ABSTRACT

Title of dissertation: ESSAYS ON ENVIRONMENTAL SHOCKS IN DEVELOPING COUNTRIES: ADAPTATION OF SOCIAL PROTECTION, MIGRATION AND LABOR

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Environmental shocks, particularly high impact natural disasters, such as earthquakes, floods and droughts, test the boundaries of social resilience and vulnerability. According to the International Disaster Database (EM-DAT), from 1975-2011 the number of natural disasters reported worldwide, along with the number of households affected, gradually increased over this period (Natural Disaster Trends, Center for Research on the Epidemiology of Disasters (CRED)). Economic status of a country did not predict the number of disasters a country faced. However, findings indicate that countries with lower incomes (Kahn (2005), Stromberg (2007), Keefer et al. (2011)) and countries with greater income inequality (Anbarci et al. (2005)) encounter more casualties and greater economic damage. It is therefore important to understand ways in which communities in lower income countries can cope with such community level shocks, as this can then point to changes that can be made to help these countries better cope with environmental shocks.

This dissertation is comprised of three applied essays focusing on identifying consequences of environmental shocks related to social protection, migration and labor in developing countries. Recent literature on environmental shocks in low-income countries have focused on improving the measurement of such shocks to avoid common identification issues. The essays in this dissertation provide empirical and methodological contributions to a growing literature on measuring and understanding the implications of environmental shocks.

In the first essay I address a gap in the current analytical literature on the effectiveness of decentralized targeting of social safety nets (often delivered in the same way as humanitarian aid) in insuring households against disaster risk. I combine survey data from Indonesia with geological earthquake data to determine if village leaders change the pattern of distribution of a subsidized rice program intended for the poor in earthquake affected villages. My findings suggest that the central government targets more safety net resources to earthquake villages, but access to these resources declines for its intended poorest beneficiaries, and targeting is worse in communities with higher social capital. I discuss how these findings may be linked to bargaining power assigned by village leaders to poor and non-poor recipients, which can be a function of the leader's personal benefit, either electorally or through reciprocity expected from social contacts or family members to whom the leader provides access.

The second essay examines migration as a key mode of adaptation to extreme floods and droughts, and investigates the impact of weather-driven internal migration on local labor markets in Nepal. In this essay the identification and methodology used by Boustan et al. (2010) is modified to a dynamic framework to fit the contextual setting of the study. We combine survey data from Nepal with $0.5x0.5$ degree gridded satellite based weather data to identify weather anomalies and then create instruments for local migration in Nepal. Our analysis of the impacts of local migration on labor markets finds native wage losses are slightly larger than those observed in the U.S. and elsewhere. Labor substitution is imperfect in Nepal, as migrants appear more skilled than the average native worker in hosting communities. These results suggest imperfect substitution coupled with fixed labor demand in the formal sector may partially explain why wage losses are more pronounced here than in other settings. We also find strong negative effects of migration on wages of low-skilled workers and informal sector employment. This is consistent with a displacement of low-skilled workers out of the labor markets. Highly skilled migrants may have to accept lower-skilled jobs to integrate into the labor markets and therefore, push low-skilled natives out of the labor markets.

The third essay identifies the detrimental impacts of crop shocks, predominantly floods and droughts, on secondary school aged youth (aged 14 to 19) in Tanzania. While a large body of literature has focused on the causes and consequences of child (aged 7 to 13) labor very little is known about the impact of transitory shocks on youth. I find that crop shocks may increase youth labor significantly, and be particularly detrimental to school attendance of youth enrolled in school. Youth enrolled in school increase unpaid labor to substitute for the paid labor taken up by others in the household. These results also indicate that female youth are disproportionately more likely to engage in unpaid labor and miss school compared to male youth. I also identify that while youth schooling outcomes are affected by shocks, child schooling is not affected. These research findings suggest that more attention needs to be paid to short and long term consequences of shocks for youth.

ESSAYS ON ENVIRONMENTAL SHOCKS IN DEVELOPING COUNTRIES: ADAPTATION OF SOCIAL PROTECTION, MIGRATION AND LABOR

by

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Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2014

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Dedication

To amma and papa, for your countless sacrifices that have given me a brighter future.

To Lasitha.

To Harin (aiya).

Acknowledgments

First and foremost I'd like to thank my advisor, Vivian E. Hoffmann, for allowing me the freedom to explore on my own, while encouraging me and giving me sound academic guidance when I faltered. She has taught me to tackle difficult roadblocks without giving up, question my thoughts, and express my ideas even when I lacked confidence to do so. I cannot thank her enough for always being available for help and advice. I am extremely grateful for the opportunity I had to work with, and learn from such a insightful and well-rounded economist like Vivian.

I am very indebted to Kenneth E. McConnell for his academic guidance, patience, and encouragement during many topic changes and tangential discussions. I am inspired by Prof. Mcconnell's intellect and his ability to make students aspire to communicate ideas clearly, while giving a lot of thought to the empirics and theory of a topic. He has shown me the value of being able to tell a simple story about your work, that can be understood by not just a fellow economist but by any individual.

I owe my gratitude to Kenneth L. Leonard, for feedback over the years. I have learnt a lot in development economics from Ken's passionate teaching and discussions. I am thankful to committee members Howard Leathers and Feinian Chen for comments and feedback, and extend my gratitude for sparing their invaluable time to review the manuscript. I am also grateful to many faculty who have contributed to my academic training and research. I would like to thank Lars Olson, Ramon Lopez, Richard Just, Pamela Jakiela and Sarah Adelman, my AREC peers, and participants of the development economics seminars for many fruitful interactions and learning experiences.

I am deeply indebted to Valerie Mueller for hiring me to work with and for her at IFPRI. Working for Valerie truly enhanced my graduate school experience. I learnt a lot from working with Valerie, and her enthusiasm, intellect and commitment to research made me rediscover my passion for development economics. I am indebted to Valerie for her mentorship. I am also very thankful for the opportunity to work with Jean-Francois Maystadt. Jean-Francois's patience with research, impressive econometric knowledge, humility and positive attitude has taught me a great deal. I am extremely grateful to Valerie and Jean-Francois for letting me use some of our research in one chapter of this dissertation.

I am grateful to my undergraduate professors, Janet Ceglowski, Harriet Newburger and Ronda Hughes, for their encouragement to pursue doctoral studies. I would like to thank my classmates, especially Romina, Pinar, Rubaba, Claudia and Yoanna for their academic and social support and friendship while pursuing this long journey with me. I would also like to thank my friends outside the department for cheering me along. I am grateful to my brother Harin for his endless support and encouragement, and being available for my last minute queries. Thank you to my husband Lasitha and my parents; my gratitude to you cannot be put into words.

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Chapter 1: Can Natural Disasters Affect Decentralized Targeting of Social Safety Nets Intended For the Poor?: Evidence from Earthquakes in Indonesia

Abstract

Natural disaster shocks are highly destructive in underdeveloped countries. Social safety nets may be particularly important in the face of such shocks for insuring households. This paper examines the targeting of a rice subsidy program (RASKIN) with decentralized village level targeting, in the aftermath of earthquakes in Indonesia. I find earthquake affected villages benefit more from RASKIN. However, access of the poorest is nearly 12% lower relative to such households in unaffected areas in the post earthquake period. The non-poor in earthquake affected villages are more likely to participate by 6% to 13% relative to the poorest. While the non-poor face larger relative losses than the poor during an earthquake, both consumption and assets of the non-poor remain above the poor in absolute terms. I also explore heterogenous effects of earthquake shocks on safety net targeting by village social capital. It is widely believed that social capital is associated with better governance. However, in this case, higher pre-disaster social capital does not increase access of the poorest to RASKIN. The results of this paper suggest that decentralized targeting of social insurance may not be effective at reaching intended beneficiaries in the context of natural disasters.

JEL Classification: Q54; I38; D63
Keywords: Natural Disasters; Earthquakes; Social Safety Nets; Social Capital; Poverty; Inequality

1.1 Introduction

Large scale disasters create a problem of asymmetric information in which identifying and targeting aid or social safety net resources towards households in need can be challenging. Nonetheless, it is important to understand how decision makers, such as village leaders, target resources in the aftermath of a crisis. In this paper I focus on the targeting of social safety net resources within villages impacted by earthquakes in Indonesia. Such safety net programs may exist within villages prior to disasters, and can be adopted to address changes household needs arising from disaster shocks (Pritchett et al. (2002),de Janvry et al. (2006), Pelham et al. (2011)).

Examining Indonesia's largest subsidized rice for poor safety net program, I first question if the central government uses the program as a coping mechanism to allocate more resources to disaster affected villages. Second, I examine if resources distributed within villages by village authorities are diverted away from the program's intended poor beneficiaries[1]. To fully explain the observed resource distribution pattern I test whether targeting of safety net resources is linked to the impact of earthquakes on households consumption and asset losses. Lastly, I consider heterogenous impacts of earthquake shocks on the distribution of safety net resources by the level of pre-disaster social capital within villages.

By addressing these questions I aim to fill a gap in the existing literature on the effectiveness of social safety nets or humanitarian aid in insuring households, mostly the poor, against disaster risk. There are a few studies that examine related issues. de Janvry et al. (2006) consider the role of Mexico's conditional cash transfer program as a safety net in affecting child schooling and labor post shocks. Morris and Wodon (2003) analyze the targeting mechanisms of disaster relief funds post hurricane Mitch

[1]The response to this question has implications not just for the distribution of decentralized safety net resources but also for distribution of post-disaster humanitarian aid,distributed in a similar way. de Silva (2009) and Aldrich (2010) discuss inequality in the distribution of aid post Tsunami in Sri Lanka and India, by ethnicity, caste, location, and bridging social capital.

in Honduras. Looking at allocation rules they find that the allocation of relief does not depend on household wealth, but is dependent on the amount of asset losses incurred by a household.

With the exception of these studies that look specifically at the distribution of aid or safety net resources, the literature on natural disasters has largely focused on the effects on child health and human capital investment (Bustelo et al. (2012), Ferreira and Schady (2009), Jensen (2000a) for a literature review), labor market consequences (Jayachandran (2006), Lopamudra (2007), Mueller and Quisumbing (2010)), and the impact on poverty and growth (Rasmussen (2004), Skidmore and Toya (2002), Jaramillo (2009), Raddatz (2007), 2007; Baez and Santos (2008), Rosemberg et al. (2008)). Most such studies on poverty and growth find persistent negative impacts of disasters on household poverty. Other studies have examined coping strategies during disasters, showing that households lacking access to formal insurance rely on own asset stocks, including grain and livestock (Paxson (1992), Udry (1995), Rosenzweig and Wolpin (1993)), liquid assets such as jewelry and financial savings (Franeknberg et al. (2003)), and social networks, but such insurance is not complete (Townsend (1994), Fafchamps and Lund (2003), Fafchamps and Gubert (2007)).

While there is a clear lack of empirical evidence measuring the impact of disasters on the distribution of social safety net resources, there are several reasons why a natural disaster might affect this inter- and intra-village distribution. First, disasters prompt changes in the identity of those in need and exacerbates the imperfect information problem on the relative degree of need. Second, increased need and decreased resources within the village can increase competition for safety net benefits. Given the limited capacity of central governments to identify need, decentralized targeting in which village leaders determine household access to a safety net program may be more efficient. However, post-disaster, village authorities may face competing demands from the needy poor who are the intended targets of safety nets, from the elite, who face larger absolute losses, and from those to which they have social ties or who belong to important voting blocs. The latter two groups are likely to overlap, posing a threat to the former. Nose (2010) finds disasters may exacerbate underlying economic bias and corruption in the distribution of resources if the number of households in need increases. Hence, disasters could exacerbate the problem of elite capture[2] discussed in the literature (Mansuri and Rao (2004), Platteau (2004), Platteau and Gaspart (2003), Baland and Platteau (1999), Bardhan (2002), Bardhan and Mookherjee (2002), Vedled (2000)).

The current literature also does not examine the role of social capital in post disaster aid distribution. Putnam (1993) defines social capital as "features of social organization, such as trust, norms and networks, that can improve the efficiency of society by facilitating coordinated actions." There is a large literature addressing whether social capital matters in issues ranging from governance to growth, to human capital accumulation and child welfare (Olken (2009a), Dipasquale (1999), Knack and Philip (1997), Narayan and Pritchett (2000)). However, the role of social capital in reducing the influence of elite capture during community level shocks has not been explored. Putnam (1993)'s seminal work on social capital suggested that declining social capital within communities could lead to ineffective governance. Stronger village networks, on the other hand, could increase the capacity of communities to respond collectively to shocks (Douty (1972)).

During disasters, higher levels of social capital may thus be expected to improve

[2]Elite capture refers to the capture of resources by groups with greater social and economic power, such as wealthier households, landowners, those with access to formal financial savings mechanisms. Studies on elite capture focus on Community Driven Development (CDD) programs in which resource allocation is determined at the village level. The problem of elite capture has been pervasive in various economic arenas, from the distribution of formal subsidized credit (Burgess and Pande (2005)), to the distribution of input vouchers in agricultural subsidy programs (Pan and Christiaensen (2012)), to the management of benefits generated from local hardwood forests (Iversen et al. (2006)). Several critiques of such programs can be found in the literature (Mansuri and Rao (2004), Platteau (2004), Platteau and Gaspart (2003)) suggesting that lack of proper implementation and oversight can lead to elite capture. Fritzen (2007) analyzes the design of CDD programs in Indonesia and finds that elite control of project decision-making is pervasive.

decentralized targeting of social safety net resources. However, higher social capital within a village may disadvantage the poor if that social capital is concentrated among the elite. Social capital is difficult to quantify but governments may use some proxy of it to figure out how it helps communities distribute resources. I use standard measures of pre-earthquake participation[3] in community meetings and the number of social groups found within a village to test how this affects the distribution of social safety net resources.

I analyze these questions using data from Indonesia (Indonesia Family Life Survey (IFLS)) because of the existence of large-scale social safety net programs, and the country's susceptibility to disasters, particularly earthquakes. The study uses panel survey data from 2000 (IFLS3) and 2008 (IFLS4). I consider a large-scale subsidized rice for poor program (RASKIN) in Indonesia designed to provide rice at a price significantly below the market price to poor households. According to Sumarto et al. (2002) need and therefore participation should be determined using specified objective criteria including household consumption, landlessness, asset ownership and other criteria on the household and its members. However, evidence suggests that participation is not always determined objectively.

Although the government mandates using nationally set poverty standards as a guideline for determining RASKIN program eligibility, several studies (Pritchett et al. (2002), Olken et al. (2001)) find that local authorities wield power over which households access the program and their level of benefits. In the RASKIN program, the central government distributes subsidized rice from warehouses to the local authorities that transport the rice to the village. Subsequently, village authorities determine eligibility, as well as the price of rice and a quantity cap for each household. I found significant inter- and intra- village variation in participation, price paid and quantity purchased within RASKIN villages.

