Internal labor migration as a shock-coping strategy: evidence from a typhoon*

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Abstract

We analyze how internal labor migration facilitates shock-coping in rural economies. Employing highly precise satellite data, we identify objective variations in the inundations generated by the most severe typhoon in Vietnam for decades, and match this treatment with a household panel survey before and after the shock. We find that, following the massive drop in income, households achieve to cope mainly through internal labor migration to urban areas: Households with settled migrants ex-ante receive more remittances. Non-migrant households react by sending new members away for work who earn less than established migrants, but remit similar amounts in the short-term.

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"The prudent embark when the sea is calm – the rash when it's stormy."

The magnitude and frequency of extreme weather events is forecasted to increase significantly over the coming decades (IPCC 2013) and developing countries will have to bear a disproportionately big share of related costs. For most of these countries, e.g. Vietnam for instance, weather shocks are already the single most important cause that pushes households below the poverty line (World Bank 2013). While affected rural households theoretically dispose of a portfolio of risk management strategies, most of those fail in the context of large covariate shocks.

In parallel, over the past decades, households in rural economies have seen – temporarily or permanently – some of their members being drawn to urban areas in search of economic opportunities. Such internal migration may diversify the income sources of rural households. In this paper, we study the role of internal labor migration in alleviating income fluctuations both as an ex-ante risk-sharing arrangement and an ex-post shock-coping strategy (with migrants being sent after the shock).

First, we study empirically how cash transfers from migrants help consumption smoothing following a large covariate shock[1] and show that such transfers are, by and large, the most active shock-coping channel. When households experience a drop in annual income per capita of 550 USD due to such kind of shock (approximately one third of total domestic income), they receive 170 USD from labor migrants[2]. All of the response comes from long-distance migrants, i.e., migrants that relocate to another district or province relative to the sending household. In contrast, we find that local networks essentially fail to provide any insurance as they are equally affected by the shock. However, the transfers from labor migrants are still insufficient to bridge the gap in income generated by the initial shock: affected households need to reduce consumption and, more importantly, they do so while having to incur some new expenditures on non-food items (including repairs). As a consequence, our affected households reduce their food expenses per capita by 170 USD.

Second, we aim to understand the exact role of urban migration in insuring households against large disruptions in the rural economic activity. We find that transfers come both from *established migrants* and from *newly-sent migrants*. On

[1]Several papers have highlighted that migration allows households to diversify income sources across space and sectors. See for instance Deryugina (2013), De Brauw et al. (2013) for some empirical evidence on internal migration, Azam and Gubert (2006), Combes and Ebeke (2011), Yang and Choi (2007), Yang (n.d.) for international migration and Stark and Bloom (1985), Poelhekke (2011) for theoretical analyses.

[2]As we are applying a continous treatment indicator in our analysis, we report all coefficients below for a change in the dependent variable due to a full flood exposure (100% inundation) compared to none (0% inundation) in the aftermath of the typhoon.

the one hand, households with a member that is already away in another district or province for working purposes receive much more on average than other households: our benchmark household having lost 550 USD would receive 370 USD. On the other hand, there is a higher probability (+19%) for our affected households without established migrants to send a new migrant. When we investigate the migration outcome of these hastily-sent migrants, we find that they earn, in general, 25% less than established migrants. This wage gap is not explained by different intrinsic characteristics (e.g. age, sex, education) but by a job-worker match of lower quality essentially due to a short-spanned job search without recourse to job agencies and privileging very big cities (Hanoi and Ho-Chi-Minh City). In spite of these differences in income, newly-sent migrants transfer approximately the same amount back to the household of origin as established ones, apparently because their earnings still remain clearly above the average earnings in rural areas: they can afford it.

One important contribution of our study is to provide a clean identification of the exposure to a large covariate shock. We focus on the passing of a typhoon – Ketsana[3] – having hit Central Vietnam in September 2009. Although not particularly strong in terms of wind speed, this storm entered the records as the most devastating one in Vietnam since 1990 because it generated torrential rains and heavily affected crop income in rural areas through intense flooding. We follow a novel approach to identify flooded areas using geophysical satellite data with a 250m precision, before, during, and after the passing of the typhoon. We treat each daily satellite image such as to assess the extent of surface water from the reflectance of near-infrared waves.[4] In parallel, we also compute daily precipitations. With both measures at hand, we know how much it rained (with a precision of 10 km), and how much of the village surroundings was flooded (with a precision of 250 m) and during how many days[5].

We then match the treatment with a unique multi-topic household panel dataset

[3]Ketsana was the eighth typhoon of the 2009 pacific typhoon season but the storm was also named *Ondoy* by the Philippine Atmospheric, Geophysical and Astronomical Services Administration (PAGASA).

[4]In that respect, we relate to the recently established micro-economic literature on the impact of weather disasters using weather data and remote sensing techniques. See Cavallo and Noy (2010) and Dell et al. (2014) for an overview of this literature. The more conventional approach is to rely only on respondents' subjective perceptions of what they consider an adverse weather shock and which degree of intensity they assign this shock (Alvi and Dendir 2011, Morris et al. 2002).

[5]Our treatment – floodings in the village surroundings – may be related to some local geographic characteristics and irrigation technologies. We provide a serie of robustness checks which show that (i) our treatment is not correlated with pre-treatment variables of interst; (ii) there are no differential trends before treatment in affected villages; (iii) our results are robust to the addition of differential trends pre- and post treatment along a large set of indicators for village topography including historical typhoon exposure, and; (iv) our results are robust to the use of exogenous variations in our treatment due to typhoon precipitation patterns.

that was conducted in 220 villages (all affected to a different extent) and two waves in 2008 and 2010 in Central Vietnam[6]. Using pre- and post-disaster observations for each household, we employ a difference-in-difference estimator to identify the causal effects of typhoon Ketsana on our outcomes of interest. Due to the richness of our household data, we can investigate a wide range of coping strategies in response to the shock. For instance, we observe migration outcomes and the job search efforts of migrants, thanks to the existence of a migrant tracking survey.

Our study contributes to two distinct strands of literature, on risk management and migration.

Starting with the risk management literature, our contribution is to highlight the crucial role of internal migration in the context of a large covariate weather shock. Households dispose of few income and consumption smoothing strategies (Morduch 1995). One way to smooth income is to increase the number and type of income generating activities (Jacoby and Skoufias 1998, Kochar 1999). Households can also smooth consumption by accumulating precautionary savings (Paxson 1992), depleting household assets in times of economic hardship (Rosenzweig and Wolpin 1993), or relying on credit markets by borrowing from formal financial institutions (Morduch 1995) or informal sources such as moneylenders (Fafchamps and Lund 2003). Publicly-provided or commercial insurance solutions[7] can also help smoothing consumption (Deryugina 2013). Another mechanism for consumption smoothing is to rely on transfers from networks of mutual support, which may involve migrant household members, the extended family or close friends in the same or distant localities (DeWeerdt and Dercon 2006, Yang and Choi 2007). However, in the case of large covariate shocks, local support networks tend to be quite uniformly affected and, therefore, ineffective (Zimmerman and Carter 2003, Zylberberg 2014). In fact we find that, in the aftermath of Ketsana, most of these risk management strategies fail except insurance provided by long-distance labor migrants, which partly compensate for incurred income losses. When total consumption can only be partially smoothed, households may incur high implicit long-term costs which tend to increase with the severity of the shock (Dercon and Bold 2007). Typical examples for long-term cost may be malnutrition because of reduced food consumption (Subramanian and Deaton 1996), mortality due to insufficient health care (Anttila-

[6] In selected robustness checks, we also rely on earlier data collected in 2007.

[7] While arrangements such as public safety nets and commercial insurance products are frequently found in industrialized countries, they often remain an exception in developing countries. In fact, despite some experimental pilot studies that have been conducted by international donor agencies, insurance products against natural disasters are currently not offered in Vietnam. Furthermore, Vietnam does not dispose of a functioning national social security system that might help smoothing income losses from large and correlated shocks (Trung 2013).

Hughes and Hsiang 2013), lack of business investment, or forgone accumulation of human capital, for example through child labor (Maccini and Yang 2009). We do find some evidence that food expenses decrease in the aftermath of Ketsana but we cannot find any robust evidence of insufficient health care in response to the typhoon (Lechtenfeld and Lohmann 2014).

As regards the migration literature, studies of internal or rural-to-urban migration go back to the 1970s when several developing economies experienced a structural change (see Sahota (1968), Todaro (1980), Cole and Sanders (1985) for a review and some empirical evidence). We contribute to this literature by showing an important, but so far unexplored, role of urban migration: Households can react to a shock by sending a member to work in urban centers and the urban labor demand can absorb quite easily the rural excess labor supply.[8] Very little is known on those issues despite being at the core of the policy debate, at least in Vietnam (Abella and Ducanes 2011). An indirect contribution of our paper is to estimate the potential hurdles to migration thanks to some exogenous variations that drive rural workers out of their villages. We show that jobs are easy to find and still provide higher wages than in agriculture. Todaro (1980) and Cole and Sanders (1985) describe cities as being constituted of a modern urban sector and a less modern sector (construction for instance) absorbing the excess labor supply. In their analysis, migrants transit through the less modern sector before starting to work in the modern sector. We find that excess labor supply, e.g. our newly-sent migrants, end up more often in the unskilled occupations, because it takes time to integrate into the modern urban sector.

The remainder of the paper is structured as follows. Section 1 briefly discusses the background of our impact evaluation, namely the patterns of typhoon impact in rural Vietnam, the data, the empirical strategy and some descriptive statistics. Section 2 presents the main results and we briefly conclude in section 3.

1 Data and Empirical Strategy

1.1 Typhoon impact in rural Vietnam

Vietnam is regularly hit by tropical storms forming in the West-Pacific basin: between one and five of them are recorded every season between May and November. However, not all regions of Vietnam are exposed to the risk of tropical storms. The geographical range of risk-prone areas in the center of Vietnam expands between the

[8] A recent contribution, Tse (2011), investigates the impact of natural disasters on migration in Indonesia and finds that floods actually decrease migration rates, although the precise channel remains unexplained.

latitudes 12^o and 22^o North, which translates into approximately 1.100 km - a distance equivalent to the one from Barcelona, Spain, to Frankfurt, Germany. Figure 1 illustrates the frequency of occurrences for each district between 1945 and 2006. Among those storms, few are actually categorized as typhoons according to the Saffir-Simpson hurricane scale (SSHS)[9]. Despite the frequent occurrence of tropical storms, the probability of being hit by a category-2 typhoon for a specific district along the risk-prone areas is relatively low. For instance, there was only one other typhoon that reached the strength of Ketsana (category-2 typhoon) making landfall in Vietnam during the survey period from 2007 to 2013. Further, even within affected districts, the variability of precipitation, wind speed and subsequent floodings may differ considerably.

In this paper, we focus on a typhoon which entered the records as the most devastating storm in Vietnam since 1990: On the 29th of September 2009 storm Ketsana hit Vietnam as a category-2 typhoon after having brought severe destruction to the Philippines. The eye of the typhoon made landfall approximately 60 km south of the city of Da Nang in Central Vietnam (Figure 1). During its course over the country Ketsana directly affected 14 provinces and brought torrential rainfall for several days. With wind gusts reaching sustained speeds between 154 to 177 km/h at some locations, Ketsana did not belong to the strongest typhoon category in terms of wind speed. In contrast, damages incurred through excess rainfall and related inundations and landslides were reportedly massive: Official estimates state that Ketsana affected roughly 2.5m people in Vietnam, killed 182 of them, and caused accumulated direct capital losses of approximately 1% of GDP[10]. Interventions of government authorities, NGOs or public organizations could not prevent or alleviate the impact of the typhoon.[11]

[9]The Saffir-Simpson hurricane scale (SSHS) classifies tropical storms into five categories distinguished by the intensities of their sustained winds. To be classified as a typhoon, a tropical storm must have maximum sustained winds of at least 119 km/h (category 1). The highest classification in the scale, category 5, is reserved for storms with winds exceeding 251 km/h.

