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Floods and spillovers: households after the 2011 great flood in Thailand

Abstract

In 2011, Thailand experienced its worst flood ever. Using repeated waves of the Thai Household Survey, we analyse the flood's economic impacts. In 2012, households answered a set of questions on the extent of flooding they experienced. We use this self-identified flood exposure, and external exposure indicators from satellite images, to identify both directly affected households and those that were not directly flooded but their communities were (the spillovers). We measure the impact of the disaster on income, expenditure, assets, debt and savings levels, directly, and indirectly on spillover households. We also analyse the flood's impacts across different socio-economic groups.

JEL-Codes: O120, Q540.

Keywords: disaster, flood, Thailand, economic impact.

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1. Introduction

In 2011, Thailand experienced a catastrophic flood. Not only did the flood cause widespread damages, and atypically for slow-moving floods, it also resulted in a considerable loss of life as well. The World Bank's Impact Assessment Report (2012) estimated there were 800 fatalities and a total loss of THB 1.43 trillion (USD 46.5 billion) making it the costliest flood ever, globally.[1] According to current GDP figures, the estimated direct loss of property and infrastructure due to the 2011 flood amounts to nearly 13% of the Thai economy. The flood affected many provinces (including most importantly the commercial hub of Bangkok). It started with very heavy rains in late July – early August 2011 monsoon season. Flooding started in the North of the country, and subsequently led to an overflow in the Chao Phraya river (the main river flowing through Greater Bangkok). Most of the impact of the flood was experienced in the last quarter of 2011, with the high water reaching Greater Bangkok in early November (Okazumi et al., 2013; World Bank, 2012)

Figure 1 maps cumulative annual rainfall in 2011, providing insight on both the severity and incidence of flooding across the country. According to the Thai Meteorological Department, mean annual rainfall reached its peak in 2011 representing a 24% deviation from normal. Alongside record-breaking rainfall, Poapongsakorn (2012) attributes the extensive damage to Thailand's inefficient water management, unplanned urbanisation and lack of a reliable flood warning system. He argued that economic and human losses could have been contained through the implementation of effective ex ante prevention and mitigation policies.

In terms of macroeconomic impacts, the sustained flooding resulted in a loss of production in the manufacturing sector accounting for an estimated 70% of the total damage (World Bank, 2012). GDP growth fell sharply in 2011 as seen in figure 2. Following manufacturing, the housing and agricultural sectors suffered the greatest losses. By some estimates, THB 110 billion were lost in wages, 1.9 million houses were affected and around 12.5% of the cultivated land in Thailand was damaged (Aon Benfield, 2012 & World Bank, 2012). With the country's rural population reliant on agriculture for their living, these impacts could have had potentially large welfare implications. Of note is that only 25% of total losses were covered by insurance.[2]

Understanding the impact such a momentous event had on Thai households is clearly important. It is necessary to evaluate such impacts carefully not only so that ex-post assistance is well designed and adequate, but so that the cost-benefit calculus of ex-ante prevention and mitigation policies are comprehensive and reflect a correct evaluation of possible scenarios. There is little assessment of the impact of large sudden-onset events on household incomes and expenditures in middle-income countries. The assessment that exists is either focused on households in high-income countries (especially in Japan and the USA for earthquakes and hurricanes, respectively) or the impact of droughts and hurricanes on rural households in low-income countries (in Sub-Saharan Africa and Central America, respectively).[3]

An investigation of the impact of a large adverse shocks on household income and expenditure patterns is crucial as their actions and experiences in the aftermath of the disaster can undermine their ability to fully recover. The risk of this possible 'disaster-poverty trap' is especially acute if households have limited external support and lack access to formal risk coping strategies such as insurance.[4] Ex post coping, for example by cutting health and education expenditures, can adversly impact their long term welfare and lead to persistent poverty.

Using data from the Thai Household Socio-Economic Survey (THSES), this paper analyses the economic impacts of the 2011 floods. In the 2012 THSES, households answered a set of questions on the extent of flooding they experienced in the 12 months prior to the survey. As the same households are followed over time—surveyed in 2005, 2006,2007, 2010 and 2012—

the timing of the survey, the detailed geographical information it includes, and its panel structure allows us to analyse household welfare before and after the flood in unusual detail. We investigate how the floods affected households that experienced direct flooding damage.

As we have information, from satellite imagery, about the extent of the flooding, we can also identify those households that lived in flood affected regions (sub-districts) but did not self-report being flooded themselves. We then can estimate how the flood impacted those who were not flooded but lived in communities that were affected by the flood. We term these households 'the spillovers'. While we are unable to exactly identify the channels of indirect impact that affected these spillover households, the detailed data we have allows us to offer some possible explanations for these identified impacts.

The rich data collected in the survey can help us measure the true indirect impact of the disaster on variables such as income, expenditure, assets, debt and savings levels as well as labour market outcomes. We also analyse flood impacts across different socio-economic groups and livelihoods, characterize the spillover effects, and validate our results with several robustness tests.

Our analysis demonstrates that business income is driving the negative impacts on flooded households. This average negative impact on business income is coupled with a (much smaller) increase in government support. Further, we identify spillover effects on households that were not directly affected by the flood – these spillover effects are almost as large as the loss experienced by directly impacted households. Further analysis, by socio-economic status, shows that the declines in business income is associated with higher-wealth households, while lower-wealth households did experience very significant decline in agricultural income. When spending is examined, we find the flood induced an increase in housing expenditure alongside reductions in spending on luxuries. Aggregate impacts are largely driven by higher-wealth households who typically work in the non-agricultural sector.

Section 2 reviews the relevant literature; it predominantly focuses on disasters that have occurred in developing countries and their effect on household welfare and poverty. Section 3 provides information on the four datasets we use; Section 4 outlines the research methodology; and Section 5 presents the key results. Lastly, section 6 concludes and presents ideas for future research in this area.

2. The Literature

The literature on natural disaster impacts has predominantly focussed on assessing impacts at the aggregate macroeconomic level (Noy, 2009; Kellenburg and Mobarak, 2011; Toya & Skidmore, 2007, Felbermayr and Gröschl, 2014). This involves examining the effect of shocks on macroeconomic variables such as GDP growth using cross country data. Although studies show mixed results in terms of the impact of disasters on GDP, there seems to be a consensus that a country's level of development and their quality of institutions play an important role in the determination of overall economic costs.[5]

However, what about economic costs at the micro-economic level? Taking the macroeconomic view does not provide insight into the possible heterogeneous impacts that may exist within countries and the distiributional impacts. It is important to understand how natural disasters may impact household welfare; focussing on such variables as income, consumption and asset accumulation. Understanding the heterogenous impacts, and their distributional consequences, through studies such as our own, can help guide government policy in mitigating (and preventing) the potentially adverse impacts of disasters on households. This is especially important for middle-income countries, where many ordinary households lack the capacity to adequately respond to shocks, but the government has the resources to adapt policy to that reality.

While the literature on households and disasters is larger in scale and scope in comparison to, for example, micro firm-level studies, it still is fairly limited in its ability to identify and characterize impacts. Anttila-Hughes and Hsiang (2013) analyse the ex-post economic effects of typhoons in the Philippines. They use household panel data alongside variation in physical storm data to identify impacts on household income, consumption, and assets. Their results show that exposure to typhoons reduce household income by 6.6%, where, surprisingly, this effect is consistent across different income groups. Again, atypically, they find that this loss in income "translates nearly one-for one" to a reduction in household expenditure. This implies an absence of consumption smoothing by households, who seemed to predominantly make adjustments to the level of their human capital expenditure. Their data precludes them from differentiating, as we do, between households that experienced direct damages, and those that were indirectly affected as they were living in communities that were damaged.

Similar to the 2011 floods in Thailand, Bangladesh experienced "the flood of the century" in 1998 (Del Ninno et al, 2001, p.15). Analysing the impact on household welfare, Del Ninno et al. find that more than half of those affected by the flood experienced a loss in assets, employment and days worked in the agricultural sector, and many households faced food insecurity and health problems. Their analysis of coping strategies showed that around 60% of households borrowed to maintain their expenditure levels.

Mueller and Quisumbing (2011) build on this work by analysing the long run impact of the flood on household wages. The authors use a household panel dataset expanding five years after the disaster in order to guage both the short term and longer term impacts. Results show that long term impacts are more damaging as households saw wages decline by 4-5% when flood depth deviated from normal conditions. De Alwis and Noy (2016) also analyse the long-term impact of a sudden onset event—in this case the 2004 Tsunami in Sri Lanka—and find that in the decade after the catastrophe, households residing in affected districts experienced a relative increase in their income, but a much smaller increase in their expenditures (and no impact on employment). Franklin and Labonne (2017) focus on labor markets and specifically on the impact of typhoons on individuals' employment in the Philippines. They find that in the formal sector, workers are typically not layed off and the labor adjustment is observed at the intensive margin (i.e. fewer hours and lower wages) rather than at the extensive margin (layoffs). Other recent papers that have examined flood impacts include Yonson (2018) and Parida et al. (2018) who estimate the impacts of floods on health in the Philippines, and on suicides in India, respectively.

Work by Janzen and Carter (2013) combines the literature on post-disaster poverty traps, assets and micro insurance. The authors evaluate the asset dynamics of households who received an insurance pay-out following a drought in Northern Kenya in comparison to those households who did not. Using instrumental variables to account for selection bias, their results show that households that received an insurance payment were 22-36 percentage points less likely to run down their assets. Further, they find a "critical behavioural threshold" (p.2). Households with asset holdings above a certain level are more likely to smooth consumption whereas those below the threshold display asset smoothing behaviour.[6] Consequently, insurance pay-outs have a heterogeneous impact; they help stabilise consumption for less wealthy households and help protect assets for those who are relatively well-off. These results provide insight on the important role insurance can play in preventing households from engaging in costly/welfare-destroying coping strategies. Unfortunately, our data on Thai households do not include any information on insurance take up, but we are able to investigate differential impacts with quantile regressions based on a socio-economic status indicator we construct from durable assets data.

In addition to this, there are a number of papers who look into analysing the impacts of excessive rainfall and drought spells rather than one-off natural disasters (see Assimwe, 2007 and Thomas et al 2010). The latter study shows how droughts can be more welfare damaging for households then floods, especially for those who work in agriculture. Lertamphainont and Sparrow (2016) conduct a very similar analysis using rainfall data matched with a cross-sectional repeated survey of Thai farming households (for the decade before the 2011 floods). They also find droughts to be more damaging than excessive rainfall events, and identify decreased ability to smooth consumption among poorer (landless) households.

As in our paper, Chantarat et al. (2016) examine the Thailand floods. They, in contrast with our retrospective work, focus on how the floods affected households perceptions about the future. In particular, how households shifted their expectations about the occurance of future events, and, using a specially designed survey they conducted, how households shifted risk preferences and general attitude to risk. Earlier Cassar et al. (2017) conducted a set of experiments in Thailand to also idenfity shifts in attitudes to risk associated with the experience of the 2004 tsunami that hit Southern Thailand.

Lastly, a recent paper by Poaponsakorn et al. (2015) is the only systemic study to look at the impact of the 2011 Thailand floods on household welfare. As well as giving an overview of the immediate impact and causes of widespread damage, Poaponsakorn et al. (2015) uses cross sectional household survey data alongside satellite images to determine the impact of the flood on household expenditure and income. Satellite data is used to determine which provinces were flooded and this information is matched with household addresses reported in the survey. Results show a negative impact, with expenditure decreasing by 6.7% for flooded households in comparison to those who were not flooded. Additionally, the stratification of households by income shows that the middle class showed a larger welfare impact in comparison to groups at the tail end. The author does acknowledge the limitations of the use of satellite images to determine impacts and proposes future research with the use of digital elevation maps. Nabangchang et al. (2015) also analyse a specially-designed household survey, this one conducted in the most heavily affected region around Bangkok and targeting specifically affected households. Their aim is to collect subjective information about the disaster's costs to households in the heavily affected region.

