trauma deaths" reported by EM-DAT across the entire population, by more than a factor of 15. These findings indicate that most of the Filipino mortality from typhoons does not result from physical exposure to the storm. Rather, the bulk of mortality occurs due

[30] We observe a death rate per woman-household of 1,130 deaths per million; 44.8% of women in the sample are non-migrants; and there were 22.3 million women aged 15-49 (the DHS age range) in 2007 according to the Philippine National Statistics Office as detailed at `http://www.census.gov.ph/data/pressrelease/2010/pr10162tx.html`.

[31] Between 1985-2006.

to deterioration of economic conditions and subsequent disinvestment in health and human capital.

5.5 Evidence of Adaptation to Typhoon Climates

Households should suffer positive typhoon loses only if adaptation to their typhoon climate is costly. Here, we briefly examine whether there is evidence of adaptation to typhoons using cross-sectional variation in typhoon climates (recall Figures 1 and 4).

5.5.1 Optimal Adaptation in Theory

We imagine that households can exert costly adaptive effort e to reduce their losses if a typhoon strikes[32]. If the cost function over e is convex, then households will exert adaptive efforts only until their marginal costs of effort equal the expected marginal benefits. Because adaptive efforts only provide benefits when a typhoon actually strikes, locations that have more frequent or more intense typhoons should have greater returns to adaptation. Thus, theory predicts that households located in relatively intense typhoon climates will invest more in costly adaptation, reducing their marginal losses when a typhoon actually strikes. Denoting a household's optimal level of adaptive effort e^*, we expect

$$\frac{\partial e^*}{\partial \bar{W}} > 0 \tag{6}$$

where $\bar{W}$ is expected typhoon wind exposure, a summary statistic for a location's typhoon-climate. Unfortunately, we cannot directly observe whether this is true because we do not observe e^*. However, increasing effort reduces marginal losses ($-\partial Y/\partial W$) to

[32]For example, households could reinforce the walls of their home.

a fixed level of actual typhoon exposure

$$-\frac{\partial}{\partial e}\frac{\partial Y(e)}{\partial W} < 0. \tag{7}$$

This enables us to infer that adaptation is occurring if we see that marginal losses decline as climates intensify. Assuming households optimize, we multiply equations 6 and 7 to obtain

$$-\frac{\partial}{\partial \bar{W}}\frac{\partial Y(e^*)}{\partial W} < 0 \tag{8}$$

a result that we now investigate empirically. For a more complete treatment of optimal adaptation to tropical cyclone climates, as well as empirical evidence from around the world, we refer readers to Hsiang and Narita (2011).

5.5.2 Cross-Sectional Evidence of Adaptation

We test Equation 8 by examining whether typhoon-induced losses vary with the typhoon climatologies of different Philippine regions. In the top panel of Figure 11, we plot the negative semi-elasticity of income ($-\partial Y/\partial W$) for each region against its average typhoon exposure ($\bar{W}$). Consistent with Equation 8, the marginal effect of typhoon exposure declines with increasingly intense typhoon climates. This suggests that populations do invest adaptive effort in response to their typhoon climate. However, we note that all regions have positive marginal losses, indicating that no region undertakes "complete adaptation" by driving their marginal damages to zero. In the lower panel we provide suggestive evidence that this adaptive response can also be observed in female infant mortality responses.

Two points regarding the top panel of Figure 11 are worth noting. First, the average losses due to cyclones remain high even for regions that exhibit high levels of adaptation.

This occurs because average exposure necessarily increases with the climatological wind speed, so more intense average exposure counteracts falling marginal losses. In Figure 12 we plot estimates for average annual losses and find that average total losses are almost constant across all climatologies[33]. The second point of note is that the slope of the OLS fit, representing the response of adaptive effort to climatological conditions, is -0.04. This implies that marginal losses decline by roughly 2.8% with each one meter per second increase in climatological wind speed. This number, estimated using only the within-Philippines cross-section, almost exactly matches Hsiang and Narita's (2011) earlier estimate (3%) which used the cross section of all countries in the world[34].

6 Summary and Discussion

We have shown that the typhoon climate of the Western Pacific imposes major economic and human costs on Filipino households. We observe this impact directly in the form of lost physical assets; measure its economic effect of depressing household income and reducing consumption and human capital investments; and lastly show evidence that these disinvestments have irreversible consequences, which we demonstrate by examining infant mortality. We discuss some implications of these results and policy options below.

The Magnitude of Typhoon Losses We summarize the magnitude of typhoon-induced losses in Table 15, where we estimate average annual losses attributable to the

[33]We fit a quadratic curve because total average cost should be quadratic if the response in Figure 11 is linear.

[34]Using the same measure of cyclones exposure (spatially averaged maximum wind speed), Hsiang and Narita (forthcoming) found that increasing average exposure by 1 m/s led to adaptive adjustments which reduced marginal damages by approximately 3% of their baseline value (when average exposure was 0 m/s). Our results, presented in Figure 11, indicate that increasing average exposure by 1 m/s reduces marginal income losses by roughly 2.8% of the analogous baseline value (marginal income losses are 0.0143 log points per m/s when average exposure is set to 0 m/s in Figure 11).

typhoon climate of the Philippines. We estimate that an average household's income was 6.57% lower in an average year due to the previous year's typhoon exposure. To place this in context, the Philippines' National Statistical Office estimates that the average family's savings rate was 14.7% in 2009[35]. These income losses are closely tracked by a 7.08% reduction in expenditures, with particularly large reductions to human capital investments such as education (13.3%) and medicine (14.3%). Food consumption is less elastic, falling 5.9% in the average year, but this masks large substitutions away from expensive and higher nutrient foods, such as meat and dairy, and towards cheaper calories, such as grains. We find that average levels of typhoon exposure raise female infant mortality rates by 1,130 deaths per million households per year. Accounting for the fact that a child is not born in every household in every year, this translates into 18.1 deaths per thousand live births. This amounts to 55.0% of the observed female infant mortality rate in our sample[36].

These results are large, and it is important to be clear when interpreting them. We calculate these losses based on mean typhoon incidence in the average province/region; so we interpret them as capturing the expected difference in outcomes between a typhoon-free year and a year experiencing average typhoon exposure (16.9 m/s). They can thus be thought of as mean losses conditional on the Philippines having the typhoon climate that it has; any major shift in that typhoon climate would necessarily lead to a host of adaptive responses that are impossible to estimate given lack of an observable counterfactual.

[35]Calculated using an average family income of 129,000 pesos and savings of 19,000 pesos for 2009. Data available at `http://www.nscb.gov.ph/secstat/d_income.asp`

[36]We calculate the ratio of deaths per million households per year to deaths per thousand infants in our sample to be 61.92. Note that the observed female infant mortality rate is our sample of 33.0 per thousand is close to the mean UN estimate for female infant mortality in the Philippines between 1985 and 2005 of 28.3 per thousand (data available from `http://data.un.org/`)

Implications for Economic Development Our results indicate that typhoons destroy existing capital and reduce investments in new capital. Both of these effects are a concern for economic development. As discussed in Hsiang (2010), Dell, Jones, and Olken (2011), and Pindyck (2011), climatic conditions that interfere with capital accumulation and economic growth are particularly pernicious because their effects are compounded over time. The repeated exposure of populations to tropical cyclones, both in the Philippines and elsewhere, probably slows the accumulation of capital stocks at the household level. Unfortunately, such long run effects are difficult to identify empirically because cross-sectional variations in cyclone-climates are correlated with unobservable omitted variables; hopefully, future research will address this challenge. Nonetheless, given our evidence, tropical cyclones should be added to the list of geographically-varying factors which may be contributing to spatial patterns in global economic development (Gallup, Sachs and Mellinger (1999); Nordhaus (2006b)).

Implications for Climate Change As previously discussed and shown in Figure 1, the Philippines has one of the most active typhoon climatologies in the world. The frequency with which typhoons impact the Philippines suggests that households must understand and incorporate typhoon risk into their economic decisions (Mendelsohn (2000); Hsiang and Narita (forthcoming)). Thus, we interpret our estimates as conditional on households having already exploited the full range of adaptive behaviors available to them. This assumption is supported by our cross-sectional evidence that levels of adaptation vary across typhoon-climates, results that match the cross-country findings of Hsiang and Narita (forthcoming) with striking precision. The fact that we continue to observe large typhoon impacts in one of the world's most intense typhoon climates, where populations have already adapted optimally, suggests that adaptation costs are so high that populations find they are better off suffering typhoon losses rather than investing in additional adaption. This has unsettling implications for future climate change policy.

In the design of climate change policy, adaptation to climatic changes is viewed as a substitute for efforts to mitigate climate changes directly (Stern (2006); Nordhaus (2008); de Bruin, Dellink and Tol (2009); Aldy et al. (2010); Patt et al. (2010)). If adaptation is generally inexpensive compared to mitigation, then the cost-minimizing strategy is to not invest heavily in mitigation and instead to rely primarily on adaptation. However, if adaptation is very costly, then mitigation should be utilized more vigorously. Our findings suggest that adaptation to tropical cyclones is extremely costly; thus policies cannot assume that adaption to changes in the future cyclone climate[37] will be cheap. This should increase the estimated social cost of greenhouse gas emissions, and concomitantly the value of mitigating these emissions. We may speculate that technological advances will reduce the future cost of adaptation, but until further evidence is martialled this remains an assumption. Moreover, if adaptation costs to tropical cyclones are representative of adaptation costs to a broader class of climatological phenomena, this would suggest that current models of future adaptation are too optimistic (de Bruin, Dellink and Tol (2009)).

[37] Knutson et al. (2010), a recent review of this topic, conclude

> [F]uture projections based on theory and high-resolution dynamical models consistently indicate that greenhouse warming will cause the globally averaged intensity of tropical cyclones to shift towards stronger storms, with intensity increases of 211% by 2100. Existing modeling studies also consistently project decreases in the globally averaged frequency of tropical cyclones, by 634%. Balanced against this, higher resolution modelling studies typically project substantial increases in the frequency of the most intense cyclones, and increases of the order of 20% in the precipitation rate within 100 km of the storm centre. (p. 157)

Thus the entire distribution of tropical cyclone events is expected to shift on average, with fewer low intensity storms but more frequent high intensity storms. However, there remains extensive uncertainty and the relationship between tropical cylcones and warming is an area of active research.

Policy Options in the Current Climate

Setting aside issues surrounding the future climate, it is important to note that there are a variety of targeted policies that might increase the welfare of Filipino households, or other typhoon-afflicted populations, in the current climate. At present, large-scale post-disaster management is almost entirely an *ad hoc* process that is strongly influenced by political concerns and the media (Besley and Burgess (2002); Garrett and Sobel (2003); Eisensee and Strömberg (2007); Yang (2008); Kunreuther et al. (2009); United Nations (2009)). However, our findings provide insight into systematic policies that could address typhoon-induced welfare loss.

Insurance Social insurance allows Filipino households to smooth their consumption over some, but not all, income shocks (Fafchamps (2003), Yang and Choi (2007)). Our observation that consumption responds strongly to typhoons, reflecting income changes, indicates that current insurance networks are not well-diversified against these events. Perhaps this occurs because typhoons are large with respect to insurance networks, a fact that would reduce the idiosyncratic component of the income shock (Townsend (1995)). Expanding insurance networks over larger spatial scales should reduce the uninsurable aggregate component of typhoon shocks; however this must be done carefully as even wealthy countries have struggled to sustainably insure tropical cyclone risk (Kunreuther et al. (2009)).

Credit Without looking specifically at credit markets, we cannot say exactly how they behave in the wake of Typhoons. However, we observe that income and consumption in low income households recover more slowly than in high income households. It is plausible that this differentially slow recovery persists because poor households are credit constrained, preventing them from efficiently rebuilding their capital stocks

(Duflo and Banerjee (2007); Noy (2009)). If this is true, subsidizing the development of credit markets for low-income households may increase their resilience.

Information It seems unlikely that the households in which female infants die are intentionally allowing these infants to perish. It is more plausible that parents believe their newborn can cope with higher-than-average levels of neglect, and that there will be limited permanent damage (Duflo and Banerjee (2011)). Unfortunately, for a small number of unlucky families, this assumption proves false. It may be the case that simply educating parents about the risks of post-typhoon neglect will be enough to mitigate a large portion of typhoons' effect on infant mortality.

Targeted Subsidies If household decisions were made to perfectly optimize household welfare, than post-disaster economic decisions would be efficient. Unfortunately, it seems that children's long-term welfare, which depends in part on their human capital, is differentially neglected in comparison to short-term consumption of goods like recreation, tobacco and alcohol. In this situation, it may be optimal to tax adults to finance human capital subsidies that specifically target children. To avoid political manipulation of these subsidies, it might be possible that they be indexed to verifiable measures of typhoon exposure (Hellmuth (2009)).

Technology Standards Because it is difficult for consumers to verify the quality of infrastructure, construction quality may not be properly priced into markets (Olken (2007)). This could introduce additional uncertainty into households' calculation of the economic risk they bear in a particular typhoon-climate. To correct for this market imperfection, it may be optimal for governments to enforce building codes or other technology standards that mandate a specified level of robustness to typhoon exposure.

Research and Develop Adaptation Technologies We find a continuous gradient in levels of adaptation that reflects the gradient in cyclone risk. This suggests that adaptation technologies are effective, however the cost of adopting additional adaptation technology is binding throughout the Philippines (Hsiang and Narita (forthcoming)). Thus, research and development that raises the effectiveness or reduces the cost of adaptive technologies should induce households to employ greater levels of self-protection.

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7 Tables and Figures

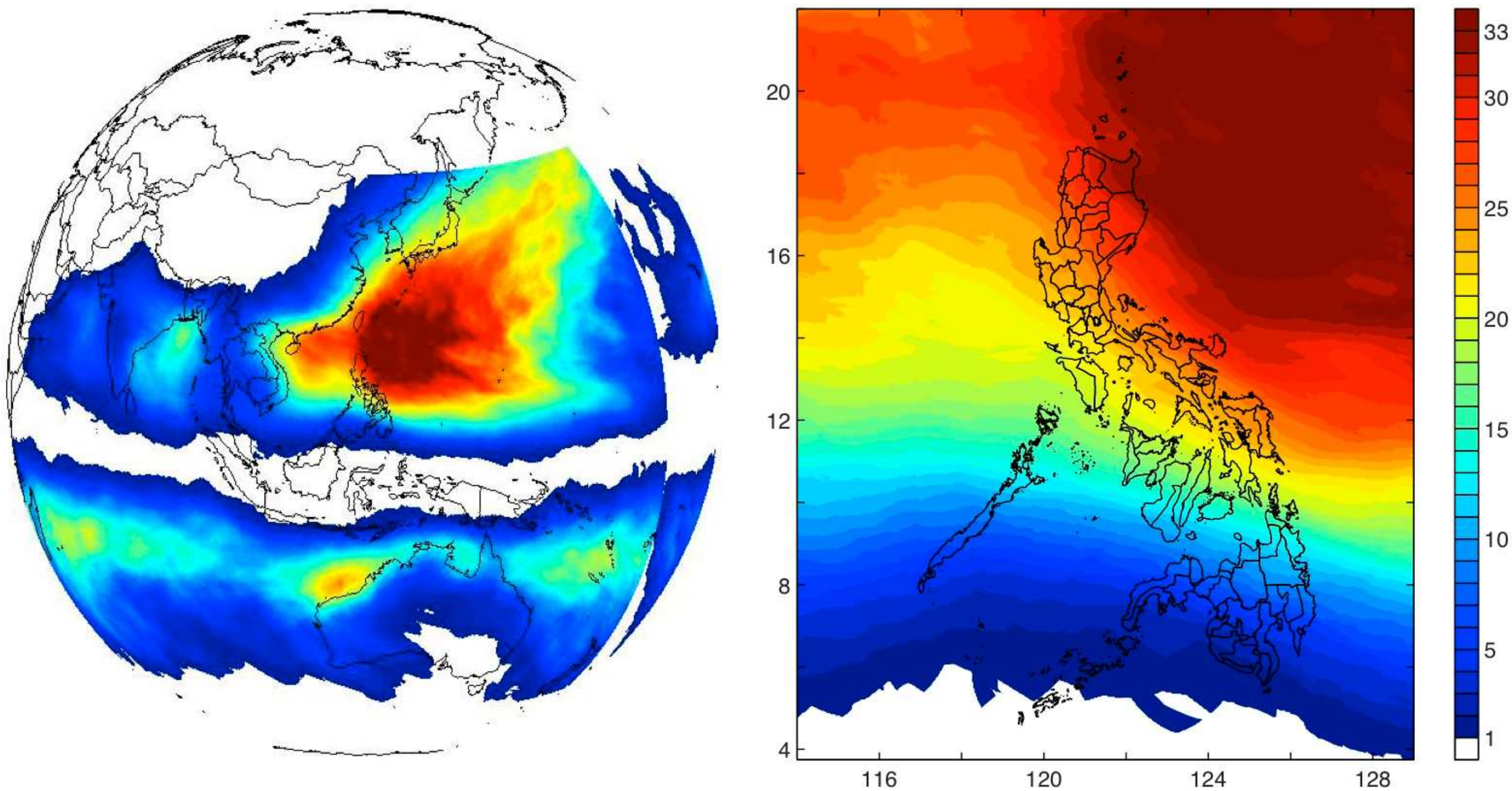


Figure 1: Annual maximum wind speed (in meters per second) averaged over 1950-2008. Data: LICRICE.

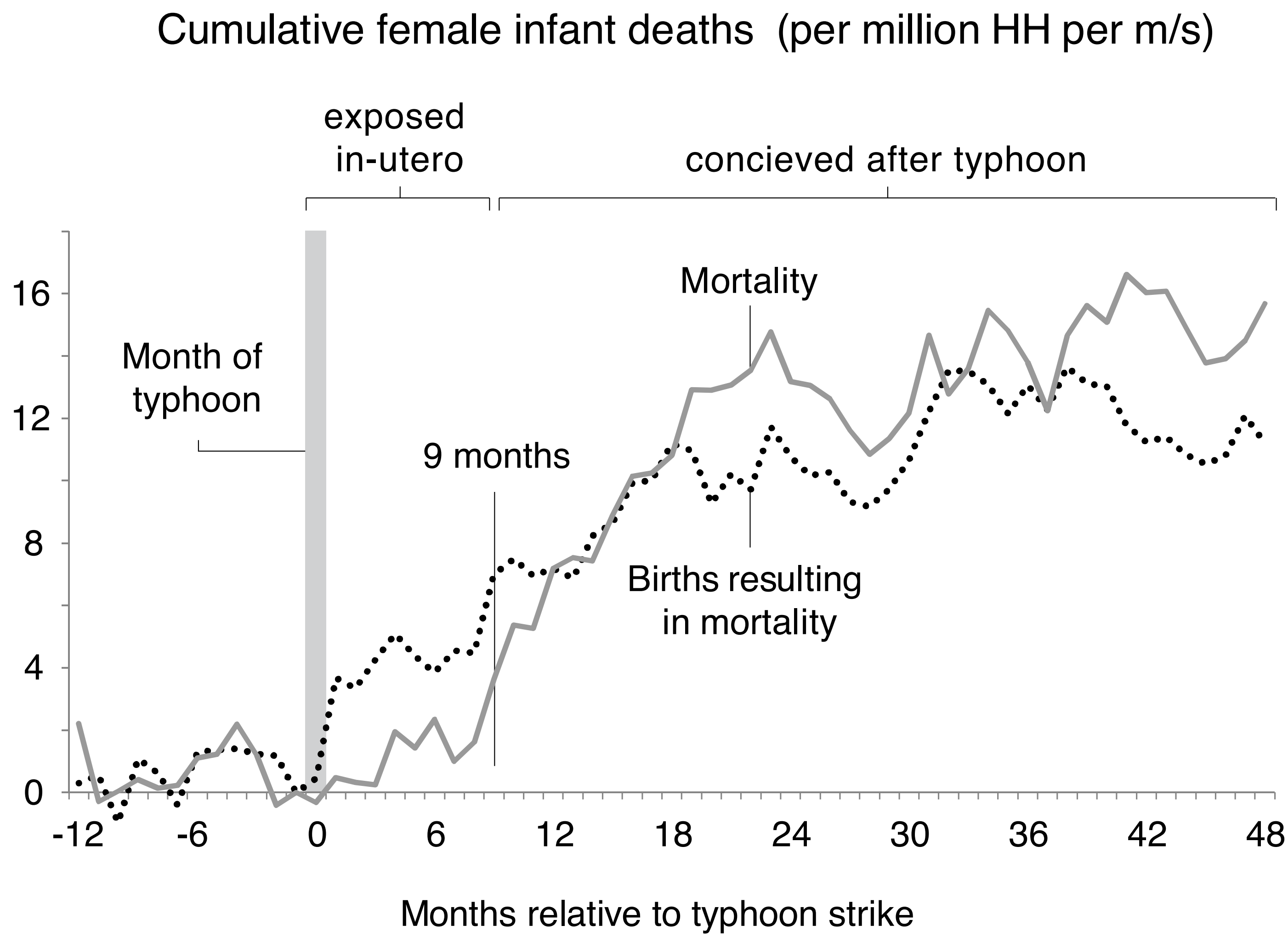


Figure 2: Cumulative impact of typhoons on number of infant female deaths (grey line) and number of female births resulting in infant deaths (dotted black line) per million households. Observation is at the regional aggregate level. Cumulative effect normalized such that month prior to typhoon impact is zero. Includes region, year, and month fixed effects, lagged temperature and precipitation controls, and lagged dependent variable.

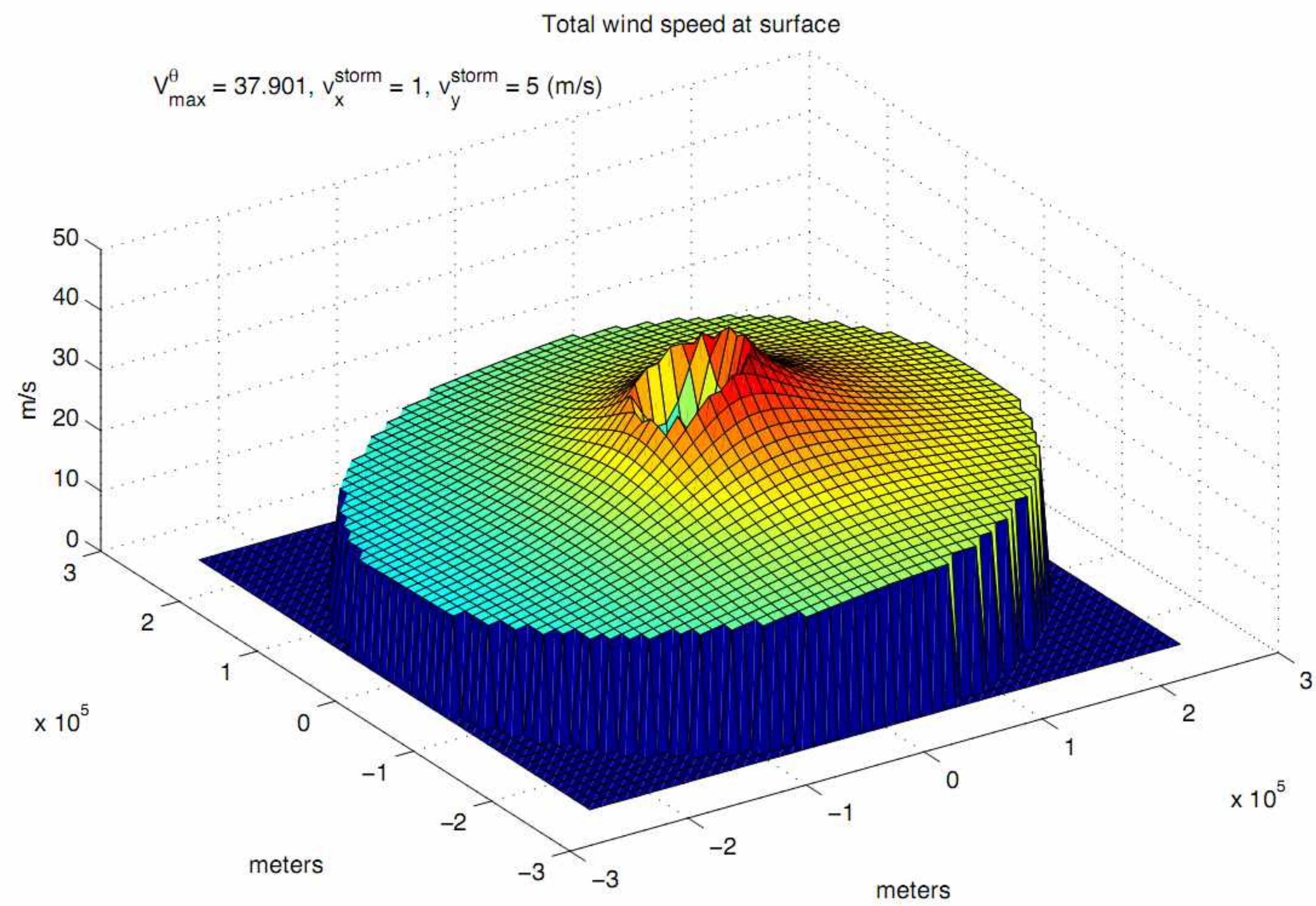


Figure 3: Snapshot of the parametrized typhoon wind field used by LICRICE to reconstruct exposure at the surface. The storm is modeled as a translating Rankine vortex, the storm pictured here is translating 1 m/s in the “x” direction (into the page and to the right) and 5 m/s in the “y” direction (into the page and to the left). The maximum windspeed on the ground (V^{θ}_{max}) is 37.901 m/s, the sum of rotational (azimuthal) winds and winds generated by the storm's translation.

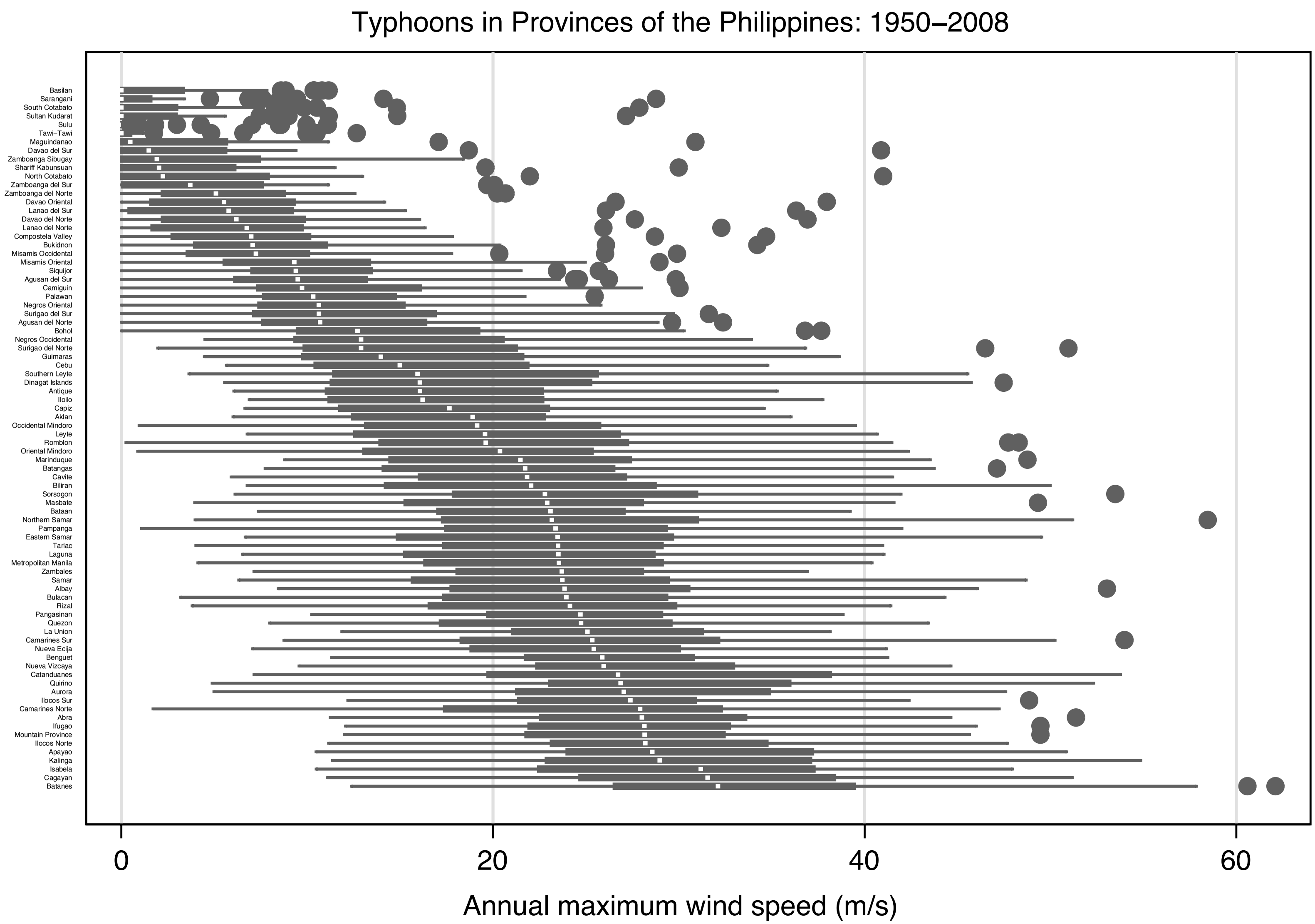


Figure 4: Box-whisker plot of provincial annual maximum wind speed measure, 1950-2008. Provinces (y axis) are ranked by median annual maximum wind speed (white dots). Shaded boxes show interquartile range of wind speed distribution; grey dots show outlier years. Data: LICRICE.

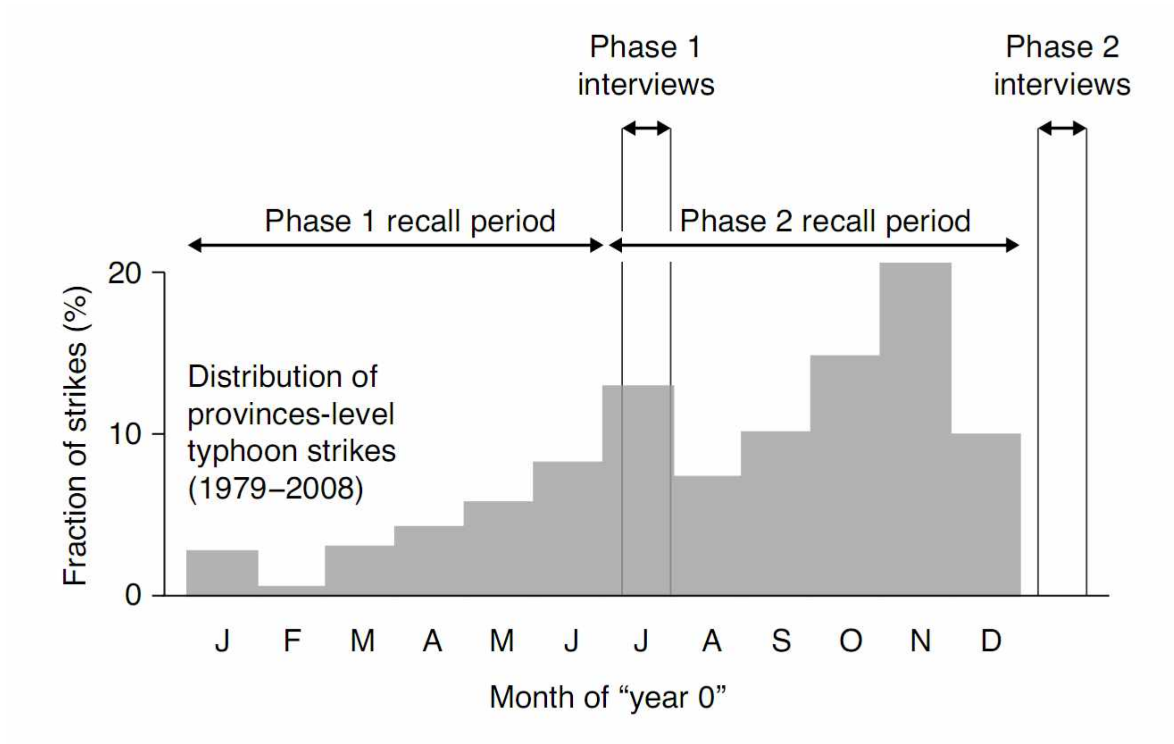


Figure 5: Timeline of FIES data collection overlaid with a histogram of typhoon events by month.