[10]EM-DAT - The OFDA/CRED International Disaster Database (www.emdat.be), University Catholique de Louvain. In our sample of rural households, we estimate *indirect* losses for the average household to be 4% of its income. Our households are closer to the typhoon than the representative Vietnamese household, and their activities are more vulnerable to floods.

[11]Disaster prevention efforts were initiated on September 27 by the Central Commitee for Flood and Storm Control (CCFSC) which also coordinated later international donor agencies' relief actions. In the morning of September 28, 24 hours before the typhoon made landfall in Vietnam, the Vietnamese prime minister issued an urgent telegraph to all ministries in possibly affected provinces, commanding the evacuation of the population most at risk. By the time the typhoon made landfall, more than 50.000 households or 300.000 individuals were evacuated and hosted in public shelters. Although the central government commanded the assistance by the local army forces to help farmers save their crops before the onset of any inundations, the effectiveness of this measure appears to having been very limited due to a lack of time for preparation. In our household data, we find no precautionary harvest activities before the disaster's onset. Similarly,

1.2 Treatment

Due to the different weather disasters associated with the passing of a typhoon (e.g. wind speed, rainfall, flooding or landslides) the construction of a uniform exposure measure which summarizes its impact is non-trivial. Given our previous description of typhoon Ketsana, we decide to account for the most important source of destruction related to this event: local inundation. In order to capture the extent to which villages were inundated, we propose a direct measure based on the analysis of satellite images, and an indirect measure, i.e., the intensity of rainfall, which we use as a robustness check. We describe both measures in the following lines.

Flooded areas In order to obtain an indicator of inundation caused by typhoon Ketsana, we proceed as follows: we collect the two daily satellite images provided by the NASA[12] for a window of 13 days (from september, 26th to october, 8th), treat them such as to extract a daily measure of water coverage in the neighborhoods of our villages, and deduce how much excess water there was in the aftermath of typhoon Ketsana compared to normal times, i.e., before and one week after the event took place.

How can we measure the presence of water at the surface from satellite images? The left panel of figure 2 displays the untreated image taken of South Indochina (Vietnam, Laos, Cambodia, and Thailand) on October 6, 2009, about a week after the passing of Ketsana. The right panel is the same image with a modification that makes visible the near-infrared waves. The depicted index is called Normalized Difference Vegetation Index (NDVI), a simple graphical indicator that is commonly used in remote sensing measurements to determine the intensity of vegetation in a given location. Due to its technical specification, however, it can also be used to detect ground water coverage. There are different methods used in the hydrological literature (see Rogers and Kearney (2004) or Sakamoto et al. (2007) for instance) to identify water from NDVI images. The bottom panel of figure 2 is obtained by applying the same filter as Sakamoto et al. (2007) which study annual flooding patterns in the Cambodian and Vietnamese Mekong Delta areas. We follow this approach throughout the paper. The Mekong Delta can easily be identified, spreading from the center to the bottom right of the land mass depicted.

Each treated satellite image gives an estimate surface water at the time during

disaster relief activities by local and international donor agencies had little effect. In our household data, cash and in-kind transfers are small and not correlated with real losses.

[12] Two satellites (Terra and Aqua) take daily images of different zones of the globe. Two subsets of images cover Vietnam, i.e., Indochina and China 5. See the MODIS subsets. http://earthdata.nasa.gov/data/near-real-time-data/rapid-response/modis-subsets

which the image was taken. Our data provides two satellite images per day, and thus two distinct daily observations of the degree of water coverage. We first generate a daily estimate by averaging these two distinct daily observations. Second, we group daily observations into different bins: the first two days after landfall are the *immediate impact*, the five following days are the *aftermath*. In our preferred specification, we use the three days before the typhoon and after the *aftermath* as the *normal* period. For each of these bins we construct simple averages of inundated area in varying radiuses of one, two, five, and ten kilometers around our surveyed villages. We divide these values by the total surface area in order to obtain the percentage of area inundated for each village. The advantage of this data-merging process is (i) to estimate the share of days/area flooded in the neighborhood of villages after the catastrophe and (ii) to reduce the noise induced by cloud coverage and the instantaneity of each image taken separately.

Figure 4 displays the sample average share of flooded areas before, on impact, in the aftermath and after the passing of Ketsana. We can see that the median household in our sample lives in a village where 12% of the surroundings areas are flooded in the aftermath of the typhoon. This number rises up to 77% for the most affected household.

In our empirical analysis, our benchmark *treatment* T_v will be the percentage of area inundated in the *aftermath* of the typhoon and within a radius of 5 kilometer around village v[13]. Why do we use only the aftermath? Due to satellite measurement error during times of heavy clouds coverage, the immediate impact measure is extremely noisy. For instance, its correlation with self-reported exposures by commune leaders is nil. In contrast, there is almost no clouds coverage from october 1st to october 6th (see figure 2). We also use the normal periods measures and define a *propensity* indicator P_v as the percentage of area inundated within a radius of 5 kilometers around a village during these normal days.

Figure 3 displays the average geographic variation in our treatment in the aftermath of the typhoon T_v. For the sake of illustration, we show the mean of 2 villages in each of the 110 sub-districts of our household survey. Even though one province (Dak Lak) is less affected than the others, there is still relatively high within-province variation in the treatment, and we will use this variation rather than differences across provinces. Our treatment may be related to some local geographic characteristics. In this regard, we control for differences in village topography (e.g. being located in coastal areas, mountains, plains, etc.). In order to purge our treat-

[13]We use the 5km-radius because the data on crop production in 2008 indicates that 95% of farming plots are located within a distance of five kilometers from the village center.

ment from local unobserved differences, we also construct an alternative treatment directly based on rainfall.

Precipitation In addition to the observation of flooded areas with a 250m-precision, we use the NOAA/Climate Prediction Center daily rainfall estimates between September, 26th 2009 and October, 7th 2009 and construct a measure of average rainfall R_v during the immediate impact, i.e., September 29th and 30th. As with flooded areas, we also construct the average rainfall before the typhoon and a week after, such as to capture the variation in rainfall during normal monsoon days P_v^r. Figure 5 displays the excess rainfall $R_v - P_v^r$ for the three different provinces in our analysis.

Compared to our inundation-treatment (figure 3), the precision is much lower. Indeed, the NOAA/Climate Prediction Center daily rainfall estimates are provided with a 10 km precision and these estimates are the outcome of an algorithm relying on 4 different sources whose minimal precision is 10 km. One advantage of this measure is, however, that it correlates very little with each village topography. In addition, it captures the immediate impact that we miss with our satellite images due to cloud coverage.

1.3 Empirical strategy

We aim at estimating the impact of typhoon Ketsana on a broad range of socio-economic outcomes for affected households. To this purpose, we use a difference-in-difference approach: We take advantage of our panel data and identify the household response from variations between the pre-treatment period and the post-treatment one.

However, our treatment may be correlated with some omitted variables, e.g., geographical characteristics, and villages that differ along those characteristics may follow differential trends. In the benchmark specification, we control for province/wave fixed effects and allow villages with different propensity P_v to have different trends (in robustness checks, we also allow villages with different topography to have different trends). We thus identify our effect on variations over time for villages of the same province and with the same propensity to be flooded.

We estimate the following baseline equation:

$$Y_{h,v,p,t} = \beta_0 + \beta_1 T_v \times \mathbb{1}_{t=2010} + \beta_2 P_v \times \mathbb{1}_{t=2010} + \gamma X_{h,t} + \delta_{p,t} + \alpha_h + \varepsilon_{h,v,p,t} \quad (1)$$

where h indexes the household, v stands for village, p for province, and t indexes time or equivalently the wave (t=2008 or 2010). $Y_{h,v,p,t}$, our dependent variable, will be either the presence of migrants (members having been absent from the household for at least 6 months per year), transfers from migrants or income depending on our specification. T_v is our treatment indicator, which reflects the exposure to inundation in a radius of 5 km around our survey villages in the aftermath of typhoon Ketsana. P_v is the water coverage within the same radius during unaffected times[14]. The vector X includes time-varying socio-demographic characteristics of the household, such as the household size, member composition, age, gender and education of the head. $\delta_{p,t}$ is a set of province-specific wave fixed-effects to account for changes in living conditions over time in each province, and α_h are household fixed effects to control for time-invariant household unobservable characteristics. Since we have household fixed-effects, we do not need to include T_v and P_v individually in our specification[15]. Similarly, we do not need to add $\mathbb{1}_{t=2010}$ separately because we have already included province-specific wave fixed effects. $\varepsilon_{h,v,p,t}$ is the error term with standard errors clustered at the sub-district level, given that there exists some spatial correlation in our treatment. Finally, remark that we allow $\beta_{2,t}$ to vary with time, such that villages with a higher propensity to be flooded are allowed to evolve differently than others.

What is the variation that we are capturing with this specification? We are conducting a difference-in-difference analysis comparing affected and unaffected villages with similar propensities to be affected, before and after the shock. We fully control for observed and unobserved time-invariant factors. As regards time-varying factors, we control for floodings before and one week after as a proxy for normal periods as well as geographic factors.

There may remain some concerns that our treatment is not fully exogenous because some unobserved geographic characteristics may explain the exceptional floods in some areas. For instance, soils may differ across their capacity to absorb water and these differences may reflect natural advantages or technological disparities. To better isolate pure exogenous variations in the exposure to the typhoon, we use our alternative treatment R_v and estimate the following two-stage panel Instrumental-Variable specification:

$$\begin{cases} T_{v,t} = b_0 + b_1 R_{v,t} + b_2 P_{v,t} + b_3 P^r_{v,t} + cX_{h,t} + d_{p,t} + a_h + e_{h,v,p,t} \\ Y_{h,v,p,t} = \beta_0 + \beta_1 \widehat{T_{v,t}} + \beta_2 P_{v,t} + \beta_3 P^r_{v,t} + \gamma X_{h,t} + \delta_{p,t} + \alpha_h + \varepsilon_{h,v,p,t} \end{cases} \tag{2}$$

where the variables $\{V_{v,t}\}_{V=T,R,P,P^r}$ denote the interactions $\{V_v \times \mathbb{1}_{t=2010}\}_{V=T,R,P,P^r}$.

[14]We use different definitions of unaffected periods in order to capture normal indundation levels: days before and long after the event, or each period taken separately.

[15]We could as well choose to estimate a specification without household but village fixed effects. In such case, our estimates do not change, but the standard errors are larger.

Compared to specification 1, we only use here the variations in floodings T_v implied by rainfall R_v once controlled for normal conditions P_v, P_v^r. One advantage of such specification is that it excludes differences in soil absorption conditions (whether natural or human-made) from our identification.

1.4 Household Data

The empirical analysis draws on a rich multi-topic dataset collected within the framework of the project "Vulnerability to Poverty in Southeast Asia", sponsored by the German Research Foundation. The project was carried out as a panel survey in three waves (2007, 2008 and 2010) and includes more than 2,000 households in 220 villages in the rural provinces of Ha Tinh, Thua Thien Hue (referred to as Hue) and Dak Lak.

Sample selection took place in a three stage process at the district, commune, and village level. The primary sampling unit was the commune assuming homogeneity within a province, which is reasonable for the chosen provinces in Vietnam, especially with regards to the geophysical conditions and employment structure. On the commune level two villages and in each of these villages 10 households were randomly selected. The sample was designed in such a way that it is representative of the target population and allows drawing conclusions on a broad range of topics affecting rural households in the selected provinces and areas with similar conditions. All provinces in the survey rank in the lowest income quintiles in the country with their population predominantly engaging in small-scale agriculture and limited self- and off-farm employment. Although all of these provinces are generally located in the Vietnamese typhoon risk prone area, their propensity of being affected differs.