[3]The measures of social capital used are the same as those used by Olken (2009b) to measure how television and radio affect social capital in Indonesian villages

The paper is organized as follows. Section 2 provides a brief discussion of Indonesia's 'rice for poor' (RASKIN) safety net program and the country's exposure to earthquakes. Section 3 is a description of the data sources and variable definitions. Section 4 provides a conceptual framework. Section 5, 6 and 7 give empirical strategy, empirical results and conclusion respectively.

1.2 Social Safety Net Programs and Earthquake Exposure in Indonesia

1.2.1 The Rice for Poor Social Safety Net Program: Inequality and Insurance

During the Asian Financial Crisis from 1997-1999 the number of households living below the poverty line in Indonesia increased dramatically (Tabor and Sawit (2001), Sumarto et al. (2002)). This lead to the creation of several safety net programs. The largest among these was the OPK - *Operasi Pasar Khusus* - rice program created in mid 1998 (later called RASKIN), as a result of soaring rice prices, food shortages, malnutrition and a decline in real household income. Rice is a staple consumed by most Indonesian households and therefore RASKIN, which ensured the affordability of rice for the poor, was a critical component of these safety net programs. The program is the largest redistributive program in the country.

For RASKIN the rice was distributed through village government authorities. On a monthly basis, the government logistics depot (DOLOG) delivered rice to the village, or village staff would retrieve the rice from the subdistrict (Kecamatan) office. According to the RASKIN food security program's guidelines, each eligible household determined to be below a poverty threshold within a village should be allowed to purchase 10kg of subsidized rice per month at a price of Rupiah 1000/kg (Sumarto et al. (2002)). Approximately half a year after the start of the program 74 million households, 15 percent of the country, were targeted (Sumarto et al. (2002)). Olken (2006) notes that during 1998-1999 official guidelines allowed households to purchase up to 20 kg of OPK rice per month at 60 percent below the market price. In survey data using the 1998 Hundred Village Survey, a nationally representative survey of 100 villages, he finds that the subsidy represented approximately 9 percent of total pre-program monthly household expenditures for households purchasing the full allotment.

Although villages were supposed to determine eligibility following national BKKBN (Population and Family Planning board) criteria, Olken et al. (2001) find that there was significant inter-village variation in the determination of eligibility and therefore whether or not poor households within a village qualified for the program. For example, setting the price too high made some households unable to afford the rice. In some villages, rice was distributed equally among all households, thereby reducing the amount of rice poor households could have access to. In certain cases, Olken et al. found that wealthier households lobbied to receive the rice despite not consuming the rice, which was found to be of lower quality. Once they received it they were able to benefit from the subsidy by re-selling it to traders. Accordingly, village authorities had almost complete authority to determine how the rice would be distributed within their villages (Olken et al. (2001)). A study by LP3ES (2000) on the OPK program in 1999 estimated that among households receiving subsidized rice, 19 percent received the full 20kg, and 68 percent received less that 10kg.

1.2.2 Earthquake Exposure

Indonesia and the surrounding region is one of the most seismically active zones found globally[4]. The 2004 Tsunami, the third largest earthquake in the world since 1900, is a well-known example of an earthquake affecting Indonesia. While this claimed 227 898 casualties, most earthquakes have a small fraction of the impact of the Tsunami.

[4]USGS Seismotectonics of the Indonesian Region: http://earthquake.usgs.gov/earthquakes/world/indonesia/seismotectonics.php

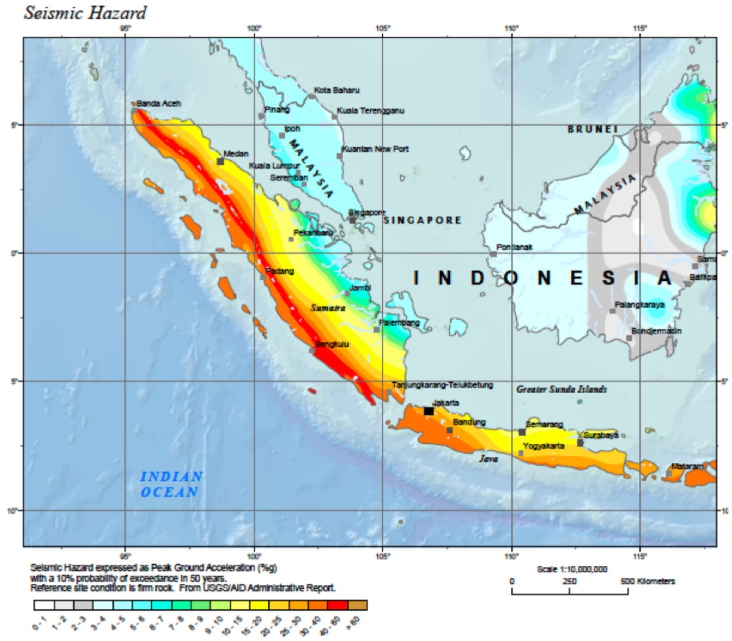


Figure 1.1: Seismic Hazard Map of Indonesia *Source: NEIC-USGS*

Although most earthquakes are not as severe, a few high magnitude earthquakes, over 5.5 on the richter scale, occurred in Indonesia during the study period. Most notable was the May 2006 Java earthquake in which human casualties were reported at 5,749, and 38,568 people were injured with an estimated 423,000 evacuated (USGS). Figure 2, above, depicts a seismic hazard map of Indonesia showing the the probabilistic maximum considered earthquake given the relative motion of different areas.

Figure 3, shows earthquake occurrences between the years 1988 and 2008 of magnitude above 5 at epicenter. The red vectors illustrates the movement of the Australia plate relative to the Sunda Plate. The green circles indicate fatal earthquakes and all colored circles represent earthquakes with main shocks over magnitude 7.7 and aftershocks occurring within 31 days. The different colors represent different earthquakes.

I spatially link US Geological Survey (USGS) data on earthquake occurrences to communities found in the Indonesia Family Life Survey (IFLS) data set. I use a difference in difference framework to identify the impact of earthquakes occurring over a period of two years prior to the 2008 survey, while treating 2000 as the pre-disaster

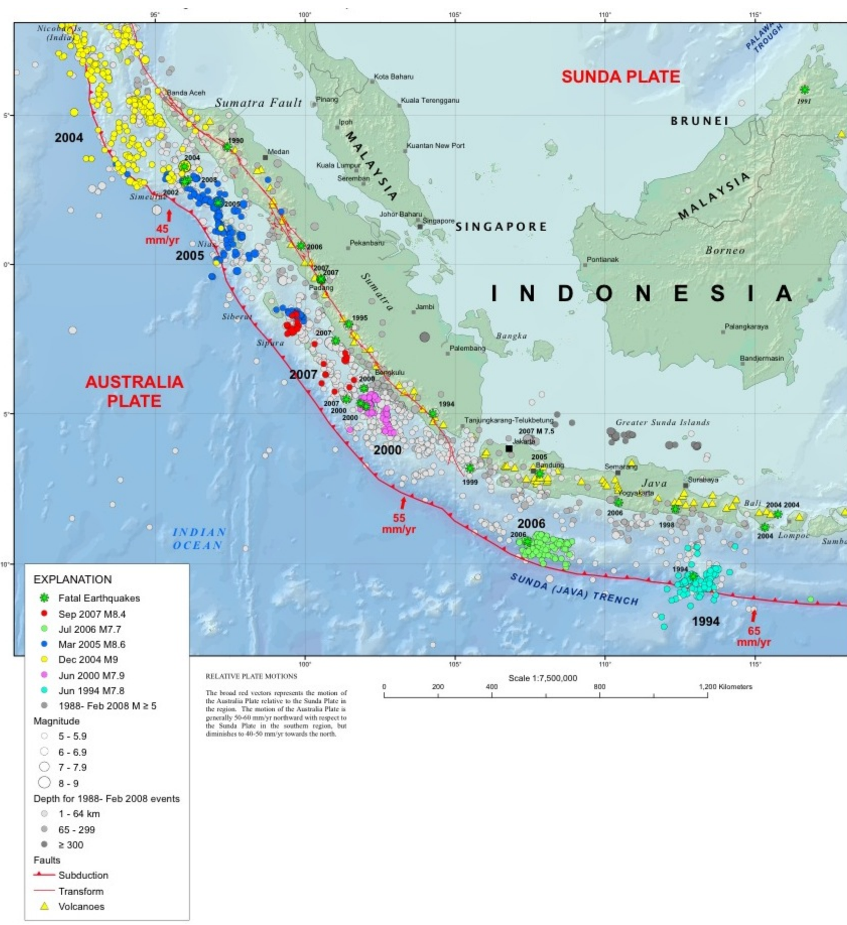


Figure 1.2: Indonesia Earthquakes 1988-2008 *Source: NEIC-USGS*

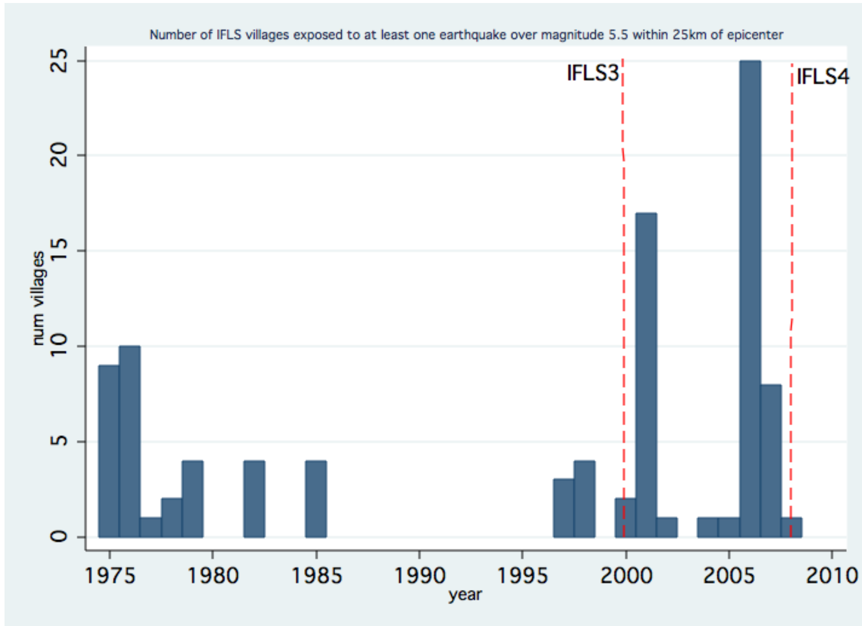


Figure 1.3: *Source: author's calculations using NEIC-USGS*

period. This is because several of the country's most catastrophic historic earthquakes occurred during this period and few villages were affected by catastrophic shocks in the 25 years prior to 2000. Using U.S. Geological Survey (USGS) magnitude and distance to epicenter data, the number of villages exposed to catastrophic earthquakes over 5.5 magnitude, and within 25 km of the community, over the period 1975 to 2008, is shown in Figure 1, below. I use a narrow definition of shocks to ensure that only extreme events are recorded. In the 2006-07 period, 33 IFLS communities were affected by a catastrophic shock using this criteria for extreme shocks[5].

Several papers find that the impact of earthquakes on households are strongly negative in the short run and likely to persist in the long run. Baez and Santos (2008) and Rosemberg et al. (2008) show that in the short run households impacted by severe earthquakes earn less income, consume less and draw down assets significantly. These studies suggest that recovery from asset losses would take several years. Given such

[5]From USGS documentation most catastrophic earthquakes in Indonesia were over 5.5 magnitude. Varying distance to epicenter up approximately 50 km does not alter the observed pattern of village exposure to shocks. Figures in the appendix show exposure to shocks within 25-50 km, 50-75 km and 75-100 km radius of communities

findings, I concentrate on earthquakes within two years of the 2008 survey because the negative impacts on household consumption and savings remain large over this period. Figure 1 shows that several communities were exposed to a shock in 2001. However, most communities were from one area, Yogyakarta, and newspaper coverage (Jakarta Post, 2001) suggests no significant damage was caused during this shock. In the analysis I control for community exposure to shocks in the 2000-2005 period and 1975-1999 period to control for the impact of past exposure to earthquakes and changes in behavior arising from changes in households expectations.

For the empirical analysis in this paper I focus on communities located along the islands of Sumatra, Java, Bali and West Nusa Tengarra sampled by the Indonesia Family Life Survey (IFLS). These areas serves as an ideal setting as they lie along a tectonic fault and are therefore more prone to earthquakes, and contain the most densely populated areas in Indonesia. Both South Kalimantan and South Sulawesi are excluded as geological survey data indicate no seismic activity in these areas during the time period of interest. Keeping the comparison within the indicated regions allows for the creation of a control group with similar characteristics as the earthquake affected population.

1.3 Data

The primary data for this paper comes from the Indonesia Family Life Survey (ILFS) which is a large scale longitudinal survey carried out in 13 of 27 provinces in Indonesia representing 83% of the Indoneisan population. The survey was first carried out in 1993/94 (IFLS1), followed by subsequent surveys in 1997 (IFLS2), 1998 (IFLS2+), 2000 (FILS3) and 2008 (IFLS4). For the purpose of this paper I will use data from the individual, household and community surveys for the 2000 and 2008 surveys[6].

[6] IFLS2, carried out from August 1997 to April 1998, is used to verify that for outcomes on consumption and assets, treated and untreated households in the matched sample follow a similar pre-disaster trend. IFLS2 does not have data on RASKIN. IFLS1 is not used as it dates too far

The availability of panel data in these survey periods allows a study of the dynamics of behavior. Among provinces being studied in this paper, the sample included 6728 IFLS3 households. In IFLS4, the re-contact rate was 95.1% of any part of IFLS3 origin households, over eight years later. This is a relatively low rate of attrition for a large survey. Overall 6329 households, 87.6% of all households were interviewed in all 4 surveys since IFLS1 (IFLS User Guide, 2009). The higher re-interview rates would lessen the risk of bias due to nonrandom attrition in the survey. For households surveyed in 2000 living in communities affected by a disaster in 2006 and 2007, the rate of attrition in 2008 is 4.29%, 31/723 . Similarly, attrition among households livings in communities unaffected by a disaster is 5.00%, 331/6728. A ttest reveals no difference in mean attrition among *potentially* affected and unaffected households in 2000, surveyed in 2008 surveys.

For the study I focus on origin households from 2000, present in 2008. Although earthquakes are exogenous states of nature, location is not strictly randomly determined due to the two year period over which earthquake occurrences are measured. It is possible that village and household characteristics are correlated with risk if such risk is correlated over time, or if households affected in the 2006-2007 somehow differed from unaffected households.

Appendix Table A1. shows differences across households in the earthquake affected and unaffected areas. For the unmatched sample, earthquake affected household heads were significantly older, with more years of schooling and fractionally longer residence in the village. Households in earthquake affected villages are also disproportionately more urban, have better access to electricity and piped water, have more water needs met in the dry season and have more midwives in the village. These differences are all significant across affected and unaffected villages, and could

back, and would have a smaller sample to select from if treated as baseline, as split households are also included in subsequent surveys. IFLS2+ in 1998 was only carried out on a 25 percent sample and is thus excluded from the study potentially affect the outcomes of interest. Some of the consumption outcomes are also significantly different across the two groups. Hence, to empirically determine the causal impact of exposure to earthquakes on household welfare and the effectiveness of social safety net programs, a difference-in-difference estimation along with matching is used.