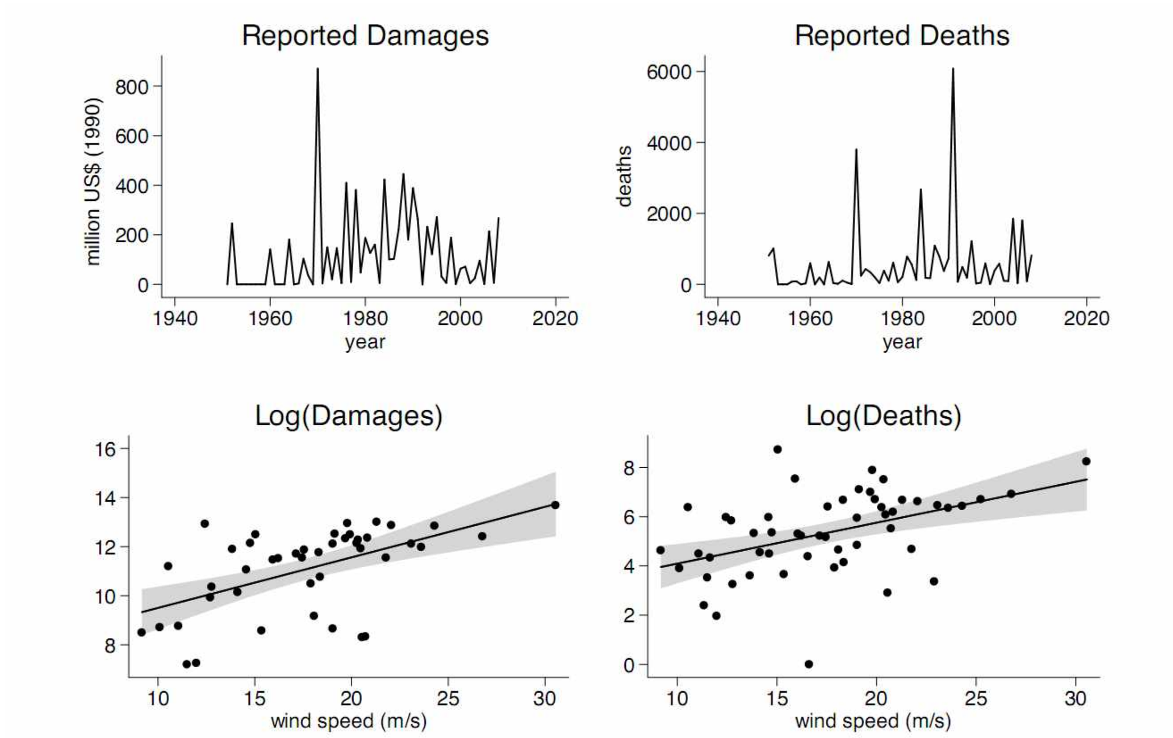


Figure 6: Country-level losses to typhoons for the Philippines. Reported damages and deaths from the EM-DAT dataset are shown as time series from 1950-2008. Scatter plots show log losses as a function of wind speed averaged nationally. Fit is OLS with confidence interval.

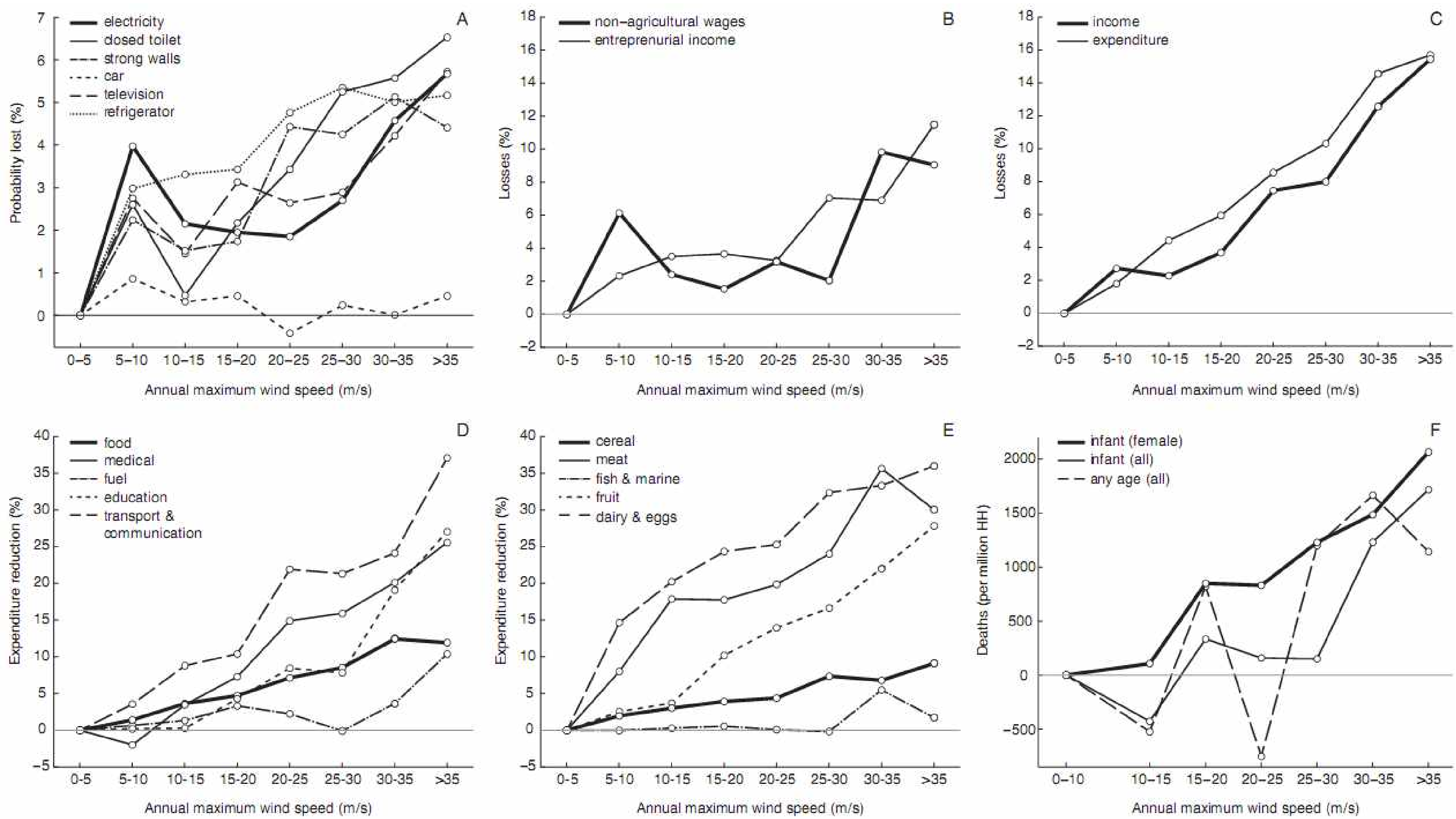


Figure 7: Household losses as a nonlinear function of previous calendar year's typhoon exposure. Losses are on the y-axis, x-axis shows coefficient on indicator variables for previous year's maximum wind speed.

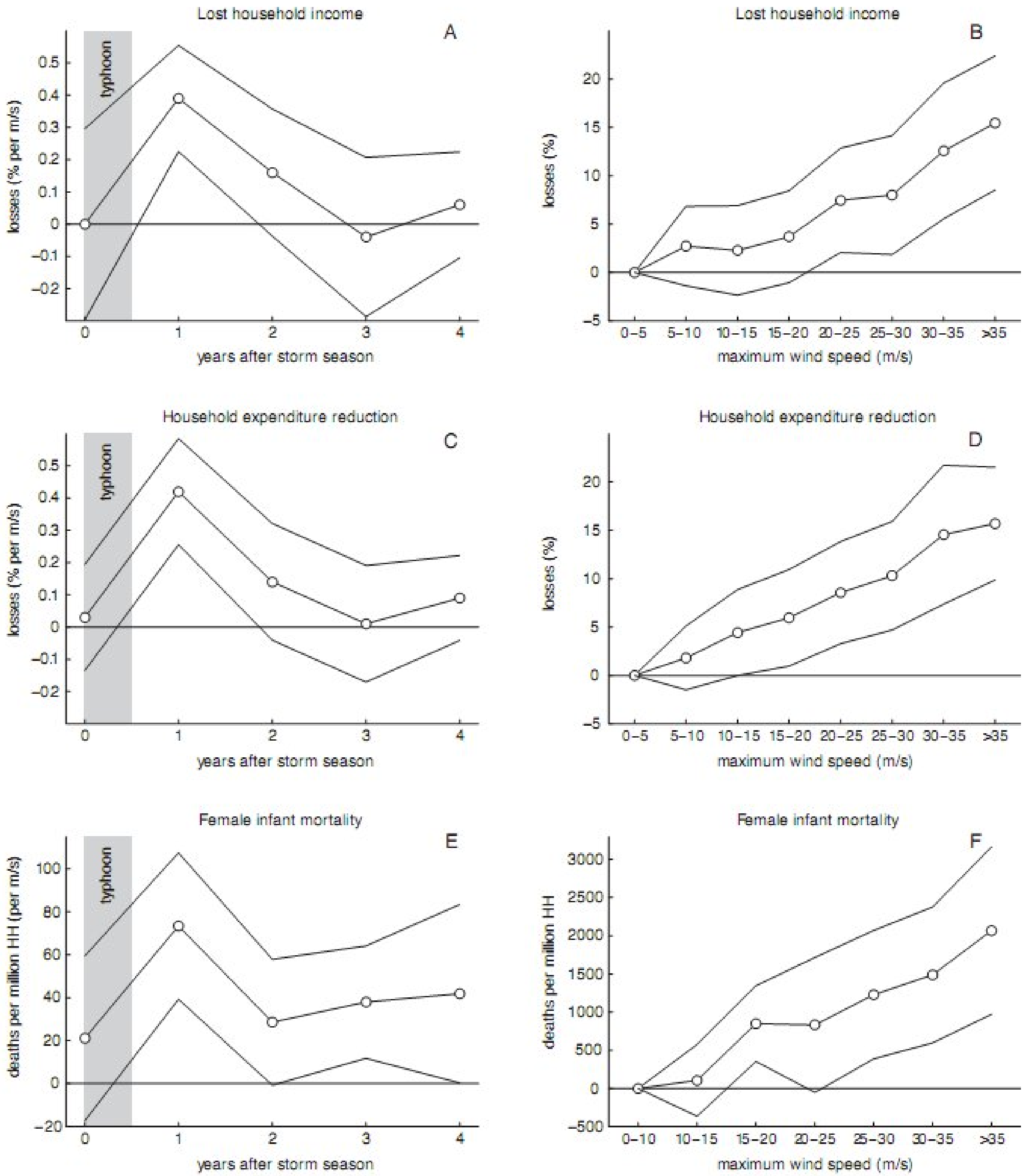


Figure 8: Similarities in the structure of typhoon losses across categories. Dynamic (left column) estimates of typhoon impacts show responses for 5 years starting in the calendar year of exposure (year 0). Nonlinear (right column) estimates show coefficients on indicator variables for previous year's (Lag=1) maximum wind speed. Thin lines are 90% CI.

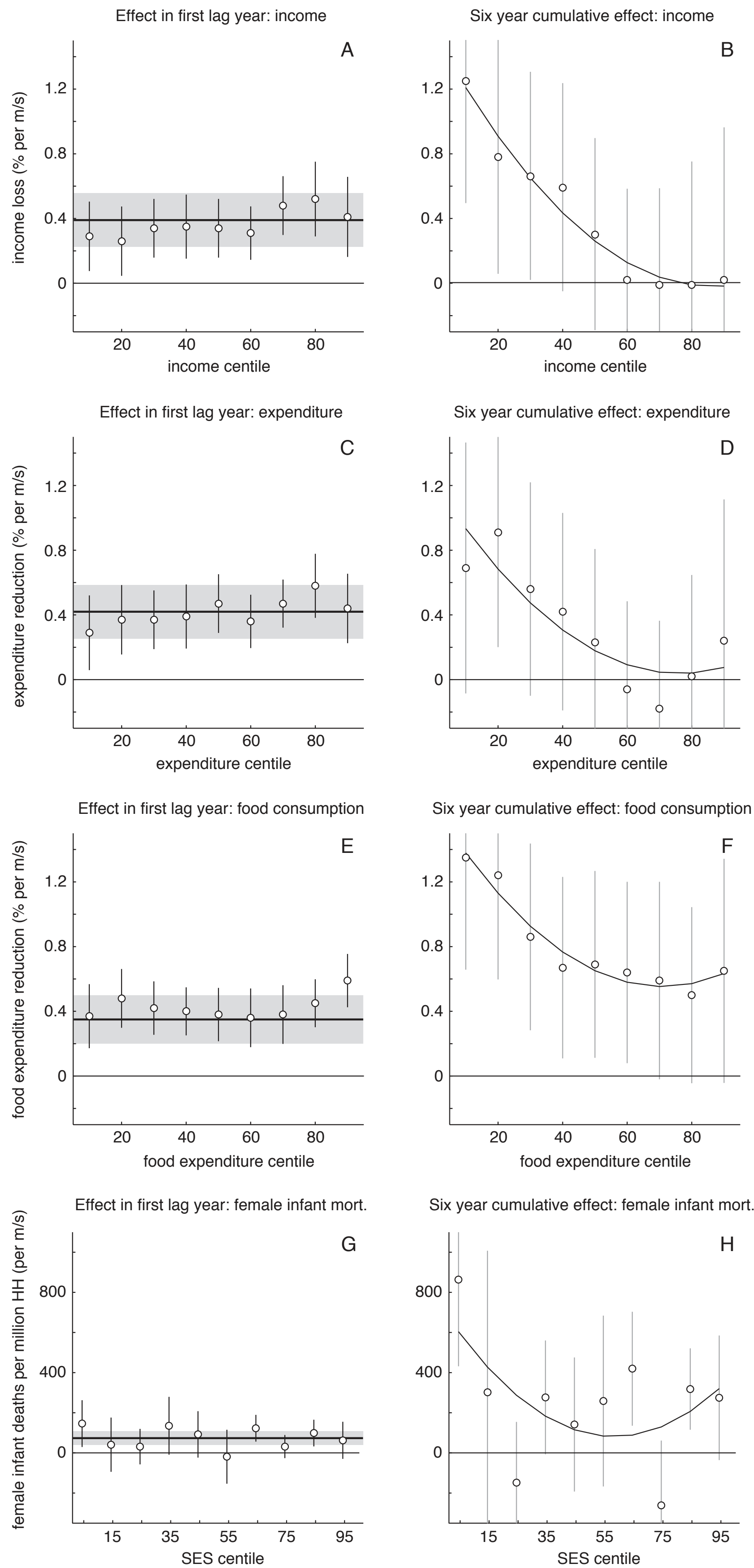


Figure 9: The effect of of typhoons on the shape of income, expenditure, and wealth distributions. Left column shows the effect of the previous year's typhoon exposure at different q-quantile cutoffs (A, C, E) and inferred socioeconomic status (G). Right column shows the cumulative impact over six years. Whiskers are 2-σ for each point-estimate. Thick horizontal lines in A, C, E, and G are the main effect from the baseline model, shaded regions are the 90% CI. Curves in B, D, F, and H are simple OLS fits to the estimated coefficients. Each coefficient is estimated in a separate province-by-year panel containing province fixed effects, year fixed effects, controls for temperature and rainfall and a lagged dependent variable.

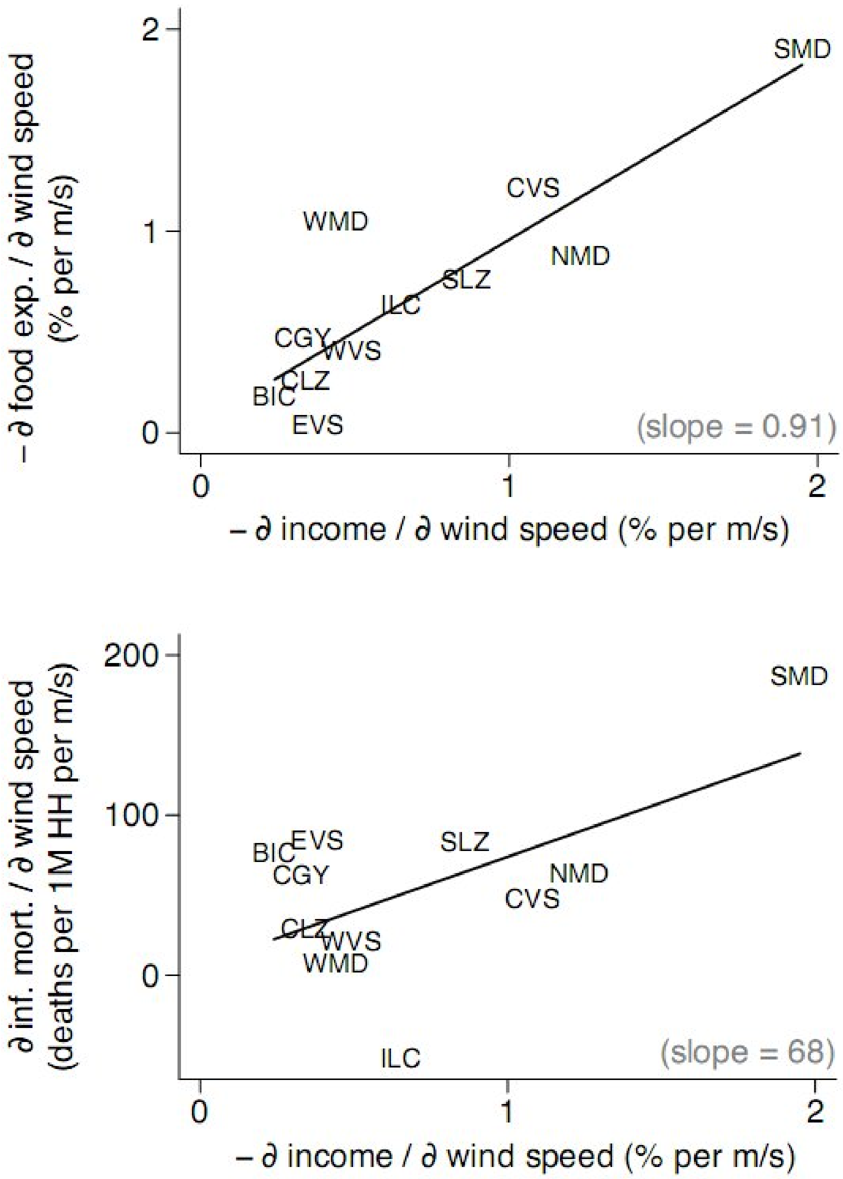


Figure 10: Cross-sectional correlation of region-specific coefficients. Regions that suffer relatively larger income losses (x-axis) also suffer larger reductions in food expenditure (top) and larger increases in infant mortality (bottom), holding the intensity of typhoon exposure fixed.

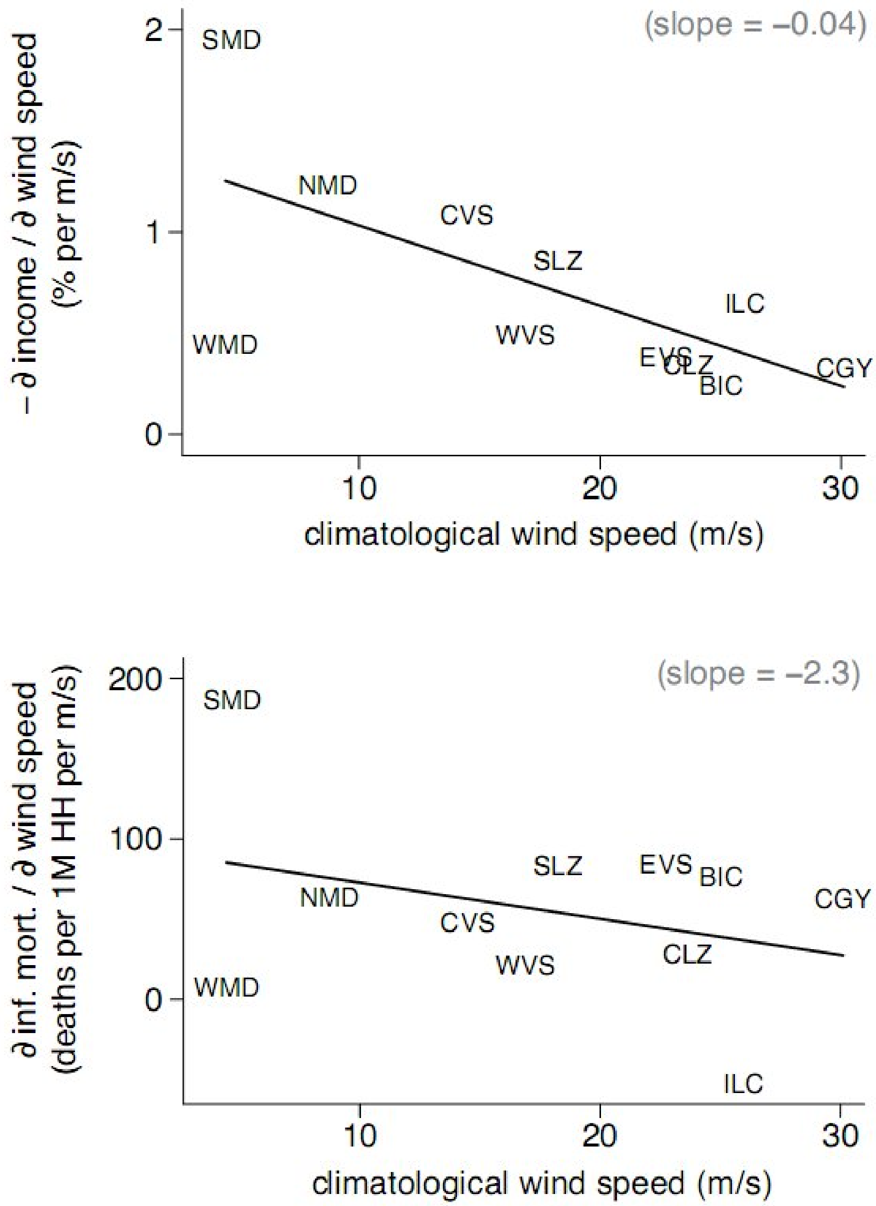


Figure 11: Cross-sectional evidence of adaptation to sub-national typhoon climates. Regions with higher mean exposure (x-axis) generally suffer smaller losses to income (top) and infant life (bottom) when the intensity of typhoon exposure is held fixed.

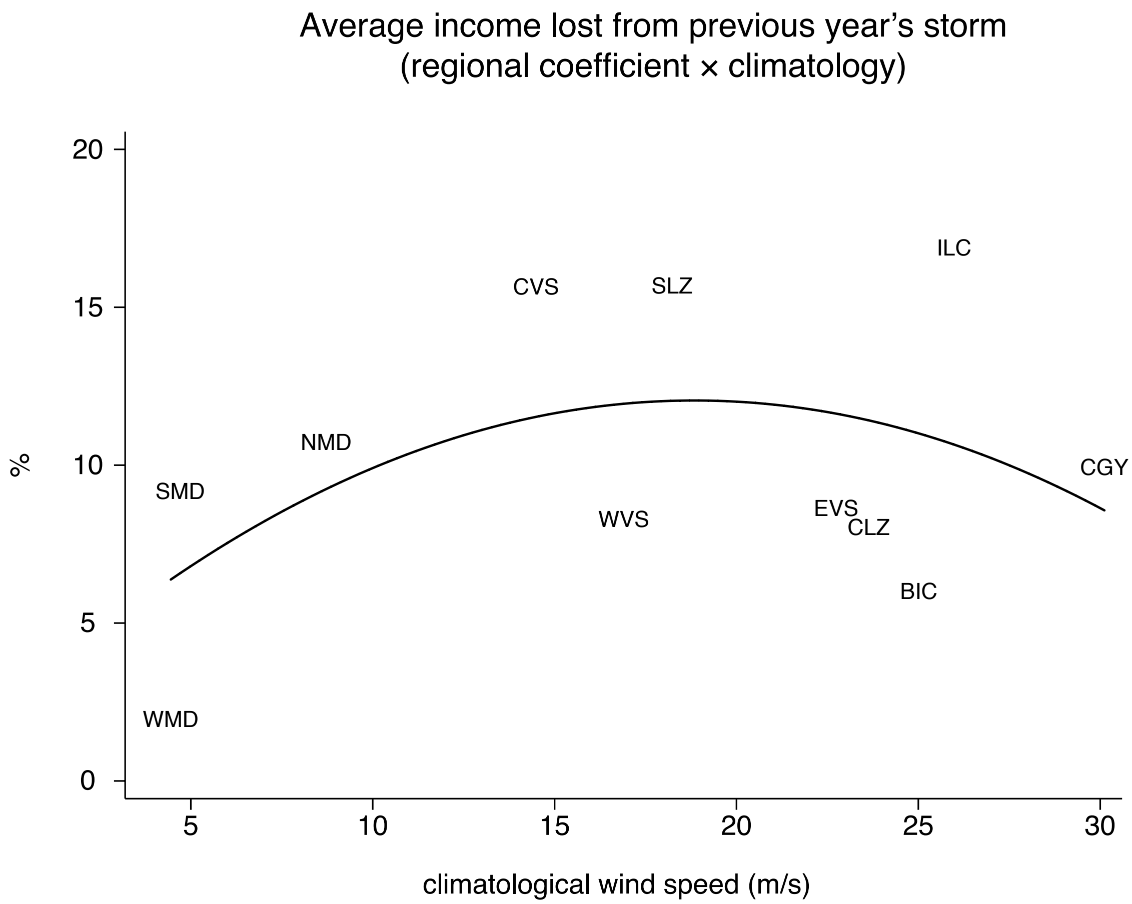


Figure 12: Average annual income loss by region. Regions exhibit marginal losses that decline as average wind speed rises (Figure 11). However, average treatment increases with average exposure, by definition. The result is an average annual loss that is quadratic in average wind speed.

Table 1: Typhoon exposure (maximum wind speed) summary statistics.

Unit of observation	Years	N	Mean	Std. Dev.	Min	Max
Province	1950 - 2008	4838	17.6	12.0	0.0	62.1
Province	1979 - 2008	2460	16.9	11.6	0.0	53.5
Region	1950 - 2008	885	16.1	11.5	0.0	47.4
Region	1979 - 2008	450	15.3	11.0	0.0	45.9
Nation	1950 - 2008	59	17.3	4.6	9.2	30.5
Nation	1979 - 2008	30	16.5	4.0	9.2	23.6

Notes: Maximum wind speed measured in meters per second.

Table 2: Summary averages for FIES households

VARIABLES	
Total number of household members	5.2
	[2.27]
Number of household members above age 15	4.1
	[1.78]
Age of household head (yr.)	47.6
	[14.16]
Household head is male (%)	85.2
	[35.5]
Household head completed no school (%)	5.7
	[23.19]
Household head completed primary school (%)	64.0
	[48.01]
Household head completed secondary school (%)	33.6
	[47.2]
Total household:	
Income (PHP)	127500
	[157600]
Expenditures (PHP)	103700
	[106000]
Food expenditures (PHP)	50500
	[33100]
Education expenditures (PHP)	4000
	[11900]
Medical expenditures (PHP)	2200
	[11300]
Observations:	142789
Household has:	
Electricity (%)	62.6
	[48.4]
Closed toilet (%)	72.2
	[44.8]
Strong walls (%)	54.5
	[49.8]
Television (%)	39.1
	[48.8]
Car (%)	6.7
	[24.9]
Observations:	107620

Notes: Standard errors shown in parentheses. Income and expenditures shown in year 2000-equivalent Philippine Pesos.

Table 3: Summary averages for DHS households

Variable	
Age	28.65
	[10.08]
Married (%)	48.32
	[49.97]
Wife has no education (%)	3.2
	[17.6]
Wife has post-secondary education (%)	28
	[44.9]
Husband has no education (%)	4.17
	[20]
Husband has post-secondary education (%)	22.5
	[41.7]
Wife's total children born	2.0
	[2.57]
Wife's total sons born	1.02
	[1.49]
Wife's total daughters born	0.95
	[1.42]
Observations	24841

Notes: Standard errors shown in parentheses. Non-migrant households only.

Table 4: Checking for balance on typhoon exposure in FIES data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Household head characteristics				
VARIABLES	Total family size	Single family dwelling (%)	Married (%)	Male (%)	Uneducated	Only primary school	Only secondary school
Max windspeed 5-10 m/s	0.0594	-0.81	0.30	-0.30	1.02	-3.06**	-0.45
	[0.0603]	[1.27]	[0.53]	[0.42]	[0.81]	[1.27]	[0.97]
10-15 m/s	0.1480*	-1.12	0.68	-0.17	0.67	-2.69*	-0.24
	[0.0781]	[1.60]	[0.66]	[0.65]	[0.85]	[1.55]	[1.27]
15-20 m/s	0.1205	-0.68	0.50	0.25	0.83	-3.00*	-1.04
	[0.0864]	[1.77]	[0.65]	[0.63]	[0.86]	[1.64]	[1.39]
20-25 m/s	0.1968*	-0.91	0.70	0.76	0.96	-3.26	0.40
	[0.1000]	[1.84]	[0.77]	[0.78]	[1.02]	[2.05]	[1.68]
25-30 m/s	0.0748	-0.15	0.28	0.85	0.24	-1.41	2.64
	[0.1099]	[1.87]	[0.81]	[0.81]	[1.06]	[2.20]	[1.88]
30-35 m/s	0.0808	0.81	0.44	0.53	0.97	-2.14	2.59
	[0.1139]	[2.03]	[1.05]	[1.04]	[1.11]	[2.09]	[1.82]
35+ m/s	0.1046	-0.49	0.67	1.55*	0.73	-1.21	3.13
	[0.1171]	[2.09]	[0.98]	[0.90]	[1.16]	[2.29]	[1.94]
Observations	142,789	142,789	142,789	142,789	142,789	142,789	142,789
R-squared	0.0145	0.02	0.01	0.01	0.08	0.14	0.24

Notes: Standard errors clustered at the treatment (province) level in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0 and 2-5 estimated but not shown. Includes province and year fixed effects and lagged temperature and precipitation controls.

Table 5: Country level typhoon damages.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Damages (log)	Damages (log)	Damages / GDP (log)	Damages / GDP (log)	Killed (log)	Killed (log)	Killed / pop. (log)	Killed / pop. (log)
Maximum wind speed (m/s)	20.62***	23.65***	26.34***	22.23***	16.63***	22.39***	20.62***	22.00***
	[4.24]	[5.05]	[4.89]	[5.75]	[3.54]	[3.65]	[3.53]	[3.84]
Observations	44	44	37	37	53	53	48	48
R-squared	0.31	0.34	0.41	0.48	0.21	0.35	0.30	0.34
Linear and quad trends	-	Y	-	Y	-	Y	-	Y

Notes: Typhoon treatment at national level. White standard errors in brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table 6: Typhoon impact on assets

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Has electricity (%)	Has closed toilet (%)	Strong walls (%)	Has TV (%)	Has car (%)
Max wind speed, T=0	-0.21*	-0.04	-0.08	-0.17*	-0.03
	[0.11]	[0.10]	[0.10]	[0.09]	[0.03]
T + 1	-0.14**	-0.16***	-0.11**	-0.12**	0.01
	[0.06]	[0.03]	[0.04]	[0.05]	[0.02]
T + 2	0.01	-0.09	-0.09	0.11*	-0.03
	[0.07]	[0.06]	[0.08]	[0.06]	[0.05]
T + 3	0.04	-0.12	0.12	-0.10*	-0.01
	[0.06]	[0.09]	[0.10]	[0.05]	[0.03]
T + 4	-0.07	-0.11	0.01	-0.15***	0.01
	[0.04]	[0.06]	[0.05]	[0.03]	[0.02]
Observations	107,620	107,620	107,620	107,620	107,620
R-squared	0.28	0.21	0.21	0.34	0.07

Notes: Notes: Standard errors clustered at the treatment (province) level in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only shown for 0-4. Includes province and year fixed effects, lagged temperature and precipitation controls, and household controls consisting of the number of members, working and non, in household as well as household head's gender and education level.

Table 7: Household income as a function of typhoon exposure and covariates

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Income (%)	Income (%)	Income (%)	Income (%)	Income (%)	Income (%)
Max wind speed, T=0 (m/s)	0.26	-0.10	0.02	-0.00	0.27	-0.01
	[0.26]	[0.14]	[0.15]	[0.18]	[0.16]	[0.15]
T + 1	-0.88***	-0.33***	-0.35***	-0.39***	-0.58***	-0.39***
	[0.19]	[0.09]	[0.09]	[0.10]	[0.14]	[0.14]
T + 2	1.04***	0.01	0.00	-0.16	-0.17	0.02
	[0.32]	[0.10]	[0.08]	[0.12]	[0.14]	[0.14]
T + 3	0.16	-0.14	-0.17	0.04	-0.22	0.29
	[0.17]	[0.14]	[0.13]	[0.15]	[0.15]	[0.18]
T + 4	-0.58**	-0.08	-0.09	-0.06	-0.03	-0.05
	[0.25]	[0.09]	[0.11]	[0.10]	[0.09]	[0.11]
Observations	142,789	142,789	142,779	142,779	174,896	367
R-squared	0.32	0.38	0.57	0.57	0.55	0.95
Exposure:	province	province	province	province	region	province
Province FE	-	Y	Y	Y	-	Y
Region FE	-	-	-	-	Y	-
HH controls	-	-	Y	Y	Y	-
Lagged temp, precip	-	-	-	Y	Y	Y
Lagged dependent var.	-	-	-	-	-	Y
SE clustered at region	Y	Y	Y	Y	Y	-
Years 1985 to ...	2000	2000	2000	2000	2006	2000
Conley (spatial) SE	-	-	-	-	-	Y

Notes: Percent change calculated as log points *100 per m/s of max wind speed. Standard errors in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only shown for 0-4. Spatial SE calculated using a distance of 250km and uniform weights. Column 6 is collapsed to the province level. Income includes all wages, salary, and net transfers.