Attrition in the panel is relatively low with rates around two to three percent per wave. For the empirical analysis, the sample is restricted to households that have been interviewed in all waves. In the main specifications, we are left with a total sample size of approximately 2,100 households.

1.5 Descriptive Statistics

Table 1 provides summary statistics for the full sample and by provinces for the pre-disaster wave 2008. Regarding household demographics, the average household in our study provinces has 4.4 permanent members excluding migrants, and 1.2 men between 16 and 59 years. Roughly two thirds report farming as their main occupation and the share of agricultural income over domestic income is relatively high at around 40 percent. Other income components constitute earnings from self-employment (15% of total income), off-farm labor earnings (13%), and public transfers from government institutions including insurance payments (7%). With a total income of 1,384 USD (PPP) per capita our sample households are significantly poorer than the average national household with 2,890 USD (PPP) per capita in 2008. This is due to the sample selection which targeted rural provinces far away from the rich urban centers of Hanoi and Ho-Chi-Minh-City. Their consumption patterns are those of poor rural households: they dedicate roughly 50% of total consumption to food expenditure while expenditures on non-food items such as cloths, personal care supplies, and fuels amount to 37%. Spending on education and health services is of lower magnitude with roughly 6 and 4% of total consumption respectively.

Our households possess very few monetary savings with 234 USD on average. They do borrow (2,832 USD per household) but those loans are mostly mortgages from formal sources such as public and commercial banks. Consumption-loans are inexistent.

Regarding migration outcomes, 38% of our sample households have at least one internal migrant and there are 1.6 migrant members on average conditional on having one migrant[16], respectively in the same district (*local*), in another district than the household (*long-distance*). Households at the origin maintain strong financial exchange with their migrants: In 2008 they received 340 USD from the migrants while sending out 168 USD to them, which implies positive net transfers of 172 USD for the average household per year. Looking at long-distance labor migration, we find that the incidence is 21% with roughly 1.4 individuals for each sending household. Net transfers from this type of migrants are of 64 USD per household on average. In contrast, incidence of local labor migration within the same district is relatively low (5%) as most labor migrants tend to target the big industrial centers such as Ho-Chi-Minh-City in the South or Hanoi in the North for off-farm employment opportunities.

There are some important differences between provinces that are worth mentioning. Starting with Ha Tinh province (column 2), the figures show that total income is below average and the share of agricultural over total income is also relatively low. One reason is Ha Tinh's geography: Being the Northernmost province in our sample with relatively low temperatures during the winter months, paddy cultivation predominantly follows a one season pattern and takes place during the summer months

[16] We classify a household member as a migrant if, within the given reference period, the person was at least 16 years of age and declared to belong to the household, but did spend more than half of the time (at least 180 days) away. Further, we classify a person to be absent for work purposes if the household's respondent declared that the person left because of a "job opportunity" or "job search".

from April to October. Agricultural income during the winter season is close to zero. This stands in contrast to the provinces further in the South where two cropping cycles per year are standard and agricultural incomes during the winter are significant. Ha Tinh also performs worst in terms of off-farm employment opportunities. As a consequence, migration incidence in Ha Tinh is high with 49% for all migrants and 31% for long-distance labor migration, and families rely heavily on cash transfers: Net transfers from these networks are also the highest among our sample with 248 USD and 120 USD respectively. Hue economy (column 3) is more diversified than Ha Tinh's with mining and tourism offering employment opportunities to the local population. This is reflected in the above average contribution of self-employment (23%) and off-farm labor income (20%) to total income. Migration is less frequent (22% of households with long-distance labor migrants and net transfers around 110 USD). In contrast to the other two provinces Dak Lak (column 4) is located in Vietnam's central highlands and does not have direct access to the sea. Due to Dak Lak's climatic conditions, agriculture plays a mayor role with many cash crops such as coffee, fruits, and vegetables being grown on a large scale. Thanks to these favorable conditions Dak Lak is the richest province in our sample with an average income per household of 8,415 USD. Agricultural activities contribute almost 60% to the total income, while earnings from self-employment and labor constitute 13% and 10% respectively. Migration plays a minor role in Dak Lak compared to the other two sample provinces.

1.6 Pre-treatment differences between affected and spared villages

Our treatment is derived from an unpredictable and random event – a typhoon hitting a particular point of Vietnam. However, villages with a large share of flooded areas may still be different than the others, even within the same province. Local variations in the treatement are likely to be related to topographic characteristics. For such reasons, we use a difference-in-difference specification and rely on some exogenous variations (rainfall) and use topography controls. In this section, we comment on two additional placebo checks.

First, we perform a comparison exercise between treated areas and untreated ones before the treatment. Since our treatment is continuous, we need to define an equivalent dummy variable that would separate two groups – the treated versus the control groups. In order to do this, we regress our benchmark treatment T_v on the propensity P_v and province fixed-effects, and we isolate T_v^r the residual of this regression. We then separate our households into 2 groups: the treated $T_v^r > \bar{T}$, and the control group $T_v^r \leq \bar{T}$. We choose arbitrarily $\bar{T}$ such that our treated group constitutes 1/3 of the total sample, and we compare the pre-treatment characteristics of the two groups in table 2[17]. As can be seen in table 2, the treated and control groups do not differ systematically regarding our important variables of interest, i.e. income and transfers. It could be noted, however, that consumption is slightly higher in the treated group in 2008. Naturally, when we repeat the comparison exercise in 2010, the two groups diverge radically. For instance, transfers from labor migrants are then 35 USD higher in the treated group. We explore these ex-post differences formally using our specification 1 in the next section.

In the following section, we sometimes restrict our specification to two subgroups: villagers with at least one labor migrant away in 2008 and villagers without any labor migrant away in 2008. When we repeat our comparison exercise within these subgroups (see tables A9 and A10 in the appendix), we still cannot find any major difference between treated and control households.

Second, we run a placebo specification between rounds 1 and 2, i.e., between may 2007 and may 2008. We replicate our benchmark strategy as if the typhoon had hit in september 2007, two years before the actual occurrence:

$$Y_{h,v,p,t} = \beta_0 + \beta_1 T_v \times \mathbb{1}_{t=2008} + \beta_2 P_v \times \mathbb{1}_{t=2008} + \gamma X_{h,t} + \delta_{p,t} + \alpha_h + \varepsilon_{h,v,p,t} \qquad (3)$$

We will provide explicitly the results of this placebo experiment for one of our main specification (remittances) in table A3.[18] This specification is a direct test for the presence of pre-treatment differential trends along our treatment.

2 Results

This section is organized as follows. First, we analyze how our treatment – excess flooded areas due to Ketsana – affects income and how transfers received by the households help consumption smoothing. Second, we focus on the migration strategies of households. In particular, we assess whether additional remittances received by affected households come from already-established migrants (*ex-ante*) or newly-sent members (*ex-post*). We then analyze potential differences between the ex-ante and the ex-post migrants: do newly-sent migrant get similar jobs as established ones and are they equally productive?

For all specifications, we restrict our sample to the 2008 – ex-ante – and 2010 –

[17]We could have chosen very different thresholds. Defining a discrete treatment is for illustrative purposes: we could also correlate our residual treatment – a continuous variable – to the pre-treatment characteristics. The general idea that the treatment is not correlated with pre-treatment characteristics would go through.

[18]The results of this placebo experiment for all our main results are available upon request.

ex-post – household observations. Since the 2010 survey defined the reference period between May 2009 and April 2010, our estimates capture the household's response between 7 to 8 months after the shock. All monetary variables are reported in USD (PPP) per capita terms, normalized by the number of permanent household members, i.e., excluding absent migrant members. For the sake of simplicity, we report all coefficients below for a theoretical change in the dependent variable due to a full flood exposure (100% inundation) compared to none (0% inundation). However, as we are applying a continuous treatment indicator which, in practice, ranges between 0 and .77, we can also interpret our estimates as follows: assuming linearity between treatment and outcomes, we discount the coefficients by .77 such as to obtain the difference induced by the treatment between the most and least affected households in our sample. Similarly, one can adjust the estimates by .2 to obtain the impact of one additional standard deviation in the treatment.

2.1 Income shocks and shock-coping instruments

In order to study how the shock affects the budget constraint of households, we first analyze how income responds to our treatment and we show which activities are mostly disrupted by the floodings. We then assess the extent to which households manage to smooth consumption. We finally describe which consumption-smoothing instruments respond to the treatment.

A way to understand this exercise is to write down the budget constraint of a representative household. In period t, the household receives a revenue $y_t = \sum_a y_t^a$ from its different activities indexed by a, receives transfers $\tau_t = \sum_s \tau_t^s$ from different sources s, and adjusts its asset position Δb_t. Transfers are negative if there is a net outflow from the household and Δb_t is negative if the household saves during the period. The household consumes $c_t = \sum_c c_t^c$ where c denotes the different categories of consumption.

$$y_t + \tau_t + \Delta b_t = c_t$$

The treatment supposedly lowers income y_t, and we want to investigate whether $\tau_t + \Delta b_t$ is sufficiently large to allow the household to maintain consumption constant. Naturally, we want to go beyond the aggregate quantities and know which activities are mostly disrupted, which transfers are responding the most and whether the consumption basket changes.

Income We estimate equation 1 for different measures of income starting with total income per capita y_t. We then restrict the analysis to income per capita generated by the most affected activity, i.e., crop income. We further narrow down our focus and investigate the response from the potentially most-affected crops: Summer/Autumn (SA) crops and Summer/Autumn paddy rice. As a placebo check, we repeat the same exercise for an activity that should not be affected by the shock given its timing, Winter crops (W) and winter paddy rice. The results on income outcomes are presented in panel A of Table 3. Our treatment predicts a strong decrease in income per capita (see column (1)). The coefficient is economically and statistically significant and indicates a loss of 550 USD per capita for a household living in a village that is completely flooded compared to one which is spared. Looking at more disaggregated income sources, it appears that the drop in total income is predominantly driven by a decrease in crop income (see column (2)). In total, this magnitude accounts for roughly 65 % of the loss in total income.

There should be some variations in the extent to which crops are affected, for instance, variations in harvesting seasons. As the main staple crop in Vietnam - paddy rice - is usually gathered between September and November depending on the local climate of the province, its production is directly affected by the typhoon. Columns (3) and (4) report the losses for income per capita generated by Summer/Autumn crops in general and paddy Summer/Autumn rice in particular (respectively 105 USD and 70 USD). In contrast, columns (5) and (6) report the estimates for winter crops and winter paddy rice. As expected, the income generated by those crops is quite uncorrelated with the treatment, given that the typhoon did not occur between the planting and harvest periods. If anything, the coefficient in column (5) is positive, because households may attempt to catch-up from the previous season losses by investing more in the following.

In table 4, we also report the impact on the income generated by livestock, hunting, wages, or self-employment and we find that none of those components are significantly lower in affected areas. In particular, local labor markets other than agriculture do not seem to be affected. One reason may be related to Ketsana's characteristics: the typhoon affected Vietnam essentially through rainfall damaging crops rather than wind gusts destroying other assets. There are also many villages of our sample in which wage labor is almost absent.

Consumption We now turn to consumption (panel B of Table 3). We do find a negative but not significant effect of typhoon Ketsana on total expenditure per capita (column(1)). However, as we can see in the following, households need to substitute between different consumption categories. Looking at column (2), we do find a robust negative effect on food consumption per capita. In contrast, the coefficient in column (3) on non-food consumption is positive. This result is consistent with the interpretation that households may spend more in the aftermath of a typhoon to repair housing damages and replace broken durable assets. Further, we also find negative but non-significant effects on education as well as on health expenditures.

In summary, while substitution between non-food and food consumption appearently takes place, net consumption per capita still decreases by around 200 USD (or 40 % of the initial income loss), mainly driven by a significant reduction in food consumption. This implies that the average households is unable to fully smooth consumption in the short-term.