Our study compliments as well as advances the work done by Poaponsakorn et al. (2015) through our use of a unique panel data set where individuals self-report being affected by the flood. The self-reporting of shocks provides for a more reliable 'treatment' group as households have a better understanding of whether they have been affected by the flood or not. In contrast, the use of satellite images may only identify 'treated' households impercisely.[7]

Overall, it seems that economic impact of a natural disaster crucially depends on a household's ability to cope with the shock ex-post and their degree of exposure and vulnerability to the shock ex-ante. Practically all of the literature studied above links external meteorological measures with household survey data in order to determine the effect of natural disasters on households using cross sectional data. In this respect, our paper provides a valuable contribution. Instead of relying on external rainfall data, which is both infrequent and imprecise, our analysis makes use of both self-reported shocks and publically-collected data to determine the impact of Thailand's worst natural disaster in recent years. Self-reported shocks are likely to be most relevant considering the floods were a result of heavy rainfall in the mountainous areas, and the most heavily impacted regions did not necessarily experience the highest amounts of rainfall. Precise satellite imagery combined with geo-coded household surveys, were they available, would also have enabled such identification.

3. Data

The data used in this paper comes primarily from the *Thai Household Socio-Economic Survey* (THSES) conducted by the National Statistical Office of Thailand. The survey was carried out over five different years (2005, 2006, 2007, 2010 and 2012). It covers around 6000 households and provides data at both the individual and household level. It tracks the same households across the five waves, providing a dynamic view of household characteristics on economic measurables such as income, expenditure, asset holdings, employment, savings and debt, and other socio-economic indicators such as health conditions. In addition to the THSES, we use data from three other sources to identify the natural hazards: list of flood-affected districts from the Thai government, satellite imagery to identify the duration of flooding, and rainfall data.

3.1. The Shock Module: Identifying Flood Impact

Additionally, the 2010 and 2012 waves included a module on shocks faced by households and the coping strategies used to overcome them. Respondents were asked whether they were affected by particular shocks (including flooding) and were then required to provide details on the extent of damage caused, the loss of income experienced and the types of strategies used during their recovery.[8] In 2012, 1067 households reported they were affected by flooding in comparison to only 122 households in 2010. It is this set of questions that enables us to distinguish between directly affected households and those indirectly affected (the spillovers).

We use these households that reported flooding in the 2012 survey as our 'treatment group' in analysing economic impacts. Our benchmark control group will be all the households that did not report being affected by flooding in the 2012 survey and did not reside in those sub-districts identified as affected by the government (using satellite imagery). The spillover group are those households that did not report being flooded, but did reside in the subdistricts that were.

We use government data that identifies the flooded sub-districts as an ‘alternative’ measure of flood in order to identify spillover effects, as we can locate households to their sub-districts. We identify indirectly flood-impacted households as those households that reside in sub-districts that the government reported as being flooded but did not themselves report being flooded. Above, we noted that 591 households in our survey self-reported being affected by the flood, of these, only 21 did not reside in these sub-districts that were affected according to the Thai government data. There were additional 1303 households that resided in the affected sub-districts but did not report being flooded. This different treatment group allows us to differentiate between directly-impacted households (the 591 households that reported being impacted), and indirectly-impacted households (the 1303 households that reported not being impacted but resided in sub-districts that were impacted).[9] Figure 3 places the locations of these three groups of households on the Thailand map.

Given the panel structure of the data, these groups can then be compared over time to analyse the economic impacts of the flood. Since we would like to use both the cross-section and time series available to us, we restrict the sample on which we conduct statistical analysis to those households which we observe for both the 2010 and 2012 waves. The survey maintains around 6000 observations per wave by adding new households if some households drop from the sample. Fortunately for us, we have about 5100 households that are observed for both 2010 and 2012. An attrition rate of around 15%, while present, is not that severe and is unlikely to bias our results by much.

Since floods are a re-occurring event in Thailand, the self-reported shock in the survey could be picking up flooding that occurred in other parts of the country and were unrelated to the Greater Bangkok mega-flood associated with the monsoon season of 2011. Poapongsakorn (2012) identifies 26 provinces (out of 77) which were most affected by the mega-flood event. We use this information to define our treatment variable so that $Flood_i$=1 if household i

reported being flooded *and* the household resides in one of the 26 affected provinces. This restriction is also applied to our alternative flood measures used to test the robustness of the self-reported shock indicator. After accounting also for some missing data, we end up with full surveys on 591 flood-impacted households and 4500 households that were not impacted by the 2011 floods.

Table 1 provides a few summary statistics for those who reported being affected by the flood in 2012. On average, households impacted by the flood were impacted for 4.2 months. All impacted households reported losses of property, while over sixty percent of affected households reported a loss of income; the average value of property damage and of loss of income were both close to 40,000 THB. Households, however, have been differentially impacted by the shocks, with very large variance in reported losses of both property and income. The reported rise in expenditures, by household, was about half as large – almost 20,000 THB.

While we do not use this information in our statistical analysis, households also reported their use of coping strategies (this information is available in Appendix Table 1). Accumulated savings and regular cash income were the dominant strategies used by households to recover from the disaster. This was followed by informal financial support from relatives and children. Very few households made use of other possible coping mechanisms, such as selling off assets. Reported expenditure reductions were spread equally over certain categories including entertainment, leisure and clothing.[10]

3.2. Other Data from Household Survey

Several adjustments were made to the data available from the survey prior to conducting the statistical analysis. Variables of interest such as income, expenditure, debt and savings were all reported at the individual level while agricultural income and asset holdings were reported at the household unit. This required aggregating data across individual household members. It is possible that this may result in some double counting; which is most concerning with the expenditure data as multiple members may report spending on big items that benefit the whole household. To account for this, we create two expenditure variables: i) expenditure by household head only and ii) expenditure by all adult household members. Both income and expenditure variables are also aggregated by different sub-categories to provide additional insights. All monetary values are adjusted for inflation using CPI data from the Bank of Thailand to allow for comparisons across different years. Further, all variables are transformed into per capita terms and an additional precaution is taken by also creating per capita adult equivalent variables (dividing the data by household members above the age of 15).

The survery does not provide a measure of the stock of wealth owned by households, and it is therefore impossible to directly determine the household's socio-economic status. However, the survey provides information on asset holdings (livestock, housing, land, consumer durables and vehicles). It is impossible to aggregate these in order to get a measure for household wealth as asset values or quantities are not reported.[11] Therefore, we use principal components analysis to create an asset index which we then use in our statistical analysis.[12] For our purposes, the variables used to construct the index include the ownership of consumer durables (TV, fridge, phone, oven etc), the type of fuel used for cooking and the source of drinking water. The latter variables were re-coded into binary indicators. Land and house ownership, housing structure and indicators of access to basic utilities were excluded from the index since they displayed very little variation across households. Additional detail on the creation of the index as well as the weights used is provided in Appendix A. Appendix Table 3 shows summary statistics on household characteristics, including the asset index, broken down by treatment status and survey wave.

3.3. Weather Data

Lastly, while the reliance on self-reported observations on flooding has some advantages, we see other advantages in using alternative measures of flood impact. Self-reported shocks are, after all, potentially endogeneous and subjective (Thomas et al, 2010; Guiteras et al., 2015).[13] Further, households may have implemented certain ex-ante strategies to ensure they are protected from any shock or have the means to cope with it ex-post. This may impact how and whether or not they report being affected. While these are all possible sources of biases, the shock module question we use from the THSES survey asks a binary question of whether the household was affected by the flood or not. This significantly reduces ambiguity and the bias associated with misreporting.

We use data derived from satellite images, from the Geo-informatics and Space Development Agency (GISTDA). These images were taken weekly May-December, 2011, with 50×50 meters resolution. They provide information regarding the location of inundated sub-districts and the flood duration. Poaponsakorn (2012) used the Thai government's definition of 2-week flood duration as the criterion for payment of compensation to define 'large floods', and identifies these affected households using the remote sensing imagery mentioned above. We use that information to identify whether long-lasting floods imposed higher costs on households (both directly and indirectly).[14] [15]

Finally, besides the survey data, the Thai government lists of affected sub-districts, and the data obtained from the satellite imagery, we also use measured rainfall data to control for any economic impact of rainfall that is unrelated to the flooding. Without accounting for rainfall, it is possible that some of our identified flood impact is just a function of the increased rainfall in affected areas. We are more interested in the catastrophic incidence of floods.

3.4. Descriptive Statistics

Table 2 describes the households as they are observed prior to the 2011 floods in 2010, and after the floods in 2012, and compares our treatment and control sub-samples. In columns 3 and 7, it describes the statistics for an alternative set of control households, that only includes households that are within a 10km band from affected sub-districts, but their own sub-districts were not reported as affected by flooding. In column 4, the table also includes information about the attrition in our sample; i.e., those households that were observed in 2010 but were not included in the 2012 sample.[16] The households in all regions are very diverse, with very large standard errors associated with all the measureable differences, so that none of the differences between the treatment and control observations detailed above are statistically signficant. In their demographic and labour force participation characteristics, the treatment and control households appear almost identical (see Appendix Table 3). We do observe that all types of income, expenditures, saving, outstanding debt, and assets are higher for the treatment group in the 2010 sample.

These differences are consistent with our observation that the flood-impacted households were generally wealthier than their counterparts that were not impacted because most of the impacted households reside in Greater Bangkok or the Central regions, and these are the wealthiest regions in the country. This is also evident when we examine the geographical distribution of households according to their socio-economic asset-index classification; see figures in appendix (the asset index was described in section 3.2). A comparision of the 2010 observations with the 2012 sample already fore-warns of many of the systematically identified results we describe below in the estimated regressions.

4. Estimation Methodology

The existence of treatment and control groups observed both before and after the flood provides an ideal setting to conduct a difference-in-difference analysis. In contrast to Poapongsakorn

(2012), who used provincial level aggregate data, we will be using household panel data which provides both a cross-sectional and time series view of key outcome variables. The panel structure has several advantages. First, it helps overcome the problem of some unobserved heterogeneity. Since we have several data points on the same household, we can take account of omitted time-invariant factors that differ across households by controlling for fixed effects. As we detailed in the previous section, this is especially important for the 2011 floods, as they occurred in the wealthiest region in Thailand. Second, the use of panel data allows us to control for long term trends or "dynamic changes" in outcome variables.

We start with a standard difference-in-difference model of the form:

$$y_{it} = \beta_0 + \beta_1 post_t + \beta_2 flood_i + \beta_3 post_t flood_i + \beta_4 X_{it} + u_{it} \qquad (1)$$

where $post_t = 1$ if the observation is from 2012 (post-flood), $flood_i = 1$ if household reports being impacted by the mega-flood of 2011 (note this is not subscripted by *t*). If we were to estimate this model, we would be identifying the impact of flood on flooded households (β_3) and controlling for time specific effect (β_1), flooded household group specific effect (β_2), other time invariant factors (C_i), and other time-varying effects (X_{it}).

Because of the panel nature of this dataset, this model can be modified to control for household specific effect (instead of only for treatment/control group effect) by replacing $\beta_2 flood_i$ with household fixed effects model (δ_i).

$$y_{it} = \beta_0 + \beta_1 post_t + \delta_i + \beta_3 post_t flood_i + \beta_4 X_{it} + u_{it} \qquad (2)$$

The potential sets of X_{it} to be included are deviation of yearly rainfall from normal (available at provincial level), and government reports of other disasters, e.g., dummy of other floods or droughts in each year. This can be extracted from the self-reporting shock module. A fixed effects regression allows us to address additional concerns due to our use of self-reported shocks. It is likely that there are unobservable time invariant factors (such as the degree of risk aversion) which may drive the reporting of shocks by households and their impact. This could generate biased results. Thus, our model assumes that this unobservable effect (δ_i), may be correlated with our variable of interest; $E(\delta_i|Flood_{it}) \neq 0$. However, the exogeneity assumption regarding the error term still holds; $E(u_{it}|Flood_{it}\delta_i) = 0$. We use additional controls in our estimation in order to account for observable time-varying effects that may impact outcomes in the absence of floods. For the benchmark results, we estimate our model for two time periods only where 2010 is the year before the flood and 2012 is the year after.