To mitigate ex-ante differences, across treated and untreated households, I Propensity Score Match (PSM) households in the sample to create balance along baseline characteristics. The sample is well suited to PSM as there is a sufficiently large control sample from which matches can be drawn. Origin households in 2000, also present in 2008, are used to create a matched sample. Among a total of 723 households in the earthquake affected treatment group, 570 are included in the matched sample. The treatment group is matched to a control sample of 1,170 households, totaling 1,740 households in the sample each survey year. In total the sample included 33 communities affected by at least one earthquake over the period 2006 to 2007 and 250 unaffected communities. The matched sample included 30 affected and 215 unaffected communities.

Summary statistics and differences in means at the 2000 baseline for the matched sample are shown in the right panel of Table A1 As shown in the statistics, household characteristics are balanced across the affected and unaffected matched sample. Household heads are on average 49 years old, with 6.25 years of schooling at the mean. Households have roughly 5.85 members and a household is established within a village on average for 45 years. One concern that arises from analyzing disasters is that such extreme events could increase migration in or out of disaster areas, leading to a bias in estimates on the impact of earthquakes. As discussed above, there is no statistically significant difference in attrition across survey years, between earthquake affected areas and unaffected areas. In addition, the matched sample ensures, that households have been present in an area for approximately the same amount of time, and have similar characteristics. Approximately 54% to 55% of households are urban, and all other characteristics including access to electricity, water, health posts balance across earthquake affected and unaffected communities. For the matched sample, in 2000 43.2% of unaffected and 44.5% of affected households were participating in the RASKIN program. There is no difference between treated and untreated households in total per capita consumption, food consumption, non food consumption and durables purchases as shown by the p values for the differences. In addition, there is no difference across the value of household assets and land ownership which are also outcomes variables.

The second source of data is from the National Earthquake Information Center, US Geological Survey (USGS) which provides historical information on magnitude, depth, latitudinal and longitudinal epicenter information for earthquakes. Using a rectangular latitude, longitude area search around Indonesia[7] I located all earthquakes with magnitude greater than 5.5 on a Richter scale in 2006 and 2007. This resulted in information on over 200 such earthquake occurrences in this area over the two year period. For each of these the distance from each community to the epicenter was spatially linked using Vincenty's Formula (Vincenty (1975)). For any year, a community level earthquake occurrence was recorded if a shock was over magnitude 5.5 within spherical distance less than or equal to 25 km from epicenter to community. While shocks of magnitude 5 and below occur quite frequently in Indonesia, these are shown historically to not have any impact on communities (USGS, historical earthquake information). I select a radius of 25km to closely link communities in sub-districts (Kecamatan) that were reported to be affected by earthquakes in the 2006-2007 using reports on identified earthquakes [8].

[7] This covered an area from and 11.5° S to 6.5° N latitude and 91.8750° E to 144.750° E longitude.

[8] Survey data shows that over 95% of households within a 100km radius of occurrences over 5.5 magnitude are within 25km of the shock. The results from the estimation are robust to changing the radius of the shock to 35 and 45 km. Interacting the the shock with magnitude of the shock has no observable effects.

To deflate expenditures, asset values, and prices of subsidized rice I used statistics from the Badan Pusat Statistik (BPS) which provides monthly price indices for up to 45 cities in Indonesia. Each city or cities are matched to Kecamatans and a price index is created around the survey months. Monetary values are all relative to 2007 base year. The price indices do not account for rural-urban price differences across Kecamatans, due to a lack of rural price data.

1.4 Village Authority Problem

In this section I introduce a simple conceptual framework to understand the distribution of subsidized RASKIN rice to villagers. The framework takes into account the welfare of two groups; non-poor and poor[9]. The objective of the central government is for the poorest households in a village to receive the rice subsidy in any state of the world[10]. However, from the IFLS data approximately 60% of the poorest get access to the program while around 40% of the non-poor also get access to subsidized rice. This suggests the village authority's optimization decision is not aligned with the central government over the distribution of RASKIN.

The village authority is assumed to be neither egalitarian or utilitarian and solves the decision over the distribution of subsidized rice, both the *quantity* and *number of households* in each group receiving subsidized rice. The agent's choice over the distribution of subsidized rice is contingent on the size of losses incurred by each group, the number of poor households, and the bargaining power of the poor and non-poor, all contingent on the state of nature. The objective function, which achieves a Pareto efficient solution, is characterized as the weighted sum of the utilities of the poor and the non-poor. The poor and the rich do not have equal say in the decision-

[9]The empirical section relies on more than two groups, however, for simplification I assume that the poor are the poorest group considered in the empirical estimation. In this case, the non-poor is synonymous with elite households. Here $VERY\ POOR_{hct}$ refers to the poor and $MOD\ POOR_{hct}$, $MOD\ WEALTHY_{hct}$, $VERY\ WEALTHY_{hct}$ refers to the non-poor.

[10]Program guidelines are constant with respect to shocks.

making process over the allocation of RASKIN. The weights on the poor assigned by the village authority are denoted $\lambda^R(\pi) > 0$ and non-poor $\lambda^P(\pi) = 1 - \lambda^R(\pi) > 0$. These weights may be a function of their personal benefit, either electorally or through reciprocity expected from social contacts or family members to whom the agent provides access[11].

I present an illustrative special case of the of the village authority's problem over the allocation of resources. In this case the village authority chooses the number of households X^P, X^R, that can participate in RASKIN only. It assumes that all program participants receive the same amount of subsidized rice normalized to one. This implies that the mean allocation to the poor or non-poor group is equivalent to the proportion of each group receiving the program. The decision of the village authority then collapses to a single one, namely how many households in each group receive subsidized rice.

The aggregate welfare functions for the rich and the poor can be written as:

$$W^P(X^P) = \frac{X^P}{M}U^P(1|Z^P(\pi)) + (1 - \frac{X^P}{M})U^P(0|Z^P(\pi)) \tag{2.1}$$

$$W^R(X^R) = \frac{X^R}{N-M}U^R(1|Z^R(\pi)) + (1 - \frac{X^R}{N-M})U^R(0|Z^R(\pi))$$

Hence, the village authority maximizes the weighted average utility over the rich and the poor, choosing X^P and X^R, for given $Z^i(\pi)$ net income (income-savings-losses)

[11]Some authors such as Bardhan and Mookherjee (2002) model weights as endogenously determined through a voting model.

of the poor or non-poor household, to solve the problem:

$$\underset{X^P, X^R}{\text{Maximize}} \quad \frac{M}{N}\lambda^P(\pi)W^P(X^P) + \frac{N-M}{N}\lambda^R(\pi)W^R(X^R)$$

$$\text{subject to} \tag{2.2}$$

$$(\phi :)\bar{X} = X^P + X^R$$

If the village authority was allocating subsidized rice according to the central government's guidelines then $\lambda^R(\pi) = 0$ for all π states of the world and $\bar{X} = X^P$ As this is not the case, and some wealthy households do get access to the program in all states, solving above provides a simple solution to the first order conditions:

$$\lambda^P[U^P(1|Z^P(\pi)) - U^P(0|Z^P(\pi))] = \lambda^R[U^R(1|Z^R(\pi)) - U^R(0|Z^R(\pi))] \tag{2.3}$$

where $U^{i\prime}(.) > 0$ and $U^{i\prime\prime}(.) < 0$.

Denote two states of nature as π_0 (no disaster) and π_1 (disaster). In the no disaster state, given

$$[U^P(1|Z^P(\pi_0)) - U^P(0|Z^P(\pi_0))] > [U^R(1|Z^R(\pi_0)) - U^R(0|Z^R(\pi_0))]$$

then, $\lambda^R(\pi_0) > \lambda^P(\pi_0)$ for $X^R > 0$ and at least some RASKIN rice to be allocated to the wealthy. In other words, inferring that the utility benefit to the poor of receiving a unit of subsidized rice is greater than the utility benefit to the non-poor in the absence of the shock, the fact that any rice is targeted to non-poor households implies that these households have greater bargaining power than the poor.

Now, assume $\bar{X}$ remains constant, and the number of poor M does not change (i.e. the pre-disaster classification of poor is kept post disaster). Then, for two states of nature π_0 and π_1 if $\triangle Z^R(\pi_0 \rightarrow \pi_1) > \triangle Z^P(\pi_0 \rightarrow \pi_1)$ then there exists some $Z^R(\pi_0) > Z^R(\pi_1) > Z^P(\pi_0) > Z^P(\pi_1)$ and $\lambda^R(\pi_1) \geq \lambda^R(\pi_0) > \lambda^P(\pi_0) \geq \lambda^P(\pi_1)$ and such that $\triangle X^R(\pi_1 \rightarrow \pi_2) = -\triangle X^P(\pi_1 \rightarrow \pi_2)$

The above states that even if non-poor households loose more wealth than poor households in a disaster, there exists some scenario in which, post losses, the non-poor still retain more assets than the wealthy, and the difference in bargaining power of the non-poor increases or stays the same ($\lambda^R(\pi_1) \geq \lambda^R(\pi_0)$) such that more non-poor get access to the program. This also means, and is shown in the empirical estimates, that the difference in the weight put on richer households by the village authority, relative what the central government would desire in any state, becomes more pronounced in the face of a natural disaster.

To show this, note that if,

$$\begin{aligned} U^{i\prime}(.) > 0 \text{ and } \; U^{i\prime\prime}(.) < 0 \\ \text{and } \; Z^R(\pi_0) > Z^R(\pi_1) > Z^P(\pi_0) > Z^P(\pi_1) \\ \text{then by the concavity of the utility function} \end{aligned}$$

$$\begin{aligned} U^R(1|Z^R(\pi_0)) - U^R(0|Z^R(\pi_0)) < U^R(1|Z^R(\pi_1)) - U^R(0|Z^R(\pi_1)) \\ < U^P(1|Z^P(\pi_0)) - U^P(0|Z^P(\pi_0)) < U^P(1|Z^P(\pi_1)) - U^P(0|Z^P(\pi_1)) \end{aligned}$$

In other words suppose net income levels post-shock are lower for both groups, but net income is higher in both states for the non-poor than poor. Then the utility benefit of receiving access to the program is always higher for the poor than non-poor and is higher for both groups post-shock. Then IF

$$\triangle Z^R(\pi_0 \to \pi_1) > \triangle Z^P(\pi_0 \to \pi_1)$$

such that

$$[U^R(1|Z^R(\pi_1)) - U^R(0|Z^R(\pi_1))] - [U^R(1|Z^R(\pi_0)) - U^R(0|Z^R(\pi_0))]$$
$$> [U^P(1|Z^P(\pi_1)) - U^P(0|Z^P(\pi_1))] - [U^P(1|Z^P(\pi_0)) - U^P(0|Z^P(\pi_0))]$$

i.e. If in addition the non-poor loose relatively more than the poor (as expected) as a result of a shock, the difference in the utility benefit from receiving the program is larger for the non-poor than the poor, it follows

$$\Rightarrow \frac{U^R(1|Z^R(\pi_1)) - U^R(0|Z^R(\pi_1))}{U^R(1|Z^R(\pi_0)) - U^R(0|Z^R(\pi_0))} > \frac{U^P(1|Z^P(\pi_1)) - U^P(0|Z^P(\pi_1))}{U^P(1|Z^P(\pi_0)) - U^P(0|Z^P(\pi_0))} \tag{2.4}$$

Given (2.4), suppose the pre-shock weights still hold, $\lambda^R(\pi) > \lambda^P(\pi)$, or although not strictly necessary, if village authorities give a larger weight to the non-poor post-shock[12] such that $\lambda^R(\pi_1) \geq \lambda^R(\pi_0) > \lambda^P(\pi_0) \geq \lambda^P(\pi_1)$. Then combining (2.3) & (2.4) if follows that the inter-temporal benefit of receiving the program is higher for the non-poor given losses in the aftermath of an earthquake if the above conditions hold:

$$\frac{\lambda^R(\pi_2)[U^R(1|Z^R(\pi_2)) - U^R(0|Z^R(\pi_2))]}{\lambda^R(\pi_1)[U^R(1|Z^R(\pi_1)) - U^R(0|Z^R(\pi_1))]} > \frac{\lambda^P(\pi_2)[U^P(1|Z^P(\pi_2)) - U^P(0|Z^P(\pi_2))]}{\lambda^P(\pi_1)[U^P(1|Z^P(\pi_1)) - U^P(0|Z^P(\pi_1))]}$$

which in turn implies

$$\triangle X^R(\pi_1 \to \pi_2) = -\triangle X^P(\pi_1 \to \pi_2)$$

i.e. assuming the total quantity of subsidized rice available to a village post earth-

[12]This could occur due to the electoral advantages of providing more services to relatively wealthier households at the expense of the poorest when the former experience hardship. It could also occur due to non-electoral motivations, simply due to stronger social or family ties of the non-poor to local leaders and either altruism or reciprocal relationships with these connections

quake remains the same the village authority would transfer some of the subsidized rice away from the poor towards the non-poor.

The above proposition simply describes that during a natural disaster the number of rich households getting access to a subsidized food security program may increase while decreasing access of the poor. This can occur if wealthier households face larger relative losses than poorer households. Under such circumstances if losses faced by wealthier households are sufficiently large such that the inter-temporal benefit is larger for the rich than for the poor, then the rich win and the poor loose.

1.5 Empirical Strategy

The identification strategy below aims to test a few specific questions on the relationship between earthquake shocks and resource allocation through the subsidized 'rice for poor' (RASKIN) program. First, what is the impact of earthquakes on affected households' access to RASKIN along the extensive margin, and intensive margin - through quantity caps and price for rice (if participating)? How does this impact vary by poverty status[13]? Second, did the high impact earthquakes within the two years prior to the 2008 survey deplete household asset stocks and generate a negative consumption response contingent on household pre-disaster within village rank[14]? Is the consumption and asset response of households sufficient to justify RASKIN allocation? Third, how does variation in intra village social capital, through the level of pre-disaster participation community meetings, the number of community groups, affect within village distribution of the rice for poor program post-earthquakes?

The distribution of earthquake communities suggests that the occurrence of shocks over the long term in the area should be random. However, because I focus on a

[13]Ex ante, within village wealth status is established for current survey year using prior IFLS survey household and community data

[14]In the case IFLS communities surveyed in Indonesia, the last earthquake affecting households' prior to 2008 was in March 2007.

two year period and capture extreme earthquakes in a few locations I find that pre-disaster household and village characteristics that may affect outcomes are correlated with exposure to earthquakes (Appendix Table 1). Hence, along with difference-in-difference, I Propensity Score Match (PSM) households along several household and community characteristics at baseline using IFLS3 (2000) to ensure pre-disaster parallel trends (IFLS2 (1997) & IFLS3)[15] and comparability (across IFLS3 & IFLS4). Using data on a panel of respondents, difference-in-difference estimation is carried out on the weighted, matched sample.