Remittances and other transfers We have provided evidence that our treatment translates into an income shock and a lower but still present consumption shock. We now focus on the second and third terms of our budget constraint $\tau_t + \Delta b_t$, i.e. transfers with third parties and changes in borrowings/savings, and investigate which of these instruments mitigate the income shock and stabilize household disposable income.

Table 5 reports the estimation of equation 1 for net remittances per capita sent by labor migrants. In affected villages, households receive significantly more than in spared villages, but it only comes from labor migrants outside of the district or province. The coefficient in column 2 indicates that households receive extra-remittances of almost 170 USD from labor migrants in another district relative to its household of origin or in another province (column 3), but they receive less from local labor migrants in the same district (45 USD, column 1). An explanation for the latter result is that some households usually benefit from a constant positive stream of remittances from local migrants, but, in the aftermath of the typhoon, these migrants are also affected and cannot contribute as much as before. In contrast, long-distance migrants are very likely not to be affected at all by the typhoon because they are usually urban migrants going to very large and unaffected cities (Ho-Chi-Minh city or Hanoi for the majority of them).

These findings support the theoretical claim put forward by Stark and Bloom (1985) that labor migration can be used as a risk reduction strategy that helps to diversify income sources across space or sectors. More diversified networks, e.g. households with long-distance migrants, are more useful: the correlation between income at the source and at the destination is then lower.

There are other networks of mutual support from whom the households may receive transfers. In the panel A of table 6, we report the response of remittances from non-labor migrants (column 1), the informal transfers given by the extended family and friends (column 2) and redistribution from social assistance and security programs (column 3) in table 6. None of these transfers help the affected households in a significant manner: extended family and friends are equally affected by the shock as they often reside close to the household's locality, non-labor migrants do not generate any revenue on their own, and state redistribution is generally too small and too weakly correlated with damages to be of any help. We also estimate how our treatment affects compensation payments (column 4 grouping insurance payments and NGOs support) and show that insurance plays a negligible role. This is primarily explained by the low coverage of commercial insurance products in rural areas of Vietnam. Finally, we examine the effects on household borrowing and dissaving Δb_t. In the panel B of table 6, we document the adjustments that the households operate in response to the treatment. Households may borrow either from informal lenders or formal institutions. The coefficient on formal borrowing is negative, suggesting that the typhoon leads to a drop in formal borrowing. The coefficient on informal borrowing suggests a moderate decrease. We also examine dissaving through the sales of liquid or tangible assets. Column 3 provides the results for liquid assets. The coefficient is positive indicating a decrease in savings due to the typhoon. We finally examine the stock of productive household assets in column 4. The coefficient here is also positive. Overall, however, none of these coefficients is statistically significant.

Robustness checks and placebo strategies We now discuss some important robustness checks.

First, in table A1, we provide the results for specification 2 explaining in a first stage variations in flooded areas by variations in rainfall. For the sake of brevity, we only report the results for our main measures of income and remittances. The first stage is strong: even controlling for wave/province fixed effects, flooded areas and rainfall are highly correlated (with a F-statistic around 16). As regards the second stage, the results are qualitatively similar to those obtained with the OLS (see tables 3 and 5). There are significant income losses that are compensated by remittances but only from long-distance labor migrants. However, the coefficients are twice larger than in the OLS specification. One explanation may be that there is some noise in our benchmark treatment and the OLS suffers from an attenuation bias that is alleviated by the 2SLS specification. In table A2, we replicate table A1 but we use the reduced-form specification in which the flood treatment, $T_{v,t}$, is directly replaced by the rainfall measure $R_{v,t}$. The results are qualitatively and quantitatively[19] similar to the benchmark specification.

[19]The maximum precipitation level recorded during september, 29th and 30th is 80 cm. Once

Second, there may exist pre-existing differential trends in income and remittances that are correlated with our treatment. For instance, our treated villages may have experimented a long-run decrease in agricultural productivity leading to more and more urban migration. In table A3, we run the placebo specification, i.e., we run our specification between 2007 and 2008 as if the typhoon had occurred in october 2007. Table A3 shows that there exist no correlation between the treatment and trends between 2007 and 2008, i.e., before the treatment.

Third, in our basic specification, we use measures of income, remittances and consumption normalized by the number of permanent household members. We can also use logarithmic specifications for variables that are mostly positive (see panel A of table A4 for total income and consumption) or we can normalize remittances by current household income (see panel B of table A4). The disadvantage of the last specification is that (i) total income is also affected by the shock which tends to bias our estimates upward, (ii) total income is a very noisy measure which makes our standard errors very large. Our results are also robust when we use non-normalized nominal values, or nominal values normalized by the number of adult equivalent members (not reported). There may be a "price bias" introduced by the use of nominal values for our income specifications. Following the typhoon, in affected villages, prices may change and nominal values would capture these variations. We construct the average rice sales price per kg at the village level as reported by each household and verify that price variations between 2008 and 2010 are not related to the typhoon (coefficient $-.014$, SE: .083 which implies a non-significant difference of 1 cent between the most and the least affected place). Indeed, rice markets are quite well integrated in Vietnam: 50% of the sample report prices in 2010 between 61 and 69 cents, consistent with the world price.

Fourth, we control for a set of demographic controls. The composition of the household may be endogenous and may respond to the catastrophe. We check that our results are similar when we drop the demographic controls. Another concern may be that there may exist omitted geographic differences between affected and spared villages which explain why those villages differ in 2010 (our main specification already includes household fixed effects absorbing any long-term differences). In panel A of table A5, we include the interaction of wave FE with a set of geographic characteristics (mountain, river, plain, elevation, slope and coast) such as to control for differential trends across different geographic zones. In panel B of table A5, we control for the long term propensity to be affected by typhoons. We construct

multiplied with the coefficient before long-distance remittances, i.e. 2, we find that additional remittances per capita in the most affected village are 160 USD higher than in the least affected village.

the average annual share of a commune that is at a certain distance to the eye of typhoons using all typhoons between 1945 and 2006 (see figure 1), and include the interaction of wave FE with such measure. We provide the results for a radius of 50 km to the eye of each typhoon, but we repeat the exercise for 30,70 and 100 km.

Finally, in our benchmark specification, we use a definition in which migrants need to be away during at least 6 months. In table A6, we replicate the exercise and redefine migrants as members having left the household for more than 3 months, thereby capturing shorter-term movements.

In all those robustness checks, our conclusions remain the same and the point estimates are very similar.

2.2 Migration as a response to the shock

From the previous analysis, we conclude that the presence of long-distance labor migrants helps households alleviate the initial income shock. That being said, most of the process through which absent members help consumption smoothing remains unknown. For instance, are households with an already-established member in 2008 more likely to receive remittances in response to the shock than their counterparts without established migrants? Do the latter send migrants in response to the shock? Do newly-sent migrants find the same jobs as the established ones?

In this section, we first determine to which extent our results are explained by already-established migrants against newly-sent migrants. We then analyze the migration outcome of newly-sent migrants, focusing in particular on their occupation and their labor search.

Consumption smoothing with already-established migrants and newly-sent migrants In this section, for each definition of migration –different district than the household, same district as the household–, we separate our sample of households into two subsamples, households with at least one migrant in 2008 and households without any migrants in 2008.[20] We then estimate specification 1, on each subsample, for (i) the presence of migrants in 2010 and (ii) total remittances received in 2010. Before presenting the results of those specifications, we provide in table A8 a test showing that income losses were similar across subsamples, i.e., households with at least one long-distance migrant in 2008 incur the same income losses as households without any migrants in 2008. Accordingly, the desire for consumption

[20]Given these definitions, we cannot differentiate any response at the intensive margin, i.e., households sending away more than one member. In the following lines, we analyze our results as if migration was a binary choice at the household level.

smoothing between subsamples should be equal.[21]

In panel A of table 7, we present the estimates for the subsamples of households without any migrants in 2008. We find that the treatment is positively and significantly correlated with the probability to have a long-distance migrant (columns 3 and 5): there is a 19% higher probability of having one such migrant. The higher incidence of migration also translates into larger remittances. The estimates indicate an average increase between 70 and 80 USD of additional remittances from long-distance migrants (columns 4 and 6). We can interpret the previous numbers as follows: When each additional migrant of the most affected villages sends 370 USD, a higher incidence of 19% is mechanically associated with average additional remittances around $370 \times .19 \approx 70$ USD. In stark contrast, we find a lower incidence of local migration in treated villages (-10%) associated with lower remittances (-30 USD).

In panel B of table 7, we present the results for the subsamples of households with at least one migrant in 2008. Within these subsamples, the incidence of migration in 2010 is never significantly correlated with the treatment. However, the signs and amplitude of the correlations are quite similar to those found for subsamples of households without any migrants in 2008 (see columns 1, 3 and 5).[22] As regards remittances, we find that remittances from long-distance migrants are significantly higher (+370 USD, column 4), a number that is very similar to our back-of-the-envelope estimates for the additional remittances received for each newly-sent migrant. Conversely, remittances from local migrants are lower in the most affected villages (column 2), even though the coefficient is not significantly different from 0.

These results imply (i) that households send additional migrants in response to the shock, and that, (ii) when they do so, these migrants are equally effective in terms of consumption-smoothing than the already-established migrants.[23]

We investigate now the migration outcome of these newly-sent migrants.

Migration outcome of newly-sent migrants To analyze each migrant's job history, we take advantage of the individual information embedded in the household

[21] We provide in table A9 (resp. A10) the same comparison exercise as in table 2 between treated households and untreated ones before the treatment but for the subsample of households with at least one migrant (resp. without any migrants) in 2008. Before treatment, we still cannot find any major difference between treated and control households.

[22] Because of sample size, the standard errors increase with these subsamples.

[23] As before, we replicate those results in table A7 for the ratios of remittances to total income and the implications are very similar. The most affected households without estblished long-distance migrants receive 3% of their total income as remittances compared to the least affected households but have a probability of 15% to have actually one migrant. As such, conditional on having one migrant in 2010, they would receive 18% of their income as remittances from such migrants.

questionnaire as well as the presence of a separate migrant tracking questionnaire conducted in 2010 alongside the post-disaster survey. This questionnaire details the activities performed by each member in the past year. The migrant tracking questionnaire is very precise but the survey is only available both in 2010 and, due to difficulties when tracking labor migrants across the country, it suffered from considerable attrition (we keep only 70% of all labor migrants). In contrast, there may be reporting error in the household questionnaire, but information is available in 2008 and 2010. We decide to use the migrant survey only as a check in order to compare the household responses and the migrant's ones, and we construct, for each migrant, a set of variables characterizing their job and their job search based on the household questionnaire.

We now limit our migrant definition to household members having spent at least 6 month in another district than the household. As regards job history, we extract the monthly wage, the total income earned over the past year, and the sector (industry, services, public administration, agriculture). We also collect the type of contract signed between the firm and the worker (permanent versus temporary) and whether the worker was hired because of specific skills (education or vocational training). Finally, in order to understand the obstacles that new migrants may face, we focus on the worker's job search, its duration and the information used to find the current job. We then collapse the data at the household level. For instance, if there are two migrants for a same household, one in an administrative job and one in construction, we construct a household level occupation variable which attributes a weight .5 to administration and .5 to construction.

In order to analyze the migration outcome of newly-sent migrants pushed away by the typhoon, we need to construct a counterfactual group. In a first instance, we abstract from the treatment and compare, in 2010, migrants from households without any migrants in 2008 to their counterparts from households with at least one migrants in 2008. In a second instance, we compare how migrants from households without any migrants in 2008 differ in their migration outcome depending on their household's exposure to the shock. Even among the new migrants, "treated" migrants are particular: the typhoon is an unexpected event that drives some of them away from the household. Their decision to migrate is taken hastily in response to floodings, and the outcome of the migration may be different than when the decision is the result of a long process and a serious preparation.