Table 8: Typhoon impact on income by source

	(1)	(2)	(3)
VARIABLES	Entrepreneurial income (%)	Non-agricultural wages (%)	Agricultural wages (%)
Max wind speed, T=0 (m/s)	-0.11	0.04	-0.10
	[0.22]	[0.22]	[0.35]
T + 1	-0.28**	-0.19	0.04
	[0.11]	[0.15]	[0.27]
T + 2	-0.15	-0.08	-0.29
	[0.15]	[0.18]	[0.30]
T + 3	0.02	0.35	-0.20
	[0.22]	[0.21]	[0.34]
T + 4	0.12	-0.02	0.34*
	[0.12]	[0.15]	[0.21]
Observations	96,989	77,754	30,773
R-squared	0.22	0.36	0.24

Notes: Percent change calculated as log points *100 per m/s of max wind speed. Standard errors clustered at the treatment (province) level in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only shown for 0-4. Includes province and year fixed effects, lagged temperature and precipitation controls, and household controls consisting of the number of members, working and non, in household as well as household head's gender and education level.

Table 9: Typhoon impact on entrepreneurial income categories

	Max wind speed, T + 1 (m/s)		
VARIABLES	% change	SE	N
Entrep. income	-0.28**	[0.11]	96,989
Crop farming / gardening income	-0.29	[0.21]	52,193
Trade income	-0.18	[0.18]	30,479
Livestock / poultry income	-0.46	[0.44]	17,158
Gambling winnings	0.28	[0.46]	10,776
Fishing income	-0.40	[0.24]	10,258
Manufact. income	0.08	[0.42]	8,715
Transport / storage income	0.15	[0.26]	7,855
Services income	-0.10	[0.45]	7,011
Forestry / hunting income	-0.44	[0.71]	2,537
N.A. / entrep. income	-1.04	[0.96]	1,454
Construct. income	-1.56	[1.24]	870

Notes: Percent change calculated as log points *100 per m/s of max wind speed. Standard errors clustered at the treatment (province) level in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only shown for 0-4. Includes province and year fixed effects, lagged temperature and precipitation controls, and household controls consisting of the number of members, working and non, in household as well as household head's gender and education level.

Table 10: Typhoon impact on expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
VARIABLES	Total Exp. (%)	Food (%)	Fuel (%)	Personal Care (%)	Clothing (%)	Travel / Comm. (%)	Medical (%)	Education (%)	Special Events (%)	Recreation (%)	Repair (%)
Max wind speed, T=0 (m/s)	-0.03	-0.22**	0.14	-0.01	-0.18	0.06	-0.20	0.20	-0.15	-0.54*	-0.11
	[0.10]	[0.09]	[0.14]	[0.20]	[0.22]	[0.24]	[0.26]	[0.28]	[0.22]	[0.31]	[0.85]
T + 1	-0.42***	-0.35***	-0.21*	-0.74***	-0.32*	-0.84***	-0.85***	-0.79***	-0.57***	-0.11	0.09
	[0.10]	[0.09]	[0.12]	[0.15]	[0.16]	[0.17]	[0.21]	[0.16]	[0.18]	[0.23]	[0.33]
T + 2	-0.14	-0.23**	0.08	0.11	-0.18	-0.31	-0.31	-0.26	-0.34	0.79**	0.78*
	[0.11]	[0.11]	[0.13]	[0.21]	[0.22]	[0.24]	[0.29]	[0.26]	[0.24]	[0.37]	[0.46]
T + 3	-0.01	-0.12	0.19	0.13	0.20	0.17	0.10	-0.24	-0.04	0.20	-0.30
	[0.11]	[0.11]	[0.15]	[0.18]	[0.23]	[0.25]	[0.27]	[0.23]	[0.26]	[0.32]	[0.79]
T + 4	-0.09	0.03	-0.01	-0.24*	0.13	0.02	-0.10	-0.20	-0.34**	-0.36	-0.46
	[0.08]	[0.07]	[0.11]	[0.13]	[0.15]	[0.16]	[0.16]	[0.15]	[0.15]	[0.26]	[0.40]
Observations	142,779	142,779	142,757	140,384	136,030	135,077	127,364	100,268	88,440	60,893	34,923
R-squared	0.61	0.66	0.54	0.53	0.29	0.37	0.17	0.30	0.28	0.24	0.21

Notes: Percent change calculated as log points *100 per m/s of max wind speed. Standard errors clustered at the treatment (province) level in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only shown for 0-4. Includes province and year fixed effects, lagged temperature and precipitation controls, and household controls consisting of the number of members, working and non, in household as well as household head's gender and education level.

Table 11: Typhoon impact on food expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Meat (%)	Fish (%)	Dairy (%)	Fruit (%)	Cereal (%)	Nonalcoholic Beverages (%)	Alcoholic Beverages (%)	Tobacco (%)
Max wind speed, T=0 (m/s)	-0.73***	-0.49**	-0.38**	-0.03	-0.07	-0.47**	-0.64**	-0.35
	[0.22]	[0.19]	[0.18]	[0.20]	[0.07]	[0.19]	[0.29]	[0.29]
T + 1	-0.74***	-0.14	-0.75***	-0.83***	-0.22***	-0.45**	-0.17	0.32
	[0.22]	[0.13]	[0.19]	[0.15]	[0.06]	[0.18]	[0.28]	[0.23]
T + 2	-0.55**	-0.37*	-0.18	-0.37**	-0.08	-0.27	-0.21	-0.03
	[0.23]	[0.20]	[0.22]	[0.16]	[0.09]	[0.25]	[0.33]	[0.28]
T + 3	-0.52**	-0.21	-0.18	0.03	-0.01	-0.62**	-0.38	-0.25
	[0.23]	[0.20]	[0.25]	[0.20]	[0.09]	[0.27]	[0.33]	[0.30]
T + 4	-0.01	0.12	-0.08	-0.02	-0.05	0.28*	0.40*	0.34
	[0.17]	[0.12]	[0.13]	[0.12]	[0.05]	[0.16]	[0.21]	[0.21]
Observations	137,495	142,361	133,913	142,421	142,684	115,746	79,381	89,148
R-squared	0.46	0.41	0.36	0.50	0.69	0.31	0.11	0.16

Notes: Percent change calculated as log points *100 per m/s of max wind speed. Standard errors clustered at the treatment (province) level in brackets. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Lags 0-5 estimated but only shown for 0-4. Includes province and year fixed effects, lagged temperature and precipitation controls, and household controls consisting of the number of members, working and non, in household as well as household head's gender and education level.

Table 12: Typhoon impact on child mortality

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Any age	Male	Female	Under 5 y.o. female	Infant female	Infant male
Max wind speed, T=0 (m/s)	-43.16	-8.765	-36.82	-24.96	21.16	-5.697
	[30.16]	[19.39]	[32.92]	[25.76]	[23.29]	[16.44]
T + 1	79.68*	26.11	58.82**	54.46**	73.37***	1.717
	[36.61]	[29.76]	[26.03]	[22.50]	[20.71]	[16.72]
T + 2	-32.82	-21.35	-5.694	-4.821	28.54	-21.54
	[39.64]	[34.04]	[22.42]	[22.27]	[17.79]	[22.19]
T + 3	-5.614	-0.695	-4.957	12.47	37.99**	-20.31
	[32.21]	[21.82]	[24.56]	[15.39]	[15.89]	[17.46]
T + 4	63.93	43.47	26.07	30.36	41.84	23.61
	[52.90]	[35.96]	[29.56]	[26.65]	[25.27]	[22.94]
Observations	265,430	265,430	265,430	265,430	265,430	265,430
R-squared	0.106	0.090	0.086	0.083	0.082	0.084

Notes: Mortality shown per million households per year. Diarrhea expressed per million households, calculated only for survey years, and indicates that the household had a child hospitalized for diarrhea within the 2 weeks prior to survey. Standard errors clustered at the treatment (region) level in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only shown for 0-4. Includes region and year fixed effects and lagged temperature and precipitation controls for all models, mother's age and age squared as well as mother fixed effects in mortality models, and household controls (mother's age and education) for diarrhea model.

Table 13: Typhoon impact on child mortality by SES group

	(1)	(2)	(3)	(4)
VARIABLES	Poorest decile	Below med.	Above med.	Top decile
Max wind speed, T=0 (/s)	98.33	9.214	36.08	87.85
	[62.12]	[32.81]	[25.34]	[58.01]
T + 1	146.1*	82.15**	63.44**	62.74
	[70.92]	[31.36]	[23.42]	[56.10]
T + 2	140.8*	31.53	28.99	-5.418
	[73.40]	[35.88]	[20.53]	[47.83]
T + 3	164.8*	54.30**	21.39	27.98
	[91.13]	[23.82]	[16.06]	[34.26]
T + 4	173.4**	58.67	20.27	13.61
	[74.24]	[34.35]	[22.20]	[45.81]
Observations	26,637	142,216	123,214	20,587
R-squared	0.084	0.086	0.071	0.072

Notes: Mortality shown per million households per year. Standard errors clustered at the treatment (region) level in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only shown for 0-4. Includes region and year fixed effects, lagged temperature and precipitation controls, and mother fixed effects.

Table 14: Typhoon impact on infant female mortality by sibling gender

VARIABLES	Max wind speed, T=1 [m/s]
First born	35.32
	[23.48]
Marginal impact of having older siblings	75.70**
	[25.35]
Only older sisters	75.80
	[78.97]
Marginal impact of having older brothers	53.18
	[67.54]
Only older brothers	121.2*
	[57.10]
Marginal impact of having older sisters	0.405
	[40.07]

Notes: Mortality shown per million households per year. Standard errors clustered at the treatment (region) level in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only shown for 0-4. Includes region and year fixed effects, lagged temperature and precipitation controls, and mother fixed effects.

Table 15: Household losses due to previous year's mean typhoon exposure

Variables	Mean typhoon impact in prev. calendar year
Income	-6.57
Expenditures	-7.08
Food exp.	-5.90
Education exp.	-13.3
Medicine exp.	-14.3
Female infant mortality rate	
deaths per million households	1130
percent of mean infant female death rate	55.0%

Notes: Losses are calculated using mean typhoon exposure between 1979 and 2008 as shown in 1 for the province level (region level for infant mortality) using coefficient on T + 1 from estimates in tables 7 (col. 4),10 (cols. 1, 2, 7, and 8) and 12 (col 5). Losses for income and expenditures are log points times 100.

Appendices

A Appendix: Data

Typhoons

The Philippines' typhoon climate Figure 1 summarizes the typhoon-climate of the Philippines by displaying annual mean wind exposure. The globe on the left is centered on the Philippines, displaying the country's location in the global cyclone climate, while the map on the right displays the within-country variation in that climate. Typhoons form in the warm waters of the Pacific, generally to the east of the Philippines, and are pushed westward by prevailing winds, striking the Philippine islands. As storms move westward, they also tend to drift northward[38], causing storms to strike the northern regions of the Philippines more often than they strike the southern regions, generating a within-country gradient in typhoon risk that is probably the strongest within-country gradient in the world.

Maximum wind speed as a measure of typhoon incidence Typhoon wind speed is simply the maximum wind speed achieved at a location during the course of a calendar year. If a location experiences multiple storms, the annual maximum is the maximum of the maximum speed achieved in each storm. Pixel-specific wind speed estimates are spatially averaged over each province[39] to aggregate exposure into

[38]The northward drift is due both to prevailing winds as well as a phenomenon known as "beta-drift," which results from an interaction between the storms angular momentum (vorticity) and the angular momentum gradient of the planet. See, for example, Wang et al. (1996).

[39]It may be possible to reduce our measurement error by using population-weights, following Jones and Olken (2010) and Hsiang et al. (2011), or capital-weights, following Nordhaus (2010), when aggregating our exposure measure. However, we fear that if populations strategically locate themselves or capital in response to typhoon risk, this may bias our estimated coefficients in some unknown way. Thus, we use area-weights because populations cannot manipulate this parameter, giving us confidence that our RHS variable is fully exogenous. This conservative approach may mean that our estimation

province-by-year observations. While there are other feasible measures of typhoon impact, Hsiang and Narita (2011) demonstrate in detail that spatially averaged wind speed has explanatory power of economic outcomes that is at least as good, if not better, than alternative physical measures. These alternative measures include total energy dissipated (e.g., Hsiang (2010)), wind speed at landfall (e.g., Nordhaus (2010) or Mendelsohn et al. (2010) or minimum central pressure (also in Mendelsohn et al. (2010)).

Family Income and Expenditure Survey (FIES)

FIES data collection and attrition FIES surveys are designed to be nationally representative and are conducted at the household level. For each survey wave, teams of surveyors deploy twice, the first phase from July 8-31 and the second phase from January 8-31 in the following year. Surveyors visit the same households in both phases, completely repeating the survey but asking respondents to recall income and consumption only over the last six months[40]. Annual statistics are computed by averaging responses to both the July and January phases. The survey asks detailed questions about income and expenditure, collapsing respondents estimates of both prices and quantities into summary statistics for total receipts. The NSO estimates that each surveyor samples 1.5 households per day, suggesting that the survey does not take longer than two-thirds of a working day. For each phase, the surveyor is instructed to return to the household at least twice (for three visits total) if the household head is missing or the survey cannot be completed for other reasons. In cases where a household does not complete one of the two phases, the household is dropped from the sample. The

is inefficient, in the sense that it does not take advantage of all available data, but this should only make our inferences more conservative.

[40] The NSO asks respondents to describe income and consumption in an "average week" for the period of recall in an effort to limit recall biases. This method has been used consistently from 1985-2006.

NSO notes that this type of attrition sometimes occurs because households cannot be located in the second phase due to the passage of a typhoon, often because the physical house containing the household no longer exists. Because the NSO does not provide attrition statistics, there is little we can do to account for this attrition other than control for household characteristics and run standard tests for balance on typhoon treatment. Balance tests for our primary measure of treatment, previous year's cyclone incidence, are shown in Table 4 in section 4. We show an expanded version of our tests for balance in Table C.1. We note that even for year-of incidence, when the NSO explicitly warns of attrition bias, we find only modest evidence of sorting. Households receiving typhoon treatment the same year as they are surveyed are mildly more likely to be headed by a male household head (0.07% per m/s, or 1.2% for average treatment), an effect that seems to persist somewhat over time. They are also mildly less likely to have completed secondary school (0.23% per m/s, or 3.9% for average treatment), an effect that seems to be reversed the following year. We include both of these variables as covariates in our analysis, and note that doing so does not appreciably change our results (see, for example, the stability of our coefficient on income after the addition of household covariates during our specification search in Table 7). We conclude that they are unlikely to be driving our results.

B Appendix: Results

Income and Expenditures Losses

Prices Reductions in expenditure of course do not directly translate into reductions in consumption without knowledge of the price response. We investigate whether the expenditure losses we observe can feasibly be interpreted as losses to consumption by testing for typhoons' impact on regional food prices for a vairety of commodities; these results are shown in appendix tables C.4, C.5 and C.6. We find little evidence to suggest that typhoon exposure materially alters food prices. The one exception to this is the price of beef, which seems to be somewhat reduced by typhoon exposure. A possible explanation for such an outcome could be shock-induced asset sales, i.e., families may be trying to smooth their income by selling off valuable assets such as cows. Regardless, our price results overall would indicate that we are not remiss in interpreting our drops in expenditures as reductions in income.

Infant Mortality

Inferring Economic Status

We follow Bollen, Glanville, and Stecklov (2002) in inferring household wealth for DHS households by tabulating the total number of asset categories present in the house, and then ranking each households' total wealth (unweighted by asset type) against other households in their region the year they were surveyed. We argue that In light of both common intuition and our results in section 5.1 it is clear that this stratification suffers from no small degree of endogeneity, since 'poorer' households with fewer assets may simply be worse hit by typhoons.

We argue that our measure of socioeconomic status is nonetheless a reasonable proxy for several reasons. First, since we rank each household against other households in its region our measure will be a poor proxy only if a household's rank *relative to other households'* is affected by typhoons nonmonotonically with wealth, and while we observe differential impacts of typhoons across by income this relationship appears to be monotonic. Second, while asset losses due to typhoons are clearly large, they are small as a proportion of total assets, implying that our potential margin for error is small. Third, we note that due to the quasi-panel nature of DHS data our concerns about typhoon impact altering our SES metric are only relevant for very recent typhoons; for the bulk of observations we are looking at several years before the DHS surveyor arrived and observed household assets, minimizing the potential for bias.

Robustness

Table C.7 shows specification sensitivity checks of our infant female mortality result. We begin by regressing the binary variable indicating whether a household in our sample reports an infant female's death in a given year against our raw measure of typhoon intensity, maximum wind speed, with standard errors clustered at the level of treatment, the region. Even in this very limited specification we find a significant (and the 10% level) and positive association between infant female mortality and typhoon intensity in the calendar year following typhoon exposure. Controlling for year fixed effects does little to change this result, but controlling for region fixed effects does, strengthening the response and incrasing the precision with which we estimate our impacts. We find no evidence that leads in our model predict female infant mortality, or that varying the lead and lag structure of our distributed lags model appreciably alters our results.

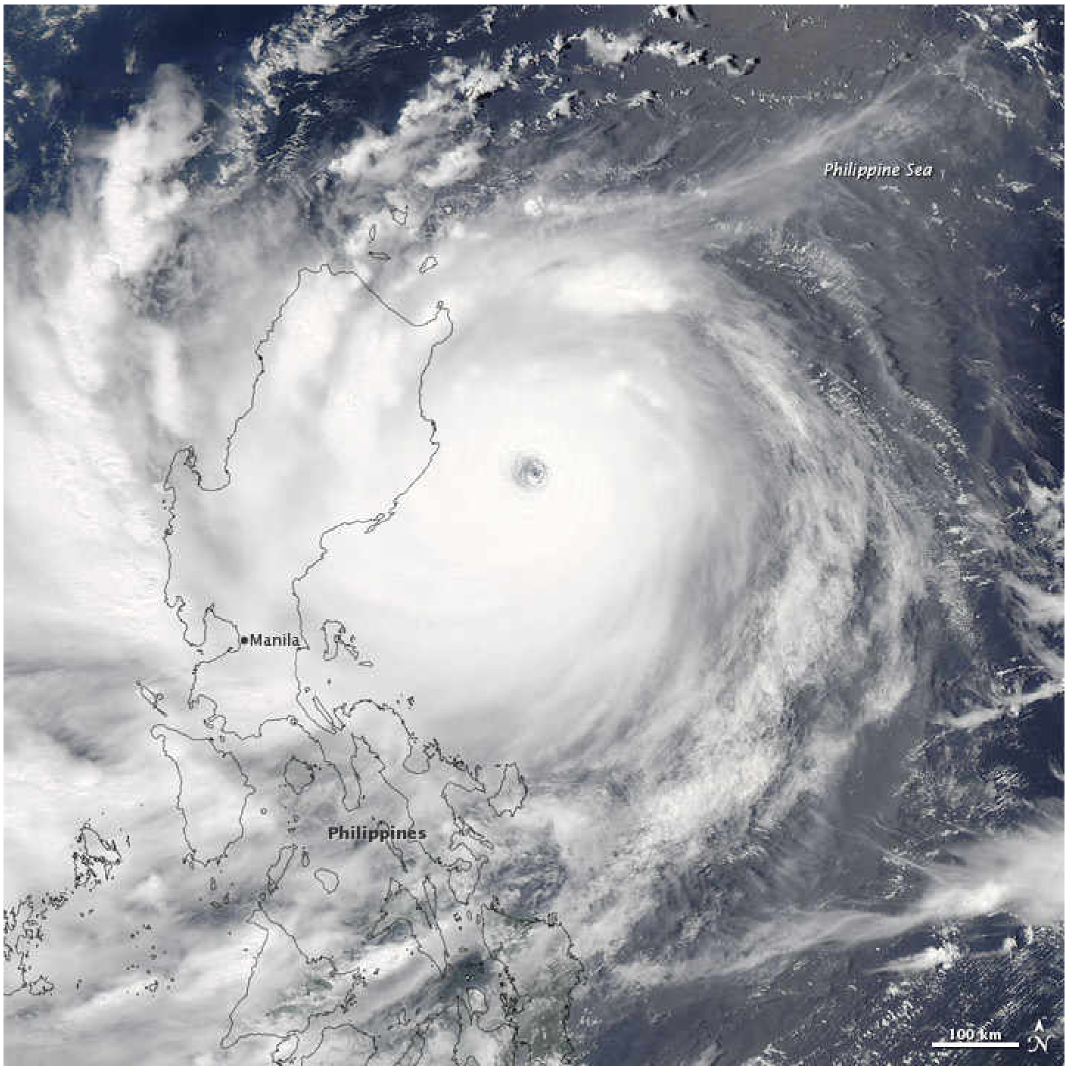
Fetal losses

The gendered nature of infant deaths in our sample may plausibly, at least for in utero effects, reflect the widely documented gender disparity in fetal deaths (Trivers and Willard (1973); Almond, Edlund, and Palme (2009); Sanders and Stoecker (2011)). It is thus feasible that the heavily female nature of the infant mortality result is due to different timing of deaths due to fetal typhoon exposure: in utero exposure could result in immediate death for affected male fetuses but only non-mortal damage to girls. Those girls would have a higher propensity to be born preterm or low birth weight, and be more likely to die later on.

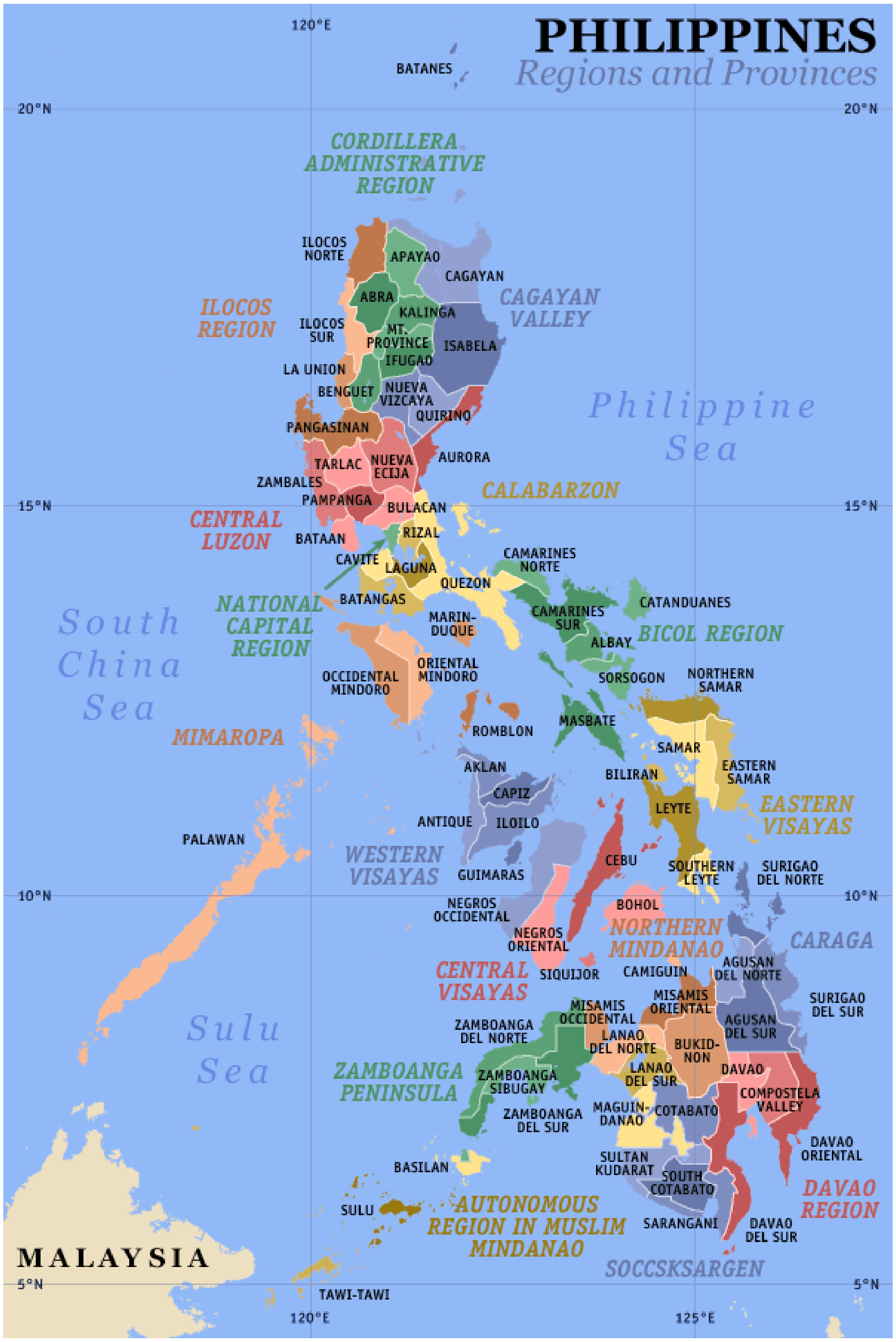
We examine in-utero exposure in Figure C.3, showing the cumulative impact of typhoons on birth rates. We find that birth rates generally fall in the 9 months after typhoon impact, particularly for males. Since this is before possible typhoon-induced changes in conceptive behavior, we conclude that this reduction in the birth rate most likely results from the typhoon's traumatic impact on the intrauterine environment, and interpret this result as supportive of the generally higher propensity of males to die in utero. We find that female births actually increase slightly after storm impact, perhaps reflecting storms' increasing the likelihood of pre-term birth, but this effect is quickly swamped by the same downward trend. We find that after the 9 month mark has passed cumulative birth rates begin to recover, suggesting possible attempts by households to make up for lost fertility. We explore whether this effect is discernable at the annual level in Table C.8. We find that Figure C.3's reduction in birth rates is apparent in a negative, but not significant, coefficients for males.

We explore whether this differential response in fetal deaths results in material changes in gender of mortality rates in Figure C.4. We find that the large decrease in male births combined with the lack of any systematic response in male mortality makes our estimate of monthly deaths sensitive to minor changes in specification, and thus normalize gendered mortality rates by the previous period's gendered birth rates. Doing so substantially reduces noise in our estimate and reveals that while female infants experience a marked increase in birth-rate adjusted mortality, the response among male infants remains flat.

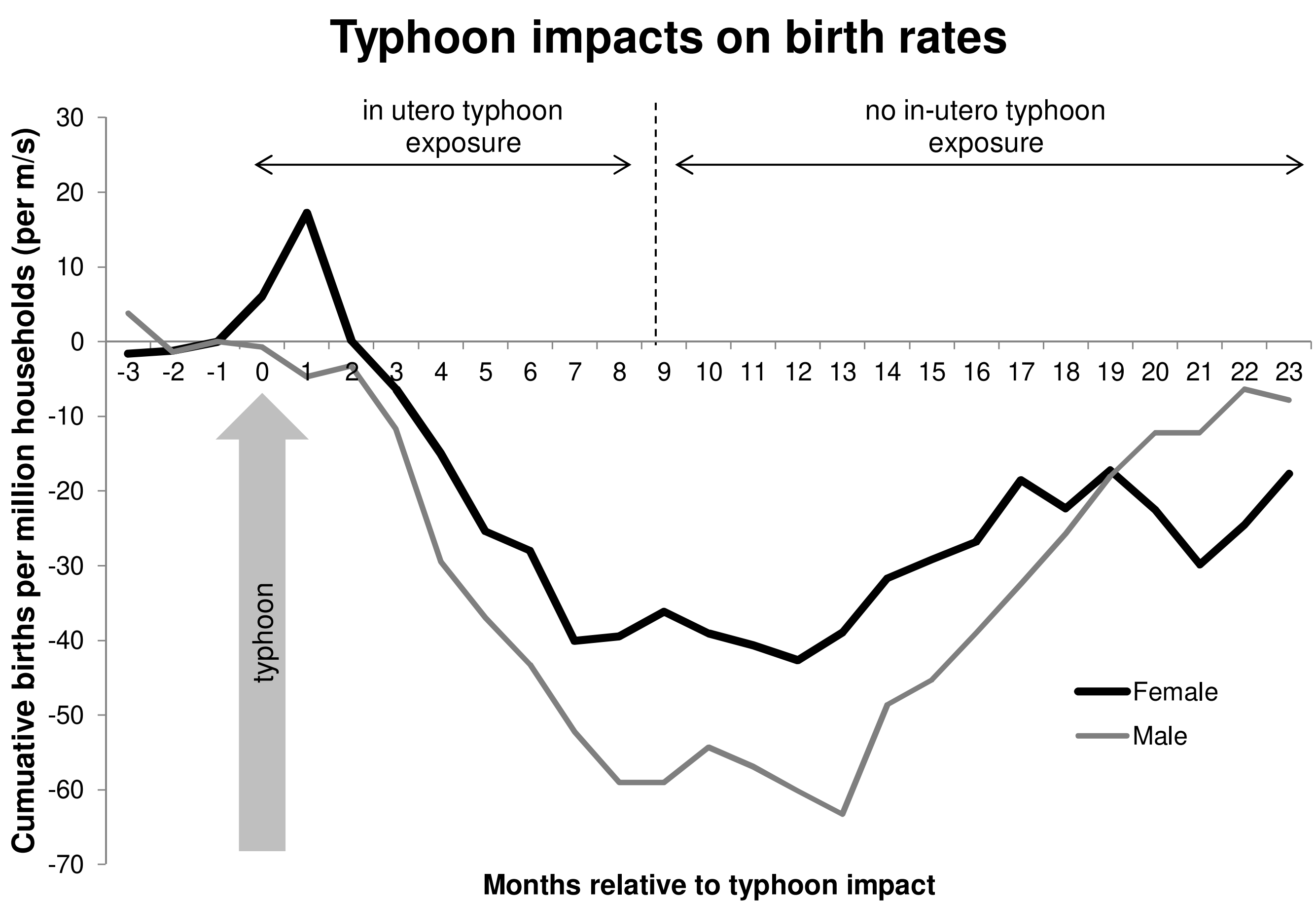
C Appendix: Tables and Figures



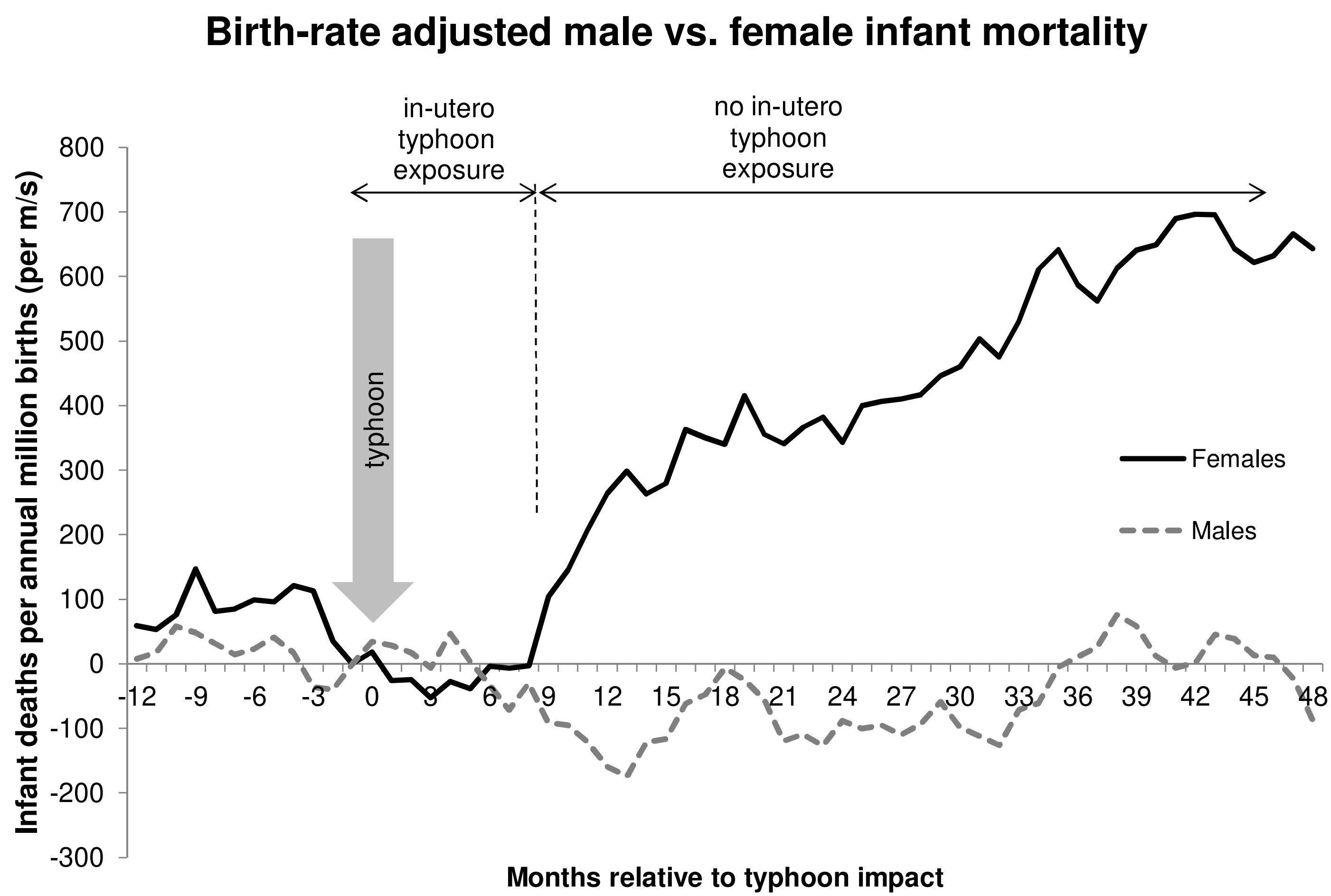
Appendix Figure C.1: Typhoon Nanmadol striking the Philippines (moving westward) in 2011.