The 'treatment' effect assumes that both groups are facing the same time trends prior to the floods. As Meyer (1995) states, although we cannot assume that both the control and treatment groups are similar in every respect, we can make the more plausible assumption that any unobserved differences between these groups are constant over time—i.e., they display parallel trends in the dependent variables before the shock. Since these unobserved time-invariant differences are controlled for in our model, we are able to estimate the true impact of the disaster on affected households. The additional variables in the specification we estimate (X_{it}) are inserted in order to control for any additional exogenous differential changes that may also affect the assumed parallel trend. These include the education level of the household head, number of dependents, age of household head, the asset (socio-economic) index, the proportion of adults working in the household in the last 12 months, the gender status of the household head, a dummy variable indicating whether the household owns their own house, the deviation from the norm in annual rainfall and the presence of any other observable natural shocks (such as droughts). The issue of parallel trends is investigated directly in figures 4-5, in additional figures in the online appendix, and indirectly in the falsification tests described below, in section 5.4. It appears that the parallel trends assumption holds quite well for all three groups (directly affected, the spillovers, and the control group).

We estimate equation (2) using a set of outcome/LHS variables (y_{it}): income, income by category, expenditure, expenditure by category, labour market outcomes, savings and debt. For the last three, we observe no statistically measurable impact of the floods. Below, we discuss these last results, but we do not report these regressions (results are available upon request).

We further differentiate between the effect of flooding on these outcomes variables across different groups; focusing in particular on socio-economic status and livelihoods (farm vs. non-farm) as determining the differential impacts. We use robust standard errors clustered at the sub-district level since we hypothesize that households residing within the same sub-district are more likely to experience similar outcomes.

5. Estimation Results

We start by presenting our results for a benchmark treatment (directly flood-impacted) variable: households that self-reported being affected by flooding in the latter half of 2011 and that reside in the 26 provinces that were affected by the 2011 mega-flood. We then discuss how these impacts are different across socio-economic status and livelihood (agriculturalists and non-ag) and spill-over impacts from affected households to unaffected households in regions that were impacted. The rest of the section is devoted to several attempts to further establish the robustness of our results using different measures of treatment and estimation techniques. In all tables, dependent variables are listed in the column headings. All variables are in real terms unless stated otherwise.

5.1. Self-Reported Shock

In our benchmark result, presented in table 3 column 1, we find that households who reported being flooded saw a negative impact on per capita income (estimated to be THB 5694; column

1, row 2). Some of the coefficients in our benchmark specification are not statistically different from zero, but those that are have the expected signs and magnitudes.

Not surprisingly, the number of adults working in the household is positively associated with household income, while the number of dependents is negatively associated with this measure. These (proportion adult and dependents) are consistently estimated in terms of their direction across the different types of income as well. The amount of land owned by the household is positively associated with aggregate income; and reassuringly this association only holds for agricultural income when examining the different types of income sources.

Overall, however, the ability of our model to explain the level of household income is fairly weak. This makes our results for the flood impact (the coefficient for *Post*Flood*) all the more remarkable, given their consistently statistically significant magnitude across many specifications detailed in the next tables. The rest of table 3 includes a breakdown of income to its various components, as they are provided in the survey. The flood appeared to have negative impact, on average, on both agricultural and non-agricultural incomes, but it is only the impact on non-agricultural income (column 2) that it is also statistically significant. When we break non-agricultural income into wage and business incomes, we find that this negative result is driven by the adverse and statistically significant impact of the floods on business income.

Lastly, in our benchmark results in table 3, we also examine the household receipts from government support. We expect government support to increase after a natural disaster of this magnitude. We indeed find that on average government support did indeed increase for flooded households (relative to non-flooded households), but that this increase was very small relative to the amount of lost income these households experienced, on average, as a result of the flood.

We also estimated the impact for household income without accounting for household size (i.e., not in per-capita terms). Results are very similar and are available on request.

Household per capita expenditures are described in table 4. Similarly, we estimate the determnants of total expenditure in column (1), and the breakdown of expenditures to its various components in the other columns of table 4. Overall, flooded households did not change their overall spending levels in any statistically observable manner (the coefficient on *Post*Flood* is small and statisitically insignificant in column 1). Households experienced, on average, decreases in income, and while they probably needed to spend more following the floods, they were also likely to be more credit constrained. We do observe that flooded households experienced an increase in spending in the 'housing' category (which includes spending on housing repair and furniture – in column 2). On the other hand, spending on luxuries decreased in similar measure (column 7). The coefficient estimates of the flood impact on spending on food, health, and education, are all negative, but small (and statistically significant only for education).[17] Beyond our key independent variable of interest, we generally find the spending is higher for households with a higher socio-economic asset index, and higher for household that have more adults working and fewer dependents.

In separate regressions, which we do not report but are available, we examined the impact of the 2011 floods on labour market variables (unemployment periods and the average number of jobs held), changes in the stock of debt, and change in accumulated savings. In all of these, the flood impact coefficient is always small and stasticially insignificant, and the models also have very poor explanatory power. We note that these results are surprising. Given the reported and estimated decreases in income, and the smaller observed decreases in consumption patterns, one would reasonably expect for these behavioural patterns to be financed by reported drawdown in saving or by reporting of accumulating debt. We suspect that the reason we find no statistically significant result is the potential noise in this reporting, but of course do not have any way to distinguish this hypothesis from any alternative explanation.

5.2. Socio-Economic Status and Livelihood

We use the durable asset index we created as a proxy for household wealth[18], to assist us in determining whether we observe heterogeneous impacts of flooding across households with differing socio-economic status. We divide households using their corresponding asset index into quartiles representing poor (Q1), middle income (Q2 & Q3) and rich (Q4) households. Table 5 provides a summary of results for our coefficient of interest (*Post*Flood*), separately for each income group; income and expenditures are in longs (results for the levels are available in the appendix). We report only the cofficient on the flood impact, but all other results are available upon request.

Results show a large and striking decrease in agricultural income for poor households which drives their decrease in total income (on average around 70% of their decline in total income appears to be associated with declines in agricultural income). For richer households (Q3 & Q4), the decline in income is mostly associated with declines in business income; the impact of the floods on agricultural income seems to be very varied; as relatively few rich households even have agricultural income so that the variance of the estimated impact is very large. Intriguingly, and maybe disappointingly, the increase in government support is most pronounced for higher SES households (even in logs) with, on average, increases in support of about THB 500 compared to THB 200 for the poorest quartile (see online appendix). As before, we observe that the magnitude of government support is not even close to being adequate in providing (implicit) insurance for households from the income shock associated with the mega-flood.

The estimation of the flood's impact on expenditure across households with different socio-economic index is also described in table 5. We do not observe a consistent pattern. As reported before, most of the change in spending appears to be associated with increases in spending on housing and decreases in spending on luxuries. The richest households (in assets) tend to increase their spending the most on housing.[19] Possible explanations for this is both that poorer households may lack the liquidity that can allow them to pay immediately for reconstruction, and that the housing for richer households is more expensive to fix.

It is important to note that our results for households with different socio-economic status are somewhat less robust as the distribution of households across the SES index is not identical for the treatment and control groups. Unlike instances of other disasters (most frequently floods or droughts) richer households were more likely to have been impacted by the 2011 floods than poorer ones – the richest quartile includes 40% of the treatment observations (flood impacted households) while only 15% of these are from the poorest quartile.

Next, we estimate our model separately for farm and non-farm households. Farm households are categorised as any household that answered 'yes' to the question 'Does anyone in your household work in agriculture?'[20] Approximately half of survey respondents work in agriculture. However, most agricultural households still reported non-agricultural earnings suggesting that households in this category diversified their income across different sources. Results show that non-farm households had a larger negative impact relative to those who were not affected and relative to flooded agricultural households. Agricultural households also experienced a decrease in business income but the magnitude was much lower. Further, agricultural income increased for this group but this result was not statistically significant. Some of the ambiguity around potentially positive flood impacts on agricultural income found in earlier literature (e.g., Fomby et al., 2013; Loayza et al., 2012) could be due to changes in agricultural product prices following the flood. It is likely that the prices of agricultural products increased following the floods, thus increasing farmers' income.[21] These regression results are available upon request.

5.3. *Spillovers*

Until now, we have estimated the impact of the floods on households that were directly impacted. However, it is likely that the floods also imposed indirect costs on households that did not suffer direct damages from the floods. These households are unlikely to be reporting having experienced the flood, but their incomes may have been affected as the regional economy suffers a slowdown, as supply chain are being distrupted, and as impacted businesses lay off workers. As their community has been affected, the flood had also placed demands on the time and resources of un-damaged households, further hampering their ability to generate income. Furthermore, the floods are also likely to have changed relative prices in the impacted regions, thereby imposing further impacts on households that have not been directly affected.[22]

In order to account for these spillover effects, we estimate a model that allows us to identify both direct effect of the flood on flooded households and separately spillover effects on unaffected households located in the flooded areas. Uniquely for this paper, our data allows us to identify the indirectly-impacted as we observe two different flood measures: household-survey self-reported flood measure $flood_i$ and the sub-district-level flood areas $flood_d$ that is obtained from the government report. A version of this sub-district level flood indicator is the typical strategy for identifying hazard effected households in the rest of the literature; as most household surveys did not include a shock module until recently. Equation (2) thus becomes:

$$y_{it} = \beta_0 + \beta_1 post_t + \delta_i + \beta_3 post_t flood_i + \beta_6 spillover_i + \beta_4 X_{it} + \beta_5 post_t C_i + u_{it} \quad (3)$$

where $spillover_i = flood_d - flood_i > 0$. These results are described in tables 6-7 for income and for expenditures, respectively.

For income, in table 6, we find that the main driver of impact, non-agriculutral income, is still the main driver for the decrease in income we observe. Intriguingly, we find that while our results for the direct impacts carry through, with an estimated average decrease in income of THB 6944 for directly impacted households, households that were indirectly impacted suffer an almost equivalent decrease in non-agricultural income of THB 6253. This result seems to be caused, as before, by a reduction in business income. Directly impacted households experience an average decrease in business income of THB 6465, while the spillovers cause other neighboring households to experience a decline in business income of THB 4018.

In our view, this is an important result. We show that accounting for the direct impacts of disasters on affected households is not a sufficient measure of the total cost of a disaster. Neighboring but directly-unaffected households also experience a decrease in incomes. This decrease in income can potentially be, as in this case, almost as large as the adverse impact on the households that were directly adversely affected by the flood. Thus, the main impact of the flood on wellbeing appears to be not directly tied to actual physical damage to individual households, but the disruption it entails to economic relationships within the affected community. This suggests that our traditional measures of disaster costs may be underestimating the true economic costs of disasters; and that this underestimation may be quite substantial.

Our claim that the costs may be relatively invisible is also reflected in our finding that government support does not increase for indirectly-impacted households; in fact the coefficient on the flood impact on government support for indirectly-impacted households is negative (though stastistically insignificant). It is not only our estimates that may be ignoring these indirect spillover impacts, but government policy ignores them as well.

In table 7, we investiage the spillover impacts on households' expenditure patterns. Here we find a decrease in expenditures that is largely driven by decreases in spending on luxuries (the decrease in spending on luxuries for the directly affected households is about twice as large as for those not directly affected). Spillover households did not need to increase their spending on housing (as their houses were not directly damaged). We therefore do not observe an increase in spending on housing for this group, and consequently the overall impact of the floods on total spending is negative and statistically significant for this spillover group.