How do newly-sent migrants compare to established ones? We show in table 8 that there exist differences between migrants sent in families where a migrant was already present in 2010 and the others. Established migrants have a significantly higher monthly income, which is not explained by their employment sectors: the proportion of workers in agriculture or public administration is similar. For any given sector, the monthly wage of newly-sent migrants is between 25 and 35% lower than established migrants. There are two explanations behind these income differences. First, established migrants are slightly older (26.4 against 25) and have a significantly higher job tenure (2 years and a half against 6 months). Second, the job-worker match is better: the fraction of established migrants who declares that they have been recruited for their skills is significantly higher (31% against 21%). In contrast, there are no obvious differences in the educational levels or the gender. Along migrant-specific observables, newly-sent and established migrants are quite similar.

How do migration outcomes correlate with the treatment? We select all the new migrants for whom we have detailed income information and separate them into a *treated* group and a *control* group. We follow the same process as in the placebo analysis: we regress our treatment T_v on propensity P_v and province fixed-effects, and consider T_v^r the residual, i.e. the excess share of flooded areas compared to normal times relatively to the province average. We then classify our households by their position relatively to $\bar{T}$, and choose arbitrarily $\bar{T}$ such that our treated group constitutes 1/3 of the total sample. In table 9, we see that new migrants related to affected households earn less than new migrants related to non-affected households. This difference does not arise from sectoral differences: there is no compositional disparity between the two groups. In each sector, new migrants sent by affected households earn 100 USD less per month than new migrants sent by non-affected ones.

The wage differential may be due to the migrant's intrinsic characteristics, or to the quality of the migrant-job match. Our results tend to favor the latter interpretation. While affected migrants and non-affected migrants have similar characteristics (age, gender or education), affected migrants declare having been selected for their skills much less (14%) than non-affected migrants (26%). Part of the reason behind the lower match quality may be that affected migrants invest much less time in their job search: only 37% declare having searched for more than a week against 63% for non-affected migrants. Similarly, only 26% had access to external information sources during their search against 46% for non-affected migrants.

Discussion Our results support a straight-forward interpretation: some members of households that were affected by the typhoon are forced to migrate hastily and get a job as quickly as possible. This hurriedness prevents them from carefully sampling job offers. However, their prospective income remains quite high, even compared to established migrants, which explains why they are able to send comparable remittances.

The description of urban migration in transition economies (Todaro 1980, Cole and Sanders 1985) is consistent with our findings. In cities, there exist two sectors, a modern urban sector and a large unskilled sector absorbing the excess labor supply. Generally, migrants transit through the unskilled sector before starting to work in the modern sector.

We do find some evidence in favor of these theories. Our migrants, and particularly the ones that are pushed away by the typhoon, end up working in the manufacturing sector, in occupations with low skill requirements. The search effort seems to be minimal (less than a week and no recourse to agencies) which is consistent with infinitely elastic labor demand schedules.

In contrast, established migrants, who had the time to screen job offers, work in more demanding occupations (the modern sector). In this sector, they may acquire some job-specific skills thanks to their higher tenure.

In this literature, the unskilled sector is often viewed as a stepping stone to modern occupations. Our findings indicate that it may also act as an efficient and flexible coping device for rural households when the rural activities are disrupted: job offers are easy to find and wages remain high relatively to agricultural wages. Sudden migration may, however, incur some costs on the household and the migrant that we completely ignore.

3 Concluding remarks

What do we learn from this paper? Drawing on a rich panel data set and an objective treatment indicator, we show that, in the aftermath of a large shock, households suffer significant negative impacts on their main source of income, but alleviate part of the losses thanks to transfers sent by internal labor migrants. Interestingly, we find evidence that the effectiveness of different support networks increases with the spatial distance between the affected household and the sender: While local networks apparently break down in the wake of the shock, networks with far-distance labor migrants seem to compensate partly for this.

Our most important result comes from the analysis of those transfers. While it is true that households with an already-established migrant receive more, households without migrants before the shock are more likely to send one when they are affected. Conditional on having one migrant, both types of households receive the same amount. Indeed, newly-sent migrants are very similar to established ones and their labor income is only slightly lower. The unique explanation behind the wage gap is that newly-sent migrants need to find jobs very quickly, invest less in their job search (they go more often to big cities and contact less job agencies) and end up in a match of lower quality. Despite these differences, the wage gap remains low: One reason is that labor demand in cities is very elastic in sectors that do not require education or specific skills such as construction. As such, labor migration seems a safe outside option when the agricultural activity is affected.

Several policy implications emerge from this study. Households are quite unable to cope with income losses except through transfers sent by labor migrants. However, our results also indicate that, even for households who receive support from their labor migrant networks, a share of uninsured risk remains. In contrast to industrialized countries, publicly provided assistance and commercial insurance solutions do not play any role for the average household. Being able to track impacts and outcomes of a typhoon in real-time today, we believe that our results might have implications for improving existing disaster relief programs in the immediate aftermath as well as for the effective design of public or commercial insurance solutions.

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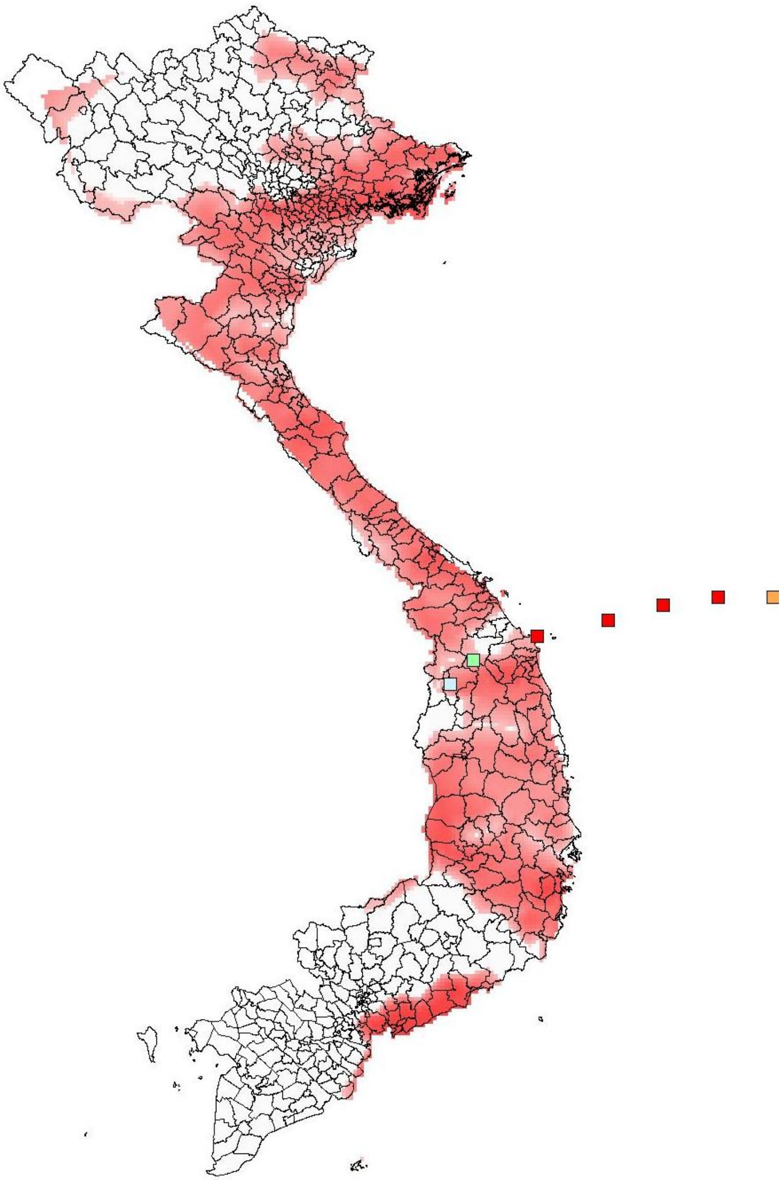
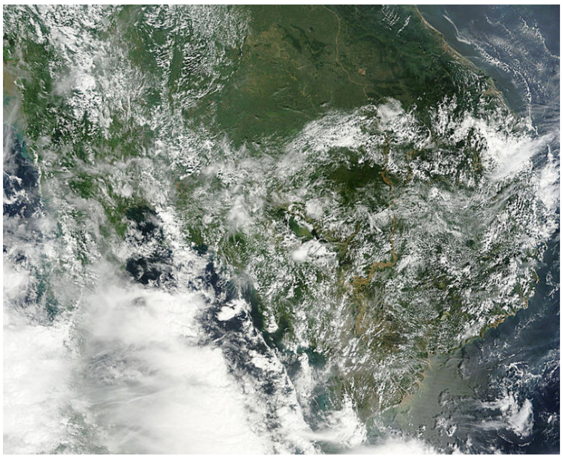
A Figures and tables

Figure 1. Track of typhoon Ketsana and the aggregate propensity to be affected (1945-2006)

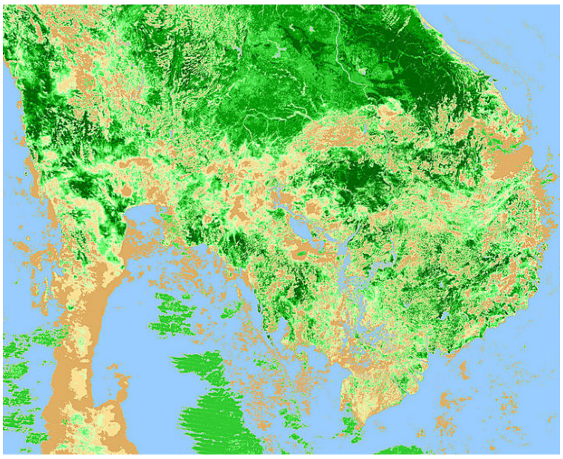


Source: Joint Typhoon Warning Center (JTWC). The propensity to be affected is the average annual percentage of a district area at most 50kms from the passing of a tropical typhoon (author's calculations). The track is represented by the location of the typhoon's eye every 6 hours.

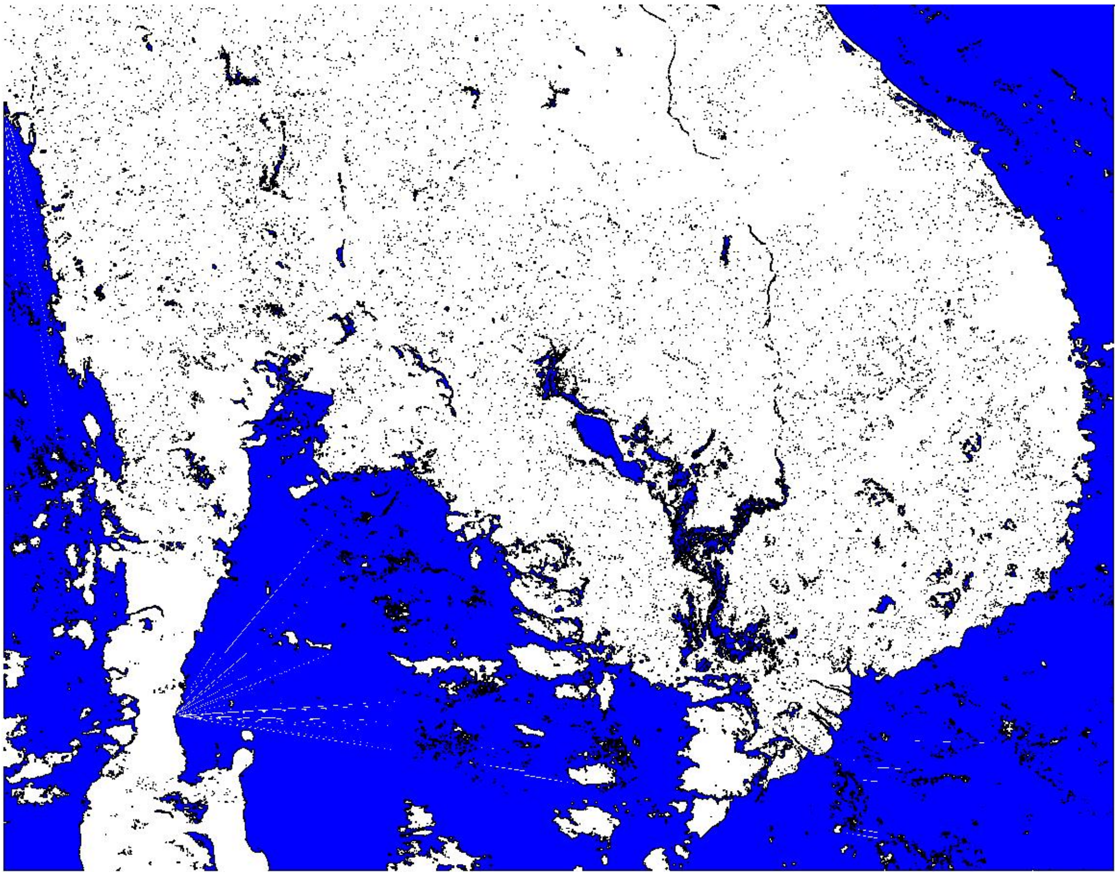
Figure 2. Satellite image – treatment (06/10/2009).