For the estimation strategy, let t_{2008} control for the change in the outcome across the survey years and take on a value equal to one in the post earthquake period. Further, let Y_{jhct} be the outcome variable with j indexing the outcome of interest, household h, in community c, at time t. Here, $Y_j \epsilon$ {logPCFE, logPCNFE1, logPC-NFE2, logPCE, logPCDE, VAL ASSETS, logVAL ASSETS, OWNLAND, log VAL JEWELRY, log VAL SAVINGS, OPK, OPK QUANTITY, log OPK PRICE}, where log denotes the natural logarithm of the variable. For a matched sample I estimate the Average Treatment Effect of the Treated (ATET), comparing treatment and control units, pre and post-earthquake:

$$\begin{aligned} E(\gamma_{jhc}|X,Z) = &\{E(Y_{jhct}|X_{hct}, Z_{ct}, EQ_{hc} = 1, t_{2008} = 1) - E(Y_{jhct}|X_{hct}, Z_{ct}, EQ_{hc} = 1, t_{2008} = 0)\} \\ &- \{E(Y_{jhct}|X_{hct}, Z_{ct}, EQ_{hc} = 0, t_{2008} = 1) - E(Y_{jhct}|X_{hct}, Z_{ct}, EQ_{hc} = 0, t_{2008} = 0) \end{aligned}$$

To estimate the ATET, the equation below is used. This tests the underlying questions of whether in the aftermath of catastrophic earthquakes household are able to recover to pre-disaster levels of consumption and asset stocks, and if affected areas received more subsidized rice through the RASKIN program:

[15]IFLS2 is not used in the empirical estimation as it does not have data on safety net program.

$$
\begin{aligned}
Y_{hct} = \gamma_0 + \gamma_1 EQ_{hc} * t_{2008} + \gamma_2 EQ_{hc} + \gamma_3 t_{2008} \\
+\gamma_4 X_{hct} + \gamma_5 Z_{ct} + \gamma_6 \sum_{2000}^{2005} EQ_{hc} + \gamma_7 \sum_{1999}^{1975} EQ_{hc} \\
+\beta_h + \eta_p + \epsilon_{hct}
\end{aligned}
\tag{1.1}
$$

where β_h here represents household level fixed effects that capture time invariant household characteristics. In addition, η_p province-time level fixed effects are included to purge the data of fixed differences across provinces in a given data period. The standard errors are also clustered at the community level to capture intra-community correlation. The primary coefficients of interest would be those on the $EQ_{hc} * t_{2008}$ reflecting the average treatment effect on the treated.

The X_{hct} household level controls used in the estimation are age of head, gender of head, years of schooling of head, marital status of head, years in village of household head, household size. Village controls, Z_{ct}, include number of health posts, proportion of households with electricity, proportion of households with piped water, access to large and small microfinance institutions in village, village population, urban, number of earthquakes 2000-2005, number of earthquakes 1975-1999.

To address the latter part of the heterogenous effect on safety net allocation by household wealth standing within the village, the triple difference equation below is estimated.

$$
\begin{aligned}
Y_{hct} = \gamma_0 + \gamma_1 t_{2008} + \gamma_2 EQ_{hc} + \sum_{k=1}^{4} \gamma_{3k} RANK_{hc,t-s} \\
+\gamma_4 EQ_{hc} * t_{2008} + \sum_{k=1}^{4} \gamma_{5k} RANK_{hc,t-s} * EQ_{hc} \\
+\sum_{k=1}^{2} \gamma_{6k} RANK_{hc,t-s} * t_{2008} + \sum_{k=1}^{4} \gamma_{7k} RANK_{hc,t-s} * EQ_{hc} * t_{2008} \\
+\gamma_8 X_{hct} + \gamma_9 Z_{ct} + \gamma_{10} \sum_{2000}^{2005} EQ_{hc} + \gamma_{11} \sum_{1999}^{1975} EQ_{hc} + \beta_h + \eta_p + \epsilon_{hct}
\end{aligned}
\tag{1.2}
$$

In the equation above let $RANK_{xhct}$ where $x = 1, 2, 3, 4$ indexes wealth standing in the village $RANK_{1hct} = 1$ if the household is classified as very poor (more than one std. dev. below the community mean). Similarly, $RANK_{2hct} = 1$ if the household is moderately poor (between mean and one std. dev. below the mean), $RANK_{3hct} = 1$ if moderately wealthy (between mean and one std. dev. above the mean), and $RANK_{4hct} = 1$ if very wealthy (more than one std. dev. above village mean). The primary coefficients of interest is γ_7 which measures the triple difference impact of various allocation rules by the social planner.

To address the impact of pre-existing social capital on disaster affected household's access to the RASKIN program, I estimate (1.2) above but for a sample stratified at various social capital cutoffs, i.e. $OPK_{hct}(|SC = x)$. In this specification, SC is the the pre-disaster level of social capital stratified into groups. In the case of proportion of households participating in community meetings x={>0.7, >0.6, >0.5, <=0.5, <=0.6 <=0.7}. For the number of community social groups, x ranges from { >=4, >=3, <5, <4}.

1.5.1 Variable Definitions

To investigate the impact of exposure to earthquake risk over a two year period, in 2006 and 2007, on households' consumption response , several components of consumption expenditures are measured. The measure $PCE_{hct} = PCFE_{hct} + PCNFE1_{hct} + PCNFE2_{hct}$ is per capita household expenditure on non-durables. Here, $PCFE_{hct}$, measures per capita monthly expenditures on households food consumption. $PCNFE1_{hct}$ is per capita monthly non-food expenditure on electricity, water, fuel, tel etc., personal toiletries, household items, domestic services(servants wages etc), transport (gasoline, bus/cab fares etc), arisan and recreation or entertainment. $PCNFE2_{hct}$ is an annual measure converted to monthly per capita non-food expenditures which includes spending on clothing for children and adults, household supplies and furniture, rituals, ceremonies, charities, gifts and taxes. In addition to these consumption variables, household expenditures on durables $PCDE_{hct}$ is also measured.

In order to estimate the effect of these specific disasters on the destruction and depletion of household assets stocks, I use measures of the value of household assets. The variable $PCVALUEASSETS_{hct}$ measures the per capita total Indonesian Rupiah value of assets including house, other house/building, non agricultural land, savings, vehicles, household appliances, furniture and jewelry. I also separately estimate the impact on $OWN\ LAND_{hct}$ which is an indicator variable of ownership of land not used in farm or other business. Impacts on $VAL\ JEWELRY_{hct}$ and $VAL\,SAVINGS_{hct}$ which are unlikely to be destroyed and more likely to be used as a buffer against shocks.

Given the primary interest in the study lies in identifying the differential impacts of the disaster by household poverty level, I construct a within village poverty measure for each household for the 1997 and 2000 survey years. Household relative deprivation in the village before the shock is measured using a distance index that compares household wealth to average wealth within the village. A household a wealth index is created using principal component analysis. The variable $WEALTH_{hct}$ is an index constructed from the ownership of assets as well as household welfare indicators including use of electricity for cooking, type of sanitation facilities and water sources are used. Based on this measure, community mean wealth factor and standard deviation are determined. A household is classified as $VERY\ POOR_{hct}$ within the village if it lies more than one standard deviation below the village mean, $MOD\ POOR_{hct}$ if between mean and one standard deviation below mean, $MODWEALTHY_{hct}$ if between mean and one standard deviation above mean, and finally $VERY\ WEALTHY_{hct}$ if more than one standard deviation above the mean. For a given survey year, household wealth standing in the prior survey is used, this is mainly to establish ex-ante status within the village for earthquake affected households.

Table A2 characterizes household ownership of various assets and status by predisaster within village wealth standing. For both earthquake affected and unaffected areas, there is a monotonic relationship between the proportion of households owning an asset, or proportion with given status, and within village wealth standing, suggesting that this is a good composite measure of household position within the village. The difference across groups in proportion ownership of an asset is larger for some assets, and these assets are given a greater weight in the determination of household wealth factor score. Table A3 provides information on the number of households, stratified by earthquake affected and unaffected areas, falling into each wealth group for the matched sample. The number of households falling into each group is sufficiently large to allow the estimation of differential effects by wealth group.

To examine the distribution of safety net transfers across affected and unaffected areas and across households within a village I use three outcomes of interest. These include OPK_{hct}, which indicates participation in the food security rice subsidy program for the poor. The other two outcomes, denoted $OPK\ QUANTITY_{hct}$, and $OPK\ PRICE_{hct}$, are quantity of purchases and price conditional on purchase in the last four weeks. One caveat is that in the 2000 baseline survey, only the total amount spent on purchase of subsidized rice ($\tilde{P}Q$) and the estimated value in the market ($\hat{P}Q$) are recorded for each household. Hence, I employ an alternate strategy to decompose the quantity and price of subsidized rice. Among households interviewed in 2000, 80 percent of interviewed households state that they usually purchase medium quality rice, with just 4 percent stating they purchase low quality rice. In the IFLS 2000 community survey, three price points for medium quality rice in the market are established using three merchants in each community. I calculate the average market price of medium quality rice ($\hat{P}$) across the three merchants. For each household, in the 2000 survey, I divide the estimated market value for the purchase of subsidized rice by the average market price of medium quality rice ($\frac{\hat{P}Q}{\hat{P}}$) to determine the quantity of subsidized rice purchased. Subsequently, I divide the estimated purchase value of subsidized rice by the estimated quantity of subsidized rice purchased ($\frac{\tilde{P}Q}{Q}$) to calculate the price of subsidized rice for the household [16].

To examine if higher or lower levels of social capital translate into differences in post-disaster resource allocation, indicating differences in governance, first, I stratify the sample at several cutoffs of pre-disaster proportion of households participating in community meetings. Household members were asked if they had participated in community meetings in the last 12 months. I classify a household as having participated if at least one member of the household participated. Second, I consider an alternative measure of pre-earthquake social capital, namely the number of social

[16] These data a verified against the quantity of subsidized rice derived another way, and results on quantity do not differ across methods. In the other method, the subsidized value of purchases ($\tilde{P}Q$) is divided by community level price per KG of rice for those participating in the rice for poor program. This assumes that all households participating in OPK rice program pay the same price for subsidized rice. While this should be the norm, data from the 2007 survey shows that for most villages, there is some variation in household reported price paid for subsidized rice, consistent with mismanagement in the program. The first method of calculating the price and quantity of subsidized rice is the preferred method, as it is expected to be subject to less measurement error.

groups found in a community among a set of 8 groups including Village Cooperative, Youth Group, Village Mobile Library and Neighborhood Watch Program. The pre-disaster number of social groups is taken as a measure of existing social capital within the village. Variable SC_{ct} measures the social capital cutoff level for villages in the stratified sample. For social capital measured as proportion of households participating in community meeting, SC_{ct} ϵ {>0.7, >0.6, >0.5, <=0.5, <=0.6 <=0.7}. For the number of community social groups, x ranges from SC_{ct} ϵ { >=4, >=3, <5, <4}.

1.6 Empirical Results

1.6.1 Distribution of Social Safety Net Programs in Earthquake Affected Villages

In Table 1, I estimate the relationship between participation in RASKIN and village earthquake status in the post-disaster period. Participation is measured both at the extensive margin - through access to the program - and intensive margin - through quantity cap and price paid for subsidized rice[17]. Columns (1) and (2) show that participation for households in earthquake villages was 12.2 - 12.7% higher than unaffected counterparts[18]. This suggests first, that the central government uses the subsidized rice safety net program as a form of insurance against disasters. Second, because village authorities distribute the resources it would also suggest that these authorities pass on at least some of the benefits received[19] in the process of distribu-

[17]The estimation controls for interview month and day to control for seasonal variation in prices and quantities, which is particularly important in the context of rice.

[18]One caveat is that the last measured earthquake occurred in May 2007, thus because the indicator measures ‘access to the program within the last 12 months’ there is a possibility that some households in earthquake villages participated in RASKIN prior to the earthquake. However, it is unlikely that such a household that participated in the last 12 months did not participate within the last 9 months. As such, the coefficient on participation in earthquake areas may be slightly underestimated

[19]Missing rice through RASKIN has been citied as one main problem of the program, Olken (2006)

tion.

Similarly, from columns (3) to (6) in Table 1, households in earthquake affected communities are likely to purchase approximately 0.35 - 0.49 KG more of subsidized rice per capita and pay a 12.3 - 13.0% lower price on the rice, conditional on purchasing rice in the 4 weeks prior to survey. Within-village prices for RASKIN rice exhibit significant variation, meaning prices too are controlled by village authorities. If prices paid by households in earthquake communities are lower, then village authorities would be responsible for increasing household access to rice through lower prices in response to disaster.

The difference-in-difference framework used to examine the difference across earthquake affected and unaffected communities is adapted to a triple difference model in order to accommodate household wealth standing within the community. Due to the endogeneity of wealth in the post-earthquake period to receipt of aid, I use lagged wealth-standing to represent household position, using household wealth standing in the 2000 survey for households in 2008, and wealth standing in the 1997 survey for households in 2000. Using wealth standing in 2000 as a proxy for pre-earthquake wealth may create some measurement error as shocks are taken only from the two years following 2006, for the 2008 survey. However, in the sample the correlation in wealth factor score between 2000 and 2008 surveys is 0.70. The same correlation between the 1997 and 2000 surveys was 0.78.

Village authorities in Indonesia were responsible for distributing these resources to the poorest households. Differentiation by within village wealth standing is provided in Table 2. In this Table post-earthquake period by earthquake affected is interacted with household wealth standing. The coefficient '2008*Earthquake' estimates the impact of the shock for poorest households within a community. Specifications (1) and (2), Table 2, suggest that the poorest of the poor households within a village affected by earthquakes, are less likely by 9.2% to 11.6%, by specification with and without fixed effects, to have participated in RASKIN over the last 12 months. Columns (1) and (2) also show, however, that for all other groups of households access to the RASKIN program increases if affected by an earthquake. From (2), the estimation that includes household fixed effects , moderately poor, moderately wealthy and wealthiest households experience a 6% (17.6-11.6) to 12.9% (24.5 -11.6) increase in access to RASKIN. The increase in participation in the post-disaster period for affected households in the moderately poor, moderately wealthy and wealthiest households comes at a cost to the poorest of the poor.

Table 2, columns (3) to (6) provides coefficient estimates for the impact of earthquakes on quantity and price of rice purchased through RASKIN, in post disaster period by household within village wealth standing. The results are conditional on household purchasing rice within the last 4 weeks. In contrast to the results on participation in RASKIN at the extensive margin, the results suggest that earthquake affected households in the poorest wealth group already participating in the program are able to purchase a larger quantity of rice per capita. Both columns (3) and (4) also suggest that as a result of the earthquake moderately wealthy and wealthiest households participating in the program are likely to receive lower quantity of rice compared to the poorest households. However, the fixed effect specification in column (4) shows that although the purchased quantity for the moderately wealthy group is lower than for the poorest, it is still positive. For the wealthiest group, the per capita quantity of purchased rice is significantly lower than for the poorest households with access to the RASKIN program. From column (4), poorest households purchase on average 0.52 KG per capita above households in non-earthquake areas, while the moderately poor and moderately wealthy households purchase 0.64 (0.52 + 0.12) and 0.26(0.52 - 0.26) KG per capita more than the average poorest household in non-earthquake areas, through RASKIN. For the wealthiest group, per capita quantity purchased is 0.25 KG lower (0.52KG -0.77 KG) than the poorest in non-earthquake areas.