As we already described, due to the widespread damage caused by the flood, households who did not report being flooded were still *indirectly* affected through a slowdown in overall economic activity, employee lay-offs, production stoppages etc. To further establish the robustness of this claim, we can test for the existence of spillover effects by modifying our model for a different control group. This modified control group excludes all households who did not report being affected but live in the 26 flood-affected provinces. For this new control group we would expect minimal spillover effects, as these households are located far away from any flooded areas. As we project, the negative impact on income with this new control group (one that is not contaminated by indirectly-affected households) is larger relative to our original control group which included non-flooded households located in flooded provinces (these results are available upon request).

It could be argued that some of the flood impact is being mediated through its affected on some of the control variables (X_{it}). To examine whether our results are sensitive to the inclusion of these controls, we re-estimated tables 6-7 but without the all the control variables. Results are available in the online appendix; and are both qualitatively and quantitatively very similar to the results we report here (in tables 6-7).

Another important insight provided in tables 6-7 is obtained by comparing them to the estimates previously reported in tables 3-4. For example, one can compare the results of table 3 (the 'standard' specification identifying the direct impact), with table 6 (the 'standard' specification but also identifying the spillover impacts), and the online appendix (the 'standard' specification but excluding the spillover households from the control group). We find that the estimated coefficients for the directly impacted households are larger for the latter two cases, than they are for the 'standard' specification (reported in table 3). This suggests that the 'standard' specification under-estimates the disaster's impact on households.

As we previously pointed out, some of the households surveyed in the 2010 wave were not re-surveyed in 2012. As we described earlier (and in results available in the online appendix), these households appear poorer than the average household in the sample. If the failure to re-survey these households is somehow associated with the flood impact—maybe because they had to migrate because of the flood—our estimates might understate the flood's adverse impacts. We provide one attempt to examine this issue by re-estimating our benchmark regressions (as in tables 6-7) but now include these 'attrition' households from the 2010 survey (about 1,100 observations). Indeed, when we compare these results (appendix table 10, columns 1-4) with the results for the standard panel sample we previously used – but inevitably now estimated without the household fixed-effects (appendix table 10, columns 5-8). Without the household fixed effects, the impact of the flood is not as precisely estimated, and there is some weak evidence of larger adverse impact on income when these 'attrition' households are included.

In a further examination of the robustness of our results, we estimated several specifications using different definition of the control group. If households that were very far away from the flooded areas are very different, they may not be the appropriate control group for the 'treated' (directly and indirectly) households. We re-define the control groups as those households that are 'close enough' to the affected districts, but were not residing in the affected districts themselves (this is the same idea behind a regression discontinuity framework). We identified those households that were within a 5km band around affected districts, a 10km band, and a 50km band. In the appendix, we both provide a map to further display this selection procedure, and the regression results. Overall, the results are qualitatively identical, but magnitudes of the estimated flood impact on income varies with the control group used in the estimations. In most cases, this more limited control group leads to larger estimated coefficient identifying the flood adverse impact on incomes.

5.4. Unobserved or Spurious Time-Varying Effects: Falsification Tests

Although our fixed effects model controlled for any unobservable time invariant factors, there could still exist observed/unobserved time varying factors which we have been unable to account for in our regression and are systematically different between treatment and control. Both treatment and control households could be affected differently by other systematic shocks between 2010 and 2012 which could be driving the negative impact on income and expenditure. After all, the directly impacted households were not randomly chosen out of the total population of Thailand, and are more concentrated in some regions and in some income, expenditure levels (see figures 3-5). We can test for any difference between the treatment and control households by examining 'placebo' floods in previous years, and use these as a falsification test for our results.

The results we presented up to now are based on using 2010 as pre-flood year; and only using the 2010 and 2012 survey waves in our estimations. We now estimate a different model using all the years of survey waves available. In other word, we estimate

$$y_{it} = \beta_0 + \sum \beta_{1y} year_y + \delta_i + \sum \beta_{3y} year_y flood_i + \beta_4 X_{it} + \sum \beta_{5y} year_y C_i + u_{it} \quad (4)$$

where $year_t$ are now a set of year dummies ($y_{2012} = 1$ if year=2012, $y_{2010} = 1$ if year=2010…using the first year as reference). The second term controls for year-specific fixed effect and the sixth term control for time invariant effects. The coefficients of interest are in the fourth term; we estimate the flood impact variable specifically for each year β_{3y}. If our controls are appropriate, we expect β_{3y} to be insignificantly different from zero in all the years prior to 2012 (as in those years the treated households were in reality not exposed to any exceptional flooding or other unique shocks to this group). We expect to find significant coefficients only in the year of mega-flood- i.e., 2012. We plot the coefficient for β_{3y} and 95% confident interval by year by outcome variables (see figure 6). One can view this as a falsification test – i.e., a strongly significant result for these false (or placebo) floods will invalidate our findings about the difference between the 2010 and 2012 treated household observations.

We find these falsification/placebo results generally confirm both our model choice and the finding that the adverse impact is concentrated in business income, that government support and spending on housing increase, and that spending on luxury goods decreased. In this case, we also present some weaker results identifying a decrease in spending on education.[23] The negative coefficient on education was present also earlier, but was not statistically signficant; we thus hesitate to draw any firm conclusions from this potentially adverse finding.[24]

5.5. The Intensity of the Flood

As we noted earlier, we also obtained a measure of the intensity of the flood experienced by households from remote sensing data. In particular, the sub-district satellite data that we use distinguishes between: (1) no flood areas, (2) flood less than 2 weeks and (3) flood more than 2 weeks.[25] This data enables us to distinguish, albeit crudely, between heavily flooded areas and less intense flooding. A better measure would also account for the depth of flooding (as a proxy for how much of the property was submerged, and as damage is also a function of flood depth), but this data is not available and satellite imagery cannot provide it. We note that only 58 out of 467 households, in this smaller sample for which we have the satellite data, appeared to have experienced a flood of less than two weeks, and unfortunately, we do not have a more detailed measure of durations longer than two weeks. In any case, we re-estimate our benchmark model, but instead of using a *flood* binary measure, we use separate binary measures for *big-flood* and *small-flood.*

We present the estimation results for flood intensity on income and on expenditures in tables in the online appendix. Maybe not surprisingly, but reassuringly, bigger floods appear to impose higher costs on households (with coefficeints almost twice as large for most income definitions). In particular, those households that experienced the bigger floods (more than two weeks of flooding) experienced decreases in non-agricultural income that was much higher than for floods of shorter-duration. This is evident especially for wage income. Business income, however, showed very similar adverse declines for both sets of flood intensities.

For expenditures, the results are somewhat less precise. We observe no difference between the increase in spending on housing between the long- and short-duration flooded households, nor between the decrease in spending on luxuries; and in both cases, the splitting of our treatment observations also meant that some of the statistical significance of our earlier results is no longer present. As before, we observe that the declines in total expenditure are much smaller, on average, than the declines in income, providing evidence of consumption smoothing.

6. Concluding Remarks

This paper uses self-reported flood impact from the Thai Household Socio-Economic Survey to analyse the economic impact of the 2011 floods on households. The analysis shows that business income is driving the negative impacts on flooded households relative to the control group. This average negative impact on business income is coupled with a (much smaller) increase in government support. Further, we are able to identify the spillover effects on households that were not directly affected by the flood. The spillover effects are almost as large as the loss experienced by directly impacted households. These spillover effects appear to be driven mostly by declines in business income, but also by declines in wage income. Further analysis, by socio-economic status, shows that the declines in business income is mostly associated with higher-wealth households, while lower-wealth households did experience significant decline in agricultural income.

We do not have direct information on the kinds of business sector income that were responsible for the observed decline, and also why we observe much lower impact on agricultural income. We suspect that this distinction has to do with the different resiliencies in agricultural households versus more urban (or semi-urban) households that rely on business income. The floods started toward the end of the monsoon season, and while their magnitude was unprecedented, floods in this season in agricultural areas are common. What was more uncommon is that this time many non-agricultural areas were flooded as well, and these had much more limited capacity to adapt to the rising water. For example, in some rural areas, agricultural households next to the river have houses that can float up with the level of the water; semi-urban households that are further away from the river, do not have this capacity to adapt to the rising water, and therefore experience more difficulties in continuing with their income-generating activities.

When spending is examined, we find the flood induced an increase in housing expenditure alongside reductions in spending on luxuries. Aggregate impacts are largely driven by higher-wealth households who typically work in the non-agricultural sector. These affected households spend significantly more on housing (relative to both the control and lower-wealth households).

The above results were found to be consistent against a series of robustness checks described in section 5. There are, however, some limitations to our analysis. Our results do not provide any insight into savings and debt levels following the floods, even though questions about saving and debt are included in the household surveys we use. Assuming households engaged in consumption smoothing we would expect a decline in savings or alternatively an increase in the amount of debt households take on. This is especially true given the excessive smoothness of the asset index and the (statistically) insignificant movement in ownership measures of other assets, such as livestock and land. We cannot identify any such impact in the data with our estimation strategy. Given that flooded households explicitly reported using savings as a 'coping strategy,' future research may want to closely analyse saving dynamics; our own analysis has not been able to quantify this channel.

We also note that households were followed at their physical location (their address). The followup in 2012 therefore did not include households that were forced to emigrate to another region as a result of the damage they experienced. We analyse this attrition in the sample, using several approaches detailed in section 5 and the online appendix, and find that it is associated with, on average, lower-income households. If this is indeed an important oversight of our research, we should interpret our findings as an under-estimate of the true impact. It is likely that the most heavily impacted households were the ones that were forced to move, and thus our failure to identify and observe them may bias our findings downward.

Missing from our analysis is a detailed identification of the channels through which the flood made such an impact on the income of affected households. We hypothesise that given the details in our finding, the most likely channel was that the flood affected small and medium sized firms (SMEs), and thus the business income of many affected households. There is plenty of anectodal evidence that SMEs find it more difficult to obtain credit for reconstruction, and maintain operations, when hit by an adverse shock. This shock can be direct damage associated with floods, or it can also be decreases in firms' revenue as spillover from directly damaged firms and households translates into lower demand for the SMEs' products.

There is very little literature that examines in detail the impacts of disasters on firms' operations in developing countries. Hallward-Driemeier and Rijkers (2013) focussed their gaze on Indonesian firms during the aftermath of the 2004 Indian Ocean tsunami, and conclude that firm exits increased. Vu and Noy (2018) documented how firms in Vietnam faced reduced sales in the aftermath of disasters, and how only some were able to ameliorate for that with increasing investment. Chongvilaivan (2012) and Haraguchi and Lall (2015) provide descriptive investigations of the impact of the Bangkok floods on international supply chains. The latter project describes how of the 800 firms they investigated—all of them flooded in October 2011—about a quarter were still not operational in mid 2012.

Understanding firm behaviour can shed some light on the employment opportunities available after a disaster, an issue that is of importance in this case given the many industrial estates that were damaged. The impact of catastrophic events of this magnitude are very complex, and a lot of it depends on the details of what was damaged, and how quickly it is fixed. In this case, it appears that the affected areas were closely connected to a network of production centres and facilities that provided employment and business opportunities. That meant that the disaster affected many households that were not directly impacted by the floods.

Research on this topic of supply chains, network effects, and post-disater firm behaviour is far from conclusive; with, for example, conflicting results identified by Todo et al. (2015) and Carvalho et al. (2016) for the same event – the catastrophic East Japan Earthquake of 2011. We would have also liked to have more data on the different components of business income (mostly the size and sector of the business). This kind of information would have been useful both to explain the exact channels through which the floods affected economic activity, and also provide very important information for policymakers when preparing for future flood events.

Insurance is another issue that we could not address in this paper given the limitations of our data. It is clear that most households were not insured for flood damage, but Haraguchi and Lall (2015) note that apparently 20% of damages were insured. This insurance is most likely associated with the largest industrial facilities and firms in the flooded region, but the impact of this insurance on the recovery trajectory of affected firms (and consequently employees) is largely un-known.[26]

Data on household insurance take up would have helped in determining differential impacts of the flood for households who were formally protected against financial risk in comparison to those who were not. These results can inform government policy by providing insight on the role of insurance in cushioning the effects of the disaster. With the growth of microinsurance and other financial risk-transfer tools in Thailand and in other middle-income countries, this type of impact analysis will be both important and relevant (Kusuma et al., 2018).