Appendix Figure C.2: Provincial map of the Philippines in 2003. Provinces are the smaller administrative unit, regions are larger and have their names in capital letters. Made by Eugene Alvin Villar and reproduced under a Creative Commons license.



Appendix Figure C.3: Inferred cumulative impact of typhoons on female (black line) and male (grey line) fetal deaths. Births are per million households per year. Cumulative effect normalized such that month prior to typhoon impact is zero. Includes region, year, and month fixed effects, and lagged temperature and precipitation controls.



Appendix Figure C.4: Cumulative impact of typhoons on birth-rate-adjusted infant female mortality (black line) and birth-rate-adjusted male infant deaths (dotted grey line). Units are infant deaths per annualized million births based on previous month's birth rate. Cumulative effect normalized such that month prior to typhoon impact is zero. Includes region, year, and month fixed effects, lagged temperature and precipitation controls, and lagged dependent variable.

Appendix Table C.1: Checking for balance on typhoon exposure in FIES data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
			Household head characteristics				
VARIABLES	Total family size	Single family dwelling (%)	Married (%)	Male (%)	Uneducated	Only primary school	Only secondary school
Max wind speed, T=0 (m/s)	0.0023	0.03	0.04	0.07*	-0.02	-0.12	-0.23***
	[0.0029]	[0.04]	[0.02]	[0.04]	[0.02]	[0.07]	[0.06]
T + 1	-0.0002	0.03	0.01	0.05**	0.01	0.02	0.08**
	[0.0021]	[0.04]	[0.02]	[0.02]	[0.01]	[0.04]	[0.04]
T + 2	0.0010	0.03	-0.01	0.03	-0.03	-0.26**	0.11*
	[0.0025]	[0.05]	[0.03]	[0.03]	[0.02]	[0.09]	[0.06]
T + 3	-0.0025	-0.01	0.02	0.07*	0.02	-0.08	-0.02
	[0.0022]	[0.08]	[0.04]	[0.03]	[0.02]	[0.08]	[0.09]
T + 4	0.0011	0.01	0.01	-0.01	-0.01	0.10*	0.03
	[0.0021]	[0.06]	[0.02]	[0.02]	[0.02]	[0.05]	[0.04]
Observations	142,789	142,789	142,789	142,789	142,789	142,789	142,789
R-squared	0.0143	0.02	0.01	0.01	0.08	0.14	0.24

Notes: Standard errors clustered at the treatment (province) level in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only shown for 0-4. Includes province and year fixed effects and lagged temperature and precipitation controls.

Appendix Table C.2: Household asset loss to lagged typhoon exposure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Has improved water (%)	Has faucet (%)	Any toilet (%)	Sealed toilet (%)	Strong roof (%)	Has radio (%)	Has stereo (%)	Has refrigerator (%)	Has AC (%)
Max wind speed, T=0 (m/s)	-0.01	0.04	0.07	0.02	-0.09	-0.02	-0.01	0.03	-0.01
	[0.08]	[0.10]	[0.06]	[0.08]	[0.09]	[0.10]	[0.08]	[0.06]	[0.01]
T + 1	0.04	-0.03	-0.04	-0.18***	-0.06	0.03	-0.13***	-0.08**	-0.00
	[0.06]	[0.07]	[0.04]	[0.06]	[0.06]	[0.05]	[0.04]	[0.03]	[0.01]
T + 2	0.11*	0.30**	-0.02	-0.02	0.04	0.01	0.06	0.11***	0.01
	[0.05]	[0.11]	[0.05]	[0.09]	[0.11]	[0.07]	[0.05]	[0.04]	[0.01]
T + 3	0.03	0.04	0.04	-0.14	0.08	0.00	0.01	-0.03	0.01
	[0.08]	[0.10]	[0.06]	[0.10]	[0.10]	[0.09]	[0.03]	[0.04]	[0.02]
T + 4	-0.03	-0.12***	-0.01	-0.14**	-0.03	-0.01	-0.09**	-0.08**	-0.00
	[0.03]	[0.04]	[0.02]	[0.06]	[0.03]	[0.03]	[0.04]	[0.03]	[0.01]
Observations	107,620	107,620	107,620	107,620	107,620	107,620	107,620	107,620	107,620
R-squared	0.13	0.16	0.16	0.24	0.24	0.06	0.11	0.23	0.02

Notes: Robust standard errors (clustered at province level) in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only 0-4 shown. Includes year and province FE, temperature and precipitation controls, and household covariates consisting of: log number of total members in household; log number of working age (¿15) household members; indicator variable for household head being male; and indicator variables for household head's education.

Appendix Table C.3: Wage rate stability to typhoon incidence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	All farm wages (%)	All farm wages (male) (%)	All farm wages (female) (%)	Palay farm wages (%)	Corn farm wages (%)	Coconut farm wages (%)	Sugarcane farm wages (%)
Max wind speed, T=0 (m/s)	0.07	-0.02	-0.09	0.17	0.08	0.03	0.17
	[0.11]	[0.11]	[0.10]	[0.13]	[0.09]	[0.14]	[0.13]
T + 1	0.02	-0.05	-0.13	0.14	0.03	-0.09	0.02
	[0.10]	[0.13]	[0.11]	[0.14]	[0.09]	[0.13]	[0.16]
T + 2	0.02	0.04	0.15	0.14	0.04	0.05	-0.03
	[0.07]	[0.14]	[0.13]	[0.10]	[0.09]	[0.16]	[0.15]
T + 3	0.11	0.05	0.06	0.17	0.03	0.09	-0.03
	[0.09]	[0.12]	[0.14]	[0.12]	[0.11]	[0.12]	[0.13]
T + 4	0.04	0.01	-0.02	0.07	-0.05	0.10	-0.00
	[0.07]	[0.16]	[0.19]	[0.08]	[0.10]	[0.09]	[0.13]
Observations	294	195	195	294	294	271	217
R-squared	0.92	0.92	0.87	0.89	0.90	0.84	0.88
Year range	1985:2008	1994:2008	1994:2008	1985:2008	1985:2008	1985:2008	1985:2008

Notes: Observations at province level. Percent change calculated as log points *100 per m/s of max wind speed. Standard errors (clustered at region level) in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only 0-4 shown. Includes year and province FE and temperature and precipitation controls

Appendix Table C.4: Meat prices as a function of typhoon incidence

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Beef lean meat (%)	Beef bone in meat (%)	Pork lean meat (%)	Pork bone in meat (%)	Pork pata (%)
Max wind speed, T=0 (m/s)	0.32	-0.01	-0.06	-0.06	-0.10
	[0.38]	[0.08]	[0.07]	[0.07]	[0.14]
T + 1	0.25	-0.25**	-0.05	-0.09	-0.11
	[0.53]	[0.11]	[0.09]	[0.10]	[0.17]
T + 2	-0.25	-0.17**	0.03	0.06	-0.11
	[0.18]	[0.06]	[0.07]	[0.08]	[0.09]
T + 3	0.29	-0.17	0.09	0.02	-0.04
	[0.45]	[0.10]	[0.10]	[0.09]	[0.09]
T + 4	0.02	-0.11	0.05	0.10	0.02
	[0.20]	[0.08]	[0.10]	[0.09]	[0.09]
Observations	266	252	266	266	266
R-squared	0.68	0.97	0.98	0.98	0.97

Notes: Observations at province level. Percent change calculated as log points *100 per m/s of max wind speed. Standard errors (clustered at region level) in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only 0-4 shown. Includes year and province FE and temperature and precipitation controls

Appendix Table C.5: Fruit prices as a function of typhoon incidence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Bananas (lakatan) (%)	Bananas (latundan) (%)	Bananas (aba) (%)	Mandarins (%)	Mango (carabao) (%)	Mango (piko) (%)	Papaya (%)	Pineapple (%)
Max wind speed, T=0 (m/s)	-0.20	-0.19	0.11	-0.50	-0.07	0.48	-0.10	0.33
	[0.29]	[0.16]	[0.23]	[0.46]	[0.23]	[0.38]	[0.27]	[0.24]
T + 1	-0.26	0.11	1.02***	-0.62	-0.14	0.68*	0.02	0.31
	[0.21]	[0.18]	[0.27]	[0.52]	[0.11]	[0.35]	[0.27]	[0.30]
T + 2	-0.29	-0.15	0.43**	-0.09	0.07	-0.17	-0.33	0.07
	[0.24]	[0.13]	[0.16]	[0.42]	[0.12]	[0.38]	[0.28]	[0.37]
T + 3	-0.33	-0.16	0.50*	0.03	0.38**	0.44	-0.02	-0.14
	[0.26]	[0.21]	[0.24]	[0.42]	[0.17]	[0.28]	[0.24]	[0.34]
T + 4	-0.45**	-0.39*	0.19	-0.03	-0.34***	-0.67**	-0.45	-0.05
	[0.16]	[0.20]	[0.26]	[0.38]	[0.11]	[0.29]	[0.29]	[0.34]
Observations	266	266	266	186	266	227	266	266
R-squared	0.94	0.95	0.90	0.80	0.87	0.79	0.89	0.89

Notes: Observations at province level. Percent change calculated as log points *100 per m/s of max wind speed. Standard errors (clustered at region level) in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only 0-4 shown. Includes year and province FE and temperature and precipitation controls

Appendix Table C.6: Grain prices as a function of typhoon incidence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Rice (fancy) (%)	Rice (premium) (%)	Well-milled rice (%)	Reg. milled rice (%)	Corn (yellow) (%)	Corn (white) (%)	Corn grits (yellow) (%)	Corn grits (white) (%)
Max wind speed, T=0 (m/s)	0.07	0.08	-0.04	-0.05	0.18	0.22	-0.31***	0.27
	[0.16]	[0.12]	[0.05]	[0.06]	[0.32]	[0.30]	[0.06]	[0.26]
T + 1	-0.10	0.12	-0.10	-0.06	-0.10	0.41	-0.33***	0.00
	[0.20]	[0.16]	[0.07]	[0.06]	[0.28]	[0.34]	[0.10]	[0.20]
T + 2	-0.05	0.14	-0.07	-0.08	0.07	0.01	-0.34***	-0.05
	[0.19]	[0.16]	[0.06]	[0.07]	[0.29]	[0.31]	[0.11]	[0.22]
T + 3	-0.15	0.05	-0.10*	-0.03	-0.18	0.21	-0.28***	0.42
	[0.22]	[0.17]	[0.06]	[0.07]	[0.20]	[0.22]	[0.08]	[0.43]
T + 4	-0.22	0.03	-0.06	-0.09*	-0.19	0.02	-0.19*	-0.29
	[0.19]	[0.13]	[0.04]	[0.04]	[0.15]	[0.12]	[0.10]	[0.22]
Observations	195	179	266	263	224	210	260	226
R-squared	0.90	0.92	0.99	0.99	0.89	0.88	0.97	0.92

Notes: Observations at province level. Percent change calculated as log points *100 per m/s of max wind speed. Standard errors (clustered at province level) in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only 0-4 shown. Includes year and province FE and temperature and precipitation controls

Appendix Table C.7: Infant female mortality specification robustness

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Infant female mortality	Infant female mortality	Infant female mortality	Infant female mortality	Infant female mortality
Max wind speed, T=0 (m/s)	-26.15	-18.11	20.31	17.13	23.01
	(16.68)	(18.25)	(17.30)	(18.22)	(23.34)
T + 1	30.44*	27.68*	63.48***	68.15***	74.68***
	(14.98)	(14.92)	(19.43)	(20.85)	(21.12)
T + 2	-7.044	-17.00	17.85	22.80	30.34
	(8.896)	(9.758)	(15.67)	(16.31)	(17.99)
T + 3	15.15	2.132	36.05**	31.74**	39.53**
	(13.02)	(14.65)	(14.29)	(13.92)	(16.20)
T + 4	5.903	1.003	39.29	35.65	42.83
	(15.87)	(18.45)	(22.65)	(25.66)	(25.25)
Observations	265,446	265,446	265,446	265,446	265,446
R-squared	0.000	0.000	0.001	0.001	0.082
Year FE		Y	Y	Y	Y
Region FE			Y	Y	
Lagged temp., precip.				Y	Y
Mother FE					Y

Notes: Mortality shown per million households per year. Standard errors clustered at the treatment (region) level in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only shown for 0-4.

Appendix Table C.8: Birth rates by gender

	(1)	(2)	(3)
VARIABLES	Any birth	Male birth	Female birth
Max wind speed, T=0 (m/s)	-150.2	-252.0	94.50
	(181.7)	(142.8)	(71.69)
T + 1	-97.47	-122.8	18.50
	(156.1)	(115.5)	(93.58)
T + 2	27.39	91.89	-75.95
	(180.1)	(113.8)	(87.46)
T + 3	91.89	0.739	90.67
	(157.0)	(147.3)	(59.45)
T + 4	389.8**	174.6	215.6**
	(170.4)	(133.0)	(77.40)
Observations	265,430	265,430	265,430
R-squared	0.168	0.117	0.117

Notes: Births shown per million households per year. Standard errors clustered at the treatment (region) level in brackets. *** p<0.01, ** p<0.05, * p<0.1. Lags 0-5 estimated but only shown for 0-4. Includes region and year fixed effects, lagged temperature and precipitation controls, and mother fixed effects.

Organization of Disaster Aid Delivery: Spending Your Donations

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Abstract

Different organizational structures in the delivery of disaster aid result in significantly different qualities of hard aid, differential willingness to share aid delivery in a village, and differential promotion of public goods and maintenance of village traditions. We analyze three waves of survey data on fishermen and fishing villages in Aceh Indonesia, following the tsunami. Some well known international NGOs delivered housing with low rates of faults such as leaky roofs and cracked walls, but others did not. Some consistently delivered boats that were not seaworthy. Some pursued social agendas that were distorted by village leaders, resulting in increased inequality.

Key words: natural disasters, tsunami, aid, disaster relief, moral hazard, social agendas, non-profit firms, organizational structures

JEL code words: F35, H4, H5, H84, L2, L3

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Extreme disasters catch the public eye, often resulting in massive infusions of aid that affect not just individual well being but the fabric of societies. We examine aid delivery in Aceh Indonesia, following the tsunami at the end of 2004. The tsunami devastated coastal villages in Aceh Indonesia, wiping out virtually all physical capital and large proportions of the population. The international response was huge. For the affected areas, aid gave 134,000 houses for 120,000 houses destroyed (Xinhua News Service, February 1, 2009); and all forms of aid totaled 7.7 billion USD (Brookings, 2008), with aid officially completed in just over 4 years. How well such massive amounts of donated money are spent is something donors would like to know. NGO reports tend to focus on quantity in delivery such as numbers of homes or people served—but not on quality. This paper focuses on a key aspect of the aid black box: how the organization of aid delivery chosen by NGOs directly and significantly impacts the quality of hard aid delivered. Besides delivering hard aid, aid agencies have social agendas. We also have results that shed light on how the pursuit of social agendas may affect aid quality and on how the pursuit of social agendas not wanted by villagers may be thwarted by village leaders. The analyses are critical to understanding the determinants of the efficacy of aid delivery.

The traditional aid literature focuses on the perspective of a generic donor country dealing with local recipients, asking, for example, how conditionality affects efficacy of delivery. This paper takes a new perspective, focusing on how the organizational structure of aid delivery affects the quality of aid realized in villages. Name brand international NGOs raise money from the public for general and specific purposes such as aid for a particular natural disaster. These NGOs, along with foreign and domestic government agencies, then act as donor agencies funding aid delivery. Delivery is carried out by implementers on the ground in devastated areas. Implementers can be either vertically integrated with donors or donors can deliver aid by bilaterally contracting with specific types of implementers.

In the analysis, we distinguish four organizational structures between donors and implementers. These are identified by implementer type (in italics) and are: (1) vertical integration, which we label *donor-implementer,* where an NGO donor does its own implementation in villages, (2) bilateral contracting by an international donor with an *international implementer*, (3) bilateral contracting by an international or domestic donor with a *domestic implementer* and (4) bilateral contracting by an international or domestic donor with an own country governmental organization, in this case *BRR* [Executing Agency for the Rehabilitation and Reconstruction of Aceh and Nias]. BRR was defined to be a short-lived implementer, spending monies from the Indonesian government and multi-donor fund (mostly foreign governments) and was disbanded in 2009 as planned. In many major disasters situations the home country appoints such an agency. Note that the statement of (2) and (3) implies that domestic donors only contracted with domestic implementers, not international ones; we will explain why later.

The choice of vertical integration versus bilateral contracts strongly affects the quality of aid delivered. We focus on the main form of hard aid, houses, and also look at boats. While we do look at NGOs' quantity choices, the main analysis concerns quality: specifically whether the houses or boats built are well constructed or not. For houses we find that in the raw data there is a strict ranking in terms of quality that follows the order of implementer types listed: donor implementer, international implementer, domestic implementer and BRR, although econometric results will be more nuanced. Given a ranking, why do different donors chose different organizational structures as opposed to the highest ranked? Part of the answer will involve considerations of donor scale and politics. But part seems to be that the form of organization may help or hinder the pursuit of social agendas in the process of delivery of hard aid.

The aid literature offers little guide as to how organizational structure might affect quality of aid delivered. There is a theoretical literature on aid delivery concerned with local government response and manipulation, focusing on issues of conditionality imposed on recipients (Collier et al. 1997, Azam and Laffont 2003, Svennson 2003, and Murrell 2002), strategic responses by recipients (the Samaritan's dilemma in Pederson 2001 and Torsvik 2005), co-ordination across agencies, village "ownership" of the process and the like (e.g., Kanbur and Sandler 1999, Easterly 2003, and Paul 2006). Empirically, the literature on large scale aid mostly utilizes country level aid data (Alesina and Dollar 2000, Burnside and Dollar 2000, (Svensson 2003, Wane 2004), rather than micro-data. [1] By looking at one situation in Aceh, we are holding constant many of the aspects this literature examines. Unlike ODAs (Official Development Assistance) or planned NGO aid projects, disaster aid is mostly unconditional and largely uncoordinated as was the case in Aceh. The oversight agency, BRR, defined its role not as coordinating and conditioning aid, but as a clearing house recording aid and filling in ex post gaps in the process.

It is the well established literatures on vertical integration and incomplete contracts (see summaries in Joskow 2010 and Perry 1989) and on reputation (Kreps and Wilson, 1982) which are most relevant to the problem. Absent issues of market structure and pricing under imperfect competition in aid delivery, we focus on the sub-literatures on transactions costs (Williamson 1979, 1989), on property rights and control (Grossman and Hart, 1986 and Hart and Moore, 1990) and on the principal agent problem (Holmstrom and Roberts 1998).

[1] Wane (2004) does look at aid quality cross-countries but focuses on the relationship between quality and a country's degree of accountability. Stromberg (2007) examines how the salience of disasters affects donation. Skoufias (2003) and Townsend (1994) examine how disasters affect household health, education, and risk management. Newhouse (1970), Epple and Romano (1998) and, Malani et al. (2003) examine how NGOs like hospitals and schools perform, but the focus is not on organizational structure.

I Conceptual underpinnings

We start with considerations based primarily on the vertical integration literature. Then we turn briefly to considerations important in aid delivery not present in that literature.

I.1 Vertical integration

We start with the example of house aid delivery. While we focus on the relationship between donors and implementers, a third set of actors, builders chosen by implementers, are important in conceptualization.

A builder has a contract with an implementer to undertake construction of so many houses in a village. Usually, the basic house design is imposed by the implementer. The builder normally buys materials[2] and hires and supervises carpenters, plumbers, day labor and the like. Not only are these contracts between implementers and builders incomplete, enforcement in court is high cost, given the slow working of the local legal system and the potential for corruption in Indonesia. Thus work proceeds in stages, with 2-4 staged payments. Local village pressure including intervention by village and district officials may offer some enforcement and dispute resolution. But the potential for contract hazards is high. Construction is site and use specific, meaning the dwelling units can't be moved once started and can't readily be put into other uses. This leads to hold-up problems. When construction is partially complete more money may be demanded in an attempt to expropriate quasi-rents (with the high costs of firing the builder and finding another to take over in mid-stream who then can also hold-out). The problem may be mitigated by contracting houses in stages—1st ten, 2nd ten and so. But some villages have unoccupied, unfinished aid houses.

Thus a crucial part of the implementer's job is to hire more reliable builders, agree on the parameters for house construction, design a contract with them, monitor work, try to enforce the provisions of the contract, and negotiate changes as difficulties arise. Their ability to do that may depend on experience in contracting in general and in Aceh in particular. But we focus on their incentives to do a good job in supervision of builders.

When donors contract with implementers they face contract hazards. The implementer in a village also has site and use specificity. To fire an implementer throws into doubt the relationships and contracts developed with builders. It may mean abandoning work started, or involve costly negotiation with the new and old implementers. Thus implementers can also halt proceedings, hold-out, and attempt to extract quasi-rents from the donor, as well as imposing delay and bargaining costs. Additionally, there is the potential for the implementer to be in league with builders to pocket money in the purchase of shoddy materials and hiring of phantom workers. As noted above, it may be hard for the donors to enforce

[2] An exception be the use of "legal" wood imposed by some NGOs, in which case the NGO supplies that. While houses are not made of wood, wood is used for window and door frames and supporting beams.

contracts in local courts. One incentive to perform is to maintain reputation if the implementer anticipates repeated business with potential donors in the future.

For the situation at hand, it would seem that vertical integration dominates bilateral contracting. In principle, vertical integration removes contract hazards between donor and implementer and makes conflict resolution much simpler, although there are caveats in the literature. [3] Thus, we might think that donor-implementers do better than donors who hire implementers where agency issues remain. However, international implementers have incentives to perform because they face international reputational costs in the context of the repeated game of contracting in aid disasters. Domestic implementers are less likely to face that incentive both because they don't operate internationally and because in Aceh many were short-lived. Thus we might expect more contracting hazards and lower quality housing with domestic implementers than international ones.

In assessing domestic implementers, there could be a distinction between what type of donor hires them. International disaster aid donors have little potential for post-aid relationships with domestic implementers absent frequent major domestic disasters and would have a hard time pursuing implementers in local courts. For domestic donors, there may be some chance of post-aid relationships with domestic implementers, and some possibility of court enforcement. However, in the results we see no difference in performance of domestic implementers hired by domestic versus international donors. Also as noted above, domestic donors don't hire international implementers, perhaps for political reasons, high contracting costs (language and culture differences), and no hope of ex post court enforcement once international implementers have delivered aid and left Aceh.

Finally BRR lies at the bottom. BRR as a government entity is even more difficult to sue in local court, is a big player which may not respond to pressure from local leaders, and definitely has no reputational concerns since it was defined to be short lived.

Given a ranking of organization types, what other considerations explain why we see all four types in practice? First for donor-implementers versus international or domestic ones, to operate as a vertically integrated unit, donor-implementers had to incur fixed costs of setting up operations in Aceh, a remote and isolated region. Overcoming these fixed costs requires a larger scale operation. For small donors, the fixed cost hurdle means bilateral contracting may be a better choice. Small donors will hire

[3] As Williamson (1975, 1989) points out vertical integration doesn't necessarily dominate bilateral contracting. Costs of vertical integration involve governance issues such as bureaucratic hierarchy and employee incentives. Bilateral contracts are more likely arms-length. Vertical integration invokes personal relationships within the firm and the potential for subversion of firm objectives and application of incentive schemes. The employee on the ground in Aceh has the potential to shirk or engage in corrupt practices because of problems in executing hypothetical reward/ punishment schemes in the face of inter-personal relationships within the firm.

either domestic or international implementers who have incurred the fixed costs themselves to run operations in Aceh. Domestic donors in Aceh were small scale, often Javanese corporations or civic organizations that wanted to show sympathy, but did not have the resources or experience to mount their own delivery. Correspondingly, there are no domestic donor-implementers in the data. For small scale international donors, then, why bilaterally contract on small amounts of aid, knowing quality might suffer? One answer is marketing: some international NGOs may have felt pressure in the fund raising market to say they were helping in such a major internationally visible disaster situation (consider all those concerts and events raising money in the West!). But they had little expertise in Indonesian operations, so they made a symbolic small scale response.

Another consideration is specialization. There can be implementers which are talented/specialized in house delivery, which then bilaterally contract with donor agencies which are talented in fund raising. Why some agencies choose to specialize and others don't is the subject of a considerable literature and we don't review that here. One obvious idea is that founders of NGOs have different talents and tastes. The largest international implementer in our sample is Habitat for Humanity (Indonesia), which in Aceh represented a consortium of donors including Mercy Corps, Plan International, and Fidelity Investment. Habitat for Humanity has a big international reputation with its well known mission to work with communities in building quality housing.

The next consideration is that in the course of selling the product of hard aid delivery in fund raising, donor NGOs bundle other dimensions with hard aid. Foremost are social agendas which have two forms: (1) sustainable development (e.g., village planning, public buildings) and (2) socio-political objectives such as proselytizing and restructuring aspects of the social order and governance of traditional villages. The first agenda is reflected in mission statements on NGO websites that talk about (italics added) "*sustainable* development", "*long term* rehabilitation", "continue to stay long after the emergency is over, working with communities to *rebuild their lives*…", or "help restore and *strengthen* their pre-disaster *capacities*".[4] The second are reflected in certain NGO statements (italics added) about having "decent communities in which people…can grow into all that *God intended*"[5] and "addressing *structural causes of poverty and related injustice"*[6].

The ability to carry out either type of social agenda depends on the dominance of the implementer in village provision of aid. Housing providers are in the village for sustained periods of time and in the

[4] The respective websites for these quotes are http://www.wvi.org/wvi/wviweb.nsf/maindocs/7A0A54FD44BC11C38825737500737C8A?opendocument , http://www.gitec-consult.com/TheCompany.htm, http://www.redcross.ca/article.asp?id=11724&tid=001, http://www.redcross.org.uk/What-we-do/Emergency-response/International-emergency-response, and http://crs.org/emergency.
[5] http://www.crwrc.org/pages/crwrc_international relief.cfm.
[6] http://www.oxfam.org/en/about/what/mission.

course of their work must be intimately involved in village planning. Thus, donors with social agendas will want to have implementers who do more than just arrange housing construction. Again, vertical integration is an obvious solution, especially when aspects of the agenda are "soft" and very hard to contract on. However, executing aspects of social agendas involves knowledge of the local culture and the local bureaucracy, and interactions with BRR which tended to be responsible for certain public goods. While domestic implementers present major contracting hazards, they know the local culture and religion, language and institutions. For some donors focused on sustainable development, this could present a reason to contract with domestic implementers. And beyond this is an objective of certain donors to engage in local "capacity building", with internally imposed guidelines to use domestic implementers.

Finally it is hard to explain why BRR would be used by international NGOs and in fact it generally wasn't. BRR's role involves sovereignty issues and politics: foreign governments channeling money through BRR, as part of maintaining good inter-governmental relations.

I.2 Another aspect of aid delivery in Aceh

In disaster aid, there are not clearly defined market forces that crisply mediate the overall level of aid offered in a disaster situation, nor any omnipotent agency regulating or coordinating that. In the case of Aceh, total house aid could be viewed as excessive: many more houses were given than destroyed, despite the huge losses in numbers of families. Village heads were often in a position of deciding how much excess housing (over and above what would house surviving pre-tsunami households) they actually wanted. Excess could have perceived costs: splitting extended families across households including moving elderly and unmarried child out of the nuclear unit, sprawling of the village, and inducement or assignment of unwanted in-migration. In an earlier version of the paper (Henderson and Lee, 2012), we developed a model where donor-implementers value dominance in aid provision and being the sole housing provider in a village in order to best carry out socials agendas. In order to deter the village head from contracting with additional providers (like BRR), donor-implementers may be induced to strategically provide excess housing to the extent wanted by the village head. Second, additional effort going into social agendas may reduce the costs of monitoring housing. These issues will arise in discussion of results, but we don't present the model here.

I.3 Identification

As noted, for houses, we find a pattern of dominance in terms of quality of aid. In analyzing the link between house aid quality and organizational set-up, we interpret our results as causal. As an average treatment effect of agency type relative to a base case (BRR), quality is the outcome of implementer type, given associated contracting hazards and reputational considerations. But actual quality realized by any implementer type does vary by village, and can be affected by village behavior, as well as other stochastic elements. In particular a diligent and persistent village head may get better quality out of any implementer and associated builders, through nagging and development of inter-personal relationships. If there is matching in the sample between the degree of village heads' persistency and types of implementer, we have a problem. For example, suppose donor-implementers were more responsive to local pressure to hire better builders and to monitor them carefully. If persistent village heads perceived this and sought out that agency type, then part of the estimated treatment will be due to village characteristics, not random assignment of type.

Later, for the very particular case of Aceh, we will find that any matching on observables was limited and that, more particularly, all observable village characteristics and very detailed sub-district fixed effects that control for local cultural, social, and political differences have absolutely no effect on aid quality, although we do not observe personal characteristics of the village head. However we don't think having meticulous and persistent village heads at time of aid delivery is consistently related to matching and hence is a threat to identification of average treatment effects for two key contextual reasons. First, because of 20 years of insurgency and military occupation (settled in 2005), Aceh was an isolated province in Indonesia with no foreign and little domestic NGO presence prior to the tsunami. Village heads had no ex ante information about what types of agencies would deliver better or worse quality aid or be responsive, and no way right after the tsunami to gather objective information on agency attributes, or even to be fully aware of the organizational structure behind an implementer. Our fieldwork suggests that in general after the tsunami, agencies showed up in villages guided by locals helping in the immediate post-tsunami clean-up process, offering to adopt those villages, and accepted on a first come first serve basis. NGOs had no prior information about villages. Regional maps of villages were highly inaccurate (Appendix 1), and centralized information on village characteristics almost non-existent.

Second, even if some wily village heads could successfully seek out agency types that were more responsive, the heads who negotiated contracts were typically no longer heads at time of aid delivery. Because of the settling of the 20 year insurgency in Aceh, village democracy in Aceh was finally imposed at the beginning of 2006 (years after the rest of Indonesia). In most villages, the post-tsunami village head was a traditional head chosen from village elites. These traditional heads did not survive elections and were replaced by younger and better educated heads chosen by the general village population (Freire, Henderson, and Kuncoro, 2011). Only 36% of these unelected heads were still in office by 2007 and 12% by 2009. Most were removed in elections in 2006 and early 2007, with most aid delivered in 2007. The elections and removal from office create a pseudo-randomization, generally severing the matching link between agency type and local propensity to monitor and be persistent at time of delivery. Still there are other dimensions on which matching could occur. Given we won't be able to directly correct for selection effects per se, following Altonji, Elder, and Taber (2005), we will examine how robust our estimates are to assumptions about selection on observables versus unobservables.

The next section describes the context and patterns in the raw data. Section 3 analyzes house aid delivery. Section 4 examines socio-political agendas. Section 5 looks at sustainable development.

II. The context and patterns in the raw data

II.1 The context

We carried out extensive fieldwork in early 2005 after the tsunami and then again in 2007 and 2009, with fieldwork and survey teams in the field for many months. We surveyed village heads and local heads of the fishermen's association (*Panglima Laot*) in those years and covered 199 fishing villages, which are intended to be the universe of fishing villages in 31 sub-districts as one moves north (-east) and south from Banda Aceh, the capital (see Appendix 1). We also surveyed fishing families, now following a panel of about 635 fishing families in 90 of those villages. In addition, we have official government and international records. Relevant aspects of the surveys, a map and the main variables used are presented in the Data Appendix.