On heterogeneity of price paid for RASKIN rice, from (5) and (6) results on changes in the price of rice provide evidence that the poorest of the poor in fact, receive a 22.5% lower price than poor households in 2006 in non-earthquake areas, significant at the 1% level, for the purchase of subsidized RASKIN rice. The difference in the price paid for RASKIN rice is insignificant for moderately poor households relative to the poorest. The moderately poor households in earthquake affected areas pay nearly 20% more for rice than all households in non-earthquake areas. Moderately wealthy and wealthiest households pay 0.3% and 5% above the poorest households in earthquake areas. However, the difference is again insignificant. The results suggest that the moderately wealthy also received as much of a benefit in the price of subsidized rice as the poorest, conditional on participation, as a result of the disaster.

The results on participation in RASKIN along the intensive margin exhibit significant heterogeneity. It is clear that the poorest of the poor participating in RASKIN receive significant benefits from the program. Similarly, while the moderately poor participating in the program may not receive any price benefits, such households still get to purchase a quantity above that of those in non-earthquake areas. Earthquake affected moderately wealthy are also able to purchase a quantity above those in non-earthquake areas, but more significantly experience the same price as that paid by poorest households.

The findings in this section indicate that the poorest of the poor, by pre-disaster standing, were less likely to participate in the rice for poor program relative to non-earthquake poor households. This is in contrast to the results of higher overall program participation in earthquake affected areas. It also stands in contrast to findings that participation increases across all other groups, the moderately poor, the moderately wealthy and wealthy, within the village. Regardless of exposure to earthquakes, program participation across households increases over time, except for the poorest of the poor in earthquake areas, making this specific case an interesting one. On the other hand, for households participating in the program, the poorest of the poor and moderately wealthy receive the greatest benefits in price and quantity access to subsidized rice.

1.6.2 Household Consumption and Buffer Stock Response: Justifying the Safety Net Distribution

Given the endogeneity of household wealth with disasters it is difficult for the econometrician to determine which households are the neediest post-disaster. However, household consumption and asset responses to earthquakes, by pre-disaster within village wealth standing, can be measured. Results in section 6.1, show that village authorities target safety net aid meant for the poorest households to wealthier groups. Hence, I hypothesize that resources may have been channelled towards households with the largest relative losses and greatest say in the authority's decision making process. As findings below will show, the poorest households were also unable to fully insure against the observed earthquakes, and consume at the lowest level among all groups. Thus in the aftermath of a shock, there is a failure to target the intended beneficiaries of the rice for poor program.

The ability of a household to recover and insure against community level shocks is likely to be contingent on the households level of asset stocks. For earthquakes, although wealthier households are expected to lose more destructible assets, the proportion of losses relative to initial wealth may not differ significantly across groups. For the period over which I analyze shocks, 2006 to 2007, the first shock occurred in May 2006, and the last in March 2007. I estimate the impact of a shock on a household's consumption response after a 9 to 19 month period[20]. The response of some groups of household's relative to others in adjusting consumption to a transitory

[20]The regressions control for interview month to avoid seasonal differences, particularly in prices and quantities for food and subsidized rice

earthquake shock could potentially justify differences in the way safety net resources are allocated.

From Table 4, households in earthquake affected villages are unable to insure fully against the impact. This result corroborates with findings from other studies such as Carter et al. (2006), Morris and Wodon (2003), who study insurance under severe environmental shocks in Ethiopia and Honduras. Consumption is measured in log terms to estimate proportional changes. From estimation (1) and (2) in Table 4, controlling for observed and time invariant household characteristics, and past shocks, consumption excluding that of durables, measured on coefficient '2008*Earthquake' is 11.8% lower in earthquake affected villages. The effect comes primarily from non-food consumption. From Table 4, columns (3) and (4), disaster villages face 10% lower food consumption, for the fixed effect specification, this effect is significant at the 5% level.

Non-food consumption is divided into two groups, the first includes spending on basic necessities such as electricity, water, fuel, transportation. The second non-food group contains items that are more discretionary, such as clothing, spending on rituals and ceremonies. The fixed effects specifications estimating the relationship between earthquakes and non-food spending, specifications (6) and (8) Table 4, shows that households reduce spending by 9.8% and 26.3% in basic and discretionary consumption respectively. The results for basic is significant at the 10% level while that for discretionary is at the 5%level. These results lend support to an empirically well documented claim that budget constrained households cannot smooth out consumption spending in the face of natural disasters.

In the short term, after being affected by an earthquake, households' may need to rebuild durable assets. However, as shown in columns (9) and (10), spending on durables is 38.6% lower in earthquake affected villages, conditional on spending a positive amount. The result provides evidence that although re-building may be a priority, there is no observed increase in this area. This could be explained by post-disaster household need to allocate resources towards competing needs in food and non-food spending. In particular for households affected by large earthquakes, governmental and disaster aid organizations may provide insufficient support at the household level.

Providing disaster affected households with rice through the existing RASKIN program, could allow households to substitute resources towards other food and non-food spending. The existing social safety net program could potentially serve as an effective way to distribute resources during a crisis. Table 5 compares consumption in earthquake villages to that in no-earthquake villages and moderately poor, moderately wealthy, wealthy households to the poorest *within* the village. Estimations (1) and (2) in Table 5, control for survey year, wealth standing in prior survey, and earthquake effect individually and interacted. For poorest households, in the fixed effects estimation, consumption post-earthquake is 12.4% lower than non-earthquake poor, significant at the 10% level. The size of the relative impact is close to zero for the moderately poor, with the difference between the poorest and the moderately poor being insignificant. The moderately wealthy and wealthiest groups are strongly impacted by earthquake shocks as expected, consuming 43% and 41% below that for the poorest households with the difference being significant at the 5% level.

The results in Table 5, columns (3) and (4) show a negative food consumption response by all groups except the moderately poor for the earthquakes studied in the paper. While the poorest and wealthiest groups reduce food consumption by 11% to 15%, the changes are not significant. Food consumption of the moderately wealthy group does not appear to be insulated during earthquake shocks. In earthquake areas, moderately wealthy households incur a proportional loss of 35% below the poorest households. Results in specifications (5) and (6) for basic non-food consumption, show lower consumption across all groups, with the poorest households consuming

12.4% less than the poorest in non-earthquake areas, and the other groups consuming 19% to 23% less than their unaffected counterparts. However, none of the differences are significant. For discretionary non-food consumption, (7) and (8), consumption for poorest group in earthquake affected areas is 24% below consumption in unaffected areas. For the moderately poor in earthquake areas, consumption in the discretionary non-food group is 10.4% higher than unaffected areas. The difference between the poorest and the moderately poor, is again insignificant. For the moderately wealthy and wealthiest groups, the difference in discretionary non-food consumption relative to the poorest group is significant, and the proportional difference is large, with spending in both groups falling by over 60% in earthquake affected areas. Specifications (9) and (10) representing a decline in durables spending are the only instances in which all groups excluding the poorest incur large declines, while the poor are insulated. In this case for households spending on durables, spending falls by 180% to 234% across the three groups.

Table 6 shows mean levels of consumption among each wealth group, with and without the earthquake in the pre-and post disaster periods. From this Table, the Rupiah value of consumption for the poorest is lower in level terms than their wealthier counterparts. The results on consumption clearly indicate that by pre disaster status, relative to the poorest of the poor, moderately wealthy and wealthiest households perform significantly worse, in terms of a relative decline in consumption. However, per capita consumption for the wealthiest and moderately wealthy is 356,0404 Rupiah and 291,984 Rupiah respectively, while just 210,727 Rupiah per capita for the poorest of the poor. In fact mean consumption for all groups, see Table 6, is significantly higher than for the poorest. The question is then whether households with larger proportional losses or households with the lowest level of consumption receive differentially greater access to RASKIN in the aftermath of a disaster. The answer may be a function of the level of say each group has over the allocation decision of

RASKIN rice.

Table 7, estimates the impact of exposure to earthquakes on household asset stocks. Across all specifications, household assets are significantly depleted post earthquake. From columns (1) and (2) the real per capita Rupiah value of assets declines significantly. From columns (3) and (4) in log assets value terms, earthquake affected areas incur a 19-27% decline over the unaffected areas. Table 5 includes estimation over an additional set of household assets, that are unlikely to be destructible in a disaster but may be used by households as a form of insurance against shocks. Results in columns (5) and (6) show that ownership of land not used for farming or non-farm business activities, thus investment or inherited land, is lower by approximately 14% in earthquake affected areas.

Two other forms of assets that can be used as a buffer stock are jewelry and saving. Franeknberg et al. (2003) have shown that jewelry in particular, was used by Indonesian households during the Asian Financial Crisis, to smooth consumption. In fact, (7) and (8) similarly suggest that households deplete jewelry stocks to insure against earthquake shocks. The fixed effects estimation shows that households' value of jewelry declined by roughly 19% in earthquake areas compared to no earthquake areas. While results in Table 7, columns (9) and (10) show that the value of households savings also declined in earthquake areas, the difference in insignificant and most households in the matched sample do not own a savings account, suggesting that the use of this mechanism by households is limited.

The impact of earthquakes on asset stocks by pre-disaster within village wealth standing is shown in Table 8. While it is expected that wealthier households would lose more assets during an earthquake, one can also assume that wealthier households are likely to have a larger buffer stock of non-destructible assets to insure against shocks. The results in estimation (1) to (4) measure the effect on value of assets and log value of assets. The preferred fixed effect specification (4) suggest that moderately wealthy and wealthiest households lose 36% and 44% of assets respectively. The loss in log value of assets is significant at the 10% level for both groups, relative to the poorest. The results on consumption and wealth effects show that for the specific earthquakes studied, moderately wealthy households fare worse than other groups, in terms of proportional losses relative to the pre-disaster level. From columns (5) and (6), Table 8, this group is also approximately 4% and 10% more likely in earthquake areas to have lost investment land relative to the moderately poor and wealthiest groups, respectively. While the moderately poor and wealthiest groups experienced a probability of de-cumulating land 16% and 21% below poorest households, the results were not significantly different from the poorest counterparts. For the log value of jewelry, earthquake affected moderately poor households observe a 23% decline in the value of jewelry.

Table 9, shows the mean level of assets by wealth group, pre and post-earthquake. As expected, there is a monotonic relationship between the size of assets losses and wealth standing for earthquake areas. However, similar to the impact of earthquakes on consumption, mean assets levels are significantly higher, even in the aftermath of a disaster for wealthier households.

Due to the nature of high impact earthquakes, it is impossible to predict a clear association between earthquakes and impact by household group. This section of results highlights the impact of a transitory shock like earthquakes on the inability of households to smooth consumption, and utilize assets, by within village household wealth standing for the observed earthquakes. The empirical estimates presented above show that differences between the poorest of the poor and moderately poor groups are minimal, and both groups have a negative consumption response to earthquakes.

The results also suggest the both the moderately wealthy and wealthiest were likely to face the largest proportional losses relative to pre-disaster consumption and asset stocks. However, in addition to the negative consumption response, the poorest of the poor in earthquake areas also consume far less, and own fewer assets, than households in other wealth standings. Thus, the results on the distribution of RASKIN post-earthquakes suggests that larger proportional losses are rewarded more than lower absolute consumption in disaster affected communities with decentralized targeting.

1.6.3 Heterogenous Effects: Social Capital and Within Village Access to RASKIN

Better social capital is thought to improve governance (Putnam (1993)). I use observed differences in pre-earthquake social capital to examine how such differences affect the distribution of rice for the poor by wealth standing. The sample is stratified by several cutoffs of the proportion of the village community participating in community meetings in prior survey year. Stratification occurs at strictly greater than, and less than or equal to 70%, 60% and 50% participation. Table 10 shows estimation results on the distribution of RASKIN, of this stratification. From columns (1) to (6) for participation exceeding 70%, 60% and 50%, the distribution of RASKIN is more strongly favored away from poorest households and towards wealthier households in earthquake affected areas. At higher levels of pre-disaster community participation, a larger proportion of poorest households are likely to be excluded from the RASKIN program relative to the poorest households in non-earthquake areas. When participation exceeds 70%, in the fixed effects specification in column (2), 34%, of the poorest households loose access to subsidized rice, while 6% to 7% of each of the moderately poor, moderately wealthy and wealthiest households gain access to the program relative to households in non-earthquake areas. By contrast, from columns (7) to (12), when pre-disaster community participation is below 50%, 60% and 70%, the poorest households in earthquake areas are more likely to gain access to the program. When community participation falls below 50%, from column (8), 27% more poorest households gain access to RASKIN over the wealthiest.

The results suggest that for communities with greater pre-earthquake participation in community meetings, in the aftermath of a disaster the poorest are more likely to be marginalized compared to communities with lower levels of pre-disaster participation [21]. This means, when more households participate in a meeting, more demands of the wealthiest and moderately wealthy households are heard, relative to the poorest, because more households from all wealth standings participate. On the other hand, when fewer households participate, fewer of the poorest are placed out of the program because fewer households from any group participate, and fewer of the wealthier participate as well.

Similar results can be seen in Table 11, which stratifies communities by the number of pre-disaster community social groups. In communities with greater than or equal to 3 or 4 social groups, the poorest households are again more likely to be excluded from RASKIN participation relative to non-earthquake areas and wealthier groups. For communities with fewer than 4 or 5 social groups, I observe a positive, insignificant effect of the shock on RASKIN participation for the poor. The moderately poor however, in earthquake areas are significantly more likely to participate in the RASKIN program regardless of the number of social groups in the village.

Putnam (1993)'s argument that higher levels of social capital improves governance, does not necessarily hold under natural disasters. When social capital - pre-disaster participation in community meeting and number of social groups - is high, in earthquake areas, the demands of wealthier groups may be more likely to be met relative to the poorest households. This suggests that the bargaining power of the wealthier is higher than that of the poor and has a differential effect on outcomes in post-earthquake environments when households face a negative consumption and asset response to shocks.

[21] Analysis of the impact of earthquakes on consumption by wealth standing shows no distinct patterns. The results are not included but may be obtained form the author.

1.6.4 Robustness Checks

1.6.4.1 Household access to RASKIN excluding 25-50 km periphery

For the purpose of the study I define the treatment group as households exposed to an earthquake over magnitude 5.5 within 25 km of the epicenter. If villages in the periphery of the 25 km radius are also affected significantly by a high magnitude earthquake in the 2006 to 2007 period, then the coefficients on household access to RASKIN for the 0-25km group may be biased. Figure A1 shows the number of IFLS villages exposed to an earthquake over magnitude 5.5 within a 25-50 km radius of the epicenter. To establish that the impact of earthquakes on villages within a 0-25 km radius without the bias discussed above, I exclude villages within a 25-50 km radius of the epicenter from the control group. This prevents any bias on the coefficient measuring the post-earthquake impact of RASKIN by incorrect assignment to the control group. The results in Table A4 uses the same specification as Table 2 but excludes the 25-50 km periphery. As with prior results the treatment group is propensity score matched to a similar group of unaffected households to form a control group.