The question of external validity - how relevant are our findings for other disaster events? – is clearly one that should also be asked. Of course any event is unique, but there are several characteristics of this event that we think make it relevant elsewhere. Most predictions of the future intensity and frequency of disaser events are fairly consistent in arguing that floods (and droughts) will increase in intensity as a result of climate change (no such consensus exists for other types of natural hazards). Many countries have a similar geographical distribution in which a central area (the most developed, industrialized and richer region) is also part of a major river delta and highly vulnerable to flooding. Examples include many of Thailand's neighbours (e.g., Vietnam and Cambodia). Furthermore, the predictibilty of the monsoon rains (even if their intensity was exceptional in 2011), suggests that mitigation levels are still far from sufficient in rapidly developing and urbanizing countries like Thailand and the other countries of South East Asia. If anything, we believe that our results raise a warning flag regarding the disaster preparedness of many countries and their ability to reduce, mitigate, or adapt to future disaster risk.

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Table 1: Summary Statistics from Flood Survey Questions

Loss/Damage (THB)	Mean	Std. Dev	Obs.
Months affected	4.2	2.88	1067
Value of property damage	37,988	229,168	1065
Value of loss in income	39,093	92,867	647
Value of expenditure rise	19,225	57,586	450

Table 2: Summary Statistics of Key Variables at Household level

VARIABLES	Group A Flooded HH reporting in 2010	Group B Control HH reporting in 2010	Group B' Control HH reporting in 2010	Group C Observed only in 2010 and not 2012 (Attrition)	Group A Flooded HH reporting in 2012	Group B Control HH reporting in 2012	Group B' Control HH reporting in 2012
Total Income							
Mean	63,956	60,442	63,678	35,518	68,747	75,417	66,884
Standard deviation	140,192	134,272	80,252	89,953	135,927	188,217	108,563
Observations	591	4500	1040	1174	591	4500	1040
Business Income							
Mean	10,255	4,774	5,825	4,388	7,280	7,069	7,875
Standard deviation	53,269	22,105	16,585	20,361	21,447	29,763	23,171
Observations	591	4500	1040	1174	591	4500	1040
Total Expenditure							
Mean	18,220	13,393	15,292	13,043	17,782	13,488	15,944
Standard deviation	10,139	13,348	13,469	14,789	15,656	13,899	15,933
Observations	591	4500	1040	1174	591	4500	1040
Average Savings per Month							
Mean	5,242	3,618	4,445	3,043	5,754	3,303	4,097
Standard deviation	10,139	10,240	13,435	9,685	20,146	6,809	8,191
Observations	591	4500	1040	1174	591	4500	1040
Outstanding Debt							
Mean	218,384	192,879	211,155	168,085	213,985	178,283	193,041
Standard deviation	662,263	589,984	600,630	543,601	599,527	505,210	496,907
Observations	591	4500	1040	1174	591	4500	1040
Value of Livestock							
Mean	117,141	51,488	40,466	86,998	150,275	66,557	69,922
Standard deviation	377,691	134,699	109,936	259,839	423,337	160,679	151,651
Observations	53	922	196	59	40	611	117
Total Government Support							
Mean	517	570	503	339	1,199	791	902
Standard deviation	832	1,256	1,040	927	1,753	4,063	6,491
Observations	591	4500	1040	1174	591	4500	1040
Total Expenditure by Head							
Mean	8,765	6,962	7,788	7,872	9,142	6,858	8,038
Standard deviation	10,050	9,729	9,862	11,848	11,936	9,709	10,648
Observations	590	4495	1040	1129	584	4443	1024
Total Income per Capita							
Mean	18,136	17,711	16,116	13,257	18,282	23,355	19,668
Standard deviation	33,431	37,056	25,432	29,250	30,233	69,632	32,762
Observations	591	4500	1040	1174	591	4,500	1040

All variables are adjusted for inflation and measured in units of Thai Baht (THB). Group A includes those households that reported being flooded in the 2011 flood. and Group C includes households that were observed in 2010 but not in 2012. The Control Group B' was constructed only from the 10km band around the affected sub-districts.

Table 3: Income Per Capita

	(1) Total Income	(2) Non-Ag Income	(3) Ag Income	(4) Wage Income	(5) Business Income	(6) Gov't Support
Post	5015.0***	2000.9***	9783.0***	700.1**	1051.6**	130.1**
	(1502.5)	(609.4)	(3715.1)	(304.4)	(525.7)	(59.75)
Post*Flood	-5694.7***	-2146.7**	-4366.0	137.7	-2006.1***	113.6**
	(2092.5)	(869.0)	(5687.5)	(597.5)	(616.5)	(45.04)
Durable Asset Index	-726.7	718.3***	-4732.3	201.7*	405.2**	-5.206
	(1041.6)	(212.3)	(3017.5)	(104.4)	(189.5)	(12.60)
Proportion Working	11926.0***	3629.6***	11278.8**	2955.2***	1455.6**	78.98
	(2002.2)	(761.9)	(5371.3)	(357.0)	(622.4)	(111.5)
Dependents	-3872.9***	-1066.7***	-4731.0***	-288.3**	-369.9**	-19.64
	(950.7)	(299.4)	(1795.5)	(137.3)	(179.8)	(16.64)
Land Owned	416.0*	-27.50**	362.8	-8.887	-10.57	-1.671
	(244.1)	(12.83)	(283.1)	(6.670)	(9.761)	(1.385)
Livestock Value	0.0092	-0.0285	0.0090	-0.0005	-0.0271	-0.0001
	(0.0605)	(0.0218)	(0.058)	(0.0014)	(0.0221)	(0.0020)
Rainfall Deviation	4.166	-0.576	5.895	-0.0430	-0.632	-0.0667
	(3.007)	(0.855)	(5.320)	(0.516)	(0.578)	(0.0821)
Other Floods	-188.6	962.3	-5541.0	903.8	-176.0	108.3
	(2881.5)	(1292.4)	(6246.2)	(1219.9)	(362.1)	(112.4)
Drought Spells	829.8	-1425.4	6263.0	-861.3	-610.2	-6.023
	(2373.9)	(1409.7)	(3824.2)	(1334.9)	(447.3)	(41.15)
Pest Infestations	-1619.1	-520.5	1120.2	-602.7	-246.3	83.06
	(2730.8)	(1431.6)	(4414.3)	(1357.8)	(451.0)	(60.09)
Constant	11952.2**	1729.0	34568.3**	141.3	-68.92	99.55
	(4802.9)	(1580.4)	(13995.9)	(1321.1)	(784.8)	(222.6)
Observations	7,334	7,334	3,140	7,334	7,334	7,334
Within R-squared	0.017	0.014	0.021	0.004	0.023	0.008
Between R-squared	0.157	0.036	0.060	0.031	0.002	0.000
Number of id	3,667	3,667	1,570	3,667	3,667	3,667

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The estimated sample includes only observations of the treated and control households (excluding the spillovers).

Table 4: Expenditure Per Capita

	(1) Total	(2) Housing	(3) Food	(4) Health	(5) Education	(6) Other	(7) Luxury
Post	268.6**	55.03	95.76**	-2.688	44.85	-7.475	-80.70
	(131.2)	(47.29)	(43.04)	(11.25)	(35.38)	(33.47)	(83.90)
Post*Flood	-152.4	254.0***	-63.84	-23.79	-65.76*	42.58	-333.4***
	(257.5)	(90.33)	(92.67)	(17.76)	(35.42)	(61.13)	(123.2)
Durable Asset Index	352.6***	60.85**	50.50**	28.60***	15.22	17.05	193.2***
	(84.37)	(28.58)	(23.42)	(8.284)	(13.42)	(18.09)	(62.56)
Proportion Working	1454.9***	226.6***	404.0***	-9.255	-86.57***	160.9***	777.1***
	(207.4)	(62.57)	(64.00)	(28.14)	(32.12)	(52.34)	(139.4)
Dependents	-699.8***	-68.54**	-194.3***	-5.567	-6.351	-74.59***	-376.4***
	(81.14)	(33.65)	(31.40)	(7.906)	(12.28)	(20.65)	(49.54)
Land Owned	-1.180	-0.00442	-2.807	2.374**	-0.0182	-3.806	3.083
	(10.49)	(1.514)	(2.081)	(1.042)	(0.880)	(3.497)	(8.029)
Livestock Value	0.0014	-0.002	-0.000	-0.0000	-0.0001	0.0000	0.0033*
	(0.0013)	(0.0022)	(0.0001)	(0.0000)	(0.0002)	(0.0002)	(0.0017)
Rainfall Deviation	0.0654	-0.0845	-0.0239	-0.0234	-0.0146	-0.0186	0.250
	(0.266)	(0.0687)	(0.0620)	(0.0241)	(0.0270)	(0.0470)	(0.213)
Flood Other	-490.1**	-244.6***	-190.0***	-20.07	32.22	-61.25	11.31
	(191.0)	(79.42)	(63.38)	(36.99)	(22.90)	(49.85)	(113.4)
Drought Spells	271.5	80.83	-18.66	4.916	74.37***	-30.50	102.5
	(182.8)	(60.93)	(58.49)	(12.49)	(24.29)	(47.76)	(94.26)
Pest Infestations	198.8	45.35	-38.53	-17.32	44.15*	-62.75	183.5
	(173.5)	(74.75)	(49.90)	(20.64)	(22.63)	(46.95)	(123.0)
Constant	2459.8***	323.4**	1432.6***	-5.780	105.8*	236.7***	461.9
	(408.8)	(145.1)	(133.5)	(49.95)	(61.63)	(80.16)	(290.9)
Observations	7,334	7,334	7,334	7,334	7,334	7,334	7,334
Within R-squared	0.021	0.017	0.023	0.009	0.003	0.005	0.014
Between R-squared	0.212	0.040	0.173	0.030	0.010	0.020	0.114
Number of id	3,667	3,667	3,667	3,667	3,667	3,667	3,667

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The estimated sample includes only observations of the treated and control households (excluding the spillovers).