Fishing villages were the most devastated by the tsunami, with almost all buildings, public works, boats and roads destroyed. Our villages account for about 30% of all house aid delivered in Aceh, with much of the balance delivered in the capital, Banda Aceh. Table 1 presents an overview of destruction in our villages, using official numbers on pre and post-tsunami populations and household counts to increase coverage.[7] Table 1 gives summary statistics for the 190 villages where we have complete information for both 2007 and 2009. Our survey counts of houses and public buildings pre- tsunami are fairly accurate given the village mapping exercises conducted soon after the tsunami, in the physical presence of remaining foundations. Boats are another matter since there is no written record of pre-tsunami boats nor physical evidence of what was destroyed. By 2007 villages tend to heavily exaggerate boats lost. We only report on villages surveyed in 2005, where we record boat, captain, and owner survival status.

Destruction is massive. In 104 villages around Banda Aceh surveyed in 2005, under 50% of the population survived; in the expanded set more survived as added villages experienced a weaker wave force. The destruction of physical capital in the overall sample is almost universal, given both the earthquake that created the tsunami and the wave following 20-30 minutes later. Mean survival rate of houses for the overall sample is 9% and that of public building is lower at 6%, noting that many public buildings such as mosques and fisherman halls are built on the waterfront. The survival rate of boats, based on 2005 survey numbers, is under 6%.

[7] We believe our survey numbers for 111 villages in 2005 are more accurate in portraying pre and post tsunami village populations than official numbers for reasons detailed in Freire, Henderson, and Kuncoro (2011). Official numbers seem to modestly undercount surviving populations. Survey numbers for the 88 added villages in 2007 on pre and post tsunami populations suffer from the fact that by then most village heads had been replaced and recollections on pre-tsunami numbers are noisy.

The immediacy and extent of aid are impressive. As Table 1 illustrates, 117% of "needed" houses were replaced by late 2007; need is the number of surviving households less the number of houses that survived. Similarly, for boats the ratio of boats in the water in 2007 to surviving captains recorded in 2005 is 105%. Finally, 80% of destroyed public buildings have been replaced by 2007 even with the significant drop in village populations. By late 2007, the massive aid process had accomplished what it intended—to replace the entire physical capital stock. Yet, given the massive response there was money left to spend. More public buildings trickled in between 2007 and 2009, although almost no boats. For public buildings by late 2009 the replacement rate was 96%. House aid continued with an eventual replacement rate of 145%, motivating the notion of excess provision.

We note that aid agencies tend to specialize. Those providing public buildings, doing official mapping and planning, and providing boats tend to differ from house agencies. Public buildings were disproportionately provided by BRR — about 38% of 642 public buildings are from BRR (compared to 18% of houses). An Australian organization, APRID, built 51 of 121 new village halls and PLAN built 11 of 111 new health facilities. Neither provides housing. Nonetheless, housing NGOs exhibit the largest and by far most sustained presence in villages, often coordinating and influencing work by other NGOs.

II.2 Patterns in the raw data

II.2.1 Organization of house aid delivery and quality

Each village head is asked to name the main aid agencies delivering housing and other forms of hard aid. For housing usually only 1 or 2 agencies are involved in a village, with occasionally there being a 3rd. We map each named agency into one of the four organization types, identified by type of implementer. Typing is done based on information on donors and implementers in the "RAN" [Recovery Aceh-Nias] database,[8] a database set up by the Indonesian government working with the UN which recorded aid delivery aspects in Aceh. Details about this mapping are in the Appendix.

The raw data indicate that the four types of implementers operated differently and delivered different qualities of aid. We start with operations. Table 2 compares the scale of housing aid operation by implementer type across the 199 villages, with individual numbers for the 8 largest housing providers in our sample. Each village has 1 to 3 housing projects led by different agencies. Table 2 reports some numbers at the project level and some at the village level. The table shows that house aid tends to be given mostly by one agency in any village.[9] Additionally, only 14% of implementer-projects involve a third project in a village; and, in 46% of villages, over 90% of housing comes from one agency.

[8] http://rand.brr.go.id/RAND/

[9] While BRR is a specific agency, other numbers are for group types. For group-types, different agencies of the same type may appear in the same village. For example, if two different donor-implementers appear in a village, although that is just one village where any donor-implementer appears, it counts as two donor-implementer projects.

BRR is the largest single overall provider, but operates on a small scale in most villages. It has a relatively high fraction of occurrences where its provision is minimal (under 15% of total house aid), and a small fraction where it is the almost sole provider (over 90%). Donor-implementers provide a sharp contrast. Their provision is much more focused: they are sole providers in a high fraction of villages and are minimal providers in only a few villages they are present in. Overall, donor-implementers are dominant [almost sole] providers in 80% [43%] of villages where they are present, compared to 34% [14%] for international implementers and 34% [17%] for BRR. The Canadian and British Red Cross's stand out as dominant providers; the Canadian Red Cross is the sole provider in 8 of its 11 villages. Note for later reference that domestic implementers at 45% [16%] compared to international ones show a modestly greater relative presence in villages they serve, although they are a more diverse group. We identified 28 different domestic implementers delivering housing in our villages compared to 12 international ones.

In the raw data how does house aid quality differ by implementer type? We report on two rankings. First for each housing aid project, the village head was asked to rate the quality of construction in terms of the likelihood of "leaky roofs, cracked walls, faulty plumbing, and mould" with 3 categories: (i) high (all houses well built) (ii) medium (some well built and some not) and (iii) low (most not). A high rating is a 3, medium 2, and low 1. In the survey, we distinguished early and later batches for each agency. Ratings are generally the same for both batches, but if not we average the ratings, so for each project our scale can be 1, 1.5, 2, 2.5, or 3. For each type of implementer we calculate the average ranking over all projects. We believe village heads tend to give high ratings (given they are involved in the process); and the measure is coarse. Second, for a smaller sample of villages, individual fisherman list house faults: leaky roof, cracked walls, poor foundation, or faulty plumbing, so each fisherman can list 0-4 faults in his house. We average across fishing families served by each type of implementer to obtain another ranking by average number of faults.

Figure 1 shows these two rankings. While the averages are not significantly different, they point to some patterns that in econometric work will be significant. BRR is the worst ranked by both village heads and individual fishermen. Donor-implementers offer the highest quality housing as rated by village heads, and correspondingly have the fewest counts of faults as reported by fishermen, reflecting what we believe to be their greater ability to deal with contracting problems. For international implementers the evidence is more mixed. Village level data suggests they have a relatively good record for house quality construction. But the averaged fisherman data suggest their number of faults is the same as domestic implementers, although in econometric results they will score better than domestic ones.

Table 3 gives detailed data on housing quality of individual agencies providing housing in our villages. We list all agencies that operate in two or more villages; many are well known agencies. Those who operate in only one village are listed in Henderson and Lee (2012). For each implementer, the table gives number of houses provided, number of villages involved, and average ratings by the village head. For international and domestic implementers, we list in brackets the donor agencies often associated with the implementing agency. Some village heads report the funding agency but not the domestic-implementer working on the ground. In this case, we list the funding agency associated with the anonymous domestic implementer. For the smaller set of villages where fishermen report in the sample, we list the average count of faults associated with the relevant implementer.

For village heads, we think an average rating near or below 2.5 isn't good and ratings at 2 or below are bad. Clearly, most domestic implementers as well as BRR have relatively low village head ratings and higher counts of faults, but some international agencies do as well. For counts of faults, there is a sharp divide with international agencies scoring below 1 and domestic ones over 1 in general.

II.2.2 Boat quality

An aspect of aid outcomes is the high failure rate of boats. Boats were ordered by implementers from a handful of workshops mostly near Banda Aceh. There are two dimensions to failure. The first reported in village level data by the local head of the fishermen's association (*Panglima Laot*) concerns immediate failure: many boats were too light-weight or improperly designed for use on the open ocean, sank upon first launching due to bad design, or fell apart after a few outings. By 2007 just after most boats had been given, the overall abandonment rate as reported at the village level was already 22%. The second dimension to failure is boats that were initially usable but fell apart after a few months of usage, in a context where minimum boat life for a traditional boat is 5 years. We estimate that by 2009 at least 30-40% of boats had failed either initially or in the subsequent 2 years.

From that village level data as reported in 2007, Table 4 provides a list of individual agencies operating in 2 or more villages. The table gives the number of boats in aid, villages, and the initial failure rate. We cannot identify comprehensively implementer type for boats, because most boat aid is not reported in the RAN database. Later we will utilize the few NGOs that can be typed as boat donor-implementers in the empirical work. As such implementer type is not the focus in our analysis of boat aid, but rather a particular social agenda discussed below. Still it is instructive to see the failure rates by agency. NGOs like Oxfam, International Medical Corps, and certain foreign governments like France, Kuwait, and the Japan International Cooperation Agency have appalling records of reported boat failure. Clearly there is a failure of many implementers to enforce construction or material standards in dealing with the boat workshops. Some agencies got good boats and others shoddy ones from the same workshop.

II.2.3 Socio-political agendas

Some international agencies that arrived in Aceh in 2005 planned to implement social agendas, through "adoption" of specific villages whereby they would provide all aid in the village. While two years later the idea of strict adoption was virtually gone, because, given the massive relief effort, specialized NGOs provided specialized forms of hard aid, what was planned in 2005 is instructive. As an example, in early June 2005 we interviewed British Red Cross officials in Banda Aceh, who were operating an intensive training program for their field workers. They were planning for their then four adopted and remote fishing villages. They articulated goals of modernization and sustainable development, which included replacing as needed lost housing, boats, and public buildings, registering all lands for formal title, requiring villagers to carry retinal scan IDs (despite a lack of use for such IDs) and learning how to operate ATMs despite the local absence of bank accounts or ATMs. One goal was to structure boat provision so that fishermen could only acquire a motorized boat through joint ownership, as opposed to the traditional form of sole ownership with crew. They wanted villagers to formally contract with potential partners and for all boats to be jointly owned.

For boat provision, the British Red Cross's objective of forcing partnership on fishermen was also shared by a number of boat implementers. For the boats which are reported in 2009 in the individual fishermen questionnaires as having been received on aid in prior years, Figure 2 illustrates the agenda of shared ownership and its relationship to boat aid quality. There are two points. For the 44% of boats given which had initial shared ownership, failure rates are much higher than for non-shared boats (53% vs. 16%). The question is why? Second, regardless of failure, joint ownership fails to persist with only 20% still sharing in 2009. While the high failure rate plays into that, among surviving boats that were initially shared, only 35% still had even nominal shared ownership in 2009. Why is the failure rate among shared boats so high? It could be that NGOs emphasizing shared ownership generally gave bad quality boats. However, econometrically we will find a key strategic reaction by villages. In the presence of within agency heterogeneity in boat quality, poor quality boats were steered by village and lagoon fishing leaders towards those upon whom shared ownership was imposed, often lower status fishermen, while better ones were put aside for sole ownership. Presumably, this was not what donors intended.

III. Empirical Evidence on the Quality of Housing Aid

In this section we estimate how aid quality differs across implementer types. We examine how the degree of agency dominance within the village affects quality. We look at outcomes from both village level data and individual fishing family data. We analyze selection issues and look at aspects of social agendas.

III.1 Base specification and covariates

We have two base specifications. First is

$$y_{pv} = c + \sum_{t} \beta_t D_{tpv} + \gamma X_v + \Lambda_k + \varepsilon_{pv}, \qquad (1)$$

where y_{pv} is the quality measure for housing aid project *p* in village *v* as reported by the village head, or by fishermen for their specific house provided under project *p*. For the quality measure, we look at village heads' subjective assessments based on construction quality and fishermen's subjective assessments of problems related to construction. D_{tpv} are indicators for whether the village project was implemented by type *t* implementer, that is, donor-implementer, international implementer, or domestic implementer. BRR serves as the base type. Λ_k is a set of district or sub-district fixed effects we discuss momentarily. We are primarily interested in β_t and expect donor-implementers and international implementers to have larger coefficient estimates than domestic providers.

The second specification further distinguishes whether a project is the dominant project in a village or the 2nd or 3rd order project by the number of houses given in aid. This order in general generally corresponds to the order of aid delivery as well: biggest projects are for the earliest donor. The specification with project order is

$$y_{pv} = c + \sum_t \sum_r \beta_{tr} D_{tpv} \cdot r_{pv} + \gamma X_v + \Lambda_k + \varepsilon_{pv}, \qquad (2)$$

where r_{pv} is an indicator for project order which can go from 1 to 3. Here we are interested in whether some implementer types shade quality for 2nd and 3rd order projects.

In both specifications, $\mathbf{X}_v$ are a vector of village level covariates. Throughout the paper, we use a common set of covariates for base village characteristics, representing village demographics (number of post tsunami households, population survival rate), distance from Banda Aceh, amount of physical capital destroyed, and pre-tsunami social capital. These are characteristics which from other work affect outcomes to do with volunteerism and labor market choices in our villages (Freire, Henderson, and Kuncoro, 2011 and Nose, 2011). We eliminate 8 villages with missing or very bad population numbers, 5 with missing numbers on houses destroyed, and 7 with other missing data (e.g., GPS readings to calculate distance to Banda Aceh or aspects of social capital).[10] When we analyze individual fishermen's assessments, we will add family controls as well. Standard errors are always clustered at the village level.

The village covariates include two direct social capital measures. The first is the pre-tsunami existence of *arisan* groups, or rotating savings and credit associations [RoSCA]. Such groups of women meet regularly, with each member contributing a fixed sum to a pot and then taking the pot on a rotating schedule. An *arisan* group is a volunteer association outside the mosque and governance structure. While the theoretical work (Besley, Coate, and Loury, 1994) suggests RoSCA's alleviate credit market imperfections, empirical work finds a strong social component to *arisan* groups (Varadharajan, 2004).

[10] For the 8 with poor government data on post-tsunami population and household counts, including those has a strong effect on the coefficient on the number of post tsunami households, which becomes much smaller presumably because of measurement error, although still positive and significant. But aid results are not affected.

The other is whether the mullah, the spiritual leader of the village, survived the tsunami or not, providing continuity in village spiritual leadership. A companion paper, Freire, Henderson and Kuncoro (2011), finds that village traditions of volunteer labor are better maintained in the 68% of our villages which had *arisan* groups pre-tsunami. That paper also finds that mullah survival is important in maintaining village traditions of volunteer labor; but that survival of village heads is not, given few heads remain in office even two years after the tsunami. Greater social capital could be associated with more cohesion in villagers dealing with implementers and builders. We also note that village size and survival rates may affect social cohesion.

We do not include village fixed effects, because these eliminate effective variation in implementer types because so many villages have just one provider and many agencies operate almost exclusively at one level or another. A basic control for cultural-institutional differences across villages is fixed effects for the 4 districts in our sample and we use this throughout. In principle a better control would be fixed effects for the 31 sub-districts (*kecematen)* for the 179 villages in the main estimating sample. This is a tight control, since sub-districts on the coast are small geographic areas of several neighboring villages usually with common geographic and social characteristics. In particular, they closely mimic lagoon divisions, where fishing activities are governed informally at the lagoon level. For our main result with its large village-project level sample, we report results with and without sub-district fixed effects; and we discuss the effect on all other reported results of adding sub-district fixed effects.

III.2 Project-level analysis: Subjective quality ratings by village heads

III.2.1 Basic results

We start with the subjective quality ratings as reported by village heads for the four implementer types. Table 5 looks at subjective quality ratings by the village head of each project on a scale of 1, 1.5, 2, 2.5 or 3, with 3 being highest as noted for Figure 1. The basic results are in column 3, where we control for base village characteristics and district fixed effects. For implementer-type effects, in column 1 we control for the provider for each specific project, regardless of order as in equation (1). Donor-implementers and international implementers bring similar positive effects, compared to domestic ones or BRR. In column 2 which represents equation (2), the base case is 1st level BRR projects, with effects for 2nd and 3rd level projects for that implementer type. We then distinguish the 3 other implementer types by project level. We note that sample sizes at 3rd level projects are tiny, with each cell containing 2.1-4.8 % of all village projects. Thus, in the end we focus on column 3 where we constrain all 2nd and 3rd level project pairs to have the same coefficient.[11]

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[11] We thought of similarities to the child quality-quantity trade-off literature (in particular Black, Devereux, and Salvanes, 2005), where parents make decisions about children's education and numbers of children and birth order matters. If villages were really in control of this, absent aid agency choices and strategic interaction with agencies, we would model quality as a function of whether a project was in a 1, 2 or 3 project village and whether "birth

The pattern we see in columns 2 and 3 is that, relative to the baseline case of 1^{st} level projects of BRR, 1^{st} level donor-implementer projects offer higher subjective quality projects by 0.44 on the scale to 3. International implementers offer higher subjective quality projects by about 0.33, regardless of project level. That is a basic result. If the implementer in the village is an international agency (donor-implementer or international implementer) they offer higher quality housing, through better monitoring and insistence on quality of construction. For other implementer types, BRR offers the same quality at its 2^{nd} and 3^{rd} level as its 1st level; and quality for domestic implementers does not differ from BRR.

However, there is a twist for donor-implementers. The gain in quality for donor-implementer 1^{st} level projects evaporates at the 2^{nd} and 3^{rd} level. This could suggest that donor-implementers act strategically to put less effort in quality as their prominence in a village fades. Or it could be “inadvertent” reduction in monitoring to the extent that personnel involved in social agendas also monitor and such personnel devoted to social agendas decline as dominance declines. However for international implementers there is no quality decline as they lose dominance; they operate and monitor in villages to offer good quality house everywhere. For donor-implementers, it seems care in dealing with contractors declines as their dominance declines, a startling finding which robustness is explored in the next table.

The discussion presumes the implementer-type effects are causal. We later detail our results on matching. Here we note that observed village characteristics have no affect on quality. Column 4 shows that village characteristics without conditioning on type have no significant effects on quality. Columns 5-7 show that, compared to column 3, implementer type effects are not influenced by any observable village characteristics. Column 5 additionally controls for whether the original village head survived the tsunami and is still in office and the education level of the village head in 2007 when housing construction was at its peak. These have no effect on quality and do not change coefficient estimates of the other variables. In column 6, adding a fine control for culture and informal institutions in the form of 31 sub-district fixed effects has little impact on implementer-type point estimates. Finally in column 7, removing all village covariates and all fixed effects also has little impact on implementer-type effects.

We can’t prove there are no unobservables that matter, but the absence of any relevance of observables in affecting quality is an important consideration. Conditional on type, agency housing quality is not influenced by any village characteristics which might be matched to agency type. We believe aid quality is largely determined by implementer policy applied uniformly across villages. Variation in a particular agency’s outcomes across villages is based on random variation in performance

order” mattered, or whether this was the 1^{st}, 2^{nd}, or 3^{rd} level project in the village. In this case, we are ordering projects by size, and while our information on order is limited, it appears that in general the largest project in a village was the first one. In such a formulation, coefficients on number of projects and project order are usually insignificant; and once we control for implementer types all traces of order and number of project effects go away.

of contractors and construction crews and in squeaky wheel behavior of democracy era village head, and in village heads' perceptions of quality.

III.2.2 The donor-implementer twist

An intriguing aspect of Table 5 is that donor-implementers appear to lower quality once they lose dominance. Here we show there appears to be a steady decline in quality as donor-implementer dominance wanes. We first took the column 2 Table 5 specification, removed the constant term, added indicator variables for each project level by type of implementer, and then for all 1st level projects by implementer type we added that indicator interacted with the fraction provided by other projects in the village. While fractions are endogenous, in the model in Henderson and Lee (2012) quality is only affected by unobservables through its effect on fractions provided. The only fraction variable across all implementer types that is significant is the one for donor-implementer.[12] We focus on a restricted, "preferred" specification in column 1 of Table 6, based on column 3 of Table 5 to show the hypothesized pattern.

In column 1 of Table 6, as the fraction of housing provided by other projects rises from 0, donor-implementers reduce quality of their first level projects. By the time that fraction of others hits 40% (generally about the maximum of others relative to a 1st level project), the advantage of quality for a donor-implementer on a 1st level project is reduced from 0.60 to 0.21. Do these effects reflect agency policies per se? We worried that they might arise because of differences in the composition of donor-implementers at different project levels and differential overall policies of those NGOs. In particular, British and Canadian Red Cross's never have 2nd or 3rd level projects and usually dominant housing provision in their villages. Maybe the results arise because they have better quality housing than all other donor-implementers. We reran the base specification in column 1 of Table 6 for two sub-samples. First in column 2, we drop the British and Canadian Red Cross villages from the sample, getting almost the same results as in column 1. Then as an extreme, we drop all villages except those where the 6 largest donor-implementers (UN, WVI, CRS, German Red Cross, Australian Red Cross, Turkish Relief) appeared who routinely operated at different levels in different villages. The results in column 3 are not highly significant given the small sample but the coefficient patterns are consistent with columns 1 and 2. Composition does not seem to be driving the results.[13] Finally, we note that replacing district by sub-district fixed effects in Table 6 has no significant effect on outcomes.[14]

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[12] The estimates of the coefficient [standard error] of the fraction variables are as follows: donor-implementers -0.98 [0.33], international implementers -0.23[0.74], domestic implementers -0.23[0.68], and BRR -0.43[0.59].

[13] More generally, we also worked to find heterogeneity of donor-implementer effects by specific grouping such as all the Red Cross's or all Christian based NGOs, but found no consistent evidence of differential effects.

[14] Column 1 and 2 results are not affected. In column 3 the coefficient on ratio of others is weakened (changed to -.425) but the sample is tiny and spatially clustered.

III.3 Individual level analysis: Quality ratings by fishermen

Are the findings from the village level data confirmed by individual micro data, for the sub-sample of 90 villages where we survey fishermen? We have several hundred fishing families who received a house, name an agency we can identify and categorize, and have corresponding information on different house quality dimensions. Before starting we note that the basic results in column 3 of Table 5 are maintained in the sample of 90 villages (Henderson and Lee 2012, Appendix 4, column 3) and that the fishermen sample is representative of the types of agencies operating in the 90 villages.[15]

Fishing families are asked about four specific faults: do they have a leaky roof, cracked walls, a poor foundation, or faulty plumbing. We have two samples, although we rely more on the second. First are 529 families, where we type housing agencies according to the agency named by the household head. Second is a smaller sample of 371, where we require an agency named by a household head to match one named by his village head so as to reduce noise in household head knowledge of implementer identity. Household heads know names of domestic implementers (with *Bahasa* Indonesian names) and BRR and the village head-household head matching has a high rate of success for these. For international NGOs matching was less successful. Village heads negotiate and sign contracts with agencies, so they have a good sense of specific names of foreign NGOs and who really were the agencies responsible for housing. Given the myriad of agencies operating in any lagoon, villagers are sometimes confused about exact foreign names and what actual implementer was responsible for supervising the contractor who built their house.[16] We match just over half the sample on name alone and add another 20% by matching by implementer type.[17] The matched sample of 371 has only 29 international implementer projects, which makes inferences for this type tenuous.

Keeping BRR as the baseline, Table 7 gives fault counts and then probits for each individual fault, having as covariates the basic village controls from before including district fixed effects, the type of implementer providing housing to the family, and basic family controls of size and age and education of the household head. None of these family and village controls are significant and we don't report their coefficients in this table. For total faults, column 1 presents a Poisson count model with robust and village clustered standard errors. We note that households don't routinely report faults in all categories: in the larger sample 52% report none, 18% one fault, 17% two, 8.6% three, and 6.7% four.

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[15] We compare the actual and expected (if randomly assigned within the village) counts of houses received by our fishermen by agency types for the larger sample of fishermen. The actual counts and expected counts for donor implementer are [174, 178], international-implementer [60, 46], domestic implementer [207, 189] and BRR [118, 146]. The fishermen data seem to represent fairly well the counts reported by the village heads.

[16] Also a village head may name one agency (say, a donor) and the villager another (say, the implementer) when both are involved, although we worked hard to overcome this problem (a specific donor typically hires just one or from a small set of implementers).

[17] When the level 1 type is the same as the level 2 or 3 type we assign it as the level 1 type. There are 34 instances of these and we also try dropping such cases but the results are similar.

Columns (1a) and (1b) report respectively for the larger and then the better matched samples. Column (1c) for the smaller sample removes all village or family controls and fixed effects to show these have no impact on implementer types results. In columns 1a-1c, consistent with village head results, donor-implementers offer lower counts of faults than BRR—a 40% reduction for the larger sample and a 60% reduction for the better matched sample in columns (1b) and (1c). International-implementers also have lower counts although results are statistically weak given their small representation in the sample. The column (1a) the estimate is small, consistent with measurement error in typing. In columns (1b) and (1c), for international implementers reductions are 81% and 91% respectively. Domestic implementers have similar counts to BRR. To column 1a, if we add sub-district fixed effects, results are unaffected. In column 1b with its smaller sample, the only effect is to strengthen (and make significant) the reduction in faults for international implementers.[18] Finally, we note that the small cell counts make it impossible to confirm the 1st versus 2nd and 3rd level projects effects we found for village heads in Tables 5 and 6; results are roughly consistent but noisy.

In Table 7, columns 2-5 report probits on whether the house has a leaky roof, cracked walls, poor foundation, or faulty plumbing, for the smaller better matched sample. In the probits, donor-implementers are significantly less likely to have 2 of the 4 faults at the 5% level, one significantly less at the 10% and one just missing the 10% mark. For international implementers, only one fault is significantly less at the 5% level and one at the 10% level. Domestic implementers show no differences relative to BRR.

The individual fishermen results reinforce those for village heads. Donor-implementers offered high quality aid and so did international implementers but effects for the latter are not precisely estimated. Domestic implementers and BRR offer houses that were more poorly constructed.

III.4 Selection

The identification question is whether given some degree of matching between implementer types and villages characteristics, the matched-on characteristics influence implementer behavior once they are in the village (relative to a randomly selected village). A natural way to proceed would be to correct for this problem with a selection correction or IV strategy. In general there are no observables that meet the exclusion restriction a priori. One possibility came from the idea that individual NGOs tend to cluster in sub-districts to reduce costs of operation. So using the RAN data, we constructed measures of the extent of clustering by agency type (outside the own village) in a sub-district; but resulting instruments were too weak instruments to be useful.[19] Thus we proceed in two ways. First as already shown, a myriad of

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[18] However, that strengthening is based on 29 international implementer observations, 13 of which are in one sub-district with no donor-implementers and only 2 BRR projects.

[19] Part of the weakness occurs because actual clustering is at the individual agency level, not at the type level.

village observables have no individual effect on quality. Here we first look for evidence of matching. Then following Altonji et al (2005) we look at potential degrees of selection and associated bias.

On matching on observables, Table 8 looks at the match between the housing implementer type and village characteristics in a multinomial logit framework. Since some villages have more than one housing aid project the number of observations is greater than the number of villages. We report the marginal effects from a multinomial logit regression, looking at the probability of a village getting a particular implementer type for a project, for each of the four types. There are 8 covariates, 5 of which depict internal village characteristics, 2 of which relate to village head characteristics, and the last being distance to Banda Aceh. District (*kabupaten*) fixed effects are included as in previous regressions. Although the overall associations are significant, only one of the 48 marginal effects (including those for fixed effects) is significant at the 5% level (and we also experimented with a number of others).[20] Villages with a pre-tsunami *arisan* group are 18% more likely to get a donor-implementer and about 10% less likely to get an international implementer or domestic implementer. It could be that having an *arisan* group (RoSCA's) represents a village as having greater social capital appealing to donor-implementers. Of course for selection effects, that only matters if social capital also influences agencies to build better houses than they otherwise would and Table 5 suggests not.

We then turn to an econometric technique that helps gauge the degree of potential bias that could arise from the unobservables. Adapting a method proposed by Altonji et al. (2005) to the multinomial treatment case, we explore as they do how large the selection on unobservables relative to the selection on observables would have to be to explain away the entire agency effects. By conditioning the error term and the index of observable variables in the outcome equation to have the same impact on the agency type variables, we estimate the selection bias in OLS. The ratio of the original OLS estimate to this estimated selection bias shows how large the selection on unobservables relative to the selection on observables would have to be to explain away the entire agency effect. Appendix 2 explains the underlying econometric procedures and Table 9 presents the results. The first row restates the OLS coefficient estimates reported in column (3) of Table 5, the second row presents the implied selection bias in the agency type effects with bootstrapped standard errors, and the last row is the ratio of the OLS estimate to the implied bias, or how large the selection on unobservables relative to the selection on observables would have to be to explain away the entire agency effects. Unlike Altonji et al., our observables individually do not affect quality and this feature manifests in aspects of Table 9. First of all, for

[20] When we exclude the district fixed effects we find that distance to Banda Aceh has a significant relation with the agency types. The district fixed effects in Table 9 are masking a distance effect. Domestic agencies and BRR appear to have an aversion to operating in more remote locations. We don't see that as a match affecting aid quality results conditional on agency type. In fact if there was a bias in finding better quality for international agencies it would be downward, as distance may make quality more expensive. But the aversion (which was also clear from fieldwork) of domestic NGOs and BRR is of interest itself.

international implementers the negative implied bias in row 2 of -0.078 is small and statistically indistinguishable from zero, resulting in a large ratio in row 3. The negative sign implies that the selection bias impacts the outcome in the opposite direction to the international implementer effect, though the standard error in row 2 is large. Second, while the point estimate of the bias, 0.549, in row 2 for donor-implementers is large relative to the OLS estimate in row 1, the standard error is also large, as are standard errors for all implementer types. This makes it difficult to accurately assess the role of unobservables; but the Table 9 results are consistent with the donor implementer and international implementer effects we find in OLS being real.[21] Lastly, for domestic implementers we get a small ratio in row 3, but the original OLS estimate is also small.

III. 5 Other aspects associated with house aid delivery

Before turning to boats, amongst the many aspects of aid delivery we have comments on three particular items. First is that some house aid came with "guarantees", in particular a test for quality of cement used in construction and/or an offer of a six month to one year for repair of defects. There is an association between the offering of guarantees and quality as reported in Appendix 4, but we can't argue the relationship is causal. We don't have evidence that failing a cement test or failing to honor a repair "guarantee" brings any penalty. While offering guarantees could mean some agencies may then build better houses, it could also mean that, in the presence of within agency heterogeneity across villages in quality, agencies only offer guarantees for the better houses they provide.

Second, we have not discussed quantity, only quality. Quantity varies at in the individual level in terms of house size and at the village level in terms of the number of houses. The basic question is whether there is an association between quantity and agency type, with possible connections to quality. For size we know about the number of rooms. Specifically, beyond the central room, we recorded the total number of bedrooms (ranging from 0-6, heavily centered on 2) and about additional special rooms, namely kitchens and bathrooms. We get a count of total additional rooms (additional to a one room box) by adding one if there is a kitchen, one if there is a bathroom, and the count of bedrooms. We estimate whether the count of additional rooms relative to a one room box is influenced by the implementer type.

Here we report on the whole set of control covariates in columns 1 of Table 10 for the smaller better matched fisherman sample. None are significant at the 5% level. In particular, a larger family size

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[21] Altonji et al. (2005) point that when the ratio in row 3 is greater than 1, one can have faith in the OLS estimates because selection on unobservables is likely less than selection on observables. Given that most of our covariates are statistically estimated at zero in equations (1) and (2) of Appendix 2, one may wonder why the ratio is not larger than the 0.55 we get. One of the assumptions in the Altonji et al. procedure is that no single variable dominate the distribution of the outcome or treatment variable. This is because the covariance between $X\gamma$ and $X\beta$ can be large when a few coefficient estimates in γ and β dominate the other coefficient estimates in magnitude. In our case, the coefficient estimates on the pre-tsunami *arisan* variable in equations (1) and (2) of Appendix 2, though statistically not different from zero, were larger than the other coefficient estimates rendering a seemingly large correlation between ε and u.

does not mean a larger house, indicating how little village and family conditions may affect outcomes. Relative to BRR, the count of additional rooms is the same across agencies except for domestic implementers. They provide on average 8% fewer rooms. As in Tables 5 and 6, results are not significantly affected by replacing district with sub-district fixed effects. Domestic implementers offer both lower quality and smaller houses.

The second dimension of quantity is the number of houses provided in each village. The last 2 columns of Table 10 look at the reduced form associations with the number of houses provided on aid in each village. Column 2 shows the number is related to basics: increasing in houses destroyed (aid claim) and numbers of surviving households (need). In column 2 the only other significant determinant is the village population survival rate, where aid is decreasing in survival rate, conditional on houses destroyed and surviving numbers of households. It may be that there was more sympathy in giving to villages with the lowest population survival rates. Distance from Banda Aceh and social capital measures have no significant effect on total house aid.