Table A4 shows the post-earthquake distribution of participation in RASKIN, quantity of subsidized rice purchased and price paid for subsidized rice by household pre-disaster wealth ranking. Table A4 excludes villages within 25-50km radius of epicenter. From the results in column (2), household fixed effects specification, the poorest are 12.9% less likely to participate in the rice for poor program relative to other groups in earthquake areas and similar households in non-earthquake areas. Simultaneously, the moderately poor, moderately wealthy and wealthiest groups are 18.8% to 20.2% more likely to participate in RASKIN relative to the poorest.

Columns (3) to (6) in Table A4 suggest that post-earthquake impacts on the distribution of rice for poor at the intensive margin have the largest significant impact on the poorest group of households. From columns (3) and (4), as a result of the earthquake the poorest are likely to purchase 0.59 to 0.89 KG per capita more of rice in the last 4 weeks compared to households in non-earthquake areas and other wealth groups. Columns (5) and (6) show that the poorest in earthquake affected villages pay 18.5 to 31.9 % lower prices for the subsidized rice purchased. The results are comparable both in magnitude and direction to the results in Table 2. These results suggest that there is no significant bias generated from including households with a 25-50 km radius of the epicenter for earthquakes over 5.5 magnitude in the control group.

1.6.4.2 Household access to RASKIN using artificial treatment groups

In order to test the hypothesis that the observed impacts on within village targeting of RASKIN were driven by the impact on the correctly selected treatment group, I create alternative treatment groups to households' falling within 25 km of an earthquake. The first artificial treatment group uses villages within the 25-50 km group as treated, while excluding those in the 0-25km group from the analysis. Similarly, the second artificial treatment group assigns villages in the 50-75 km group as treated and excludes all villages within 50km of the epicenter of a 5.5 or higher magnitude earthquake. This ensures that the observed results on household access to the rice for poor RASKIN program both at the extensive and intensive margins, for households' affected by catastrophic earthquakes, can be attributed to the true earthquake effect. For the difference-in-difference estimation the artificial treatment groups were matched to similar household units using the same PSM method described in the appendix.

Results are shown in Table A5 where the first panel assigns households as exposed to earthquakes if within 25-50 km of epicenter and the second if within 50-75 km of the epicenter. The first two columns of the first panel shows no significant impact of being assigned to the artificial treatment group on household access to RASKIN. The signs on participation in RASKIN is positive even for the poorest households and contradicts the result found using the actual treatment group. Similarly, columns (3) and (4) show no significant coefficients on quantity of subsidized rice purchased through RASKIN. The signs on quantity of subsidized rice purchased by the poorest and moderately poor in the artificial treatment group of the first panel, relative to other groups and the control group, are in the opposite direction to the signs displayed in Table 2 for the actual treatment. Column (6) in the first panel of Table A5 shows a negative significant impact on the post earthquake impact on moderately poor and moderately wealthy groups on price paid for subsidized rice of the artificial treatment group. While this result is unexpected, the direction of the results are the opposite of results in Table 2. The result suggests that for villages in the periphery of the earthquakes of 2006 to 2007, the moderately poor and moderately wealthy may have benefitted from lower prices relative to the poorest households. Very similar results can be observed in the second panel of Table A5 using villages within 50-75 km of epicenter as the artificial treatment group. As with the results in the first panel, relative to the poorest households the moderately poor pay a lower price for the subsidized rice, statistically significant at the 10% level in the household fixed effect specification. While one may expect households within 25-50 km of the epicenter of an over 5.5 magnitude earthquake, it is unlikely that households in a 50-75 radius are also affected. This suggests that some other factor, other than being affected by an earthquake, is driving the observed results on prices for the artificial treatment groups.

1.7 Conclusion

This paper examines whether safety nets are effective in insuring households during disasters. The empirical findings indicate that the poorest of the poor households in earthquake affected villages are significantly more likely to be placed out of the rice for poor program examined, relative to other households within the village and similar households in unaffected villages [22]. Wealthier households, on the other hand, gain greater access to such programs after disasters. The safety net program is designed to provide rice at a subsidized price to the poorest households within a village. Therefore, shifting resources towards less poor households during a crisis should be justified by increased need among the less poor. I show that the non-poor do loose a greater proportion of pre-disaster consumption and of assets, than the poorest households. However, a comparison of level of consumption among poorest and less poor show that the poorest are still poorer post earthquake than wealthier household groups. For the poor that participate in RASKIN, the per capita quantity of rice purchased is higher and the price paid for subsidized rice is lower in earthquake areas.

These findings provide some evidence that reliance on social safety net programs using decentralized targeting in the aftermath of a disaster may be ineffective. In addition, such programs may funnel resources away from the neediest households. From a policy perspective more attention needs to be placed on the way in which such programs are utilized in high disaster risk areas. Alternative designs of such programs and use of other aid programs post disasters may be more effective in

[22] One may argue that an alternative explanation for increased (or decreased) access of households' to safety net resources may be due to increased (or decreased) central government oversight rather than cooperative behavior and subsequent community influence. There are a few reasons why this explanation is unlikely. The empirical analysis relies on a difference in difference (DD) framework that compares earthquake affected areas to similar areas not affected by earthquakes, implying that government oversight would have to increase only in the earthquake affected areas. However, most parts of Indonesia are plagued by several different crisis, making it unlikely that government would not increase oversight on all areas. In addition, even if government increased (or decreased) oversight in just earthquake areas, this would mostly fall into the hands of the local authorities to report, which would once again lead to the problem of their corruption. In-between distribution of rice to the village head and it reaching households, rice goes missing or is not accessible to poor households (Olken (2006)) and increasing oversight would be difficult as village authorities can report doing something different than what actually happens with distribution. In other words, the problem would stem from community influence on the decisions of the local authority driven by cooperation or fragility of the village which works hand in hand with increased or decreased government oversight. The literature on Indonesian safety net programs shows no qualitative evidence of an increase in government oversight in earthquake affected areas.

targeting the neediest households.

Finally, I address how variations in pre-disaster levels of social capital affected the distribution of subsidized rice through the RASKIN program. I find that greater participation in community meetings by villagers, or more social groups in a village, does not lead to an improved outcome for the poorest households. Results in this paper suggest that higher levels of social capital within a village are more likely to lead to a larger diversion of RASKIN resources away from the poorest. Greater participation in community meetings is linked to greater participation among all wealth groups in the village. Intuitively, under greater participation if more elite, or wealthier groups of households have more say in the allocation social safety net resources during a disaster, this could explain the observed result.

1.8 Tables

Table 1: Difference-in-Difference estimation of impact of exposure to earthquakes on household access to RASKIN rice for poor program, propensity score matched sample, using 2000 and 2008 balanced panel

	OPK/ RASKIN (Rice for poor program)					
	Purchased in last 12 months		Per Capita Quantity (KG)		Log Price per KG Real, 2007 Rupiah	
			(| Purchased rice in last 4 weeks)			
	(1)	(2)	(3)	(4)	(5)	(6)
Relative to No Earthquake Community						
2008*Earthquake	0.122*	0.127*	0.347**	0.485**	-0.123***	-0.130***
	(0.065)	(0.073)	(0.170)	(0.206)	(0.041)	(0.051)
Earthquake Area	-0.022		0.046		0.106*	
	(0.064)		(0.206)		(0.061)	
t2008	0.279	0.327**	-0.579**	-1.061***	-0.343***	-0.543***
	(0.196)	(0.105)	(0.255)	(0.402)	(0.110)	(0.130)
HH Controls	YES	YES	YES	YES	YES	YES
Village Controls	YES	YES	YES	YES	YES	YES
HH FE	NO	YES	NO	YES	NO	YES
Province*t2008 FE	YES	YES	YES	YES	YES	YES
Village Clusters	YES	YES	YES	YES	YES	YES
Observations	3,342	3,342	1,200	1,200	1,190	1,190
Number of hhid		1,738		910		900
R-squared within	0.17	0.166	0.286	0.156	0.312	0.528

Notes: Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%
In all subsequent regression tables household controls include age of head, gender of head, years of schooling of head, marital status of head, years in village of household head, household size.
Village controls include number of health posts, proportion of households with electricity, proportion of households with piped water access to large and small microfinance institutions in village, village population, urban, number of earthquakes 2000-2005, number of earthquakes 1975-1999.
Quantity of household purchase of subsidized rice in 2000 derived by dividing the estimated market value of rice purchased through OPK by the average market price of rice. Subsidized price paid by household in 2000 is constructed by dividing the estimated value of purchased subsidized rice by the estimated quantity. Note, for 2007, the quantity and price of subsidized rice are actual reported values.

Table 2: Difference-in-Difference estimation of impact of exposure to earthquakes on household access to RASKIN rice for poor program by within village wealth standing, propensity score matched sample, using 2000 and 2008 balanced panel

	OPK/ RASKIN (Rice for poor program)					
	Purchased in last 12 months		Per Capita Quantity (KG)		Log Price per KG Real, 2007 Rupiah	
			(| Purchased rice in last 4 weeks)			
	(1)	(2)	(3)	(4)	(5)	(6)
Relative to No Earthquake Community						
2008*Earthquake	-0.092	-0.116*	0.452**	0.519**	-0.244***	-0.225***
	(0.060)	(0.067)	(0.311)	(0.236)	(0.086)	(0.083)
Relative to Poorest Households within village (at t-1)*						
2008*Earthquake* Moderately Poor t-1	0.138*	0.176*	0.481	0.122	0.093	0.191
	(0.080)	(0.098)	(0.363)	(0.229)	(0.085)	(0.159)
2008*Earthquake* Moderately Wealthy t-1	0.149*	0.203*	-0.184	-0.261	0.06	0.003
	(0.076)	(0.107)	(0.339)	(0.386)	(0.008)	(0.112)
2008*Earthquake*Wealthiest t-1	0.182	0.245**	-0.141	-0.767**	0.035	0.051
	(0.117)	(0.118)	(0.398)	(0.379)	(0.101)	(0.173)
Earthquake Area	0.046		0.178		0.116*	
	(0.085)		(0.187)		(0.052)	
2008*Moderately Poor t-1	-0.061	-0.109*	-0.042	-0.074	-0.071	-0.082
	(0.070)	(0.064)	(0.277)	(0.238)	(0.045)	(0.078)
2008* Moderately Wealthy t-1	-0117*	-0.131**	0.188	0.262	-0.049	-0.029
	(0.067)	(0.063)	(0.252)	(0.258)	(0.045)	(0.076)
2008*Wealthiest t-1	-0.185**	-0.195***	0.304	0.221	-0.080*	-0.136
	(0.083)	(0.069)	(0.241)	(0.320)	(0.044)	(0.091)
Earthquake*Moderately Poor t-1	-0.084	-0.037	-0.409	-0.269	-0.016	-0.091
	(0.081)	(0.095)	(0.310)	(0.302)	(0.055)	(0.112)
Earthquake* Moderately Wealthy t-1	-0.027	-0.007	-0.132	-0.278	0.002	0.121
	(0.076)	(0.094)	(0.280)	(0.296)	(0.068)	(0.114)
Earthquake*Wealthiest t-1	-0.208**	-0.146	-0.595	-0.242	-0.06	-0.003
	(0.085)	(0.135)	(0.506)	(0.424)	(0.069)	(0.143)
Moderately Poor t-1	-0.003	0.008	0.148	0.304*	0.025	0.034
	(0.051)	(0.058)	(0.189)	(0.157)	(0.034)	(0.054)
Moderately Wealthy t-1	-0.164***	-0.134**	-0.077	-0.237	-0.016	-0.009
	(0.043)	(0.063)	(0.148)	(0.197)	(0.034)	(0.065)
Wealthiest t-1	-0.153***	-0.158**	0.082	0.421*	0.003	0.048
	(0.054)	(0.066)	(0.262)	(0.244)	(0.038)	(0.097)
t2008	0.32	0.369***	0.346	0.301	-0.284**	-0.479***
	(0.201)	(0.127)	(0.413)	(0.612)	(0.110)	(0.146)
HH Controls	YES	YES	YES	YES	YES	YES
Village Controls	YES	YES	YES	YES	YES	YES
HH FE	NO	YES	NO	YES	NO	YES
Province*t FE	YES	YES	YES	YES	YES	YES
Village Clusters	YES	YES	YES	YES	YES	YES
Observations	3,342	3,342	1,200	1,200	1,190	1,190
Number of hhid		1,738		900		900
R-squared within	0.173	0.187	0.314	0.182	0.317	0.553

Notes: Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1% Quantity of household purchase of subsidized rice in 2000 derived by dividing the estimated market value of rice purchased through OPK by the average market price of rice.
Subsidized price paid by household in 2000 is constructed by dividing the estimated value of purchased subsidized rice by the estimated quantity. Note, for 2007, the quantity and price of subsidized rice is actual reported quantity.

Table 3: Access to the rice for poor program for households in earthquake affected and unaffected areas, by within village wealth standing

	Proportion (/100) households purchased from RASKIN (rice for poor) in last 12 months						
	Communities not affected by earthquake			Earthquake Affected Communities			
Within village wealth standing	Pre-earthquake, 2000 (n=1170)	Post-earthquake, 2008 (n=1161)	**Difference (1)**	Pre-earthquake, 2000 (n=570)	Post-earthquake, 2008 (n=568)	**Difference (2)**	**Difference (2)- Difference (1)**
Poorest	0.570	0.826	0.256	0.582	0.747	0.165	-0.091*
	(0.496)	(0.411)		(0.500)	(0.425)		
Moderately Poor	0.523	0.672	0.149	0.508	0.726	0.218	0.069*
	(0.500)	(0.470)		(0.501)	(0.447)		
Moderately Wealthy	0.404	0.529	0.125	0.440	0.583	0.143	0.018**
	(0.491)	(0.497)		(0.498)	(0.494)		
Wealthiest	0.397	0.408	0.011	0.194	0.454	0.26	0.249**
	(0.491)	(0.499)		(0.397)	(0.493)		

Table 4: Difference-in-Difference estimation of impact of exposure to earthquakes on household consumption smoothing, propensity score matched sample, using 2000 and 2008 balanced panel

	Log Per Capita All Consumption (excl. durables)		Log Per Capita Food		Log Per Capita Non Food 1		Log Per Capita Non Food 2		Log Per Capita Durables (| > 0 spending)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Relative to No Earthquake Community										
2008*Earthquake	-0.105*	-0.118**	-0.102*	-0.107**	-0.082	-0.098*	-0.217**	-0.263**	-0.306**	-0.386**
	(0.058)	(0.053)	(0.054)	(0.053)	(0.052)	(0.056)	(0.092)	(0.115)	(0.152)	(0.166)
Earthquake Area	0.017		0.004		-0.081		0.136		0.239	
	(0.056)		(0.053)		(0.078)		(0.081)		(0.265)	
t2008	0.009	0.067	-0.042	-0.06	0.229***	0.375***	-0.199***	-0.089	0.131	0.665**
	(0.038)	(0.046)	(0.036)	(0.046)	(0.061)	(0.057)	(0.076)	(0.097)	(0.173)	(0.265)
HH Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Village Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
HH FE	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Province*t2008 FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Village Clusters	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	3,347	3,347	3,347	3,347	3,347	3,347	3,347	3,347	1,686	1,686
Number of hhid		1,738		1,738		1,738		1,738		1,239
R-squared within	0.263	0.123	0.228	0.134	0.300	0.221	0.194	0.128	0.085	0.054

Notes: Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1% Results on 2008*Earthquake are robust to the inclusion of the 1998 panel. Household real per capita consumption exclude durables consumption, 2000 data adjusted using CPI. Non-food1 expenditure (monthly) includes spending on 1)electricity, water, fuel, tel etc., 2)personal toiletries, 3)household items, 4)domestic services(servants wages etc), 4)transport (gasoline, bus/cab fares etc), 4)arisan 5)recreation & entertainment.