Table 5: Breakdown by Socio-Economic Status

Income (log value)

	Total Income	**Non-Agri Income**	**Agricultural Income**	**Wage & Salar Income**	**Business Income**	**Govt Support**
Q1	-0.163	-0.132	-0.820**	-0.0394	0.0941	0.289**
	(0.138)	(0.146)	(0.387)	(0.123)	(0.244)	(0.140)
Q2	-0.117	-0.176	0.00733	0.201	0.0205	0.484**
	(0.123)	(0.128)	(0.255)	(0.186)	(0.388)	(0.215)
Q3	-0.115	0.0946	-0.211	0.00690	-0.533	0.644***
	(0.0900)	(0.150)	(0.136)	(0.103)	(0.352)	(0.221)
Q4	-0.0542	-0.0166	-0.516*	0.0716	-0.179	0.0805
	(0.0632)	(0.0603)	(0.290)	(0.0637)	(0.147)	(0.129)

Expenditure (log value)

	Total	**Housing**	**Food**	**Health**	**Education**	**Luxuries**
Q1	0.114	0.362*	0.0795	0.515	-0.178	0.0936
	(0.0852)	(0.187)	(0.137)	(0.320)	(0.422)	(0.120)
Q2	-0.147	0.319**	-0.0664	0.0468	-0.822	-0.217*
	(0.0957)	(0.159)	(0.123)	(0.331)	(0.565)	(0.128)
Q3	-0.117	-0.0589	-0.188**	0.452	0.467	-0.201*
	(0.0765)	(0.184)	(0.0942)	(0.298)	(0.395)	(0.118)
Q4	0.0566	0.398***	-0.00264	0.159	-0.0174	-0.0387
	(0.0517)	(0.101)	(0.0685)	(0.256)	(0.196)	(0.0763)

Breakdown is using durable asset index where Q1 represents a household with the lowest socio-economic status and Q4 the highest. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Income Spillovers

	(1) Total Income	(2) Non-Agri Income	(3) Agri Income	(4) Wage Income	(5) Business Income	(6) Govt Support
Post	13133.5**	5941.0***	7192.6	2314.6***	3390.1***	405.9**
	(5114.8)	(1355.6)	(4955.3)	(738.0)	(1088.2)	(203.1)
Post*Flood	-11093.4	-6943.8***	-4149.6	296.6	-6465.0***	360.9**
	(8629.3)	(2343.5)	(8293.0)	(1464.4)	(1706.3)	(157.4)
Post*Spillover	-6554.6	-6252.9***	-301.7	-1499.9*	-4018.2***	-125.9
	(6067.6)	(1511.9)	(5831.8)	(879.5)	(1221.2)	(125.7)
Durable Asset Index	5809.9*	3414.6***	2395.3	1153.7***	1574.1***	-23.48
	(3243.3)	(684.1)	(3166.4)	(311.9)	(606.6)	(48.99)
Proportion Working	19731.5***	6993.3***	12738.2**	6740.1***	2774.7*	-20.77
	(6382.6)	(1744.9)	(6069.4)	(767.6)	(1420.2)	(245.8)
Dependents	-126.7	1149.9	-1276.6	832.1**	236.2	101.6*
	(2900.4)	(853.9)	(2797.2)	(397.5)	(644.7)	(58.86)
Land Owned	1871.3**	18.03	1853.2**	46.93	-24.79	-1.910
	(933.1)	(80.20)	(873.7)	(72.51)	(26.00)	(3.833)
Livestock Value	0.149	-0.0611	0.210	0.0003	-0.0634	-0.0004
	(0.229)	(0.0544)	(0.213)	(0.0022)	(0.0545)	(0.0006)
Rainfall Deviation	12.00*	-2.285	14.29**	-0.695	-1.481	-0.314
	(6.479)	(1.943)	(6.247)	(1.269)	(1.277)	(0.279)
Flood Other	372.5	1025.8	-653.3	426.2	-391.4	245.3
	(5622.4)	(2121.9)	(5296.3)	(1660.3)	(1172.0)	(228.6)
Drought Spells	2622.1	-2595.6	5217.6	-59.91	-2279.5*	-30.54
	(7724.6)	(1705.0)	(7490.0)	(1064.9)	(1271.1)	(90.28)
Pest Infestations	-866.8	-1160.5	293.7	122.6	-1292.3	53.96
	(5234.4)	(1544.3)	(4893.2)	(998.9)	(1130.1)	(113.3)
Constant	5652.5	2641.5	3011.0	1151.3	-1127.5	484.1
	(13097.5)	(3669.0)	(12534.2)	(2074.0)	(2879.5)	(439.5)
Observations	9,898	9,898	4,468	9,898	9,898	9,898
Within R-squared	0.0225	0.0283	0.0212	0.0122	0.0251	0.00803
Between R-squared	0.284	0.152	0.262	0.0912	0.00926	0.00186
Number of id	4,949	4,949	2,234	4,949	4,949	4,949

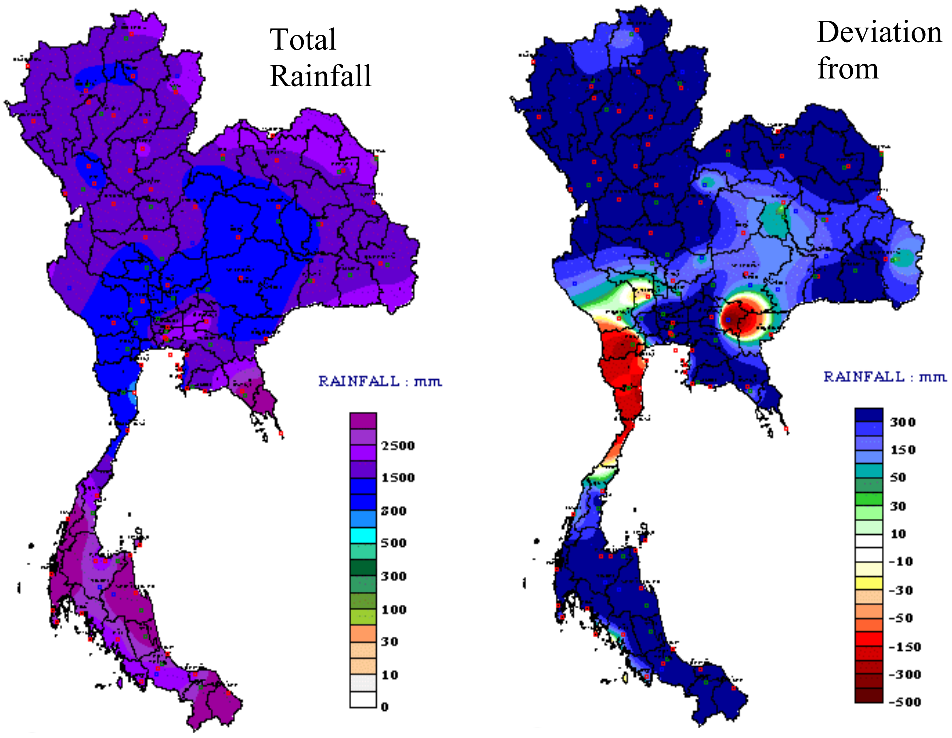
Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Includes the whole balanced sample for which income data was available.

Table 7: Expenditure Spillovers

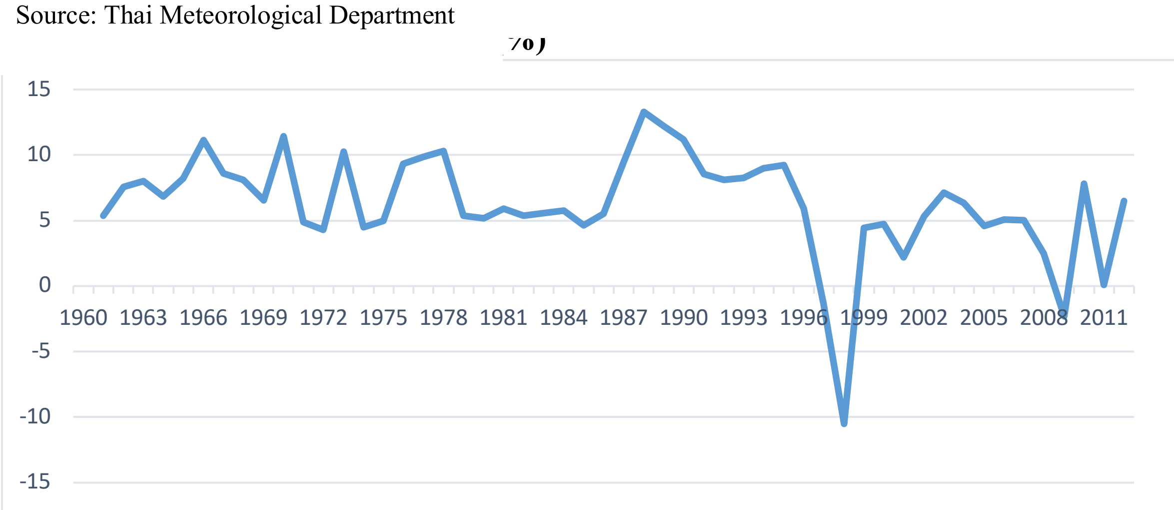
	(1) Total	(2) Housing	(3) Food	(4) Health	(5) Education	(6) Other	(7) Luxury
Post	480.0	91.13	201.8	-15.64	118.5	52.21	-498.6**
	(403.3)	(144.2)	(129.2)	(33.89)	(104.0)	(82.89)	(242.2)
Post*Flood	-1285.7	581.0*	-348.0	-57.68	-234.5	-25.62	-1378.3***
	(1027.6)	(324.2)	(362.8)	(58.38)	(158.5)	(155.2)	(435.1)
Post*Spillover	-1271.2***	-47.62	-208.8	-101.0	-76.64	-275.1***	-700.0**
	(477.3)	(145.7)	(208.6)	(63.92)	(101.1)	(97.72)	(316.6)
Durable Asset Index	2274.5***	315.2***	618.7***	51.54**	65.47	108.5**	1165.5***
	(237.9)	(74.91)	(78.67)	(22.03)	(44.04)	(48.94)	(157.3)
Proportion Working	2391.5***	294.5**	888.0***	-108.1*	-350.6***	197.6*	1471.2***
	(492.3)	(135.8)	(207.4)	(64.30)	(88.73)	(112.5)	(320.3)
Dependents	140.5	46.67	337.5***	43.18*	86.91*	9.202	-391.6**
	(262.6)	(93.36)	(128.0)	(23.05)	(50.00)	(54.80)	(154.2)
Land Owned	19.31	0.726	-0.459	5.173**	0.152	-9.279	23.80
	(29.49)	(4.417)	(6.866)	(2.411)	(2.546)	(8.314)	(20.67)
Livestock Value	0.0015	-0.0032	0.0000	-0.0013	-0.0002	0.0007	0.0055
	(0.0033)	(0.0032)	(0.0006)	(0.0009)	(0.0006)	(0.0005)	(0.0037)
Rainfall Deviation	-0.228	-0.281	-0.0285	-0.0441	-0.0054	-0.273**	0.473
	(0.612)	(0.228)	(0.194)	(0.0517)	(0.0953)	(0.115)	(0.389)
Flood Other	-1334.8***	-611.1***	-408.5**	3.394	42.94	-189.2	-120.4
	(503.4)	(221.1)	(169.2)	(51.90)	(101.3)	(121.1)	(264.6)
Drought Spells	899.9	200.8	42.41	54.25	174.1*	-5.754	273.7
	(571.0)	(180.1)	(206.2)	(59.54)	(92.20)	(101.5)	(258.4)
Pest Infestations	-160.5	-23.82	-119.4	-53.68	73.90	-314.3***	56.62
	(412.1)	(155.0)	(165.5)	(50.11)	(68.61)	(95.30)	(272.7)
Constant	3903.0***	618.4*	2438.0***	121.8	420.4*	676.2***	-83.89
	(1139.3)	(373.9)	(438.4)	(117.6)	(228.8)	(224.0)	(732.0)
Observations	9,898	9,898	9,898	9,898	9,898	9,898	9,898
Within R-squared	0.040	0.013	0.026	0.010	0.003	0.008	0.040
Between R-squared	0.435	0.104	0.355	0.026	0.055	0.060	0.435
Number of id	4,949	4,949	4,949	4,949	4,949	4,949	4,949

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Includes the whole balanced sample.