In Henderson and Lee (2012), we argue that in a context of surplus housing and a willing reserve supplier to meet any demand, that if all villages have the same tastes for excess housing, they will opt for the same amount of housing regardless of the tastes of their main implementer. Here we simply ask two questions. Is overall quantity provided associated with having more agencies and less domination in the village? Second, does the type of main provider matter and affect overall supply to the village? To answer these questions, in column 3 we add in controls for having two or more housing providers and for the main implementer type. None of these variables are significant at the 5% level, although having a second provider is significant at the 10% level.[22] Implementer type having no impact is consistent with there being a reserve provider. Adding sub-district fixed effects has no impact on implementer-type coefficients. If we add agency fixed effects as a more general control on agency tastes for housing provision, the multiple provider coefficient is reduced (coefficient (s.e.) of .060 (.084)).[23]

The last issue concerns whether implementer type is associated with social agendas, where the main house agency might play a role. Is there a hint that domestic implementers are associated with better social outcomes, which might offer a clue why, say international donors would choose them, given they provide poorly constructed houses. House implementers are deeply involved in village planning and have

[22] This weak effect of a second provider is consistent with heterogeneity in village tastes for housing quantity is in the model in Henderson and Lee (2012), where high taste can move the main provider to not strategically attempt to crowd out the reserve provider

[23] Correlations in the data do not suggest a “quantity-quality trade-off”, but neither have we modeled how that would play out. We computed residuals for excess housing from column 3 of Table 8 and added them to a column 1 Table 5 quality specification (with the full or reduced set of village covariates, but no implementer types). Adding that measure of excess housing and that measure interacted with whether a village has two or more providers produces zero coefficients on the excess housing variable and any interaction with multiple providers. It seems excess housing per se is not associated with lower quality.

the most sustained presence in the village. Planning can involve public buildings, road provision, environmental concerns and the like. Since generally house NGOs don't provide these facilities, it means that the main house provider works with other implementers to help a village get particular public buildings or infrastructure. For example, donor-implementers with their suggested social agendas might try to influence provision of public goods in the villages in which they are so involved. Domestic implementers might "work better with the community" in the pursuit of village social goals, given their greater contacts with domestic institutions and common culture with villagers? The correlations we present meet no reasonable standard of causality and matching is now an explicit problem where village measured covariates influence outcomes.

We specifically ask if the count of public buildings, the count of cooperatives and village enterprises, the fraction of roads paved, and whether coastal protection initiatives that were undertaken for the sub-sample of coastal villages through planting of mangroves, pines and grasses are associated with house implementer type.[24] Public buildings include mosques, village halls, fishermen halls, Islamic and state schools and health facilities. Results are summarized in Table 11. The most obvious finding is the very poor outcomes in all dimensions associated with BRR. They are dominated by all other implementer types in almost all dimensions. The second is the good outcomes associated with domestic implementers. Overall in the four dimensions, they do as well as donor-implementers and arguably better than international implementers. Adding sub-district fixed effects does not alter the general results in columns 1-4.[25] Table 11 associations are at least consistent with the idea that some donors hire domestic implementers to help work more towards improving village quality of life by exploiting their local contacts and institutional knowledge.

IV Imposition of socio-political beliefs: boat aid

For boats, social agendas of certain aid agencies imposed shared ownership on a substantial fraction of aid boats, to try to force a move away from the traditional captain-owner-crew social and economic structure. In Figure 2, we saw that sharing did not survive well with time, but the examination is complicated because boat failure is associated with sharing. It could be that the agencies where sharing was a strong social agenda happened to also give poor quality boats. However, there is heterogeneity of boat quality within agencies and aid boats were distributed by village fishing leaders. The question we explore here is whether the fishing leaders who allocated boats further thwarted the unpopular sharing agenda by assigning poor quality boats to shared ownership. That is, for boats from the same agency, better quality ones were not shared, while worse ones were. Further, sharing is not random across

[24] About 45% of coastal villages plant mangroves, pines and grasses.
[25] Column 1 results show little change in coefficients. In column 2 the domestic implementer coefficient is halved.. Column 3 coefficients are reduced by 25-50%. Column 4 coefficients rise by about 15%.

fishermen; it was disproportionately imposed on lower status fishermen. We assume (1) that leaders knew which boats would fail, which is plausible since construction materials and ex post quality as well as design are all observable to these experienced fishing leaders and (2) sharing per se did not induce failure, a possibility we directly address.

We use individual fishermen data, which cover 88 villages in the estimating sample. These data allow us to tease out the sharing-failure association in detail, which we can't do with the village level data. For these 88 villages, two international agencies, Triangle Generation Humanitaire (TGH) and International Medical Corps (IMC), dominate boat aid, providing over 45% of boats to the fishermen we sample, with no other individual agency providing more than 5.5%. Donor-implementers as identified in RAN for boats are a small group. Despite the limitation on villages covered, we have strong findings.

IV.1 Was sharing targeted?

In Table 12 columns 1 and 2, we look at the correlates of sharing as reported in 2007 by fishermen. Family or fishermen characteristics are related to sharing— previous ownership and higher education lower the likelihood of shared ownership. So sharing to some extent seems to have been imposed on lower status fishermen, who had poorer claims to ownership. Across villages, keeping in mind that we cover a limited sample of villages, sharing increases in villages with higher initial social capital (*arisan* group), which have been more willing to better accept the equality agenda underlying shared ownership. Sharing declines as boat aid rises in a village, suggesting not surprisingly that, in villages with fewer aid boats relative to need, sharing was more likely to occur.

In terms of aid agencies which favored sharing, in the small sample of villages, it is difficult to separate NGO effects from the 3 district fixed effects let alone 31sub-district fixed effects; and we include no fixed effects. IMC which gave 16% of boats in the estimating sample with an 87% share rate are all in one district. TGH which gave 29% of boats with only a 36% share rate is entirely in another district. As Table 12 shows, IMC has significantly more and TGH significantly less than typical sharing (compared to the base of small, ungrouped boat NGOs). British Red Cross (BRC) which favored imposed shared ownership appears only 3 times in the sample. Given all agencies besides IMC and TGH appear infrequently, we tried other groupings. Getting a boat from a boat donor-implementer identified in RAND (3.6% of boats), or from BRR (8.8% of boats) are unrelated to sharing. Regardless, what drove differences in sharing is not critical to the basic results in columns 3 and 4.

IV.2 Assigning sharing to failure

In columns 3 and 4 of Table 12, we turn to failure of aid boats, as recorded after aid was done in 2009. Failure is not related to household or village observables. However, sharing an aid boat significantly increases the likelihood of boat failure, 30% more in column 3.

Column 4 shows specifically that failure is associated with being assigned a shared boat, not with an implementer type per se. In column 4, we examine how sharing versus non-sharing, in association with the NGO group classification in column 2, affects failure. The base case is non-shared boats given by "all other" agencies; there are no BRR boats left in the sample. No non-shared IMC boats fail so there is no estimated coefficient. TGH and IMC shared boats fail at significantly higher rates, by about 40%, compared to the base and even more compared to TGH non-shared boats. Non-shared boats by donor-implementers fail less than other non-shared boats, consistent with the house results, again the idea of better resolved moral hazard problems.

Overall the results suggest that fishing leaders in allocating boats in villages knew which were good and bad boats at the point of assignment. They then assigned the low quality boats to be shared, to satisfy the shared ownership objectives of NGOs. Such sharing was assigned to villagers lower in the social hierarchy (lower education, not pre-tsunami owners).[26]

A concern is that people did not like sharing and so they could have used the boats very hard to raise money to buy out partners. Could they have used them so hard that rather than starting to fail after 5 years they failed within 1-2 years or less? This appears not to be the case. In a smaller matched sample of 2007 and 2009 fishermen, we control for intensity of use in 2007 in terms of length and numbers of trips per week to make sure that higher usage of shared boats is not driving later failure rates. As reported in columns 5 and 6 of Table 12, the two usage variables have tiny coefficients and are completely insignificant; other results are the same as in columns 3 and 4. Failed boats were just badly built.

V. Conclusion

In recent years, many countries have experienced major natural disasters and the massive accompanying humanitarian aid efforts have not been well scrutinized at a micro level. Understanding and analyzing the organization of aid delivery is essential to evaluating aid efficacy and how donor monies are spent.

We find that donor-implementers offer the highest quality housing as rated by village heads and have fewer counts of faults as reported by fishermen, reflecting their greater ability to deal with moral hazard issues. However, evidence suggests they shade in quality as they lose dominance as the leading aid agency in a village. International implementers fare relatively well in providing housing quality, regardless of degree of dominance. In contrast, domestic implementers provide lower quality housing, but are associated with more construction of public buildings and seashore conservation. They also are associated with better maintenance of village religious and occupational traditions.

[26] A "reduced" form for failure (remove sharing variables in columns 3 and 4) gives anticipated effects by status but they are not statistically significant.

We investigate the interaction of aid quality and social agendas by examining boat aid. Shared ownership was the primary social agenda pursued by many agencies delivering boat aid, perhaps in the hopes of reducing within village inequality and improving contractual practices. However, boat aid was extremely heterogeneous with many boats literally failing. We find that village leaders steered poor quality boats towards those upon whom shared ownership was imposed, often lower status fishermen. In other words, under heterogeneity of boat quality, shared ownership which was imposed as a means to decrease inequality resulted in the contrary, where higher status fishermen receive better boats and the lower status poorer quality boats.

What are the policy implications? The behavior of an aid agency that operates on the ground is a composite outcome of the organizational structure of the donating and implementing arms. Depending on that organization, the quality of hard aid and the delivery of social agendas may vary considerably within the same disaster area. Monies from international governments and multi-lateral funds funneled through the receiving country's national government may be poorly spent. Better dissemination of the links between form of delivery and aid outcomes may help private citizens and organizations who donate to NGOs make more informed choices.

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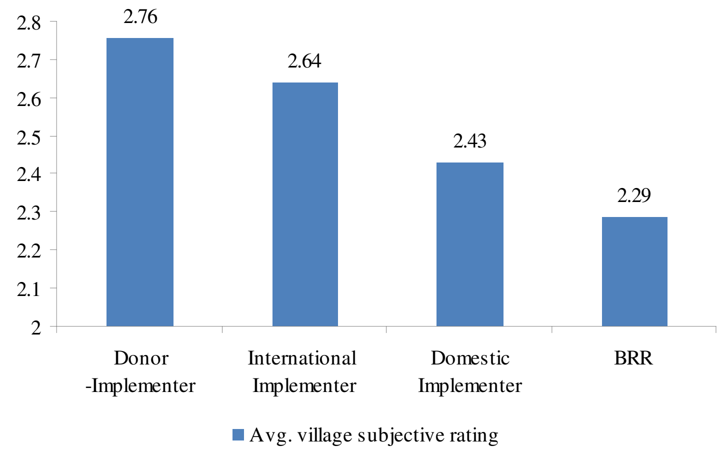
Figure 1. Housing Aid: Quality by types of aid agencies, village head and fishermen 2009



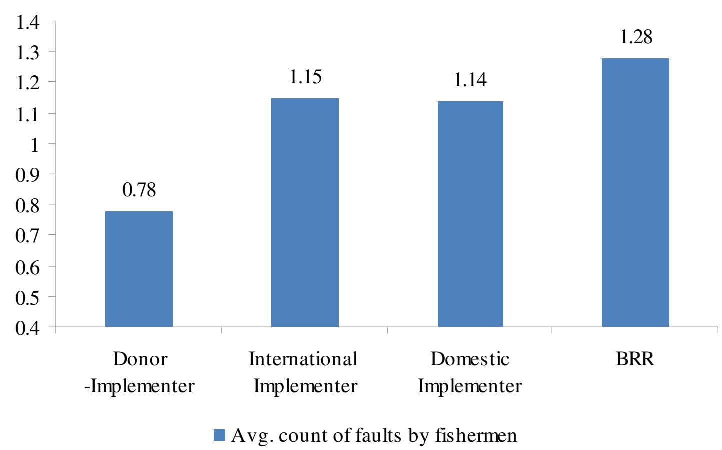


Figure 2. Boat aid: Shared ownership and boat failure, fishermen 2009

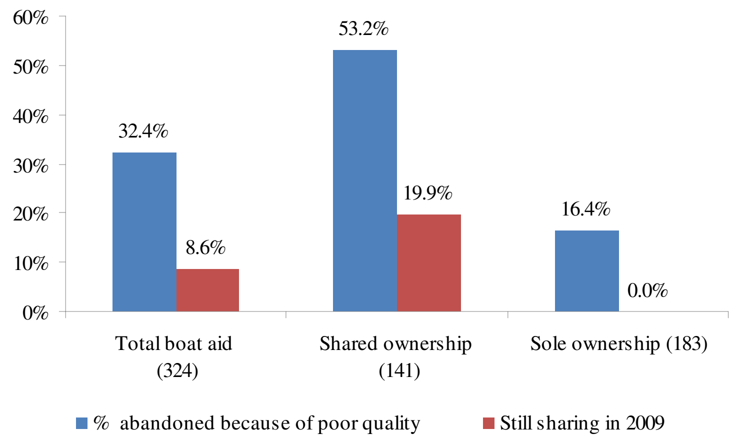


Table 1. Destruction of population and housing

Survival	
Pre-tsunami population [a]	171783 (official)
Survival rate of population [b] [original 05 villages, 104 covered]	**65%** [49%]
Post-tsunami households, official	32876
House aid	
Number of houses survive tsunami, survey	5399
Survival rate houses	**9%**
Number of temporary aid houses built (‘07 survey)	6529
Number of permanent aid houses built (’07 survey)	32277
Replacement rate by late 2007 [c]	**117%**
Number of permanent aid houses built by late 2009	39899
Other aid	
Survival rate public buildings	**6%**
Replacement rate, public buildings by late 2007	**80%**
Replacement rate, public buildings by late 2009	96%
Survival rate of boats [‘05 sample of villages]	**[6%]**
Replacement rate, boats [2007 survey for 96 villages surveyed in ‘05][d]	**[105%]**

Note: Based on 190 villages where there is both 2007 and 2009 information

a. Official population counts pre-tsunami are from the P4B, a 2004 government pre-election census.
b. The official survival rate is the 2006 PODES count divided by the count in P4B. The PODES is a tri-annual government inventory of village populations and facilities. The 2006 PODES in Aceh was conducted in the Spring 2005. It has lower counts of population and households compared to our 2005 survey (Summer and Fall, 2005). This may be partly a “9/11 phenomenon”; as time goes on more missing families are discovered.
c. The replacement rate is the number of houses given in aid divided by the number of surviving households less the number of surviving houses. Includes mosques, village halls, fishermen halls, public and Islamic elementary schools, health facilities.
d. Defined as boats on water by late 2007/surviving captains 2005.

Table 2. Housing aid agencies

Agency	No. of houses given	No. of projects [No. of villages]	Houses per project	Percent villages where present, where dominant provider	Percent villages where present, where almost "sole" provider (> 90%)	Percent projects where minimal provision (< 15%)
BRR	**7541**	**117**	**66**	**34**	**17**	**34**
All Donor-Implementers	**18009**	**115 [107]**	**158**	**80**	**43**	**7**
Canadian Red Cross	2358	11	214	100	91	0
Catholic Relief Service	2282	18	127	83	33	6
United Nations	2087	16	130	75	56	0
World Vision International	1977	12	165	75	42	0
British Red Cross	1247	7	178	100	57	0
All Domestic Implementers	**10772**	**96 [85]**	**112**	**45**	**16**	**17**
Uplink	1390	15	97	73	33	7
All International Implementers	**4517**	**61 [56]**	**74**	**34**	**14**	**23**
Habitat for Humanity (Indonesia)	1392	14	99	50	21	14

Table 3. House implementers

Name of housing agency	Type	No. of village projects	No. of houses	Village Head reports: Mean quality	Village Head reports: Mean quality (weighted)	Fishermen reports: Mean count of faults	Fishermen reports: No. of fisher-men
Canadian Red Cross	Donor-Imp.	10	1758	3.00	3.00	0.81	27
German Information Technology Executive Council(GITEC)[1]	Donor-Imp.	4	856	3.00	3.00	0.78	9
World Vision International	Donor-Imp.	11	1977	2.73	2.89	0.67	12
Spanish Red Cross	Donor-Imp.	2	250	2.75	2.84		
UN	Donor-Imp.	14	2087	2.82	2.83	0.50	6
Catholic Relief Service	Donor-Imp.	18	2282	2.89	2.83	0.00	12
British Red Cross	Donor-Imp.	8	1247	2.63	2.82	0.43	7
German Red Cross	Donor-Imp.	4	652	2.75	2.78		
Turkey[2]	Donor-Imp.	8	842	2.50	2.58	0.83	23
Australian Red Cross	Donor-Imp.	6	493	2.58	2.49		
CARE	Donor-Imp.	3	544	2.17	2.40		
Samaritan's Purse	Donor-Imp.	5	1232	2.30	2.05		
Save the Children	Donor-Imp.	2	75	1.50	1.93		
Concern Worldwide	Donor-Imp.	2	9	1.00	1.00		
GenAssist/CRWRC [Tearfund UK, Mennonite Central Committee]	Int'l Imp.	10	398	2.60	2.93	0.33	3
International Organization for Migration [Various Governments]	Int'l Imp.	5	328	2.70	2.93		
CHF International [Direct Relief International, USAID]	Int'l Imp.	7	380	2.86	2.84	0.00	2
Emergency Architects [French Red Cross, French Government]	Int'l Imp.	3	325	2.83	2.69		
Oxfam [UK Disaster Emergency Committee]	Int'l Imp.	9	514	2.67	2.66	0.89	18
Habitat for Humanity Indonesia [Mercy Corps International]	Int'l Imp.	13	1392	2.62	2.57		
Church World Services [ACT Alliance]	Int'l Imp.	2	192	2.00	2.00		
Muslim Aid Indonesia [Oxfam]	Int'l Imp.	6	390	2.33	1.92		
KOMPAK [s]	Domestic Imp.	8	599	2.88	2.92		
Caritas [d]	Domestic Imp.	5	890	2.60	2.90		
Education and Information Center for Child Rights(KKSP) [Terre des Hommes]	Domestic Imp.	3	600	2.67	2.77		
Indonesian Government Agencies [d]	Domestic Imp.	5	842	2.30	2.64		
Diakonie Emergency Aid [Katahati Institute]	Domestic Imp.	3	97	2.67	2.53		
United Methodist Committee on Relief [d]	Domestic Imp.	3	31	2.67	2.52		
Uplink Indonesia [Canadian Government]	Domestic Imp.	8	1390	2.44	2.42	1.23	31
Asian Development Bank [d]	Domestic Imp.	5	388	2.40	2.37	0.40	5
SOS Desa Taruna Indonesia [SOS Kinderdorf International]	Domestic Imp.	3	520	2.33	2.23	1.13	32
Aceh Relief Fund [Compassion International]	Domestic Imp.	4	198	1.38	1.69	3.00	4
Salam Aceh [s]	Domestic Imp.	2	172	1.50	1.68	1.75	8
MAMAMIA [Caritas]	Domestic Imp.	6	1068	1.42	1.33	1.50	16
Serambi Kasih/Serasih Indonesia [s]	Domestic Imp.	2	177	1.50	1.25	1.50	2
Nor Link/North Link [World Relief]	Domestic Imp.	2	66	1.00	1.00	2.36	14
BRR	BRR	112	7241	2.33	2.32	1.45	86

Notes: For international and domestic implementers the main donor agencies are listed in brackets.
d. Agencies named by the village head that are primarily donor agencies. In this case, implementing agencies are domestic implementers unnamed by the village head.
s. Agencies named in the survey by the village head but that does not show up in the RAN database.
1. GITEC includes the German Technical Cooperation (GTZ) and the German Development Bank (KFW)
2. Turkey includes ABS Turkey, the Istanbul International Brotherhood and Solidarity Association (IBS), and the Turkish Red Crescent

Table 4. Boat NGOs

Name of boat agency	No. of village projects	No. of boats provided	Failure rate	Failure rate (weighted)
Mercy Corps (several)	8	177	0	0
Church World Services	5	82	0	0
Samaritan's Purse	3	55	0	0
CARDI/NRC(Norwegian Refugee Council)	5	43	0	0
CHF International	3	32	0	0
Asian Development Bank	9	25	0	0
TRIKONI	2	24	0	0
Yayasan Tanggul Bencana di Indonesia	2	22	0	0
Yayasan Panglima Laot	5	18	0	0
Austin International Rescue Operation	4	15	0	0
Padi Nusatra (California Origin)	2	11	0	0
Oman	3	8	0	0
GenAssist/CRWRC	2	3	0	0
International Red Cross	11	67	0.09	0.01
Triangle Generation Humanitaire	38	502	0.17	0.08
Salam Aceh - Greeting Aceh	10	131	0.12	0.12
Austrian Tourism Export Council	3	52	0.33	0.15
Government[1]	50	326	0.31	0.19
World Vision International	6	31	0.17	0.32
BRR	9	21	0.50	0.38
Rumah Zakat Indonesia	3	7	0.33	0.43
International Medical Corps	15	101	0.50	0.50
Japan International Cooperation Agency	2	9	0.50	0.56
Kuwait	5	42	0.62	0.57
Africa Islamic AL-AMIN	3	19	0.72	0.58
France	2	36	0.50	0.83
Oxfam	6	215	0.42	0.84
Yayasan PUGAR	3	19	0.67	0.95
Serambi Kasih/Serasih Indonesia	2	10	1	1

Notes: 1. Government includes various Indonesian government agencies including the Minsitry of Fishing Affairs.

Table 5. Quality of housing

Dependent Variable:	Subjective Quality						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ln(no. households post-tsunami)	0.00713	0.00430	0.0140	0.00822	-0.00185	0.0609	
	(0.0608)	(0.0582)	(0.0592)	(0.0648)	(0.0601)	(0.0852)	
Survival rate population	-0.0913	-0.103	-0.110	-0.0790	-0.0929	-0.0906	
	(0.0920)	(0.0871)	(0.0875)	(0.0933)	(0.0851)	(0.0843)	
Mullah survive	0.130	0.157	0.154	0.117	0.146	0.107	
	(0.0942)	(0.0962)	(0.0956)	(0.0996)	(0.0943)	(0.110)	
Pre-tsunami arisan group	0.109	0.0928	0.0930	0.124	0.0730	0.106	
	(0.101)	(0.0994)	(0.0981)	(0.107)	(0.0953)	(0.0949)	
Ln(distance to Banda Aceh)	0.0915	0.0743	0.0783	0.0983	0.104	0.0711	
	(0.103)	(0.102)	(0.0995)	(0.106)	(0.0983)	(0.260)	
Ln(no. houses destroyed)	-0.0261	-0.0300	-0.0328	-0.0265	-0.0329	-0.0495	
	(0.0347)	(0.0322)	(0.0327)	(0.0381)	(0.0329)	(0.0380)	
Village head survive and in office					-0.0467		
					(0.0816)		
Current village head graduated high school					0.0957		
					(0.0821)		
Provider: Donor-Implementer	0.300***						
	(0.0951)						
x 1st project		0.444***	0.443***		0.461***	0.482***	0.496***
		(0.138)	(0.137)		(0.141)	(0.152)	(0.129)
x 2nd project		-0.0732					
		(0.186)					
x 3rd project		0.295					
		(0.314)					
x 2nd or 3rd project			0.0240		0.0134	0.00445	0.100
			(0.181)		(0.183)	(0.193)	(0.188)
Provider: International Implementer	0.312***						
	(0.101)						
x 1st project		0.330*	0.330*		0.325*	0.412**	0.349**
		(0.182)	(0.180)		(0.182)	(0.204)	(0.176)
x 2nd project		0.364**					
		(0.168)					
x 3rd project		0.335*					
		(0.173)					
x 2nd or 3rd project			0.352**		0.347**	0.313*	0.361**
			(0.147)		(0.150)	(0.162)	(0.142)
Provider: Domestic Implementer	-0.0331						
	(0.120)						
x 1st project		-0.187	-0.188		-0.170	-0.206	-0.199
		(0.188)	(0.186)		(0.185)	(0.186)	(0.186)
x 2nd project		0.102					
		(0.186)					
x 3rd project		0.332*					
		(0.171)					
x 2nd or 3rd project			0.166		0.179	0.135	0.162
			(0.162)		(0.160)	(0.169)	(0.155)
Provider: BRR x 2nd project		0.0819					
		(0.155)					
x 3rd project		-0.0728					
		(0.197)					
x 2nd or 3rd project			0.0433		0.0571	0.105	0.0631
			(0.147)		(0.150)	(0.154)	(0.144)
Kabupaten fixed effects	Yes	Yes	Yes	Yes	Yes		
Kecamaten fixed effects						Yes	
Observations	322	322	322	322	322	322	341
R-squared	0.116	0.163	0.153	0.064	0.159	0.237	0.109

Notes: Robust standard errors clustered at the village level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6. Donor-implementer quality shading, robustness to composition of NGOs

Dependent Variable:	Subjective Quality		
	(1)	(2)	(3)
Provider: Donor-Imp. x 1st project	0.601***	0.536***	0.802*
	(0.139)	(0.154)	(0.421)
x ratio of others	-0.972***	-0.740**	-0.801*
	(0.331)	(0.365)	(0.425)
x 2nd or 3rd project	0.0199	0.0181	0.675*
	(0.181)	(0.183)	(0.379)
Provider: Int'l Imp. x 1st project	0.340*	0.341*	0.925**
	(0.181)	(0.182)	(0.426)
x 2nd or 3rd project	0.347**	0.343**	0.684
	(0.148)	(0.150)	(0.458)
Provider: Dom Imp x 1st project	-0.196	-0.199	0.454
	(0.186)	(0.185)	(0.474)
x 2nd or 3rd project	0.148	0.144	0.711*
	(0.162)	(0.162)	(0.405)
Provider: BRR x 2nd or 3rd proj	0.0344	-0.0376	0.272
	(0.147)	(0.149)	(0.473)
Kabupaten fixed effects	Yes	Yes	Yes
Observations	322	299	106
R-squared	0.169	0.165	0.273

Notes: All specifications include village characteristics variables as in Table 7. Robust standard errors clustered at the village level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Housing quality from fishermen data

Dependent variable:	Count of faults			Leaky roof	Cracked Walls	Poor foundation	Faulty plumbing
	(1a)	(1b)	(1c)	(2)	(3)	(4)	(5)
Provider: Donor-Implementer	-0.417	-0.596**	-0.673**	-0.192***	-0.168*	-0.111	-0.139**
	(0.257)	(0.271)	(0.276)	(0.0695)	(0.0919)	(0.0681)	(0.0698)
Provider: International Implementer	-0.171	-0.812	-0.908	-0.265***	-0.170	-0.123*	-0.0981
	(0.370)	(0.526)	(0.582)	(0.0346)	(0.133)	(0.0666)	(0.0905)
Provider: Domestic Implementer	0.0606	0.0602	-0.0826	-0.0927	0.0176	0.0623	0.0702
	(0.170)	(0.207)	(0.204)	(0.0690)	(0.0888)	(0.0889)	(0.0878)
Household controls	Yes	Yes		Yes	Yes	Yes	Yes
Village controls	Yes	Yes		Yes	Yes	Yes	Yes
Kabupaten fixed effects	Yes	Yes		Yes	Yes	Yes	Yes
Observations	529	371	371	371	371	371	371

Notes: In addition to the same village level controls in Table 5, household size, age, and household head education level are included. For columns (2)-(5), reported coefficients are marginal probabilities from a probit regression. Robust standard errors clustered at the village level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8. Matching villages to types of agencies : Multinomial logit

	(1) Donor-Imp.	(2) Int'l Imp.	(3) Domestic Imp.	(4) BRR
Ln (no. households post-tsunami)	0.0360	-0.0191	0.0167	-0.0336
	(0.0474)	(0.0314)	(0.0391)	(0.0453)
Survival rate population	0.0352	0.0257	-0.0233	-0.0376
	(0.0657)	(0.0494)	(0.0702)	(0.0802)
Mullah survive	-0.00364	-0.0148	-0.0286	0.0470
	(0.0590)	(0.0510)	(0.0546)	(0.0568)
Pre-tsunami *arisan* group	0.181***	-0.1000*	-0.0935	0.0122
	(0.0580)	(0.0572)	(0.0600)	(0.0614)
Ln (distance to Banda Aceh)	0.0254	0.0357	-0.0646	0.00349
	(0.0589)	(0.0486)	(0.0530)	(0.0628)
Ln (no. houses destroyed)	-0.0205	0.0192	0.0355	-0.0341
	(0.0310)	(0.0246)	(0.0289)	(0.0276)
Village head survive	0.0397	-0.0736	-0.0123	0.0463
	(0.0714)	(0.0677)	(0.0698)	(0.0721)
Surviving village head graduated	-0.0501	0.0645	0.0479	-0.0623
from high school	(0.0638)	(0.0582)	(0.0620)	(0.0664)
Kabupaten dummies	Yes	Yes	Yes	Yes
Observations	349	349	349	349

Notes: Coefficients are marginal probabilities reported from a multinomial logit regression. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9. Amount of selection on unobservables relative to selection on observables required to attribute the entire agency effect to selection bias

Treatment variable:	Donor Implementer	International Implementer	Domestic Implementer
OLS estimate	0.300	0.312	-0.033
	(0.095)	(0.101)	(0.12)
Implied bias under equality of selection	0.549	-0.078	-0.381
	(0.416)	(0.400)	(0.396)
Ratio of estimate to bias	0.55	-4.00	0.09

Notes: Standard errors are in parentheses. Bootstrapped standard errors are reported for the implied bias estimates. The ratio of estimate to bias is the ratio of selection on unobservables relative to the selection on observables needed to explain away the specific implementer type effect.