Non-food 2 expenditure (annual-converted monthly) includes 1)clothing for children and adults, 2)rituals, ceremonies, charities, gifts 3) taxes.

Table 5: Difference-in-Difference estimation of impact of exposure to earthquakes on consumption smoothing by within village household wealth standing, propensity score matched sample, using 2000 and 2008 balanced panel

	Log Per Capita All Consumption (excl. durables)		Log Per Capita Food		Log Per Capita Non Food 1		Log Per Capita Non Food 2		Log Per Capita Durables (| > 0 spending)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Relative to No Earthquake Community										
2008*Earthquake	-0.105	-0.124*	-0.104	-0.11	-0.109	-0.124	-0.121	-0.244*	0.15	0.464
	(0.075)	(0.076)	(0.120)	(0.144)	(0.136)	(0.116)	(0.111)	(0.143)	(0.241)	(0.476)
Relative to Poorest Households within village (at t-1)*										
2008*Earthquake* Moderately Poor t-1	0.046	0.126	0.027	0.037	0.183	0.228	-0.128	-0.152	-0.419	-1.807*
	(0.118)	(0.127)	(0.135)	(0.141)	(0.197)	(0.194)	(0.178)	(0.234)	(0.654)	(1.059)
2008*Earthquake* Moderately Wealthy t-1	-0.234*	-0.305**	-0.114	-0.239*	-0.168	-0.223	-0.290*	-0.405***	-0.598	-2.326**
	(0.123)	(0.151)	(0.129)	(0.136)	(0.216)	(0.263)	(0.149)	(0.167)	(0.639)	(0.957)
2008*Earthquake* Wealthiest t-1	-0.227	-0.285*	-0.13	-0.154	-0.173	-0.187	-0.13	-0.355**	-1.374**	-2.342**
	(0.153)	(0.165)	(0.153)	(0.162)	(0.123)	(0.134)	(0.129)	(0.168)	(0.622)	(1.054)
Earthquake Area	-0.087		-0.058		-0.146		0.118		0.308	
	(0.083)		(0.081)		(0.150)		(0.150)		(0.355)	
2008* Moderately Poor t-1	-0.046	-0.049	-0.005	-0.037	-0.023	-0.08	-0.181	-0.106	0.162	0.501
	(0.105)	(0.118)	(0.102)	(0.119)	(0.152)	(0.183)	(0.193)	(0.210)	(0.410)	(0.786)
2008* Moderately Wealthy t-1	-0.002	-0.034	-0.004	0.071	0.052	0.033	0.038	0.104	0.2	0.603
	(0.100)	(0.111)	(0.097)	(0.107)	(0.159)	(0.192)	(0.178)	(0.143)	(0.446)	(0.647)
2008* Wealthiest t-1	-0.006	-0.097	-0.007	-0.126	0.057	0.039	-0.124	-0.154	-0.107	-0.326
	(0.098)	(0.115)	(0.099)	(0.109)	(0.153)	(0.196)	(0.191)	(0.232)	(0.405)	(0.608)
Earthquake* Moderately Poor t-1	0.164*	0.287**	0.083	0.190*	0.111	0.136	0.164	0.397	0.106	0.959
	(0.098)	(0.115)	(0.095)	(0.105)	(0.156)	(0.197)	(0.158)	(0.241)	(0.409)	(0.904)
Earthquake* Moderately Wealthy t-1	0.160*	0.297**	0.109	0.218*	0.176	0.139	0.126	0.419*	0.183	2.221**
	(0.096)	(0.116)	(0.098)	(0.116)	(0.142)	(0.194)	(0.167)	(0.233)	(0.473)	(1.017)
Earthquake* Wealthiest t-1	0.094	0.158	0.220**	0.186	0.019	0.052	0.097	0.347	0.344	1.515
	(0.107)	(0.165)	(0.109)	(0.188)	(0.165)	(0.237)	(0.190)	(0.291)	(0.425)	(1.210)
Moderately Poor t-1	0.073	0.033	0.066	0.03	0.184**	0.115	0.245**	0.222*	0.089	0.225
	(0.049)	(0.080)	(0.073)	(0.077)	(0.090)	(0.121)	(0.105)	(0.135)	(0.252)	(0.571)
Moderately Wealthy t-1	0.286***	0.05	0.246***	0.048	0.480***	0.118	0.550***	0.182	0.38	0.904
	(0.066)	(0.084)	(0.065)	(0.068)	(0.092)	(0.123)	(0.117)	(0.160)	(0.244)	(0.609)
Wealthiest t-1	0.574***	0.04	0.492***	0.018	0.890***	0.1	0.961***	0.043	1.094***	1.331**
	(0.077)	(0.104)	(0.078)	(0.123)	(0.109)	(0.141)	(0.143)	(0.184)	(0.258)	(0.654)
t2008	0.1	0.11	-0.044	0.05	0.16	0.379**	-0.115	-0.54	0.049	0.241
	(0.079)	(0.097)	(0.080)	(0.097)	(0.132)	(0.159)	(0.153)	(0.690)	(0.309)	(0.611)
HH Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Village Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
HH FE	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Province*t2008 FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Village Clusters	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	3,347	3,347	3,347	3,347	3,347	3,347	3,347	3,347	1,686	1,686
Number of hhid		1,738		1,738		1,738		1,738		1,239
R-squared within	0.316	0.149	0.268	0.161	0.358	0.229	0.236	0.141	0.104	0.094

Notes: Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1% Results on 2008*Earthquake are robust to the inclusion of the 1998 panel. Household real per capita consumption exclude durables consumption, 2000 data adjusted using CPI. Non-food1 expenditure (monthly) includes spending on 1)electricity, water, fuel, tel etc., 2)personal toiletries, 3)household items, 4)domestic services(servants wages etc), 4)transport (gasoline, bus/cab fares etc), 4)arisan 5)recreation & entertainment.

Non-food 2 expenditure (annual-converted monthly) includes 1)clothing for children and adults, 2)rituals, ceremonies, charities, gifts 3) taxes.

Table 6: Change in real mean per capita consumption for households in earthquake affected and unaffected areas, by within village wealth standing

	Real Household Per Capita Consumption, Indonesian Rupiah						
	Communities not affected by earthquake			Earthquake Affected Communities			
Within village wealth standing	Pre-earthquake, 2000 (n=1170)	Post-earthquake, 2008 (n=1161)	**Difference (1)**	Pre-earthquake, 2000 (n=570)	Post-earthquake, 2008 (n=568)	**Difference (2)**	**Difference (2)-Difference (1)**
Poorest	203,222	227,618	24,396	198,368	210,727	12,359	-12,037*
	(17660)	(33893)		(13510)	(27991)		
Moderately Poor	215,950	230,474	14,524	226,680	232,248	5,568	-8,956
	(11627)	(12596)		(20420)	(49519)		
Moderately Wealthy	285,140	308,346	23,206	306,521	291,984	-14,537	-37,743**
	(22356)	(13769)		(30501)	(16959)		
Wealthiest	370,652	384,363	13,711	362,737	356,040	-6,697	-20,408**
	(18055)	(21684)		(29686)	(33411)		

Table 7: Difference-in-Difference estimation of impact of exposure to earthquakes on household asset stocks, propensity score matched sample, using 2000 and 2008 balanced panel

	Total Value of Assets, Real, (Rupiah, x10,000)		Log Value of Assets		Land Ownership (not used for farm or nonfarm business, 0/1)		Log Value of Jewelry (|own jewelry)		Log Value of Savings, CD or stocks (| have savings)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Relative to No Earthquake Community										
2008*Earthquake										
	-1,630.233*	-3,228.872**	-0.197*	-0.270***	-0.126***	-0.140***	-0.071	-0.185*	-0.27	-0.267
	(955.094)	(1337.665)	(0.113)	(0.087)	(0.044)	(0.039)	(0.053)	(0.100)	(0.265)	(0.317)
Earthquake Area	-232.29		-0.131		0.106*		-0.063		-0.084	
	(1088.794)		(0.112)		(0.059)		(0.135)		(0.205)	
t2008	807.039	1,519.75	0.152**	0.173**	-0.022	-0.013	-0.175*	-0.112	0.121	0.318
	(834.477)	(935.124)	(0.072)	(0.086)	(0.026)	(0.032)	(0.095)	(0.140)	(0.198)	(0.415)
HH Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Village Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
HH FE	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Province*t FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Village Clusters	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	3,328	3,328	3,328	3,328	3,328	3,328	1,812	1,812	764	764
Number of hhid		1,735		1,735		1,735		1,252		609
R-squared within	0.148	0.118	0.223	0.141	0.079	0.133	0.164	0.142	0.143	0.073

Notes: Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1% Results on 2008*Earthquake are robust to the inclusion of the 1998 panel. Household value of assets is the Rupiah value sum of house and land occupied by household, other house or building, land (not used for farm non-farm business), poultry, livestock, vehicles, appliances, savings/CDs/stocks, receivables, jewelry, household furniture and utensils, other assets.

Table 8: Difference-in-Difference estimation of impact of exposure to earthquakes on assets by within village household wealth standing, propensity score matched sample, using 2000 and 2008 balanced panel

	Total Value of Assets, Real, (Rupiah, x10,000)		Log Value of Assets		Land Ownership (not used for farm or nonfarm business, 0/1)		Log Value of Jewelry (|own jewelry)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Relative to No Earthquake Community								
2008*Earthquake	719.042	1,469.12	0.375	0.298	-0.032	-0.033	0.325	0.651
	(1974.689)	(1709.006)	(0.277)	(0.308)	(0.080)	(0.100)	(0.367)	(0.640)
Relative to Poorest Households within village (at t-1)*								
2008*Earthquake*	-2,193.524*	-1,753.57	-0.512*	-0.375	-0.075	-0.155	-0.208	-0.225*
Moderately Poor t-1	(1279.879)	(1617.544)	(0.306)	(0.364)	(0.085)	(0.113)	(0.204)	(0.130)
2008*Earthquake*	-2,549.414**	-2,419.800*	-0.625**	-0.644*	-0.150**	-0.257**	-0.151	-0.297
Moderately Wealthy t-1	(1244.552)	(1248.407)	(0.308)	(0.356)	(0.075)	(0.101)	(0.422)	(0.763)
2008*Earthquake*	-5,879.707**	-5,197.293*	-0.646**	-0.741*	-0.145	-0.219	-0.559	-0.439
Wealthiest t-1	(2628.635)	(3033.490)	(0.314)	(0.389)	(0.095)	(0.145)	(0.468)	(0.696)
Earthquake Area	-1,657.81		-0.258		0.052		-0.429	
	(1073.856)		(0.214)		(0.063)		(0.364)	
2008*	957.649	2,249.58	-0.027	-0.107	0.044	0.091	-0.151	-1.510***
Moderately Poor t-1	(1071.728)	(1419.956)	(0.218)	(0.279)	(0.038)	(0.071)	(0.549)	(0.549)
2008*	708.012	1,775.05	-0.057	-0.215	0.045	0.107	-0.452*	-1.444**
Moderately Wealthy t-1	(1231.644)	(1176.095)	(0.174)	(0.153)	(0.056)	(0.078)	(0.254)	(0.607)
2008*	-618.168	-837.061	-0.115	-0.527*	0.049	0.037	-0.298	-1.550***
Wealthiest t-1	(1,762.590)	(1,842.658)	(0.224)	(0.275)	(0.065)	(0.083)	(0.309)	(0.577)
Earthquake*	2,666.954**	3,094.00	0.632***	0.785***	0.085	0.122	0.513*	0.524
Moderately Poor t-1	(1,224.848)	(1,962.338)	(0.235)	(0.276)	(0.066)	(0.092)	(0.298)	(0.685)
Earthquake*	1,406.77	3,362.81	0.502**	0.625**	0.086	0.031	0.531*	0.88
Moderately Wealthy t-1	(1,785.147)	(3,316.138)	(0.229)	(0.295)	(0.056)	(0.089)	(0.284)	(0.599)
Earthquake*	4,762.98	2,727.62	0.486**	0.449	0.045	0.026	0.665*	0.728
Wealthiest t-1	(4,258.414)	(5,412.760)	(0.236)	(0.362)	(0.076)	(0.126)	(0.345)	(0.675)
Moderately Poor t-1	757.057	1140.995	0.199	0.341	0.025	0.002	0.126	0.408
	(651.269)	(985.745)	(0.169)	(0.206)	(0.044)	(0.048)	(0.182)	(0.335)
Moderately Wealthy t-1	3,133.188***	1,526.58	0.774***	0.262	0.018	0.013	0.709***	0.15
	(1,733.032)	(1,692.269)	(0.161)	(0.207)	(0.045)	(0.053)	(0.201)	(0.324)
Wealthiest t-1	8,607.498***	1715.022	1.294***	0.229	0.072	0.005	1.026***	0.287
	(1,692.577)	(1,792.772)	(0.175)	(0.256)	(0.048)	(0.071)	(0.224)	(0.340)
t2008	-940.327	-236.993	0.15	0.365	-0.053	-0.073	0.139	0.199
	(882.034)	(956.583)	(0.182)	(0.237)	(0.052)	(0.072)	(0.254)	(0.301)
HH Controls	YES	YES	YES	YES	YES	YES	YES	YES
Village Controls	YES	YES	YES	YES	YES	YES	YES	YES
HH FE	NO	YES	NO	YES	NO	YES	NO	YES
Province*t2008 FE	YES	YES	YES	YES	YES	YES	YES	YES
Village Clusters	YES	YES	YES	YES	YES	YES	YES	YES
Observations	3,328	3,328	3,328	3,328	3,328	3,328	1,812	1,812
Number of hhid		1,735		1,735		1,735		1,252
R-squared within	0.189	0.033	0.286	0.065	0.09	0.137	0.242	0.048

Notes: Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1% Results on 2008*Earthquake are robust to the inclusion of the 1998 panel. Household value of assets is the Rupiah value sum of house and land occupied by household, other house or building, land (not used for farm non-farm business), poultry, livestock, vehicles, appliances, savings/CDs/stocks, receivables, jewelry, household furniture and utensils, other assets.