Figure 1: Annual Rainfall in Thailand 2011

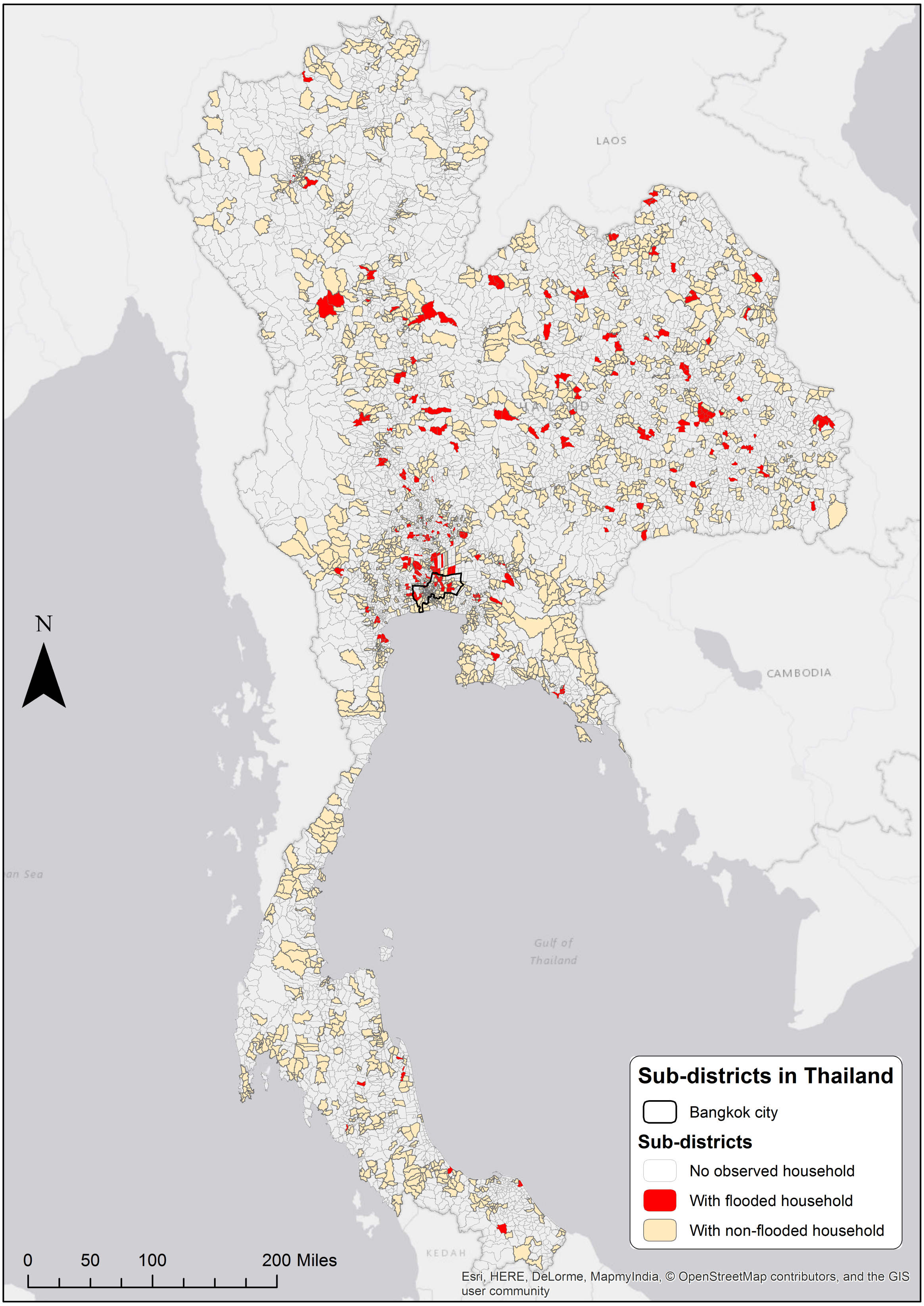


Source: Thai Meteorological Department



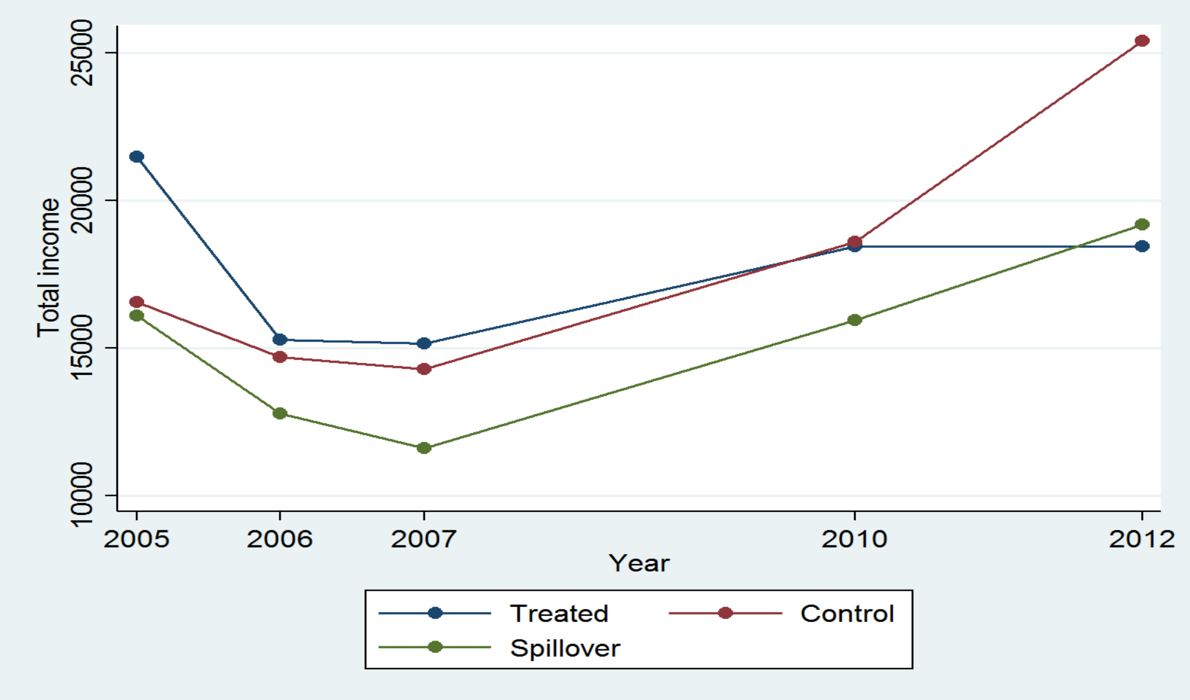
Source: World Bank

Figure 3: Flooded sub-districts in Thailand map



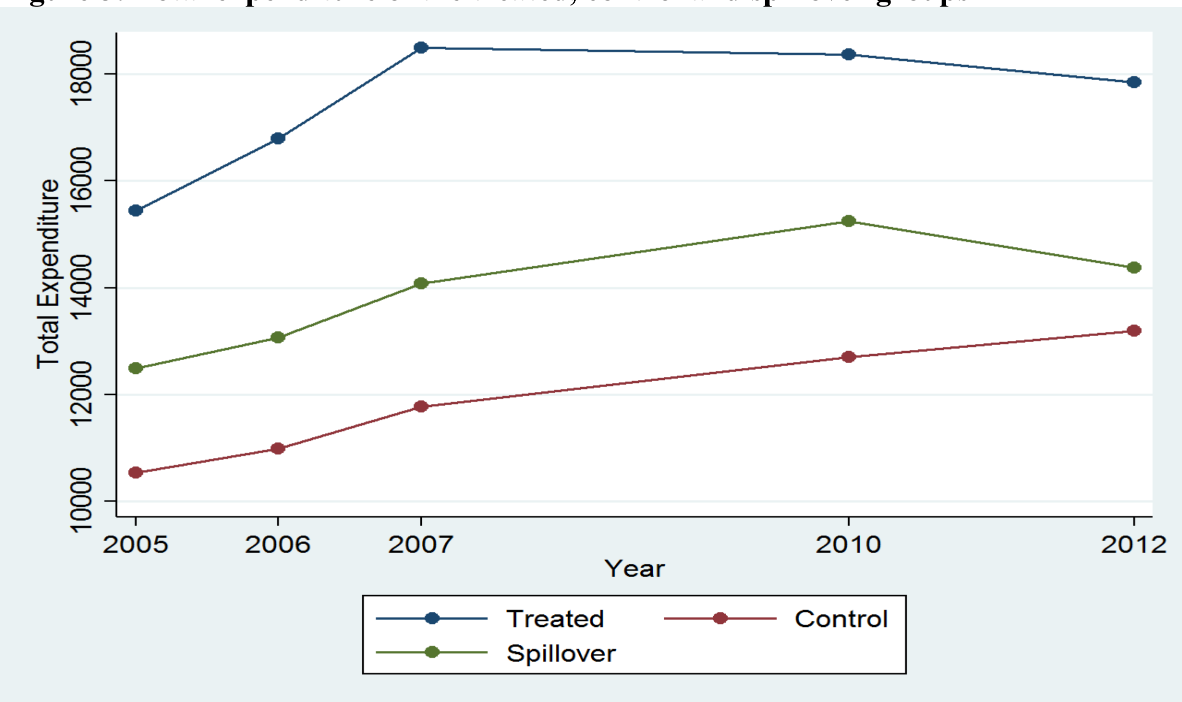
Note: There are 107 sub-districts with flooded household out of 718 sub-districts surveyed.

Figure 4: Total income of the treated, control and spillover groups



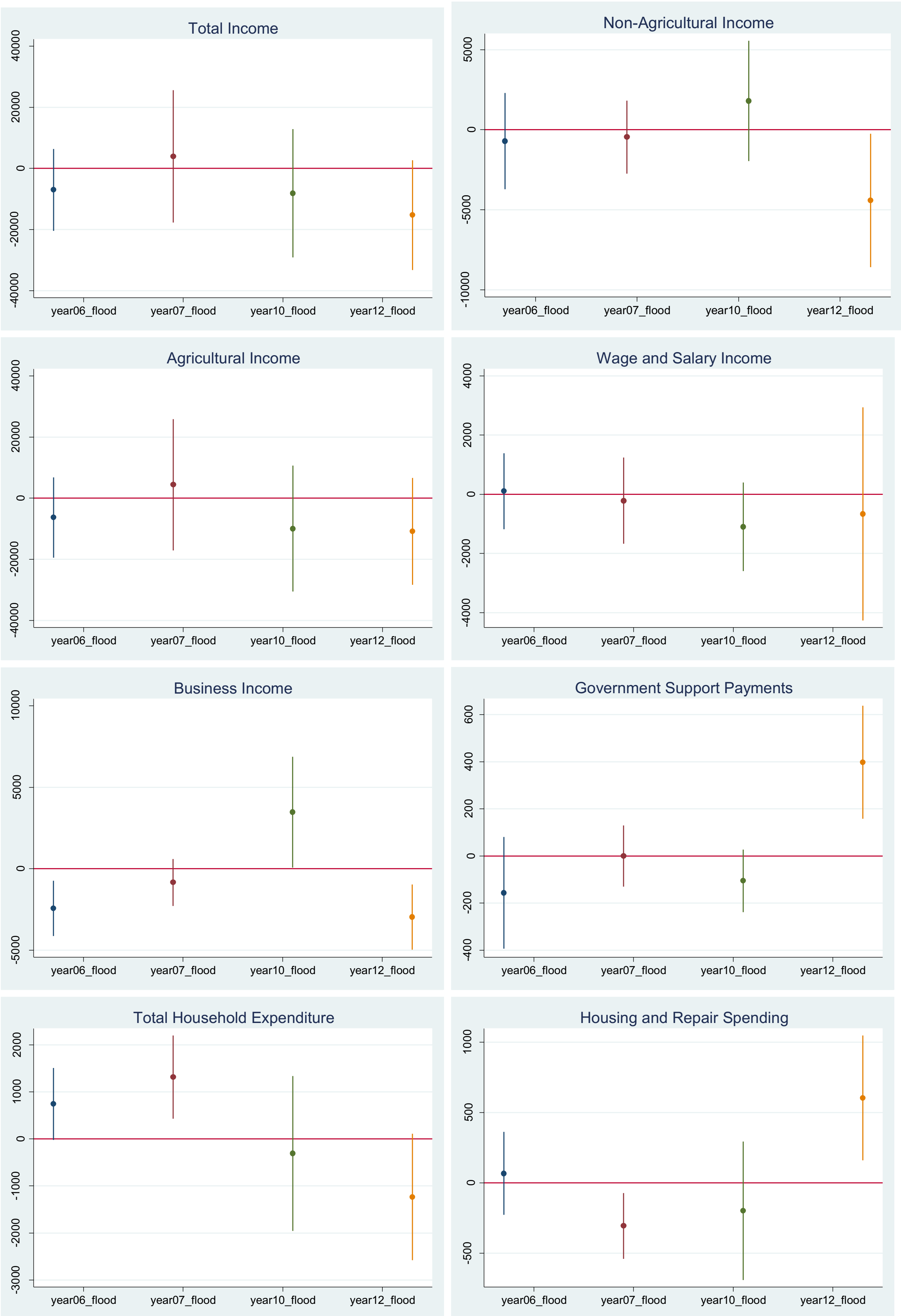
Note: There are 564, 1254, and 3026 households defined as treated, spillover, and control (based on the 2011 floods) and that are followed in all five waves of the survey.