(a) Raw.



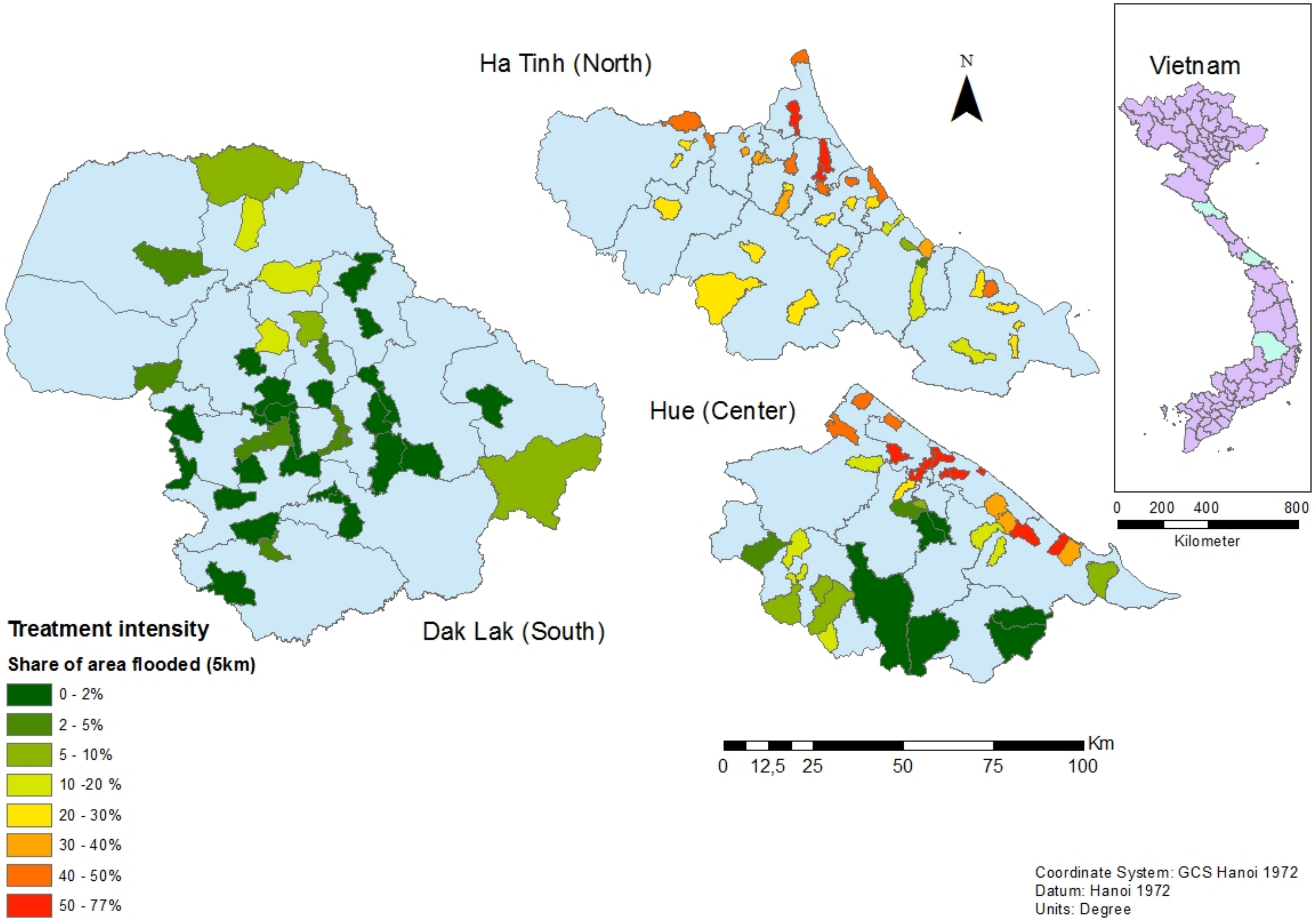
(b) NDVI band visualization.



(c) Surface water.

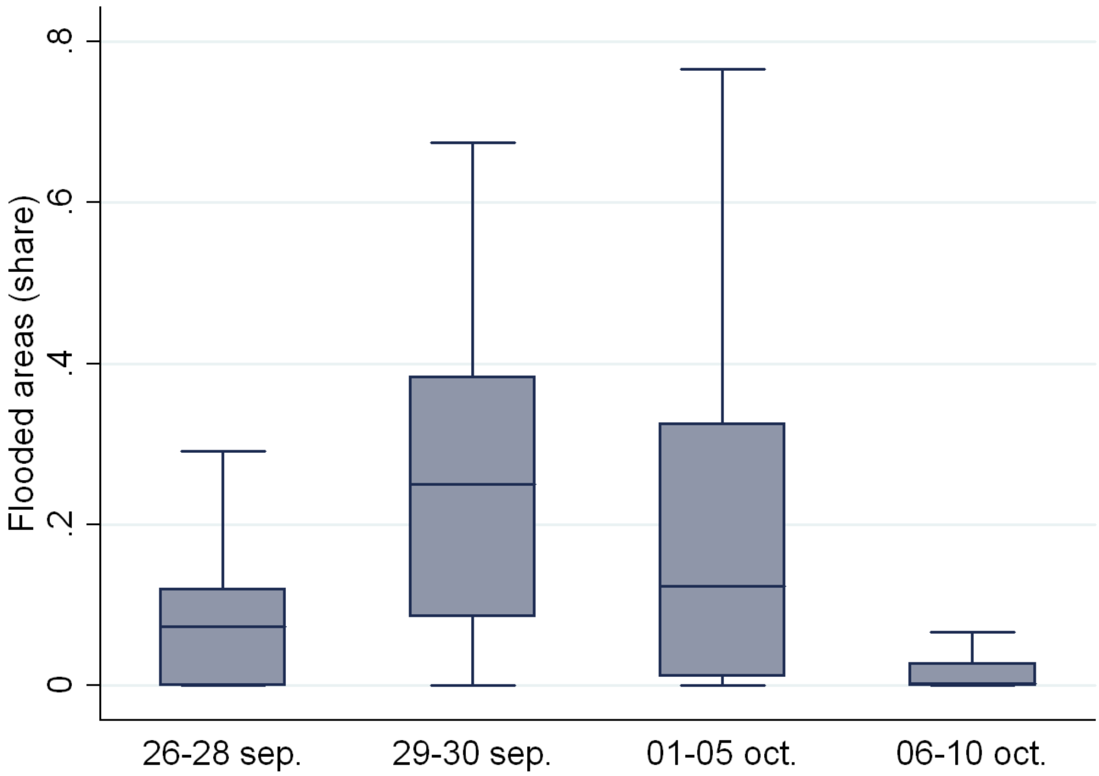
Sources: MODIS subsets (Indochina, 6/10/2009), http://earthdata.nasa.gov/data/near-real-time-data/rapid-response/modis-subsets.

Figure 3. Treatment intensity across subdistricts.



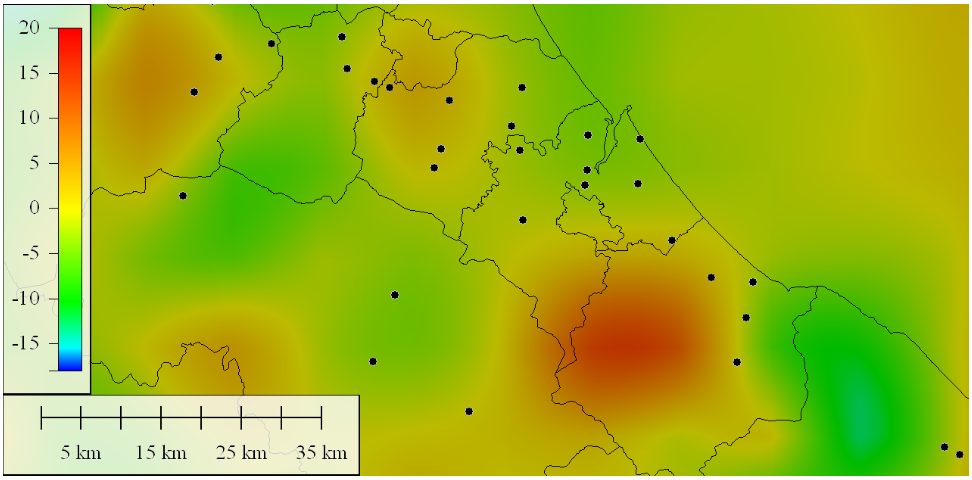
Source: Authors's calculations based on MODIS inundation data in the aftermath, i.e. day three to eight after landfall of the typhoon in Vietnam. Each cluster represents a sub-district illustrating the average treatment intensity (share of are flooded around sample locations) for two villages following our preferred specification of 5 km radius.

Figure 4. Share of area flooded across time (distribution over the sample).

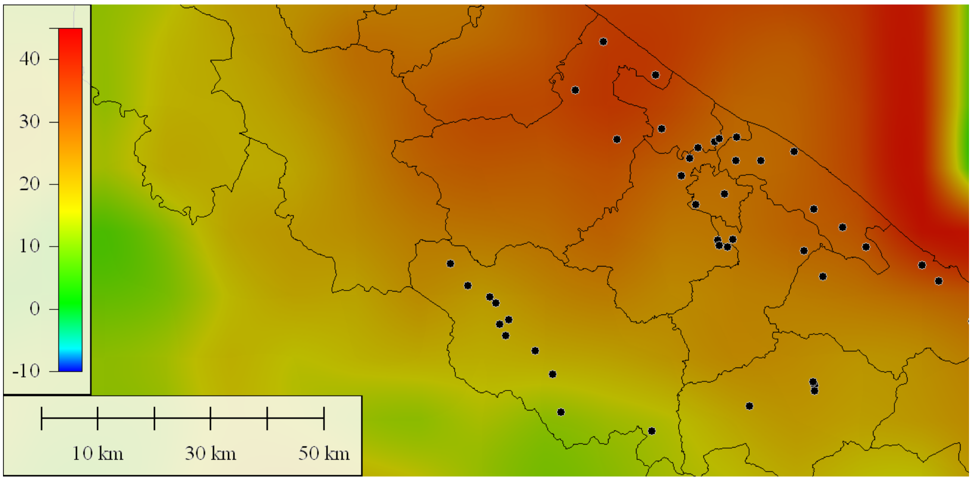


Sources: Author's calculations based on MODIS inundation data. Reminder: our treatment measure is the share of area flooded between 01-05 october, our propensity measure is the share of area flooded between 26-28 september and between 06-10 october. We exclude observations on 29-30 october from the analysis.