Table 10. Other elements of house aid delivery

Dependent Variable:	Count of rooms	Ln(Count of houses in aid in 2009)	
	(1)	(2)	(3)
Family size	0.0128*		
	(0.00748)		
Age of household head	-0.000237		
	(0.00131)		
Education of HH head (levels 1-8)	0.00872		
	(0.0108)		
Ln(no. households post-tsunami)	0.0206	0.576***	0.568***
	(0.0213)	(0.0892)	(0.0894)
Survival rate population	0.0284	-0.190***	-0.185***
	(0.0213)	(0.0727)	(0.0694)
Mullah survive	0.00731	0.00180	0.000658
	(0.0323)	(0.0720)	(0.0697)
Pre-tsunami arisan group	0.0252	0.0504	0.0570
	(0.0291)	(0.0676)	(0.0721)
Ln(distance to Banda Aceh)	0.0102	-0.0895	-0.0768
	(0.0254)	(0.0667)	(0.0706)
Ln(no. houses destroyed)	-0.00203	0.239***	0.234***
	(0.00951)	(0.0715)	(0.0708)
Two or more housing provider			0.107*
			(0.0646)
Agency type: Donor-Implementer	0.0442		0.0179
	(0.0415)		(0.0924)
Agency type: International Implementer	-0.0253		-0.0304
	(0.0492)		(0.112)
Agency type: Domestic Implementer	-0.0779*		0.0384
	(0.0418)		(0.105)
Kabupaten fixed effects	Yes	Yes	Yes
Observations	342	179	179

Notes: Implementer types are for the major housing provider in the village. Coefficients are reported from Poisson regression in column (1) and OLS in the others. Robust standard errors clustered at the village level are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 11. Public buildings and the environment

	Count public bldgs	Count co-op bldgs	Percent roads paved	Plant pines or grasses
	(1)	(2)	(3)	(4)
Major housing provider: Donor-implementer	0.256**	1.083	16.07**	0.241*
	(0.102)	(0.817)	(7.509)	(0.133)
Major housing provider: Int'l implementer	0.175	-0.183	22.89**	0.296*
	(0.122)	(1.003)	(10.10)	(0.171)
Major housing provider: Domestic Implementer	0.198*	1.532*	20.64***	0.351***
	(0.120)	(0.868)	(7.765)	(0.135)
Additional village controls	Public bldgs destroyed	Buildings destroyed	Houses destroyed	
Kabupaten fixed effects	Yes	Yes	Yes	Yes
Observations	184	184	174	147

Notes: Columns (1) and (2) are Poisson regressions, Column (3) is OLS, and Columns (4) is a probit regression. All specifications include the first five village variables in Table 7. Column (4) is based on coastal villages only. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

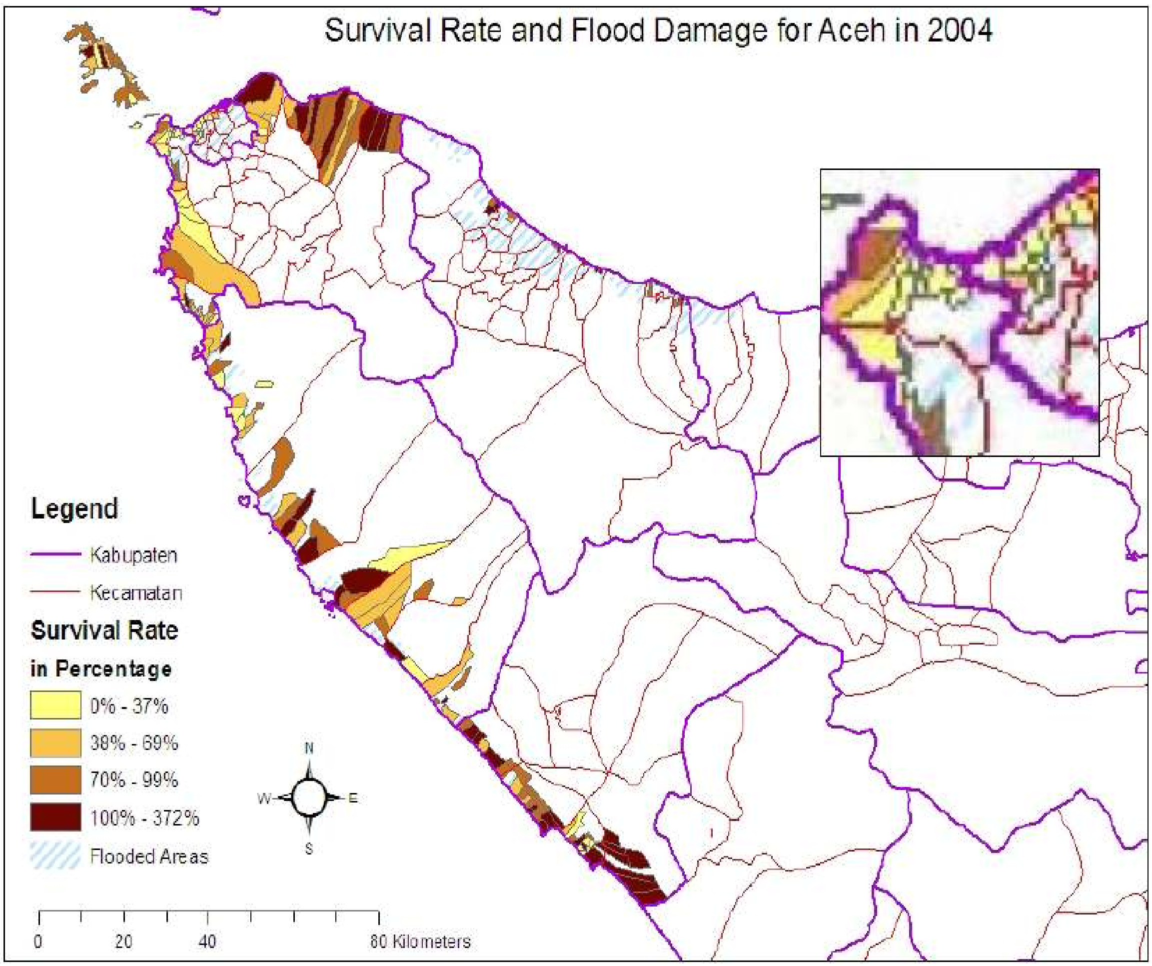
Table 12. Sharing and boat failure in fishermen data

Dependent variable:	Shared boat ownership in 07			Discard boat because of poor quality in 09			
	(1)	(2)		(3)	(4)	(5)	(6)
Family size	-0.0205	0.000334		-0.0179	-0.0362	-0.0143	-0.0273
	(0.0188)	(0.0180)		(0.0222)	(0.0273)	(0.0233)	(0.0310)
Age of household head	-0.00111	-0.00238		0.00140	0.00384	4.86e-05	0.00382
	(0.00244)	(0.00263)		(0.00343)	(0.00335)	(0.00379)	(0.00392)
Education of HH head (levels 1-8)	-0.0326	-0.0466*		0.0102	0.0239	0.0319	0.0477*
	(0.0238)	(0.0251)		(0.0224)	(0.0255)	(0.0221)	(0.0263)
Pre-tsunami boat owner	-0.0929	-0.137**		-0.0396	-0.113	-0.0216	-0.0924
	(0.0623)	(0.0613)		(0.0656)	(0.0718)	(0.0745)	(0.0903)
Ln (boat aid 07)	-0.151***	-0.122***		-0.0335	-0.0272	-0.0264	-0.0256
	(0.0446)	(0.0419)		(0.0339)	(0.0396)	(0.0420)	(0.0479)
Ln (no. fish families 07)	0.0345	0.0422		0.0123	0.0224	0.0332	0.0485*
	(0.0256)	(0.0265)		(0.0171)	(0.0174)	(0.0266)	(0.0290)
Ln(no. households post-tsunami)	-0.0607	-0.188**		0.0379	0.0494	-0.0236	-0.0233
	(0.0743)	(0.0762)		(0.0564)	(0.0625)	(0.0600)	(0.0695)
Survival rate population	-0.139	-0.0475		-0.430**	-0.303*	-0.432**	-0.222
	(0.0967)	(0.115)		(0.189)	(0.176)	(0.202)	(0.192)
Mullah survive	0.0520	0.0513		0.0511	0.0581	0.0821	0.116
	(0.0921)	fishing		(0.0929)	(0.0958)	(0.0987)	(0.100)
Pre-tsunami arisan group	0.196**	0.166**		-0.0739	-0.0716	-0.135	-0.161
	(0.0762)	(0.0727)		(0.0801)	(0.0836)	(0.0985)	(0.0985)
Ln(distance to Banda Aceh)	0.206***	0.0811		0.122**	0.115	0.163***	0.133
	(0.0647)	(0.0773)		(0.0542)	(0.0714)	(0.0574)	(0.0876)
			Shared ownership	0.292***		0.295***	
				(0.0892)		(0.0954)	
Boat NGO : TGH		-0.183**	TGH*Share		0.425***		0.381*
		(0.0901)			(0.151)		(0.212)
Boat NGO : IMC		0.482**	IMC*Share		0.370**		0.457***
		(0.193)			(0.154)		(0.162)
Boat NGO: Boat D-I		0.150	Boat D-I*Share		0.151		0.0971
		(0.221)			(0.233)		(0.205)
Boat NGO: BRR		0.0946	Rest*Share		0.107		0.180
		(0.137)			(0.132)		(0.166)
			TGH*Not share		-0.137		0.0121
					(0.107)		(0.150)
			IMC*Not share		n.a.		n.a.
			Boat D-I*Not share		-0.312***		-0.298***
					(0.0629)		(0.0648)
			Hours per fishing trip			0.00724	-0.00567
						(0.0150)	(0.0160)
			Number of trips per week			0.0492	0.0785
						(0.0506)	(0.0551)
Observations	389	389		281	257	212	192

Notes: Robust standard errors clustered by village are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix 1. Map and survey area

The figure shows a map of the survey area, with a blow-up (right side in figure) of the Banda Aceh area (upper-left part of coastal area). [1] We cover all villages in three contiguous districts (Banda Aceh, Aceh Jaya, and Aceh Besar) going south and north-east of the capital Banda Aceh. In addition we covered the fishing villages in two other districts, up to a defined geographic limit moving east from Banda Aceh into Pidie (the last sub-district surveyed is Meurah Dua) and moving south into Aceh Barat (the last sub-district surveyed is Meuruebo). These include villages on islands offshore. The map shows household survival rates by village (yellow being the worst). Unfortunately, the map is based on the post-tsunami government rendering of village boundaries which is grossly inaccurate. We took GPS readings of the center (the mosque) of the living area of each village. In only 6% of the cases is the GPS reading within the supposed village boundaries. In 15% of the cases, it is over 10 kilometers away. Coastal villages are drawn as non-coastal and vice-versa which explains why, in parts of the map, a yellow (low survival) village may be shown next to a supposed coastal village which is dark (high survival). Nevertheless the map depicts the general survey area.



Appendix 2. Estimation procedure for Table 9

We estimate the selection bias under the assumption that the selection on unobservables is equal to the selection on observables by adapting Altonji et al. (2005) to the multinomial treatment case. We consider the following linear regressions where T_J $(J=1,2,3)$ represents the three implementer types, X the covariates, and y house quality, our outcome of interest.

$$y = \sum_J \alpha_J T_J + X\gamma + \varepsilon \qquad (1)$$

$$T_J = X\beta_J + u_J \qquad (2)$$

Now consider the linear regressions of the treatment variables on the observed and unobserved components in (1)
$T_J = c_J + \varphi_J X\gamma + \delta_J \varepsilon + e_J$ for $J=1,2,3$.

The condition that the selection on unobservables equals the selection on observables implies $\varphi_J = \delta_J$ in the above, which can be expressed as

$$\frac{Cov(\varepsilon, u_J)}{Var(\varepsilon)} = \frac{Cov(X\gamma, X\beta_J)}{Var(X\gamma)} \quad (3).$$

Next, to understand the role of selection bias, we combine equations (1) and (2) to get

$$y = \alpha_1 u_1 + \alpha_2 u_2 + \alpha_3 u_3 + X(\alpha_1\beta_1 + \alpha_2\beta_2 + \alpha_3\beta_3 + \gamma) + \varepsilon$$

and under the simplifying assumption that $Cov(u_i,u_j)=0$,

$$\widehat{\alpha_J} = \alpha_J + \frac{Cov(u_J, \varepsilon)}{Var(u_J)} \quad (4)$$

for $J=1,2,3$. The second term in the right hand side of equation (4) is the bias term which we can estimate using the condition in equation (3). These estimates are reported in row 2 of Table 9. Based on this we can ask, assuming that there is no agency effect ($\alpha_J = 0$), how large the left hand side of equation (3) relative to the right hand side has to be to explain away the estimated impact we find under OLS. In other words, we take the ratio of the OLS estimate in equation (1) and divide it by the bias term in (4). These ratios are reported in row 3 of Table 9.

Data Appendix

The village surveys in summer and fall 2005, fall 2007 and fall 2009 ask questions about education, experience, and survival of village and religious leaders; population composition by sex and age both before and after the tsunami; migration; occupational structure; destruction of village lands, seawalls, aquaculture areas, docking areas and mangroves; pre- and post-tsunami data on political, legal, and social institutions; pre and post tsunami information on physical capital (houses, boats, public buildings); detailed information on initial and ongoing operations of NGOs, local governments, and relief agencies providing housing, boats, public buildings and restoration of the coast line; and detailed information on the village fishing industry pre- and post-tsunami, including questions on marketing, fishing fleet composition, catch composition and boat replacement. The 2005 survey of 111 villages focused on benchmarking destruction and village conditions. The 2007 and 2009 surveys of 199 villages (including the original 111) focused on aspects of the aid effort and institutional transformation of villages, such as the democratic evolution and quality of aid as related to different types of aid agencies.

The fishermen surveys ask about family structure, occupations, social status, income and aspect of debt and wealth, housing and boat destruction and aid, fishing productivity, and family participation in village activities. The 2005 survey focused on 475 original boat owners and captains in 77 villages (about 40% of surviving captains and owners in those villages), benchmarking family destruction of people, housing and boats, as well as pre-tsunami productivity. The 2007 and 2009 surveys follow these families, marking their rebuilding of families, new occupational choices, aid received, re-establishment or not of fishing activities, and evolving family participation in village life. In the second wave as followed in the third, besides the original families we extended village coverage and added a module for new boat owners— villagers given an aid boat who had never owned a boat. In the second wave (2007) we have about 700 families in 96 villages and in the third wave (2009) after some sorting and attrition we drop coverage to about 635 fishing families in 90 villages. Here our focus is on the quality of aid received and response to low quality boat aid.

We need to empirically classify the aid agencies in Aceh. The agencies we focus on are those reported by the village head in 2009 as having delivered aid in their villages. We classify each agency that shows up in the survey as whether it is international or local, government or private, and its religious affiliation if it has any. However, we are unable to classify each agency's principal or agent, i.e., donor or implementer status in Aceh simply based on agency names. For this we utilize an additional source of data. The Indonesian government, working with the UN, has also recorded aid delivery aspects in the "RAN" [Recovery Aceh-Nias] database [http://rand.brr.go.id/RAND/reference]. We use the RAN database to classify agency types, particularly whether it was an implementing agency or a funding agency or both in Aceh. In RAN, for each project in a village a first level implementer is named as well as the underlying donors, often many in number. A first level implementer is the leading agency that either directly hires the labor to be used in construction or monitors any sub-contractors.

We classify an agency named by the village head as a donor-implementer if it appears as both a donor and implementer in at least 30% of the villages it provides housing in RAN.[27] Although we drew a 30% cutoff, almost all agencies we classify as donor-implementers are *always* both donor and implementer in our villages. Donor-implementers typically have on the ground operations with central offices in Banda Aceh (capital of Aceh), and large teams of trained people in the field. All agencies in this category are considered international agencies as well[28].

We define an international implementer, if the agency named by the village head is an international first level implementer representing a different, usually international donor in RAN. While their donors face the agency problem of monitoring the quality of aid delivered by the implementer, these implementers have international reputations at stake.

The domestic implementer category occurs when a village in our survey names an aid agency that is a domestic implementer or an agency which according to RAN is just a donor and not an implementer. As such, the underlying domestic or international donors must hire a domestic implementer. As the visible aid agency, sometimes the village head names the international donor rather than the domestic implementer— perhaps a function of greater visibility. For example, some international donors (such as religious NGOs) sent delegations for short visits to villages where their money was being spent. Some international donors and their domestic implementers are intertwined by village heads. Either their names are explicitly linked, or in one year one agency is named and in another the other is named. Common examples include NORLINK/Salam Aceh and Caritas/Mammamia.

The Appendix table below summarizes aspect of the data and the results of the agency classification based on RAN.

[27] In some cases an NGO has multiple projects in the same village. We require for at least one of those they are both the donor and implementer

[28] One organization, the Bakrie Group, is actually a domestic agency. However, unlike the many temporary local NGOs, the Bakrie Group is one of the largest Indonesian business conglomerates and is not short lived and has a reputation to take care for. Hence, we classify it into the donor-implementer category rather than including it in the domestic implementer category.

Variables	Obs	Mean	Variable	Obs	Mean
A. Village level variables			**C. Fishermen level variables**		
Total housing aid in 09	190	199.211	Count of faults in house	643	1.036
No. of households post-tsunami	190	177.105	Count of additions in house	643	2.300
Survival rate population	188	0.749	Count of rooms	577	1.920
Mullah survive	189	0.651	Family size in 09	643	4.005
Pre-tsunami arisan group	190	0.684	Household head age in 09	640	42.923
Distance to Banda Aceh	188	67.659	Household education in 09	637	3.551
No. of houses destroyed	185	210.984	Agency is D-I	587	0.317
Dominant provider is BRR	190	0.200	Agency is BRR	587	0.104
Dominant provider is D-I	190	0.432	Agency is Int'l Imp.	587	0.210
Dominant provider is Int'l Imp.	190	0.116	Agency is Dom Imp.	587	0.370
Dominant provider is Dom Imp.	190	0.247	1st level D-I	289	0.208
Number of housing projects	190	1.947	2nd or 3rd level D-I	289	0.014
Percent centrally piped water	180	40.706	1st level Int'l Imp.	289	0.028
Number of public buildings	190	3.679	2nd or 3rd level Int'l Imp.	289	0.014
Number of Co-op buildings	190	0.516	1st level BRR	289	0.135
Percent roads paved	185	53.319	2nd or 3rd level BRR	289	0.166
Plant pines, grasses	160	0.450	1st level Dom Imp.	289	0.370
			2nd or 3rd level Dom Imp.	289	0.066
B. Project level variables			Have leakyroof	643	0.260
Subjective quality	341	2.4619	Have cracked wall	643	0.328
Objective quality	370	0.7351	Have poor foundation	643	0.207
Agency is D-I	570	0.1912	Have faulty plumbing	643	0.241
Agency is BRR	570	0.1930	Have kitchen	643	0.628
Agency is Int'l Imp.	570	0.1070	Have bathroom	643	0.890
Agency is Dom Imp.	570	0.1579	Have plumbing	643	0.782
1st level D-I	570	0.1439	Evening pray count	632	4.220
2nd level D-I	570	0.0351	Household size in 09	643	4.005
3rd level D-I	570	0.0123	Fishermen in 09	643	0.664
1st level BRR	570	0.0667	Previous boat owner	643	0.499
2nd level BRR	570	0.0982	Fishing family succession	645	0.081
3rd level BRR	570	0.0281			
1st level Int'l Imp.	570	0.0386			
2nd level Int'l Imp.	570	0.0421			
3rd level Int'l Imp.	570	0.0263			
1st level Dom Imp.	570	0.0825			
2nd level Dom Imp.	570	0.0526			
3rd level Dom Imp.	570	0.0228			

The Impact of Microinsurance on Asset Accumulation and Human Capital Investments: Evidence from a Drought in Kenya

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Abstract:

When natural disasters strike in developing countries, households often face large negative shocks to productive assets. If these assets are their main or only source of livelihood, recovery can be difficult. Recovering from such shocks often requires reduced consumption, which can come at a high cost to health and nutrition both in the short run and the long run. In this paper we ask: can insurance transfer risk in a way that reduces the need for households to rely on costly coping strategies that undermine their future productive capacity? Since 2010, pastoralists in northern Kenya have had the opportunity to purchase a novel index-based insurance contract to protect against drought risk. We take advantage of an insurance payout induced by a drought in 2011 to analyze the immediate impacts of this microinsurance pilot on expected asset accumulation and human capital investments. Our results show that insured households are 18-50 percentage points less likely to draw down assets, improving their ability to recover after the drought. In addition, insured households are 8-41 percentage points less likely to reduce meals than their uninsured counterpart. Instead of compromising health and nutrition, most insured households expect to use a portion of the insurance payout to purchase food, maintaining their current consumption of food, and making continued investments in the human capital of future generations. By improving food security during a drought, we also find that insured households are 41-51 percentage points less dependent on food aid and 3-31 percentage points less reliant on other forms of assistance.

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Whenever extreme drought strikes northern Kenya, the effects can be devastating. Livestock, the primary asset and source of livelihood, weaken and often die. Distressed sales of livestock flood the market, causing downward pressure on livestock prices. The combination of livestock loss and destocking herds debilitates the household's main productive resource, making recovery after the drought all the more challenging. In an effort to maintain assets, households may instead choose to cut back on meals. Yet by reducing consumption, households undercut critical investments in human capital, inhibiting both current and future productivity. In these ways a single negative shock can lead to chronic poverty by restricting the ability of households to generate current and future income. In this paper we assess whether insurance offers an effective alternative to these costly coping strategies which make recovery so difficult.

Insurance has been widely heralded in the past decade as a market-based risk transfer mechanism that has the potential to act as a safety net, preventing against catastrophic collapse. Although development of insurance pilot projects have been widespread, little is known about their impact. In this paper we ask: Can insurance transfer risk in such a way that it reduces the need for households to rely on costly coping strategies that undermine future productivity? That is, are insured households less likely to sell livestock or reduce consumption? In addition, are insured households more self-sufficient, relying less on food aid or assistance from others?

Our analysis offers one of the first empirical assessments of the impact of a marketed index-based insurance contract on households ability to cope with shocks in developing countries. We report the impact results from the index-based livestock insurance (IBLI) pilot in Marsabit district of northern Kenya. Since 2010, pastoralists in northern Kenya have had the opportunity to purchase a novel index-based insurance contract to protect against livestock mortality losses due to drought. A harsh drought swept the Horn of Africa in 2011 activating the first IBLI payout. We use household expectations at the time of the payout to empirically study the impact of the index-based livestock insurance on pastoralist households' asset accumulation and human capital investment decisions.

Our results reveal that, relative to uninsured households, insured households will radically reduce their dependence on costly coping strategies. Our major findings are threefold: 1) Insuring against losses results in an 18-50 percentage point reduction in the number of households who anticipated selling further livestock to cope with the wake of the 2011 drought (overall a 50% reduction), improving their ability to recover after the drought. In addition, insured households move from being net sellers to net buyers of livestock. This shift may help stem a price collapse, resulting in positive spillover effects to uninsured households. 2) Insured households are 8-41 percentage points less likely to reduce meals than their uninsured counterpart (an overall reduction of about one third). This behavioral change implies a reduction in the number of undernourished and malnourished individuals, including women and children, in this food insecure region. 3) As food security improves, insured households are 41-51 percentage points less dependent on food aid and 3-31 percentage points less reliant on other forms of assistance. Together, these results suggest that insurance can help households to protect assets during crises, without having a deleterious effect on human capital investments.

The rest of this paper is organized as follows: Section 1.1 reviews some of the relevant literature on risk in developing countries and its permanent consequences. Section 1.2 then provides an overview of the literature studying how insurance might help households to cope with uninsured risk and vulnerability, particularly in developing countries. In Section 2, we provide background on the research setting, discuss some of the limitations of our data, and then present our estimation strategy. We employ a number of different techniques: difference-in-differences, matching methods, Heckman correction, and instrumental variables to control for selection bias in the decision to insure. In Section 3, we present and discuss our main finding: that insurance dramatically reduces the need for a household to depend on costly coping strategies which undermine its future productivity. Section 4 follows, in which we consider more carefully what the results mean by also studying how households expect to use the insurance payout. We conclude in Section 5, and also make some suggestions for future research.

1 Background Literature

1.1 Shocks and their Permanent Consequences

Uninsured risk and vulnerability can be an unavoidable part of daily life for households in developing countries. Not only can shocks give rise to temporary consequences, but there is growing evidence to suggest that shocks can result in permanent consequences. This finding has developed into a wide literature of poverty traps. A poverty trap has been defined as "any self-reinforcing mechanism which causes poverty to persist." (Azariadis and Stachurski, 2005). This literature has often focused on multiple equilibrium poverty traps, which are characterized by at least one equilibrium associated with a poor standard of living, and another associated with a high standard of living. The existence of multiple equilibria also implies the existence of a "threshold" or "tipping point" at the boundary between the two regions.

If a threshold exists, at which we observe a bifurcation of equilibrium outcomes, then a shock will result in permanent consequences whenever it propels a household across the threshold. Building on this concept, Carter and Barrett (2006) develop an asset-based approach in which they distinguish transitory poverty from chronic structural poverty by using a dynamic asset poverty line. In this framework, if assets fall below a critical threshold in any period, then households will find it difficult to accumulate assets; they become trapped in poverty.

The asset-based approach to understanding persistent poverty suggests an important behavioral response to critical thresholds. Zimmerman and Carter (2003) use stochastic dynamic programming techniques to show that households above the threshold will optimally choose to smooth consumption, whereas poorer households around the threshold will choose to smooth assets instead, because asset preservation is crucial to future consumption. Hoddinott (2006) provides evidence that in the wake of the 1994-1995 drought in Zimbabwe, richer households sold livestock in order to maintain consumption. In contrast, poor households with one or two oxen or cows were much less likely to sell livestock, massively destabilizing consumption instead. In Ethiopia, Carter et al. (2007) also find evidence of asset smoothing by the poor, as households coping with a drought attempted to hold onto their livestock at the cost of consumption. Carter and Lybbert (2012) find similar evidence in Burkina Faso. They empirically estimate an asset threshold, and show that households above the estimated dynamic asset threshold almost completely insulate their consumption from weather shocks by drawing down assets, whereas households below the threshold do not.

The dilemma, as Hoddinott (2006) points out, is that even though asset smoothing is an attempt to preserve assets, consumption is an input into the formation and maintenance of human capital. Hoddinott poignantly argues that, "The true distinction lies in households' choices regarding what type of capital - physical, financial, social or human (and which human) - that they should draw down given an income shock." While asset protection strategies are designed to avoid a poverty trap, they likely come at a very high cost of immediately reduced consumption, with potentially irreversible losses in child health and nutrition (Carter et al., 2007).

The outcomes of undernutrition and malnutrition are widely known. In children, these conditions can lead to muscle wastage, stunting, increased susceptibility to illness, lower motor and cognitive skills, slowed behavioral development, and increased morbidity and mortality (Ray, 1998; Martorell, 1999). Those that do survive suffer functional disadvantages as adults, including diminished intellectual performance, work capacity and strength. In women, undernourishment during childhood can be the cause of lower adult body mass, which means increased risk of delivery complications and lower birthweights for the next generation (Martorell, 1999). These outcomes set the stage for a pernicious intergenerational cycle of undernutrition and its destructive effects. Moreover, undernourishment during adulthood further diminishes muscular strength and increases susceptibility to disease. Such undernourishment in adults can also lead to a nutrition-based poverty trap if it decreases the capacity to do productive work (Dasgupta and Ray, 1986)

This dilemma points to a need for a productive safety net that protects vulnerable households from 1) losing productive assets, and 2) engaging in costly coping strategies which impair the human capital of current and future generations. Insurance is a market-based product which has the potential to act as a safety net (Barrett et al., 2007; Skees and Collier, 2008). It offers an alternative means of coping with negative shocks, allowing smoothing of consumption and nutrition, as well as avoidance of costly asset depletion (Dercon et al., 2008).

1.2 The Potential Impacts of Microinsurance

A growing literature has been devoted to studying the benefits of insurance for poor households in low income countries. This type of insurance (targeted to poor households, and available at low cost) has become known as microinsurance. Barnett, Barrett, and Skees (2008), Dercon et al. (2008) and Cole et al. (2012) provide summaries of the literature. The literature highlights two primary avenues through which insurance might bring about positive impacts. These avenues reflect the fact that households make both *ex ante* risk management decisions and *ex post* risk coping decisions.

In the absence of insurance, there are several potential avenues for *ex ante* risk management. One option is to simply allocate resources toward activities with lower risk. However, a tradeoff exists: these lower-risk activities generally produce a lower return. Another option is to build up precautionary savings, but such savings must come at the cost of (often critical) investment or consumption today. Households may also choose to reduce their risk exposure by diversifying crop choice, assets or activities, but such diversification is not always possible, and can only be beneficial if the risk involved is not perfectly correlated across the various activities (Dercon et al., 2008).

Insurance provides an alternative risk management tool that may reduce the use of these and other *ex ante* risk management strategies. By altering the ability of households to cope with risk *ex post*, insurance may change optimal behavior before a shock is actually observed. To demonstrate this effect, de Nicola (2011) estimates a dynamic stochastic model of weather insurance. The model predicts that insurance will increase the adoption of riskier but more productive seeds, and also stimulate decreased investment, as households shift towards higher levels of consumption. This may reflect the idea that investment is a form of precautionary savings in her model. Janzen, Carter, and Ikegami (2012) use similar methods to show that when you account for a critical asset threshold, around which optimal behavior and equilibrium outcomes bifurcate, increased investment occurs around the threshold as households assume greater risk in order to attain higher productivity and a higher equilibrium. The same model shows that households above the threshold follow de Nicola's prescription: decreased investment and increased consumption as households move away from holding assets as precautionary savings.

Cole et al. (2012) conduct a systematic review of the effectiveness of microinsurance, and specifically index-based insurance, in helping smallholders manage weather-related risks. Their review identifies a substantial evidence gap in the literature on the impact of index-based microinsurance. Several papers have attempted to bridge this gap empirically, but all papers known to the authors focus on the impact of insurance on household's *ex ante* risk management strategies. These papers all show that insurance encourages investment in higher risk activities with higher expected profits. Cai et al. (2012) find that insurance for sows significantly increases farmers' tendency to raise sows in southwestern China, where sow production is considered a risky production activity with potentially large returns. Karlan et al. (2012) find that farmers who purchase rainfall index insurance in Ghana increase agricultural investment. Cai (2012) finds that tobacco insurance increases the land tobacco farmers devoted to risky tobacco production by 20% in China. This implies reduced diversification among tobacco farmers. The same paper also finds that insurance causes households to decrease savings by more than 30%, suggesting that households were building up extra savings in order to better smooth consumption in the case of a shock. Hill and Viceisza (2010) use experimental methods to show that in a game setting, insurance induces farmers in rural Ethiopia to take greater, yet profitable risks, by increasing (theoretical) purchase of fertilizer.

While the impacts of insurance on *ex ante* risk management decisions are important, none of these papers is able to assess how an insurance payout directly influences the ability of households to recover after a shock. This paper represents one of the first attempts to fill this gap by studying the impact of insurance on *ex post* risk coping decisions. We do so by empirically analyzing whether the index-based livestock insurance contract in northern Kenya successfully functioned as a safety net by preventing costly coping strategies which might otherwise have crippled future productivity.

2 Research Setting, Data and Estimation Strategy

More than 3 million pastoralist households live in northern Kenya's arid and semi arid lands. The vast majority of these households rely on livestock for their primary livelihood. When drought hits this region, households dependent on livestock must cope with large livestock losses. According to the data used for this paper, in the recent drought that devastated the Horn of Africa in 2011, families lost on average more than one third of their animals. During and after a drought, cash-strapped food-insecure households often face a difficult choice: sell off remaining livestock or reduce consumption. Sometimes both strategies are necessary for survival. Both of these strategies undercut future productivity, often reinforcing the poverty impacts of uninsured risk.

In recent years, two large scale initiatives were introduced in this region as an attempt to help pastoralists manage risk. First, in 2008 the Hunger Safety Net Program (HSNP) was launched in four districts of northern Kenya, including parts of Marsabit district. The HSNP provides reliable cash transfers to poor households, in an effort to enhance their capacity to make critical investments in their future prospects. Subsequently, in January 2010 the index-based livestock insurance (IBLI) pilot project was launched in Marsabit District of northern Kenya as a collaborative project of the International Livestock Research Institute, Cornell University, Syracuse University and the BASIS Research Program at the University of California at Davis in an effort to help pastoralists manage drought risk.

The IBLI index insurance contract uses satellite-based NDVI (normalized difference vegetation index) measures of available vegetative cover to predict average livestock mortality experienced by local communities. The IBLI index has been shown to be highly correlated with actual livestock mortality losses experienced by pastoralists in the region (see Chantarat et al., 2010, 2012 for details). Households choose the number of livestock they wish to insure, with the contract expressed in tropical livestock units (TLU), so that a single annual contract accommodates the various livestock species common in the region: goats, sheep, cattle, and camels.[1] The premium households pay depends on the risk associated with the geographic region in which they live (Upper Marsabit is more susceptible to extreme drought, so households insuring in this region are required to pay a higher premium). Insured households receive a payout at the end of each dry season (at the beginning of October and again early in March) if the predicted average livestock mortality rate reaches 15%, with the payout equal to the value of all predicted losses greater than 15%. In October-November 2011, a harsh drought swept across the Horn of Africa, and the first IBLI payouts were made to households who had purchased insurance earlier in the year. Households in our study received an average payout of about 10,000 Kenyan Shillings (or roughly $150).

[1] In the IBLI contract, a goat or sheep is equal to .1 TLU, cattle are equal to a single TLU, and a camel is equal to 1.4 TLU.

2.1 Data

Marsabit district was randomly assigned as the district to begin role out of the IBLI pilot. Although households must self-select into purchasing insurance, by randomizing treatment assignment, we can compare the insurance treatment population to a control population in order to determine the intention to treat (ITT) impacts of insurance. Both HSNP and IBLI were implemented in connection with an integrated impact evaluation to allow researchers to determine these ITT impacts. As part of the evaluation, households in each of the following geographic regimes were randomly selected to participate in a panel household survey: 1) Control locations (no access to either HSNP or IBLI), 2) HSNP-only locations, 3) IBLI-only locations, 4) Locations with access to both HSNP and IBLI. This long-term research design will allow researchers to explore both the long term impacts of the cash transfers on pastoralists' livelihoods (Jensen, 2012), and the long term impacts of insurance on both *ex ante* risk management and *ex post* coping strategies. In addition, researchers will be able to compare the insurance impacts to those resulting from the cash transfer program.

For this paper, we would ideally like to compare the IBLI-only group (3) to the control group (1). However, the nature and timing of surveys differ between regimes (1)-(2) and (3)-(4). This difference limits our ability to use the pure control group (1) to assess the immediate impacts of the 2011 insurance payout on the ability of households to cope with the shock *ex post*. Instead, for this analysis we are limited only to groups (3)-(4), in which all households had the opportunity to insure their livestock. Since households must self-select into purchasing insurance, we must account for selection bias in the analysis.

The data available includes household-level information collected annually (beginning in 2009) for 924 randomly selected households living in various sublocations across Marsabit district with access to IBLI. In each round of the survey, households were asked to answer questions about health, education, livestock holdings, herd migration, livelihood activities, income, consumption, assets, and access to credit. Each household also participated in an experiment to elicit their risk preferences. In the surveys following the baseline, households were also asked questions about insurance purchases, access to information about insurance, and tested on their level of insurance understanding.

Within this sample, two additional levels of randomization occurred at the household-level. First, as part of an encouragement design, in each period 60% of surveyed households were randomly selected to receive coupons offering a 10-60% discount on the first 15 TLU insured. Second, some households were randomly selected to participate in experimental games, which were used as a means of communicating the complex concepts of index insurance. The games were designed to demonstrate the inter-temporal benefits of insurance by simulating herd dynamics over multiple seasons. They demonstrated that insurance would have to be purchased before the season began, and for each subsequent season that coverage was desired. In addition, the games conveyed that indemnity payments were triggered by droughts, that IBLI would not cover non-drought-induced losses, and that if a drought did not trigger payments, the premium would not be returned (see McPeak, Chantarat, and Mude, 2010 for details). Non-participants heard about IBLI from other participants, through village assemblies, by word of mouth or through local village insurance promoters.