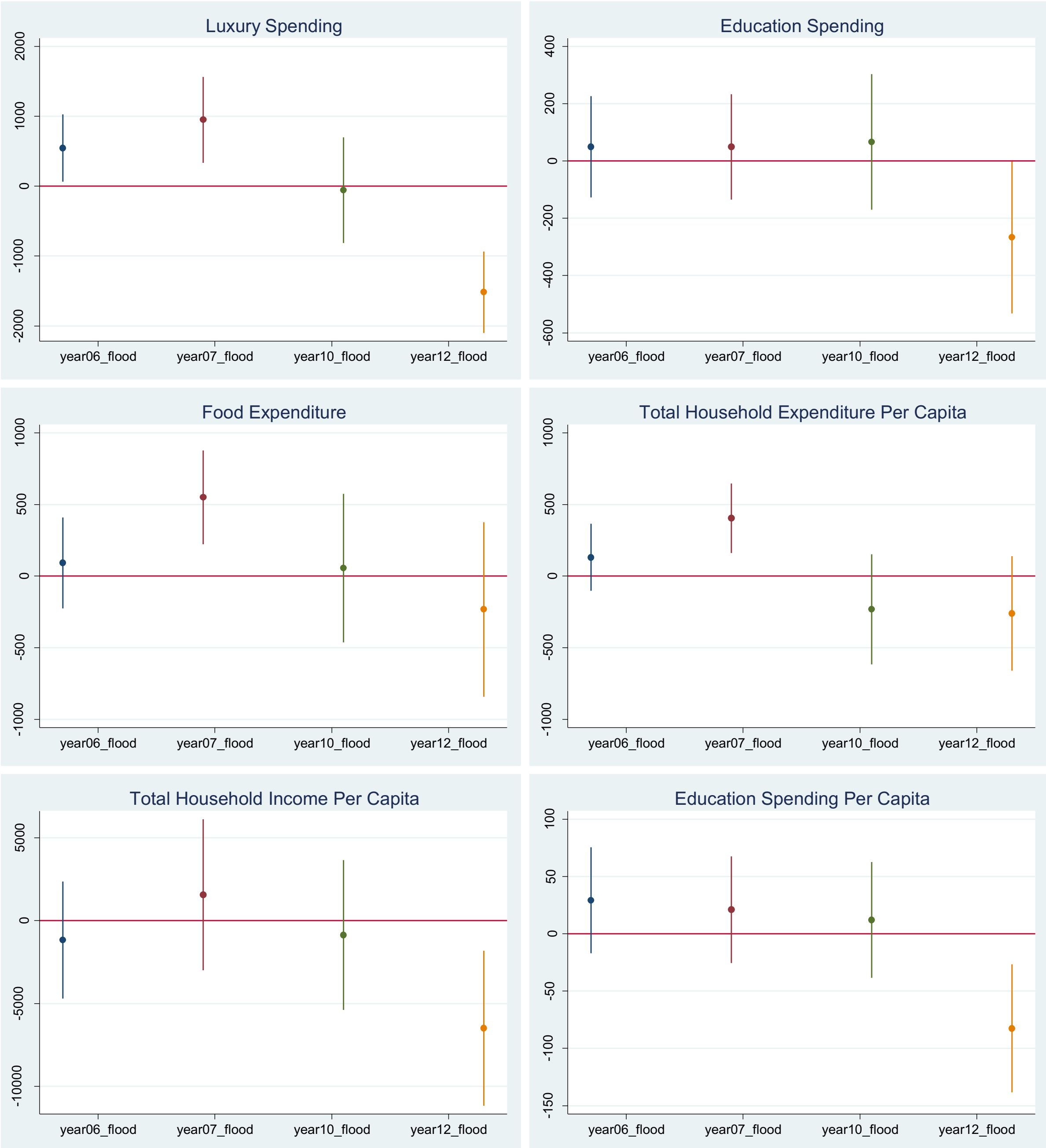
Figure 5: Total expenditure of the treated, control and spillover groups



Note: There are 564, 1254, and 3026 households defined as treated, spillover, and control (based on the 2011 floods) and that are followed in all five waves of the survey.

Figure 6: Falsification Test: Treatments in Previous Survey Waves, Coefficient on Flood Impact




Luxury Spending
2000
1000
0
-1000
-2000
year06_flood
year07_flood
year10_flood
year12_flood
Education Spending
400
200
0
-200
-400
-600
year06_flood
year07_flood
year10_flood
year12_flood
Food Expenditure
1000
500
0
-500
-1000
year06_flood
year07_flood
year10_flood
year12_flood
Total Household Expenditure Per Capita
1000
500
0
-500
-1000
year06_flood
year07_flood
year10_flood
year12_flood
Total Household Income Per Capita
5000
0
-5000
-10000
year06_flood
year07_flood
year10_flood
year12_flood
Education Spending Per Capita
100
50
0
-50
-100
-150
year06_flood
year07_flood
year10_flood
year12_flood

Footnotes

[1] According to EMDAT (www.emdat.de), the Thailand 2011 floods caused damages worth USD 40.3 Billion. The second costliest flood in the dataset (covering globally 1900-2016) is the 1998 flood in China, costing USD 30 Billion.

[2] This lack of insurance is typical for middle-income countries; and may suggest the prospect of a prolonged recovery period (Munich Re, 2012). Middle-income countries do not have well functioning markets for insuring natural hazard risks (there is virtually no hazard insurance in low-income countries). Even many high-income countries are signficantly under-provided with natural hazard insurance products (including flood insurance).

[3] See Karim and Noy (2016a & 2016b) for a regression meta-analysis and a qualitative survey of this literature. Hallegatte et al. (2016) provide further analysis of disaster risk within the context of development and climate change.

[4] See Hallegatte et al. (2016) for a thorough discussion on the links between disasters and poverty traps and associated meaurment tools.

[5] Cavallo and Noy (2011) provide a survey of the earlier literature; and Noy and duPont (2018) a more recent one. Lazzaroni and van Bergeijk (2014) provide a regression meta-analysis of more recent works. Lima and Barbosa (2018) use aggregate municipal level data to examine a specific case study of a flood in Brazil, and examine spatial spillovers of the economic impact to nearby municipalities.

[6] Kurosaki (2014) similarly identifies the differential impacts of floods in Pakistan on different types of households with varying access to financial and other resources.

[7] We use the satellite data when we identify the flood duration in each sub-district.

[8] The exact question in the survey is: "Did the household experience flood damage during the last 12 months?" The focus of the question is thus to ask if the household experienced any direct damage from flooding rather than indirect impact. Our analysis assumes respondents understood this distinction; but if they did not, this will bias our findings with respect to spillover effects downward. See also footnote #12 for a related discussion on misreporting.

[9] In the survey, there are households from 817 sub-districts. There are 12 surveyed household in each sub-district on average. Overall, Thailand has 7,416 sub-districts (the unit above village level). Based on 2010 Census, there are on average 8,830 people in each sub-district and the average area of a sub-district is 69.2km^2.

[10] Below, we examine actual reported changes in expenditure patterns by comparing 2012 to 2010 expenditure patterns, rather than rely on the self-reported behaviour from the shock module.

[11] The total values for the vehicles and livestock owned by households before the 2011 floods (in the 2010 wave) are reported; this data is described in table 2.

[12] Principal component analysis is a method of data reduction commonly used for binary indicators in socio-economic surveys. The method uses the variation in asset ownership across households to assign weights or factor scores to each variable (Filmer and Pritchett, 2001; McKenzie, 2005). An asset which is owned by nearly all households will receive a lower weight than one which is owned by a select few. The weights are also dependent on the correlations between different assets and can take on negative values. For example, if owning a bicycle is correlated with assets of low socio-economic status (such as a mud house) it will receive a lower weight. These weights can then be used to construct a household index (Vyas and Kumaranayake, 2006).

[13] Households may perceive interviewers as represtantive of assistance organizations, and misreport being affected by the flood or overestimate the damages caused in order to gain compensation.

[14] As noted by Poaponsakorn (2012), all households in Bangkok are treated as flooded households, despite the fact that some sub-districts in Bangkok were not flooded, because one cannot use the satellite images to identify floods in densly built environments.

[15] Chantarat et al. (2016) use this remote sensing data to identify households that were affected by the flood in the sample of households they collected. They rely on surveys they conducted and have precise geo-location of households. They can thus precisely distinguish which households were flooded (and in a discontinuity design) which households were located just outside the flooded areas. The household survey we use, while much more comprehensive, does not include the geo-location of households, but only the sub-district in which they are located.

[16] In the online appendix, we include an examination of the attrition households, to test whether any bias in our results can be attributable to this 'lost' sub-sample. As can also be seen in table 2, we find that when estimating a

limited depenent variable model (probit) of the households in the 2010 sample (where attrition households are classified at =1) the only systemic difference is that these households have lower income. As has been noted in many of the papers surveryed in Karim and Noy (2016a and 2016b), poorer households are generally more vulnerable to the impact of disasters, so that if anything, this attrition biases our results toward zero. Thus, our results can be viewed as a lower-bound of the true adverse impact associated with the flood.

[17] In household surveys, some expenditure items are typically calculated based on the past month, and some on the past year of spending. In the 2013 survey, food consumption, most luxury spending (except for vehicles/communication durables), and the spending on dwelling and renovations were surveyed based on expenses during the previous month. Education and 'others' are calculated based on expenses during the past 12 months. It is possible that this distinction may have led to biases if the immediate post-disaster replacement spending is captured by the year-long variables but not the past month. However, we do not find significant impact of the flood on these year-long spending variables, so do not think this distinction is of concern for our results.

[18] See Filmer and Pritchett (2001) and McKenzie (2005) for similar uses of this type of information.

[19] This result about higher increases in expenditures are not found when we estimate log-variants of these equations (estimating expenditure increases as share of past expenditure).

[20] This is, admittedly, an ad-hoc definition of agricultural households. At the very least, however, it differentiates between households that reside in dense urban areas and the rest.

[21] The price of rice exports did not change much in the aftermath of the floods, even though the flood peaked in the central region at around the harvest season. As it was a slow-moving flood, some of the rice may have been harvested early in anticipation of the high water. Agricultural production of sugar cane and palm oil, however, apparently benefitted from heavier rainfall (Bank of Thailand, 2011). The rise in some production and in some prices appeared to have improved farm income and can explain why we find an impact on agricultural income only among the poorest quartile of households. It is important to note we do not have data on the types of crops grown by each farming household, so we are unable to identify these impacts more precisely.

[22] Determining how extensive these spillovers are, spatially, can be arbitrary. Other research generally find little observable macroeconomic impact at the aggregate national level, suggesting that spillovers are not as geographically wide as affecting the whole country. Our definitions of spillovers is based on the geographic location data we have, and is admittedly ad hoc. We define as spillover households those households that still reside in the same sub-district as the directly impacted households. A more expansive definition is, of course, possible as well. We note, however, that a more spatially limited definition of spillovers is likely to lead to an under-estimation of spillover impacts (as some spillover-affected households are included in the control group).

[23] These results are not exactly comprable to the results we presented earlier in tables 3-7, as the regression specifications and samples are different.

[24] For some of the expenditure variables, we observe that the 2007 coefficient is statistically significant and positive. 2007 in Thailand, like almost anywhere else, was a boom year; the last year just before the global bust. We suspect that the positive coefficient on spending (most notably spending on luxuries) is associated with this last hurrah.

[25] The satellite readings are only available for Central and Northeastern provinces, and during the mega flood (Aug-Nov) only. Any estimates using this variable thus use a smaller sample.

[26] See Poontirakul et al. (2017) for some insights about insurance impact on firm recovery for the New Zealand earthquake of 2011.