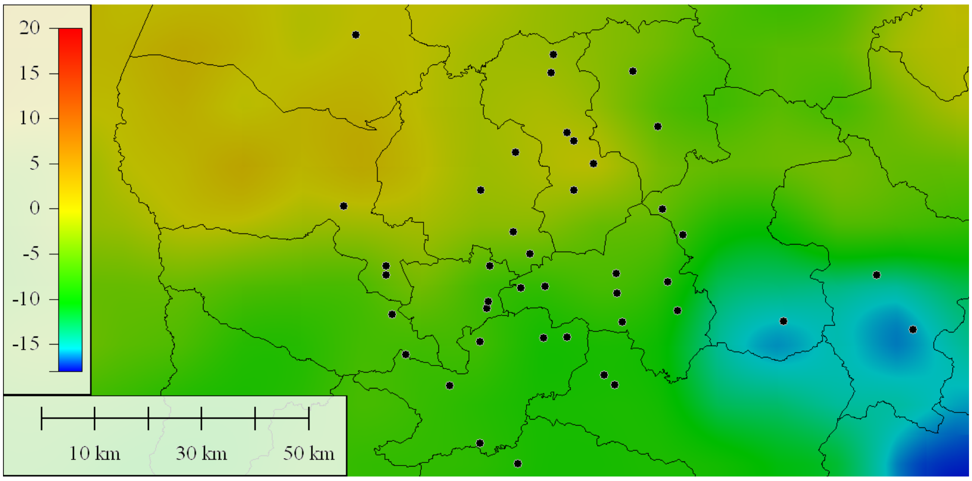
Figure 5. Rainfall intensity during the passing of Ondoy.



(a) Ha Tinh province.



(b) Tienh Hue province.



(c) Dak Lak province.

Sources: NOAA Climate Prediction Center (South Asia, Excess rainfall estimates on 29-30 sep. 2009 compared to 26-27 sep. and 01-06 oct.)

Table 1. Descriptive statistics by province in 2008.

Sample	All		Ha Tinh		Hue		Dak Lak	
	Obs.	Mean	Obs.	Mean	Obs.	Mean	Obs.	Mean
Household Demographics								
Household Size	2,000	4.4	657	3.9	644	4.5	699	4.75
No. Men (16-59)	2,148	1.22	713	1.02	699	1.22	736	1.41
Dependency ratio	2,148	.63	713	.57	699	.67	736	.64
Household Head								
Main occupation: farmer	2,197	.65	720	.67	699	.56	756	.70
Age	2,148	49.0	713	52.4	699	49.4	736	45.2
Years of schooling	2,079	6.7	707	8.37	689	5.50	683	6.27
Female	2,148	.16	713	.17	699	.17	736	.15
Household Income								
Total income (USD)	2,073	6,090	700	5,481	662	4,236	711	8,415
Agricultural income (USD)	2,148	2,467	713	1,450	699	890	736	4,951
Crop income Summer /Autum (USD)	2,148	417	713	403	699	385	736	460
Paddy income Summer/Autum (USD)	2,148	219	713	179	699	234	736	242
Crop income Winter (USD)	2,148	212	713	70	699	255	736	309
Paddy income Winter (USD)	2,148	148	713	7	699	161	736	270
Self-employment income (USD)	2,148	920	713	645	699	1,015	736	1,095
Labor income (USD)	2,148	793	713	692	699	828	736	857
Formal transfers (USD)	2,148	434	713	564	699	429	736	314
Transfers relatives (USD)	2,148	181	713	246	699	192	736	108
Household Finance								
Total borrowing (USD), stock	2,197	2,832	720	2,783	720	1,882	757	3,782
Total savings (USD), stock	2,197	234	720	262	720	120	757	316
Household Expenditure								
Total expenditure (USD)	2,000	4,976	657	4,362	644	4,708	699	5,797
Food expenditure (USD)	2,000	2,556	657	2,126	644	2,528	699	2,986
Non-food expenditure (USD)	2,000	1,829	657	1,576	644	1,730	699	2,157
Education expenditure (USD)	2,000	283	657	382	644	186	699	280
Health expenditure (USD)	2,000	180	657	171	644	132	699	233
Migration								
Migration incidence (% of hh)	2,148	.379	713	.49	699	.36	736	.29
No. migrants per hh	2,148	.614	713	.76	699	.64	736	.45
Total transfers received (USD)	2,148	340	713	533	699	129	736	354
Total transfers sent (USD)	2,148	168	713	284	699	130	736	91
Long-distance labor migration incidence	2,148	.21	713	.31	699	.22	736	.11
No. long-distance labor migrants	2,148	.30	713	.41	699	.37	736	.14
Net transfer long-distance labor migrats (USD)	2,148	64	713	121	699	106	736	-31
Local labor migration incidence	2,148	.048	713	.045	699	.034	736	.06
No. local labor migrats	2,148	.062	713	.062	699	.043	736	.08
Net transfer local labor migrats (USD)	2,148	6.8	713	-6.5	699	-4.3	736	30.2

Source: Panel - 2008. All monetary variables are expressed in total USD (PPP) per household.

Table 2. Treated versus control districts in 2008.

	Treated [696]	Control [1,304]	Difference	
			Value D	$P(\|D\| > 0)$
	Household Income			
Income per cap.	1403.82	1312.10	91.72	[0.250]
Crop	496.05	453.47	42.58	[0.260]
Crop (Summer)	101.50	89.23	12.27	[0.234]
Wage	189.51	205.83	-16.32	[0.510]
Subsidies	128.86	124.18	4.68	[0.785]
	Consumption			
Consumption per cap.	1302.7	1223.4	79.32	[0.073]
Food	663.75	610.45	53.29	**[0.007]**
Non-food	244.41	248.52	-4.11	[0.797]
Health	60.71	44.40	16.30	**[0.017]**
Education	71.51	70.84	0.67	[0.930]
	Remittances			
Remittances per cap.	20.71	31.81	-11.10	[0.637]
labor migrants (same district)	2.67	1.43	1.24	[0.560]
labor migrants (other district)	22.37	15.18	7.18	[0.347]
labor migrants (other province)	20.82	13.41	7.40	[0.231]
	Other smoothing instruments			
Transfers from friends per cap.	44.14	33.09	11.04	[0.337]
Savings per cap.	65.68	59.29	6.38	[0.458]
Borrowing per cap.	558.78	567.42	-8.64	[0.489]

Source: Panel - 2008. All variables are expressed in USD (PPP) per capita, i.e., adjusted by the number of permanent household members excluding migrants.

Table 3. Income and consumption losses p.c. due to the treatment.

PANEL A	Income	Crop income				
		All	SA	SA pad.	W	W pad.
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-547.32*	-357.33**	-102.50**	-71.29**	47.45	12.11
$T_{v,2010}$	(321.34)	(176.31)	(52.69)	(31.79)	(79.76)	(58.23)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,926	3,925	3,922	3,925	3,925	3,925

PANEL B	Consumption				
	Total	Food	Non-food	Health	Educ.
	(1)	(2)	(3)	(4)	(5)
Treatment	-204.82	-173.63*	48.85	-27.53	-19.61
$T_{v,2010}$	(175.52)	(100.06)	(31.97)	(41.81)	(49.36)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	3,926	3,926	3,926	3,926	3,926

Robust standard errors in parentheses, clustered at the sub-district level. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. All variables are expressed in USD (PPP) per capita, i.e., adjusted by the number of permanent household members excluding migrants. SA/W stand for Summer-Autumn/Winter and pad. indicate paddy production.

Table 4. Income losses due to the treatment – other activities.

	Income			
	Livestock	Hunting	Wages	Self-employment
	(1)	(2)	(3)	(4)
Treatment $T_{v,2010}$	-28.15	16.85	-69.77	-88.71
	(97.77)	(35.00)	(151.69)	(146.62)
Controls	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes
Observations	3,925	3,921	3,923	3,913

Robust standard errors in parentheses, clustered at the village level. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. All variables are expressed in USD (PPP) per capita, i.e., adjusted by the number of permanent household members excluding migrants.

Table 5. Transfers from labor migrants in response to the treatment.

	Labor migrant transfers		
	same dis.	diff. dis.	diff. pro.
	(1)	(2)	(3)
Treatment	-42.67**	166.16***	98.91**
$T_{v,2010}$	(18.36)	(56.63)	(44.35)
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Observations	3,926	3,926	3,926

Robust standard errors in parentheses, clustered at the sub-district level. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. Labor migrants are members having spent at least 6 months away for work purposes in the same district relative to the household of origin (column 1), different district (column 2) or different province (column 3). All variables are expressed in USD (PPP) per capita, i.e., adjusted by the number of permanent household members excluding migrants.

Table 6. Transfers from other third parties (non-labor migrants, friends, public redistribution, insurance) and borrowing/dissaving.

PANEL A	Transfers			
	Non-labor mig.	Family/Friends	Public	Insurance
	(1)	(2)	(3)	(4)
Treatment	-134.69	89.99	-104.70*	6.37
$T_{v,2010}$	(92.62)	(103.31)	(61.05)	(12.88)
Controls	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes
Observations	3,926	3,925	3,925	3,925

PANEL B	Borrowing		Dissaving	
	Formal	Informal	Liquid assets	Tangible assets
	(1)	(2)	(3)	(4)
Treatment	-266.35	-38.49	217.65	69.54
$T_{v,2010}$	(214.09)	(153.81)	(138.36)	(237.07)
Controls	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes
Observations	3,926	3,926	3,921	3,920

Robust standard errors in parentheses, clustered at the sub-district level. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. All variables are expressed in USD (PPP) per capita, i.e., adjusted by the number of permanent household members excluding migrants.

Table 7. Transfers from labor migrants in response to the treatment – subsamples without/with established migrants in 2008.

PANEL A	Labor migrant transfers					
	same dis.		diff. dis.		diff. pro.	
	Presence	Amount	Presence	Amount	Presence	Amount
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.104*	-29.12*	0.186**	81.17**	0.192**	68.78**
$T_{v,2010}$	(0.055)	(15.74)	(0.092)	(35.87)	(0.087)	(29.49)
Sample	*Without s.d. migrants*		*Without d.d. migrants*		*Without d.p. migrants*	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,744	3,744	3,092	3,092	3,195	3,195

PANEL B	Labor migrant transfers					
	same dis.		diff. dis.		diff. pro.	
	Presence	Amount	Presence	Amount	Presence	Amount
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.221	-362.52	0.101	370.31***	0.023	196.37*
$T_{v,2010}$	(0.520)	(302.21)	(0.168)	(137.61)	(0.212)	(120.08)
Sample	*With s.d. migrants*		*With d.d. migrants*		*With d.p. migrants*	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	182	182	834	834	731	731

Robust standard errors in parentheses, clustered at the sub-district level. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. Labor migrants are members having spent at least 6 months away for work purposes in the same district relative to the household of origin (column 1&2), different district (column 3&4) or different province (column 5&6). All monetary variables are expressed in USD (PPP) per capita, i.e., adjusted by the number of permanent household members excluding migrants.

Table 8. New labor migrants versus established labor migrants in 2010.

	Established [233]	New [149]	Difference	
			Value D	$P(\|D\| > 0)$
Monthly icome	495.57	363.37	132.20	**[0.000]**
	Services and industry			
Fraction of migrants	.828	.845	-.017	[0.657]
Monthly income	507.20	374.60	132.59	**[0.000]**
	Agriculture			
Fraction of migrants	.064	.046	.017	[0.477]
Monthly income	331.57	253.79	77.77	[0.318]
	Public sector			
Fraction of migrants	.120	.114	.006	[0.857]
Monthly income	405.93	304.58	101.35	**[0.090]**
	Migrant characteristics			
Age	26.43	25.14	1.29	**[0.090]**
Male	.608	.605	-.003	[0.939]
Education (¿9th grade)	.513	.554	-.041	[0.414]
	Match migrant-job			
Hanoi, Ho-Chi-Minh City	.358	.359	-.001	[0.976]
Tenure	2.55	.564	1.90	**[0.000]**
Permanent contract	.405	.353	.051	[0.310]
Skilled job	.310	.213	.097	**[0.037]**
	Job search			
Search more than 1 week	.521	.520	.001	[0.976]
Average search time (weeks)	1.57	1.05	.51	**[0.094]**
Information (own search)	.456	.386	.065	[0.176]

Source: Panel - 2010.