Most of the data used for this analysis comes from the third round of the panel survey, completed in October-November 2011. The only exception is the non-livestock asset index, which uses information collected in the previous year. This index was constructed from the first principle component using factor analysis. Table 1 reports summary statistics on key variables by treatment and control. The treated group refers to the population which chose to purchase insurance. All households had the opportunity to insure, but only 24% actually purchased insurance. Variables reported include the level of education of the household head, a dummy variable for whether a household is risk-taking or risk-moderate (as determined from an experiment eliciting risk preferences), a non-livestock asset index, the number of livestock owned, livestock losses in the past year, expected livestock losses in the next year and whether households indicated that it is difficult to acquire a loan. In addition, we show summary information on IBLI-specific variables of interest: dummy variables for indicated that they learned about IBLI from the game, a village insurance promoter, or from the survey, whether or not they received a discount coupon and its value, the number of ways they heard about IBLI (to control for their awareness of IBLI) and a final variable capturing their level of knowledge and understanding about IBLI. This knowledge/understanding variable was constructed by counting the number of correct responses provided in a short test about IBLI.

As we can see, the insured population appears relatively similar to the uninsured population with few observable statistically significant differences between the treatment and control. The encouragement design appears to have been effective, with the treated population being more likely to have received a coupon (and one of larger size). The game, on the contrary, does not appear to have influenced insurance adoption.

The third round of the panel survey occurred around the same time as the October-November 2011 IBLI payout. At that time every household was asked about the ways in which they had been coping with the drought over the prior three months. Households were asked if they had engaged in specific behaviors, including selling livestock, reducing meals, relying on food aid or assistance from others, pulling children out of school, increasing non-livestock activities, or migrating to look for work outside the community. They were then asked how they anticipated coping with the drought in the upcoming three months. Insured households were asked this second question after being told exactly how much they should expect to receive as an insurance payment if they hadn't already received one. Most payouts were received within days or weeks of the survey, but a few households had already received the payout.

Our results are based on these anticipated behavioral changes after receipt of the October 2011 insurance payouts. By comparing the immediately anticipated behavioral changes made by insured households with those of their uninsured peers, we can measure the immediate impact of drought insurance on household well-being. Table 2 shows a list of actions that both insured and uninsured households could have taken to cope with the drought. Column

Table 1: Summary Statistics for Key Variables of Interest

Variable	Insured Qtr 3	Uninsured Qtr 3	Difference in Means Qtr 3
Highest level education, household head	.76	1.18	.416
	(.20)	(.15)	(.294)
Risk-taking	.24	.29	.049
	(.03)	(.15)	(.041)
Risk-moderate	.50	.45	-.054
	(.04)	(.02)	(.046)
Non-livestock asset index	.15	.00	-.150
	(.10)	(.05)	(.098)
TLU Owned	16.22	18.86	2.646
	(1.40)	(1.29)	(2.423)
TLU losses in past year	7.67	7.53	-.137
	(.89)	(.49)	(1.001)
Difficult to acquire a loan	.42	.37	-.043
	(.04)	(.02)	(.045)
Expected TLU losses in next year	6.98	7.56	.579
	(.62)	(.37)	(.748)
Participated in IBLI game	.28	.25	-.030
	(.04)	(.02)	(.040)
Received IBLI discount coupon	.87	.55	-.320***
	(.03)	(.02)	(.043)
Value of IBLI discount coupon	23.85	16.53	-7.32***
	(1.85)	(.97)	(2.01)
Heard about IBLI from Village Insurance Promoter	.71	.47	-.238***
	(.04)	(.02)	(.045)
Heard about IBLI from survey	.56	.50	-.062
	(.04)	(.02)	(.046)
Number of IBLI information sources	2.16	1.89	-.269
	(.07)	(.05)	(.092)
IBLI Understanding (Results from IBLI test)	1.96	1.45	-.502
	(.08)	(.05)	(.105)

Table 2: Summary of Behavior to Cope with 2011 Drought in Marsabit

Action	Insured Qtr 3	Insured Qtr 4	Uninsured Qtr 3	Uninsured Qtr 4	Difference in Means Qtr 3
Sell livestock	.33	.12	.28	.32	-.042
	(.04)	(.03)	(.02)	(.02)	(.04)
Reduce the number of meals eaten each day	.64	.33	.74	.70	.102**
	(.04)	(.04)	(.02)	(.02)	(.04)
Rely more on food aid	.92	.43	.92	.92	.005
	(.02)	(.04)	(.01)	(.01)	(.02)
Rely on assistance from others	.35	.15	.44	.45	.096**
	(.04)	(.03)	(.02)	(.02)	(.05)
Pull children otherwise in school, out of school	.11	.08	.11	09	.006
	(.03)	(.02)	(.01)	(.01)	(.03)
Increase non-livestock activities like petty trade	.26	.22	.20	.26	-.061
	(.04)	(.03)	(.02)	(.02)	(.04)
Send family members to look for work elsewhere	.04	.04	.05	.07	.010
	(.02)	(.02)	(.01)	(.01)	(.02)

2 shows the proportion of insured households reporting that they engaged in a particular behavior in the prior 3 months. For ease of exposition, we describe this period as the 3rd Quarter of 2011. Column 3 shows the proportion of insured households who expected to do so in the next 3 months (during what we refer to as Quarter 4) after receiving their insurance payout. Columns 4 and 5 do the same for uninsured households who were expecting no insurance payout. As can be seen, substantial majorities of both insured and uninsured households cut back on meals and use more food aid to deal with the drought. Roughly a third in each group sold livestock from their already diminished herds.

The last column of Table 2 reports the difference between the percentage of the treated (insured) population who answered yes to using a given coping strategy in the 3rd quarter of 2011, prior to receiving an insurance payout, to the control (uninsured) population. A difference between the two implies one of two things: treated/insured households are coping differently in anticipation of a payout (which we would expect if insurance stimulates *ex* *ante* behavioral changes), or insured households are intrinsically different from uninsured households. If the latter is true, then we need to control for selection bias in our estimation strategy. This comparison is calculated in the last column of Table 2. We find that there are some statistically significant differences. In particular, insured households are less likely to have reduced the number of meals eaten each day and less likely to have relied on assistance from others in the 3rd quarter of 2011. These differences force us to think critically about selection bias.

2.2 Estimation Strategy

One way to control for pre-existing differences is to use a difference-in-differences (DD) estimator. Let p_3 denote the proportion of households saying that they used a particular coping strategy in the third quarter of 2011, and p_4 be the proportion anticipating using that strategy in the fourth quarter. A superscript I indicates insured households, while a U superscript indicates data from non-insured households. Thus, p_3^I represents the proportion of the insured population who engaged in a particular coping strategy during the 3rd quarter of 2011, whereas p_4^I represents the proportion of insured households anticipating using a strategy soon, knowing that they were about to receive an IBLI payout. Similarly, p_3^U represents actual behavior in the 3rd quarter by uninsured households, while p_4^U represents uninsured households anticipated coping behavior in the 4th quarter.

To estimate the impact of insurance on ability to cope, we can use the following DD estimator:

$$DD = (p_4^I - p_3^I) - (p_4^U - p_3^U) \tag{1}$$

The estimate can be interpreted as a percentage point difference. By assuming a common trend, the DD estimator controls for biases in second period comparisons between treatment and control groups that could be the result of permanent differences between those groups, as well as biases from comparisons over time in the treatment group that could be the result of trends. In our case, this means the DD estimate controls for differences that could be the result of households behaving differently in anticipation of a payout. As such, the DD impacts capture only the impact of insurance on households ability to cope with drought *ex post.*

By incorporating the DD estimator into a regression framework, we are able to control for initial conditions and estimate standard errors. Let $action_{i,t}$ be the coping strategy in period t, $insured_i$ is the non-random treatment variable, $post_t$ is a dummy variable that takes on a value of 1 after insurance payouts have been made, and $(insured_i * post_t)$ is the interaction between being insured and the *post* indicator variable. In our case, this interaction term is equal to 1 if a household receives a payout. The DD estimator, controlling for household baseline characteristics $\mathbf{X}_i$, is obtained by estimating the following specification, where the coefficient of interest is β_3:

$$action_{i,t} = \beta_0 + \beta_1 \, insured_i + \beta_2 \, post_t + \beta_3 \, (insured_i * post_t) + \mathbf{X}_i \, \delta + \theta_V + \epsilon_{i,v,t} \qquad (2)$$

A basic assumption behind the simple implementation of DD is that other covariates do not change between the 3rd and 4th quarter. Although our data includes a rich panel, with annual data collected for 4 years, we only have information at the quarterly level regarding coping strategies. As such, between period 3 and 4, we must assume that all other covariates do not change. Nonetheless, we can increase power if we control for these initial conditions ($\mathbf{X}_i$) which might affect a household's coping strategy.[2] In addition to controlling for $\mathbf{X}_i$, we can also control for geographic or ethnicity fixed effects. These fixed effects are captured by θ_V in Equation 2. The error term $\epsilon_{i,t}$ captures unobserved characteristics or shocks. Using fixed effects, we cluster the error term at the group level, by either location or ethnicity.

[2]We control for an index of non-livestock assets (which is an indicator of household wealth), livestock owned, the eduction of the head of household, risk aversion, the difficulty of obtaining a loan (an indicator of a credit constraint), actual livestock losses experienced by the household in the previous year, and expected livestock losses in the next year.

One option to further control (and test) for selection bias is to use the Heckman correction with difference-in-differences. Using this method we estimate a probit model, in which we regress the treatment variable on a number of exogenous variables affecting treatment, including at least one variable which belongs in the selection equation but does not appear in the equation of interest. The estimated parameters are then used to calculate an inverse Mills ratio, which captures the unexplained variation correlated with sample selectivity. For this reason, we can include the inverse Mills ratio as an additional explanatory variable in the DD estimation. If the estimated coefficient for the inverse Mills ratio is different from zero, then we should be concerned about selection bias.

Although the DD estimator is a straightforward method given the nature of our data, it relies on an assumption that unobserved heterogeneity between the treatment and control is time invariant. That is, after controlling for level differences among households, we assume they would have exhibited the same trends in how they cope with drought in the absence of insurance. Unfortunately, our data provides little opportunity to test that assumption, and there may be reasons to believe that the assumption is invalid. If the assumption is invalid, we need to control for time-varying unobserved heterogeneity.

One promising alternative that doesn't require an assumption of time invariance is to use matching methods. Instead of making the common trends assumption which is necessary to estimate DD, matching methods assume that unobserved factors do not affect participation. If we can control for all the factors that affect participation, then matching provides consistent estimates of the impact of insurance, although it doesn't provide any information on additional relationships of interest.

A number of matching methods exist. In this paper we present the results for nearest neighbor matching as well as propensity score matching using radius matching. For nearest neighbor matching, we seek to find the closest comparison group from a sample of non-insured households to a sample of insured households by matching on the initial value of the outcome of interest (a given behavior in the 3rd quarter). This method is an obvious choice in our case since the primary differences between treatment and control are observed across the various outcomes of interest, rather than through other channels. As an alternative, we also present the results using propensity score matching, in which we match based on wealth and other household characteristics.

We may have reason to believe that unobserved factors, such as motivation or entrepreneurship, affect a household's decision to insure their livestock. If so, matching methods are not appropriate. In this case, an instrumental variables (IV) approach is a preferable alternative because it allows for endogenous insurance participation. IV estimation requires a carefully selected instrument that is highly correlated with insurance participation, but not correlated with unobserved characteristics affecting outcomes.

The encouragement design implemented with IBLI provides three suitable instruments: participation in an insurance game, receipt of an insurance coupon and the subsequent value of the discount coupon. All are the result of randomization, so none should be correlated with coping strategies ($action_{i,t}$), but we expect all to be highly correlated with insurance uptake. Table 1 suggests that the coupon (both receipt of and value) is a good instrument. Unfortunately, participation in the insurance game is not as highly correlated with insurance uptake as we might expect, and may be a weak instrument. Using IV we obtain the local average treatment effect of insurance on coping strategies. As a final robustness check, IV can be combined with DD as long as the same instruments are correlated with demand for insurance but not outcomes over time. Under the combined IV-DD approach, we obtain the local average treatment effect of insurance on the *change* in coping strategies: ($action_{i,t=1} - action_{i,t=0}$).

3 Results

In this section we present the results of the impact analysis using DD, Heckman correction, IV, and matching methods across various outcomes. We focus on the impact of insurance on four primary outcomes of interest: livestock sales, reduction in the number of daily meals consumed, reliance on food aid, and dependence on assistance from others. The results are presented for each outcome in Tables 4-7 respectively.

In each of these tables, the first four columns show the estimation results of equation 2, where the difference-in-differences coefficient of interest is β_3, corresponding to the interaction term $insured_i * post_t$. The first column is the basic DD model without controlling for other variables. The second column adds control variables and fixed effects. In the third column we use a Heckman correction technique, in which we include the inverse Mills ratio of the insurance selection equation to correct for selection bias.

In columns (4) and (5) we present the results using an instrumental variables approach. In both cases we use three IVs: participation in the insurance game, receipt of an IBLI discount coupon, and the value of IBLI discount received. Each of these were the result of randomization, so we can be reasonably certain that they do not influence a household's response to the drought, except through the purchase of insurance. In column (4) we use IV with DD, whereas in the fifth column of each table we disregard information about previous behavior, and only use cross section data. For this reason, in column (5) the coefficient of interest belongs with $insured_i$.

The remaining two columns report the average treatment effect on the treated households using matching methods. In column (6), we follow Hill and Viceisza (2010) and use nearest neighbor matching to obtain the average treatment effect. For each outcome, we match on the 3rd quarter behavior for the same outcome. This allows us to compare outcomes of "similar" treatment and control populations. In the final column we present the results using propensity score matching (using radius matching), in which we match based on wealth and other household characteristics. The details of the first stage probit regressions for the Heckman correction, the instrumental variables method, and the propensity score matching results are provided in Table 3.

3.1 Impact on Livestock Sales

One way households often deal with large negative shocks is to sell their assets in order to purchase food and other necessities. In Marsabit, assets are primarily held as livestock. By the time the drought is severe enough to necessitate such sales, livestock are often weak and of little value. In addition, since drought generally affects a large geographic area, the massive sell-off of livestock throughout the region further reduces the market price of livestock so that income earned from livestock sales generally provides little purchasing power. When the rains return and the drought lifts, the lack of productive assets further increases the difficulty of coping with a drought's aftermath.

The results presented in Table 4 suggest that insurance substantially reduces the probability that a household will choose to sell livestock during a drought. This improves the post-drought income-generating potential of insured households. The DD results imply a 25 percentage point reduction in the number of households who anticipated selling further livestock to cope with the 2011 drought. This represents an overall reduction of about one half, relative to previous behavior. The estimates obtained using matching methods are also highly significant, although slightly smaller, suggesting an 18-22 percentage point decrease in a household's tendency to sell livestock. Using instrumental variables the impact is even larger than the DD estimates, with insurance causing a 35-50 percentage point reduction in the number of households who expect to sell additional livestock in the next quarter. Overall, the results suggest that insured households are much less likely to sell livestock during a drought, improving the possibility of a successful recovery.

3.2 Impact on Consumption

When poor households endeavor to maintain finite productive assets during a shock, it often imposes a high cost on consumption. Undernutrition and malnutrition not only impose temporary hunger, but are likely to result in irreversible consequences to long run welfare. Table 5 considers the impact of IBLI on daily household consumption. Using the simplest specification, insurance (and receiving an insurance payout) results in a 27 percentage point drop in the number of households that decrease the number of meals eaten each day when under stress from a drought. Overall, this is a reduction of about one third. This result suggests that insurance improves food security; insured households are much less likely to be malnourished or undernourished during a drought. The matching estimates are somewhat larger: insured households are 33-42 percentage points less likely to reduce the number of meals eaten each day. The result loses statistical significance when we use instrumental variables, but the sign and interpretation are the same: receiving an insurance payout makes it less likely that a household will cut back on meals. These results coupled with the results of Section 3.1 suggest that insurance can promote asset smoothing without having the deleterious effect on consumption.

3.3 Impact on Self-reliance

To assess whether insured households have greater food security during a drought, we also consider whether insurance increases self-reliance. That is, do insured households depend less on food aid or assistance from others? Table 6 considers the impact of insurance on food aid dependence. Together, the DD, matching and IV results suggest that insurance causes a 43-51 percentage point drop in the probability that a household depends on food aid (more than normal) during a drought. These estimates are highly statistically significant across each specification.

Similarly, Table 7 suggests that insured households may be less likely to rely on assistance from others. The difference-in-difference estimates predict a 21 percentage point reduction in reliance on others. Matching estimates suggests a 27-31 percentage point reduction in this form of dependence. While these last results are not robust to IV estimation, the results presented in Tables 6 and 7 combined imply that insured households may be more self-reliant during a drought, reducing their reliance on handouts by more than half.

Table 3: Demand for Insurance: First Stage Probit Regression

	Heckman (1)	DD+IV (2)	IV (3)	PSM (4)
Heard about IBLI from game	-0.0360	-0.0510	-0.0851	
	(0.205)	(0.158)	(0.177)	
Received IBLI discount coupon	1.293***	1.055***	1.323***	
	(0.186)	(0.158)	(0.181)	
Value of IBLI discount coupon	-0.00555*	-0.00364	-0.00423	
	(0.003)	(0.003)	(0.003)	
Highest level education, household head	-0.0488**		-0.0459*	-0.0448**
	(0.025)		(0.024)	(0.021)
Risk-taking	-0.0281		-0.0326	-0.136
	(0.175)		(0.172)	(0.126)
Risk-moderate	0.120		0.159	
	(0.156)		(0.152)	
Non-livestock asset index	0.155**		0.153**	0.116**
	(0.075)		(0.073)	(0.056)
TLU Owned	-0.00452		-0.00380	-0.00467
	(0.004)		(0.003)	(0.003)
TLU losses in past year	0.0113		0.0104	0.00742
	(0.008)		(0.007)	(0.007)
Difficult to acquire a loan	-0.116		0.00334	0.126
	(0.136)		(0.128)	(0.113)
Expected TLU losses in next year	-0.00791		-0.00797	
	(0.009)		(0.008)	
Heard about IBLI from Village Insurance Promoter	0.359**			
	(0.165)			
Heard about IBLI from Survey	0.0729			
	(0.155)			
Number of IBLI Information Sources	0.0430			
	(0.092)			
Knowledge of IBLI	0.179***			
	(0.063)			
Location fixed effects	no	no	yes	no
Ethnicity fixed effects	no	no	yes	no
Observations	631	649	632	634
Pseudo R-squared	0.216	0.122	0.122	0.0156

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

Column (1) reports the first stage selection estimates used to calculate the inverse Mills ratio for the third column of Tables 4-7. Column (2) and (3) are the first stage estimates used to obtain the IV-DD and cross section IV estimates in columns (4)-(5) of Tables 4-7. Column (4) reports the first stage results of the propensity score matching estimates presented in the final column of Tables 4-7.

Table 4: Impact of Insurance #1, Sell livestock

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
*insured*post*	-0.250***	-0.252***	-0.252***	-0.352*			
	(0.054)	(0.059)	(0.055)	(0.207)			
post	0.0384	0.0353	0.0353	0.0629			
	(0.029)	(0.022)	(0.030)	(0.057)			
insured	0.0421	-0.00452	0.0495	-0.165	-0.493***		
	(0.043)	(0.044)	(0.045)	(0.147)	(0.119)		
Highest education (head)		0.00307			0.000195		
		(0.004)			(0.006)		
Risk-taking		0.0259			0.0286		
		(0.041)			(0.047)		
Risk-moderate		-0.0260			0.00946		
		(0.026)			(0.042)		
Non-livestock asset index		0.0120			-0.00701		
		(0.021)			(0.022)		
TLU Owned		-3.21e-05			0.000957		
		(0.001)			(0.001)		
TLU losses in past year		0.00337*			0.00559**		
		(0.002)			(0.002)		
Difficult to acquire a loan		-0.0189			0.00836		
		(0.084)			(0.036)		
Expected TLU losses		-0.00539*			-0.00361*		
		(0.003)			(0.002)		
Inverse Mills Ratio			0.00411				
			(0.019)				
Average Treatment Effect						-.221***	-.184***
(Matching Estimates)						(0.033)	(0.040)
Location fixed effects	no	yes	no	no	yes		
Ethnicity fixed effects	no	yes	no	no	yes		
Observations	1,302	1,268	1,266	1,302	634	497	651
R-squared	0.021	0.184	0.021	0.011	0.126	-	-

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

Column (1) is the basic DD model without controlling for other variables. Column (2) adds control variables and fixed effects. In column (3) we include the inverse Mills ratio of the insurance selection equation to correct for selection bias. In columns (4) and (5) we instrument for *insured*. First stage regressions for columns (3)-(5) are reported in Table 3. Columns (6) and (7) report the average treatment effect estimates from both nearest neighbor and propensity score matching, respectively.

Table 5: Impact of Insurance #2, Reduce the number of meals eaten each day

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
*insured*post*	-0.272***	-0.277***	-0.277***	-0.0839			
	(0.061)	(0.062)	(0.062)	(0.212)			
post	-0.0424	-0.0456***	-0.0457	-0.0874			
	(0.028)	(0.014)	(0.029)	(0.057)			
insured	-0.102**	-0.0800	-0.0995**	-0.0440	-0.243*		
	(0.043)	(0.057)	(0.046)	(0.144)	(0.131)		
Highest education (head)		-0.00278			-0.00216		
		(0.006)			(0.007)		
Risk-taking		-0.0678*			-0.0813		
		(0.035)			(0.052)		
Risk-moderate		-0.0154			-0.0394		
		(0.046)			(0.048)		
Non-livestock asset index		-0.0327			-0.0473**		
		(0.029)			(0.023)		
TLU Owned		0.00114***			-7.29e-05		
		(0.000)			(0.001)		
TLU losses in past year		-0.00355**			-0.00317		
		(0.001)			(0.002)		
Difficult to acquire a loan		0.0936			0.0846**		
		(0.068)			(0.038)		
Expected TLU losses		0.00370			0.00176		
		(0.004)			(0.002)		
Inverse Mills Ratio			0.00925				
			(0.019)				
Average Treatment Effect						-.332***	-.416***
(Matching Estimates)						(0.039)	(0.049)
Location fixed effects	no	yes	no	no	yes		
Ethnicity fixed effects	no	yes	no	no	yes		
Observations	1,302	1,268	1,266	1,302	634	497	651
R-squared	0.075	0.210	0.079	0.014	0.115	-	-

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

Column (1) is the basic DD model without controlling for other variables. Column (2) adds control variables and fixed effects. In column (3) we include the inverse Mills ratio of the insurance selection equation to correct for selection bias. In columns (4) and (5) we instrument for *insured*. First stage regressions for columns (3)-(5) are reported in Table 3. Columns (6) and (7) report the average treatment effect estimates from both nearest neighbor and propensity score matching, respectively.

Table 6: Impact of Insurance #3, Rely more on food aid

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
*insured*post*	-0.489***	-0.502***	-0.502***	-0.406***			
	(0.049)	(0.073)	(0.049)	(0.149)			
post	0.00202	0.00207	0.00208	-0.0178			
	(0.017)	(0.009)	(0.017)	(0.038)			
insured	-0.00455	0.0182	0.00700	0.0194	-0.435***		
	(0.025)	(0.026)	(0.026)	(0.087)	(0.112)		
Highest education (head)		-0.0102**			-0.00835		
		(0.004)			(0.006)		
Risk-taking		0.00484			0.0282		
		(0.027)			(0.042)		
Risk-moderate		0.0382			0.0601		
		(0.031)			(0.039)		
Non-livestock asset index		-0.0537**			-0.0581**		
		(0.023)			(0.023)		
TLU Owned		-0.000410			-0.000536		
		(0.000)			(0.001)		
TLU losses in past year		0.000306			0.000397		
		(0.001)			(0.002)		
Difficult to acquire a loan		0.0238			0.00659		
		(0.025)			(0.032)		
Expected TLU losses		-0.00239			-0.00113		
		(0.002)			(0.002)		
Inverse Mills Ratio			0.000513				
			(0.013)				
Average Treatment Effect						-.492***	-.513***
(Matching Estimates)						(0.035)	(0.044)
Location fixed effects	no	yes	no	no	yes		
Ethnicity fixed effects	no	yes	no	no	yes		
Observations	1,302	1,268	1,266	1,302	634	497	651
R-squared	0.215	0.291	0.220	0.038	0.100	-	-

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

Column (1) is the basic DD model without controlling for other variables. Column (2) adds control variables and fixed effects. In column (3) we include the inverse Mills ratio of the insurance selection equation to correct for selection bias. In columns (4) and (5) we instrument for *insured*. First stage regressions for columns (3)-(5) are reported in Table 3. Columns (6) and (7) report the average treatment effect estimates from both nearest neighbor and propensity score matching, respectively.

Table 7: Impact of Insurance #4, Rely on assistance from others

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
*insured*post*	-0.209***	-0.208***	-0.208***	-0.0284			
	(0.057)	(0.032)	(0.058)	(0.221)			
post	0.0101	0.0104	0.0104	-0.0331			
	(0.032)	(0.013)	(0.032)	(0.060)			
insured	-0.0963**	-0.0434	-0.0779	-0.0362	-0.0631		
	(0.044)	(0.054)	(0.047)	(0.158)	(0.127)		
Highest education (head)		-0.0104			-0.00688		
		(0.006)			(0.007)		
Risk-taking		-0.0709			-0.0411		
		(0.053)			(0.052)		
Risk-moderate		-0.0145			-0.0132		
		(0.039)			(0.048)		
Non-livestock asset index		0.00739			-0.00587		
		(0.019)			(0.021)		
TLU Owned		-0.000438			0.000203		
		(0.001)			(0.001)		
TLU losses in past year		-0.000790			-0.00408*		
		(0.003)			(0.002)		
Difficult to acquire a loan		-0.0459			-0.0704*		
		(0.056)			(0.040)		
Expected TLU losses		0.00262			0.00219		
		(0.007)			(0.002)		
Inverse Mills Ratio			0.0260				
			(0.021)				
Average Treatment Effect						-.273***	-.309***
(Matching Estimates)						(0.036)	(0.044)
Location fixed effects	no	yes	no	no	yes		
Ethnicity fixed effects	no	yes	no	no	yes		
Observations	1,302	1,268	1,266	1,302	634	497	651
R-squared	0.041	0.140	0.042	0.002	0.081	-	-

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

Column (1) is the basic DD model without controlling for other variables. Column (2) adds control variables and fixed effects. In column (3) we include the inverse Mills ratio of the insurance selection equation to correct for selection bias. In columns (4) and (5) we instrument for *insured*. First stage regressions for columns (3)-(5) are reported in Table 3. Columns (6) and (7) report the average treatment effect estimates from both nearest neighbor and propensity score matching, respectively.

3.4 Additional Impacts

Table 2 shows that selling livestock, reducing meals, and relying on food aid or assistance from others are the major coping strategies employed during a drought. In addition to these options, we might expect that more households would have removed children from school, so that children could instead engage in productive labor to improve the household's consumption options. We do not show that to be the case; 11% of the total population (including both insured and uninsured households) pulled children out of school in the 3rd quarter of 2011 as a way of coping with the drought, and insurance appears to have no impact on this decision. This coping strategy is probably not often utilized because supplemental school feeding programs exist to keep food-insecure children in school. In fact, it seems likely that households are in fact more likely to send previously unenrolled children to school in order to receive supplemental feedings during times of heightened food insecurity.

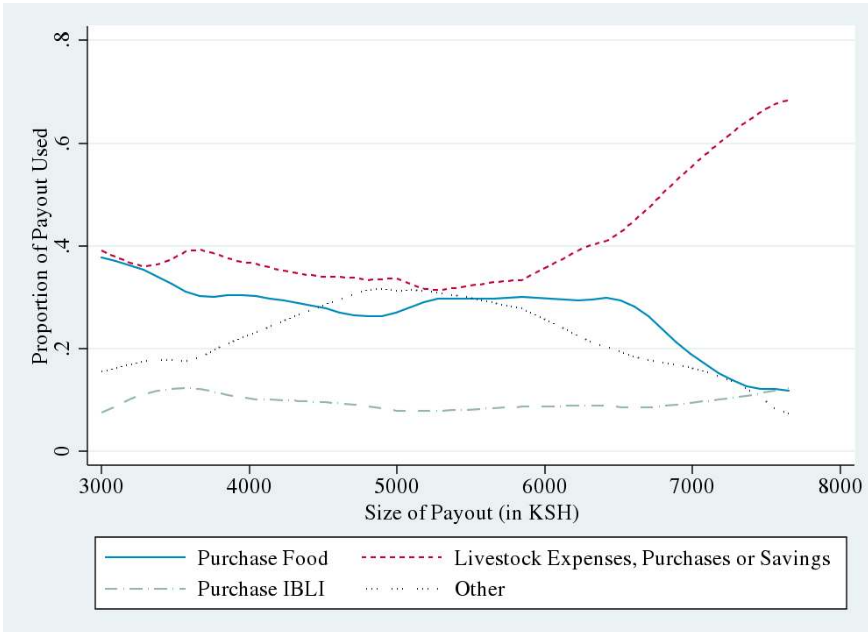
Approximately one quarter of the population attempted to diversify into non-livestock activities during the drought. However, insurance appears to have no impact on the choice to diversify. Another seldom-used option available to households is to send household members to look for work outside their community. Insurance appears to have no impact on whether households choose to migrate.

4 Accumulation and Market Effects

Another way to assess the impact of insurance is to consider how insured households spend the IBLI payout. For each surveyed household, we calculated an expected IBLI payout based on the number of livestock units they had insured. Every insured household was then asked how they expect to spend the estimated payout.

Figure 1 demonstrates how heterogeneous households expect to use the insurance payout. The vertical axis shows the fraction of a payout that a household anticipates spending on different goods (food, livestock purchases, etc.). The horizontal axis shows the total amount

Figure 1: IBLI Payout Allocation



of the payment to be received in Kenyan Shillings[3]. As we would expect, the proportion spent on different categories varies with the size of payout. Notably, a larger share is spent on food when the payout is small, whereas a larger share is spent on purchasing livestock or other savings when the expected payout is larger. Regardless of the size of payout, households on average expect to spend 10% of the IBLI payout on purchasing livestock insurance again.

In the previous section, we showed that insured households were 50% less likely to sell livestock to cope with the wake of the 2011 drought. Here, we show that insured households also indicated a willingness to spend on average between 30% and 70% of the insurance payout on livestock purchases, or livestock-related expenses. This finding suggests that IBLI shifts households from being net sellers to net buyers of livestock. This new source of demand for livestock will help maintain current livestock prices, halting the price collapse which typically occurs during droughts. This outcome is likely to have positive spillover impacts on uninsured households by increasing the purchasing power of those who depend on livestock sales to purchase food and other necessities.

[3]1000 KSH is approximately $12.

5 Conclusion

When adverse shocks strike in developing countries, poor households are often forced to choose between drawing down productive assets or human capital. Either way, uninsured risk can result in permanent consequences if the household's choice undermines its future productivity. In this paper we assess whether insurance can function as a safety net, preventing household asset depletion and improving the human capital of future generations.

Our findings suggest that IBLI payouts in Marsabit district of northern Kenya during the drought of 2011 provided substantial immediate benefits to insured households. Insured households were much less likely to sell livestock, improving their chances of recovery. Rather than sell livestock, these same households appear to shift from net sellers to net buyers of livestock. This shift has the potential to result in positive spillovers to uninsured households wishing to sell livestock by increasing demand for livestock, stemming the oft-observed drought-induced price collapse. Insured households also intend to use a portion of their anticipated payouts to purchase food. By using part of the payout to purchase food, most insured households expect to maintain their current food consumption, rather than reduce meals like their uninsured neighbors. This makes insured households more self-reliant (less likely to rely on food aid or assistance from others) and more food secure. Moreover, our results suggest that insurance can promote asset smoothing without having the deleterious long term consequences of destabilized consumption.

These results come at a critical time for policymakers. There has recently been a grand push from development agencies to scale up microinsurance pilots with the goal of reaching a larger number of households. This push has transpired in spite of an incomplete understanding of microinsurance impacts. This paper provides some empirical evidence that insurance can improve outcomes when negative strikes occur, but the results are not definitive. The findings are based in part on immediate expectations regarding a specific insurance pilot project. If these expectations closely follow true behavior, then the highly anticipated long term positive welfare impacts of IBLI are likely to be observed in the near future. Regardless, further impact analyses are necessary in order to generalize the results more broadly. While we wait to observe long run impacts of a variety of insurance pilots, the results presented here seem a strong indicator that microinsurance can be a helpful strategy for households coping with risk in developing countries.

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