Table 9. New labor migrants (treated) versus new labor migrants (non-treated) in 2010.

	Treated [57]	control [92]	Difference	
			Value D	$P(\|D\| > 0)$
Monthly icome	318.11	391.73	-73.61	**[0.071]**
	Services and industry			
Fraction of migrants	.877	.826	.051	[0.404]
Monthly income	313.09	415.60	-102.51	**[0.023]**
	Agriculture			
Fraction of migrants	.035	.054	-.019	[0.592]
Monthly income	192.98	278.12	-85.13	[0.171]
	Public sector			
Fraction of migrants	.087	.130	-.042	[0.857]
Monthly income	418.32	257.19	161.12	[0.110]
	Migrant characteristics			
Age	25.15	25.13	0.02	[0.986]
Male	.676	.519	.106	[0.184]
Education (¿9th grade)	.513	.574	-.055	[0.516]
	Match migrant-job			
Hanoi, Ho-Chi-Minh City	.481	.289	.191	**[0.018]**
Permanent contract	.385	.347	.038	[0.640]
Skilled job	.140	.260	-.120	**[0.082]**
	Job search			
Search more than 1 week	.368	.630	-.262	**[0.001]**
Average search time (weeks)	.728	1.23	-.510	[0.103]
Information (own search)	.263	.467	-.204	**[0.012]**

Source: Panel - 2010.

II. Appendix

Table A1. Income losses and transfers from labor migrants in response to the treatment (IV).

FIRST STAGE	Treatment $T_{v,2010}$
Rain (cm, 29-30/09/2009)	.003578***
$R_{v,2010}$	(.00130)
Controls	Yes
Fixed effects	Yes
Observation	3,848
Cragg-Donald F statistic	16.289

SECOND STAGE	Crop income		Labor migrant transfers		
	All	Paddy (summer)	same dis.	diff. dis.	diff. pro.
	(1)	(2)	(3)	(4)	(5)
Treatment	-1148.50	-291.28**	-157.20*	560.82**	458.94**
$T_{v,2010}$	(755.59)	(135.22)	(84.75)	(263.21)	(233.34)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	3,848	3,848	3,848	3,848	3,848

Robust standard errors in parentheses, clustered at the sub-district level. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. All variables are expressed in USD (PPP) per capita, i.e., adjusted by the number of permanent household members excluding migrants. Labor migrants are members having spent at least 6 months away for work purposes in the same district relative to the household of origin (column 3), different district (column 4) or different province (column 5).

Table A2. Income losses and transfers from labor migrants in response to the treatment (treatment is rainfall instead of flooded areas).

	Crop income		Labor migrant transfers		
	All	Paddy (summer)	same dis.	diff. dis.	diff. pro.
	(1)	(2)	(3)	(4)	(5)
Rain (cm, 29-30/09/2009)	-4.369*	-1.054**	-.630*	2.102**	1.770**
$R_{v,2010}$	(2.316)	(.442)	(.344)	(.946)	(.855)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	3,915	3,915	3,915	3,915	3,915

Robust standard errors in parentheses, clustered at the sub-district level. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. All variables are expressed in USD (PPP) per capita, i.e., adjusted by the number of permanent household members excluding migrants. Labor migrants are members having spent at least 6 months away for work purposes in the same district relative to the household of origin (column 3), different district (column 4) or different province (column 5).

Table A3. Transfers from labor migrants in response to the treatment – placebo checks with the waves 2007 and 2008.

	Labor migrant transfers		
	same dis.	diff. dis.	diff. pro.
	(1)	(2)	(3)
Treatment	17.29	-44.09	-14.64
$T_{v,placebo}$	(15.05)	(42.95)	(39.75)
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Observations	3,913	3,913	3,913

Robust standard errors in parentheses, clustered at the sub-district level. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. All variables are expressed in USD (PPP) per capita, i.e., adjusted by the number of permanent household members excluding migrants. Labor migrants are members having spent at least 6 months away for work purposes in the same district relative to the household of origin (column 1), different district (column 2) or different province (column 3). We run our baseline specification but we consider the waves 2007 and 2008 instead of 2008 and 2010.

Table A4. Income, consumption and transfers along the treatment – robustness checks (logarithmic specifications and normalized transfers).

PANEL A	Income (log)	Consumption (log)	Food consumption (log)
	(1)	(2)	(3)
Treatment	-0.257	-0.174	-0.261*
$T_{v,2010}$	(0.206)	(0.121)	(0.140)
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Observations	3,745	3,926	3,926
PANEL B	Labor migrant transfers (transfer/income)		
	same dis.	diff. dis.	diff. pro.
	(1)	(2)	(3)
Treatment	-.0045	0.0650*	0.0531*
$T_{v,2010}$	(0.0028)	(0.0336)	(0.0307)
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Observations	3,926	3,926	3,926

Robust standard errors in parentheses, clustered at the sub-district level. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. Labor migrant transfers are remittances per unit of household income from members having spent at least 6 months away.

Table A5. Transfers from labor migrants in response to the treatment – robustness checks controlling for mountains, coasts, elevation, slope, valley and rivers (panel A) and long-term propensity to be affected by typhoons (panel B).

PANEL A	Labor migrant transfers		
	same dis.	diff. dis.	diff. pro.
	(1)	(2)	(3)
Treatment	-28.92*	177.03***	102.08**
$T_{v,2010}$	(17.11)	(60.49)	(46.57)
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Observations	3,926	3,926	3,926

PANEL B	Labor migrant transfers		
	same dis.	diff. dis.	diff. pro.
	(1)	(2)	(3)
Treatment	-5.07	125.36***	92.80**
$T_{v,2010}$	(15.76)	(48.04)	(39.39)
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Observations	3,926	3,926	3,926

Robust standard errors in parentheses, clustered at the sub-district level. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. All variables are expressed in USD (PPP) per capita, i.e., adjusted by the number of permanent household members excluding migrants. Labor migrants are members having spent at least 6 months away for work purposes in the same district relative to the household of origin (column 1), different district (column 2) or different province (column 3).

Table A6. Transfers from labor migrants in response to the treatment – robustness checks for migrants more than 3 months away.

	Labor migrant transfers		
	same dis.	diff. dis.	diff. pro.
	(1)	(2)	(3)
Treatment	-31.54**	167.11***	104.81**
$T_{v,2010}$	(13.24)	(52.35)	(40.87)
Controls	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes
Observations	3,926	3,926	3,926

Robust standard errors in parentheses, clustered at the sub-district level. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. All variables are expressed in USD (PPP) per capita, i.e., adjusted by the number of permanent household members excluding migrants. Labor migrants are members having spent at least 3 months away for work purposes in the same district relative to the household of origin (column 1), different district (column 2) or different province (column 3).

Table A7. Transfers from labor migrants in response to the treatment – subsamples without established migrants – robustness checks (ratio net remittances to total income).

	Labor migrant transfers/income					
	same dis.		diff. dis.		diff. pro.	
	Presence	Ratio	Presence	Ratio	Presence	Ratio
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.104*	-0.002	0.167*	0.030	0.198**	0.031*
$T_{v,2010}$	(0.055)	(0.002)	(0.095)	(0.019)	(0.087)	(0.017)
Sample	*Without s.d. migrants*		*Without d.d. migrants*		*Without d.p. migrants*	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,744	3,744	3,099	3,099	3,195	3,195
PANEL B	Labor migrant transfers/income					
	same dis.		diff. dis.		diff. pro.	
	Presence	Ratio	Presence	Ratio	Presence	Ratio
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.221	-0.051	0.101	0.158*	0.023	0.142
$T_{v,2010}$	(0.520)	(0.054)	(0.168)	(0.091)	(0.212)	(0.097)
Sample	*With s.d. migrants*		*With d.d. migrants*		*With d.p. migrants*	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	182	182	834	834	731	731

Robust standard errors in parentheses, clustered at the sub-district level. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. Transfers are expressed as ratios to total income.

Table A8. Income and consumption losses due to the treatment – comparison between households with and without established migrants.

	Crop income			
	All		Paddy (summer)	
	(1)	(2)	(3)	(4)
Treatment	-423.49**	-375.84*	-63.93*	-57.81*
$T_{v,2010}$	(209.84)	(206.65)	(35.21)	(34.75)
Treatment × Migrant (diff. dis.)	224.56		-22.92	
$T_{v,2010}$	(187.02)		(40.81)	
Treatment × Migrant (diff. pro.)		83.74		-46.84
$T_{v,2010}$		(165.39)		(43.13)
Controls	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes
Observations	3,925	3,925	3,925	3,925

Robust standard errors in parentheses, clustered at the sub-district level. ***: $p < 0.01$, **: $p < 0.05$, *: $p < 0.1$. All variables are expressed in USD (PPP) per capita, i.e., adjusted by the number of permanent household members excluding migrants. Migrants are members having spent at least 6 months away for work purposes in the same district relative to the household.

Table A9. Treated versus control districts in 2008 for the sample without migrants in 2008.

	Treated [512]	Control [1,066]	Difference	
			Value D	$P(\|D\| > 0)$
	Household Income			
Income per cap.	1380.79	1263.52	117.26	[0.222]
Crop	512.86	461.52	51.33	[0.252]
Crop (Summer)	84.63	83.91	.721	[0.942]
Wage	195.06	211.78	-16.71	[0.576]
Subsidies	119.04	119.87	-.821	[0.963]
	Consumption			
Consumption per cap.	1256.30	1150.91	105.39	**[0.028]**
Food	648.04	594.68	53.36	**[0.018]**
Non-food	236.45	225.87	10.57	[0.337]
Health	55.97	41.41	14.55	**[0.040]**
Education	64.38	61.99	2.39	[0.758]
	Smoothing instruments			
Transfers from friends per cap.	45.33	25.14	20.19	[0.107]
Savings per cap.	62.24	62.19	.047	[0.998]
Borrowing per cap.	518.57	556.27	-37.70	[0.398]

Source: Panel - 2008. All variables are expressed in USD (PPP) per capita, i.e., adjusted by the number of permanent household members excluding migrants.

Table A10. Treated versus control districts in 2008 for the sample with migrants in 2008.

	Treated [184]	Control [238]	Difference	
			Value D	$P(\|D\| > 0)$
	Household Income			
Income per cap.	1465.85	1522.40	-56.55	[0.670]
Crop	450.59	417.47	33.11	[0.630]
Crop (Summer)	148.35	113.01	35.33	[0.256]
Wage	174.08	179.21	-5.13	[0.898]
Subsidies	156.18	143.50	12.67	[0.780]
	Consumption			
Consumption per cap.	1431.97	1548.20	-116.22	[0.275]
Food	707.44	681.08	26.35	[0.522]
Non-food	266.54	349.95	-83.41	[0.172]
Health	73.89	57.80	16.09	[0.387]
Education	91.35	110.50	-19.14	[0.372]
	Transfers			
labor migrants (other district)	84.62	75.58	9.03	[0.784]
	Other smoothing instruments			
Transfers from friends per cap.	40.83	68.71	-27.87	[0.319]
Savings per cap.	75.26	46.34	28.92	[0.392]
Borrowing per cap.	669.78	617.36	52.41	[0.557]

Source: Panel - 2008. All variables are expressed in USD (PPP) per capita, i.e., adjusted by the number of permanent household members excluding migrants. Labor migrants are members having spent at least 6 months away for work purposes in a different district.