Table 9 :Change in real mean per capita consumption for households in earthquake affected and unaffected areas, by within village wealth standing

	Real Total Household Value of Assets, Indonesian Rupiah (x10, 000)						
	Communities not affected by earthquake			Earthquake Affected Communities			
Within village wealth standing	Pre-earthquake, 2000 (n=1170)	Post-earthquake, 2008 (n=1148)	**Difference (1)**	Pre-earthquake, 2000 (n=570)	Post-earthquake, 2008 (n=562)	**Difference (2)**	**Difference (2)- Difference (1)**
Poorest	3,582	4,033	451	3,252	5,097	1,845	1,394
	(525)	(565)		(694)	(547)		
Moderately Poor	5,123	6,001	878	4,633	5,110	477	-401
	(408)	(570)		(654)	(1610)		
Moderately Wealthy	7,708	9,495	1,787	9,146	8,041	-1,105	-2,892**
	(830)	(721)		(1211)	(1115)		
Wealthiest	16,525	17,748	1,223	18,051	14,043	-4,008	-5,231**
	(1663)	(1479)		(3114)	(1877)		

Table 10 : Difference-in-Difference estimation of impact of exposure to earthquakes on household access to OPK rice for poor program by pre-disaster village participation in community meetings, propensity score matched sample, using 2000 and 2008 balanced panel

	OPK/ RASKIN (Rice for poor program) Purchased OPK rice in the last 12 months					
	Sample stratified at cutoff proportion of pre-disaster participation in community meeting					
	Above 0.7	**Less than or equal to 0.7**	**Above 0.6**	**Less than or equal to 0.6**	**Above 0.5**	**Less than or equal to 0.5**
	(1)	(2)	(3)	(4)	(5)	(6)
Relative to No Earthquake Community						
2008*Earthquake	-0.335*	0.228	-0.319**	0.23	-0.241	0.265**
	(0.171)	(0.159)	(0.125)	(0.183)	(0.118)	(0.133)
Relative to Poorest Households within village (at t-1)*						
2008*Earthquake* Moderately Poor t-1	0.398*	0.043	0.399**	0.063	0.277**	0.072
	(0.217)	(0.450)	(0.170)	(0.230)	(0.137)	(0.122)
2008*Earthquake* Moderately Wealthy t-1	0.407**	0.539	0.314**	0.335*	0.320**	-0.188
	(0.160)	(0.422)	(0.149)	(0.137)	(0.127)	(0.161)
2008*Earthquake*Wealthiest t-1	0.405*	-0.418**	0.376**	-0.072	0.304*	-0.04
	(0.210)	(0.190)	(0.179)	(0.264)	(0.161)	(0.156)
t2008	0.289**	0.363*	0.318**	0.192*	0.401**	0.274***
	(0.147)	(0.213)	(0.153)	(0.108)	(0.197)	(0.103)
HH Controls	YES	YES	YES	YES	YES	YES
Village Controls	YES	YES	YES	YES	YES	YES
HH FE	YES	YES	YES	YES	YES	YES
Province*t2008 FE	YES	YES	YES	YES	YES	YES
Village Clusters	YES	YES	YES	YES	YES	YES
Observations	1,086	1,374	1,547	1,795	1,968	2,256
Number of hhid	678	1004	1,031	1,187	1,283	1,304
R-squared within	0.177	0.178	0.147	0.169	0.189	0.21

Notes: Interactions - post-year (2008)*wealth level, earthquake*wealth level, and wealth standing -are not shown in the table but included in the regressions, for clarity, and are available from author. Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%

Table 11: Difference-in-Difference estimation of impact of exposure to earthquakes on household access to OPK rice for poor program by pre-disaster number of community groups in village, propensity score matched sample, using 2000 and 2008 balanced pane

	OPK/ RASKIN (Rice for poor program) Purchased OPK rice in the last 12 months			
	Sample stratified at cutoff number of pre-disaster community groups			
	Groups≥4	Groups<5	Groups ≥3	Groups<4
	(1)	(2)	(3)	(4)
Relative to No Earthquake Community				
2008*Earthquake	-0.217*	0.225	-0.115*	0.22
	(0.121)	(0.190)	(0.064)	(0.247)
Relative to Poorest Households within village (at t-1)*				
2008*Earthquake* Moderately Poor t-1	0.136	0.175	0.262**	0.297*
	(0.146)	(0.167)	(0.122)	(0.156)
2008*Earthquake* Moderately Wealthy t-1	0.181	0.071	0.216*	0.208
	(0.225)	(0.202)	(0.116)	(0.264)
2008*Earthquake*Wealthiest t-1	0.357*	-0.039	0.205*	-0.057
	(0.179)	(0.167)	(0.122)	(0.212)
t2008	0.208	0.593***	0.323*	0.432**
	(0.230)	(0.174)	(0.165)	(0.183)
HH Controls	YES	YES	YES	YES
Village Controls	YES	YES	YES	YES
HH FE	YES	YES	YES	YES
Province*t2008 FE	YES	YES	YES	YES
Village Clusters	YES	YES	YES	YES
Observations	1,905	2,431	2,768	1,437
Number of hhid	0.214	0.209	0.199	0.202
R-squared within	1,358	1,529	1,636	1,091

Notes: Interactions - post-year (2008)*wealth level, earthquake*wealth level, and wealth standing -are not shown in the table but included in the regressions, for clarity, and are available from author. Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%

1.9 Appendix A

A.1 : Summary Statistics exposure to earthquakes over magnitude 5.5, within 25 km of epicenter for matched and unmatched samples

	Unmatched Sample					**Matched using IFLS3 Sample**				
	Unaffected HH		Affected HH		Difference in Means	Unaffected HH		Affected HH		Difference in Means
	(N=6005)		(N=723)			(N=1170)		(N=570)		
	Mean	Std. error	Mean	Std. error	P-value	Mean	Std. error	Mean	Std. error	P-value
Household Characteristics										
Age of HH head	47.607	(0.191)	49.385	(0.605)	0.003	48.813	(0.472)	49.639	(0.633)	0.296
Gender of HH head (proportion male)	0.791	(0.005)	0.812	(0.015)	0.186	0.827	(0.013)	0.835	(0.016)	0.686
Education of HH head										
Primary or less (% /100)	0.672	(0.006)	0.585	(0.019)	0.000	0.621	(0.018)	0.621	(0.021)	0.693
High school (% /100)	0.274	(0.006)	0.329	(0.018)	0.002	0.293	(0.017)	0.298	(0.019)	0.452
University (% /100)	0.053	(0.003)	0.084	(0.010)	0.001	0.083	(0.010)	0.078	(0.011)	0.614
Years of Schooling HH head	5.794	(0.059)	6.66	(0.183)	0.000	6.301	(0.171)	6.25	(0.203)	0.744
Years in Village HH head	43.45	(0.249)	44.729	(0.785)	0.091	44.429	(0.621)	45.911	(0.790)	0.141
Number of HH members	5.601	(0.035)	5.459	(0.105)	0.183	5.89	(0.083)	5.81	(0.121)	0.523
Community Characteristics										
for Sample HH										
Urban (% /100)	0.411	(0.007)	0.548	(0.021)	0.000	0.541	(0.018)	0.552	(0.021)	0.695
Electricity (% /100)	0.778	(0.393)	0.833	(0.966)	0.000	79.502	(1.154)	80.677	(1.155)	0.472
Piped Water (% /100)	0.224	(0.006)	0.325	(0.019)	0.000	0.253	(0.016)	0.236	(0.018)	0.480
Number of Health Posts Per Capita	0.0002	0.000	0.001	0.000	0.000	0.001	0.000	0.001	0.000	0.664
Midwife in Village	0.555	(0.007)	0.772	(0.020)	0.000	0.76	(0.014)	0.757	(0.018)	0.873
Village Size (Ha.)	61064	(4961.000)	56548	(6205.000)	0.754	2372.76	(440.974)	2119.53	(302.506)	0.676
Total Population	10054	(274.000)	9417	(268.000)	0.422	9392.955	(328.251)	9494.586	(283.495)	0.815
Outcome of Interest (Real, 2010 Rupiah)										
Monthly Per Capita Consumption (Total, excl. durables, Rupiah)	261743	(3944.000)	287620	(11862.000)	0.099	270656	(12292.000)	274070	(13877.000)	0.854
Monthly Per Capita) Consumption, Food (Rupiah)	182899	(2247.000)	204301	(6884.000)	0.094	185608	(5908.000)	185706	(7956.000)	0.992
Monthly Per Capita Consumption, Non Food (Rupiah)	82569	(2550.000)	91329	(6568.000)	0.155	84884	(9355.000)	87866	(7881.000)	0.847
Monthly Per Capita Durables (Rupiah)						23654	(5633.000)	25026	(5592.000)	0.582
Total Value of HH assets, (Rupiah)	$6.27x10^6$	$(0.232x10^6)$	$7.74x10^7$	$(0.684x10^6)$	0.196	$7.89x10^7$	$(0.517x10^6)$	$8.40x10^7$	$(0.856x10^6)$	0.611
HH Wealth (factor score)	-0.125	(0.023)	0.043	(0.063)	0.012	0.132	(0.059)	0.083	(0.070)	0.593
Own Land, (Non-farm and non-business) (% /100)	0.162	(0.005)	0.303	(0.017)	0.000	0.181	(0.014)	0.208	(0.018)	0.200
Purchased rice from RASKIN program in last 12 months (% /100)	0.399	(0.018)	0.424	(0.006)	0.213	0.452	(0.021)	0.435	(0.017)	0.598
Per capita Q of RASKIN rice (| purchase in the last 4 weeks (a))	1.457	(0.034)	1.451	(0.091)	0.689	1.448	(0.080)	1.426	(0.104)	0.554
Price of subsidized rice (Rupiah/KG)	2012.397	(36.127)	2614.296	(78.254)	0.016	2195.692	(24.880)	2428.47	(40.500)	0.211

A.2 : Household ownership of assets by within village wealth standing, pre-disaster 1998 and 2000 assets, propensity score matched sample

Proportion of households owning given asset	Household within village wealth standing* - 1998				Household within village wealth standing* - 2000			
	Poorest	Poor Poor	Moderately Wealthy	Wealthiest t	Poorest	Moderately Poor	Moderately Wealthy	Wealthiest Wealthiest
	(n=303)	(n=558)	(n=601)	(n=278)	(n=274)	(n=574)	(n=577)	(n=315)
Owns house occupied by household	0.861	0.884	0.872	0.897	0.793	0.814	0.86	0.913
	(0.347)	(0.320)	(0.334)	(0.339)	(0.406)	(0.390)	(0.348)	(0.283)
Owns other house/ building	0.026	0.049	0.121	0.261	0.039	0.067	0.155	0.277
	(0.160)	(0.216)	(0.326)	(0.429)	(0.370)	(0.362)	(0.431)	(0.448)
Owns non-agricultural land	0.27	0.308	0.335	0.464	0.163	0.155	0.246	0.349
	(0.445)	(0.462)	(0.472)	(0.500)	(0.384)	(0.480)	(0.492)	(0.477)
Vehicles (cars, boats, bicycles, motorbikes)	0.233	0.38	0.634	0.721	0.179	0.359	0.59	0.717
	(0.423)	(0.485)	(0.482)	(0.449)	(0.482)	(0.445)	(0.233)	(0.451)
Owns TV	0.131	0.376	0.792	0.939	0.124	0.48	0.802	0.96
	(0.339)	(0.485)	(0.406)	(0.239)	(0.330)	(0.550)	(0.398)	(0.197)
Owns refrigerator	0.127	0.172	0.361	0.543	0.107	0.162	0.306	0.555
	(0.334)	(0.378)	(0.481)	(0.499)	(0.310)	(0.369)	(0.469)	(0.498)
Owns jewelry	0.199	0.441	0.694	0.875	0.183	0.462	0.707	0.903
	(0.400)	(0.497)	(0.455)	(0.331)	(0.388)	(0.499)	(0.407)	(0.296)
Savings/ CD/ Stocks	0.045	0.104	0.301	0.643	0.068	0.169	0.329	0.698
	(0.208)	(0.305)	(0.459)	(0.480)	(0.252)	(0.376)	(0.470)	(0.460)
Uses electric or gas stove for cooking	0.004	0.011	0.066	0.282	0.007	0.022	0.103	0.361
	(0.061)	(0.108)	(0.249)	(0.451)	(0.071)	(0.148)	(0.303)	(0.481)
Main water source located inside the house	0.198	0.277	0.442	0.508	0.27	0.368	0.499	0.629
	(0.399)	(0.448)	(0.497)	(0.501)	(0.444)	(0.482)	(0.500)	(0.484)
Household has toilet with septic tank	0.18	0.325	0.569	0.761	0.195	0.35	0.588	0.729
	(0.385)	(0.469)	(0.496)	(0.427)	(0.397)	(0.477)	(0.493)	(0.445)
Household sewage drained via flowing drainage ditch	0.289	0.3	0.471	0.546	0.363	0.458	0.5	0.579
	(0.454)	(0.459)	(0.500)	(0.498)	(0.482)	(0.500)	(0.500)	(0.494)

A.3 : Household within village wealth standing for 2000 sample, by pre-disaster 2000 assets, propensity score matched sample ? proportion affected by earthquake vs. unaffected

	HH in village unaffected by earthquake	HH in village affected by earthquake
Poorest	207	96
Moderately Poor	371	187
Moderately Wealthy	408	193
Wealthiest	184	94
	1,170	570

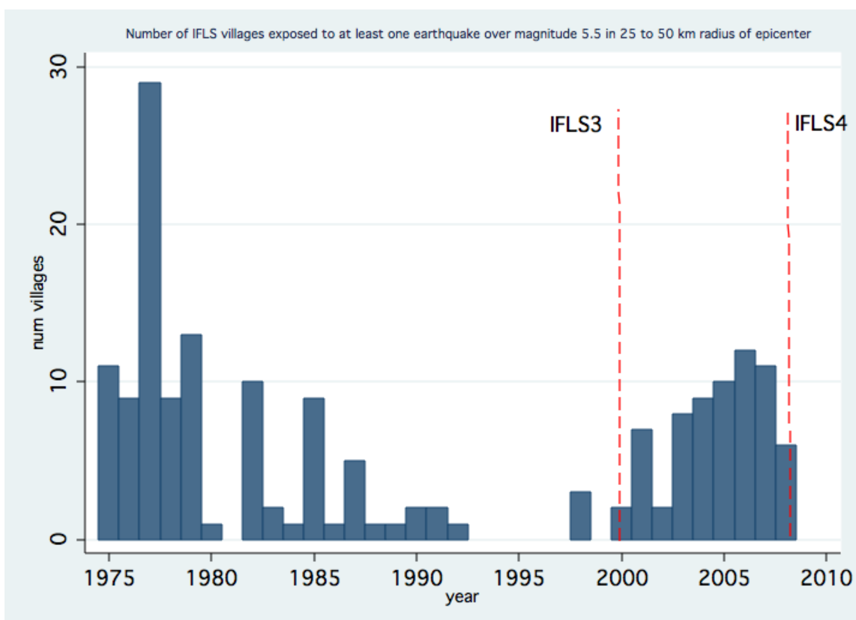


Figure A1. Number of IFLS villages exposed to at least one earthquakes over magnitude 5.5 in 25 to 50 km radius of epicenter

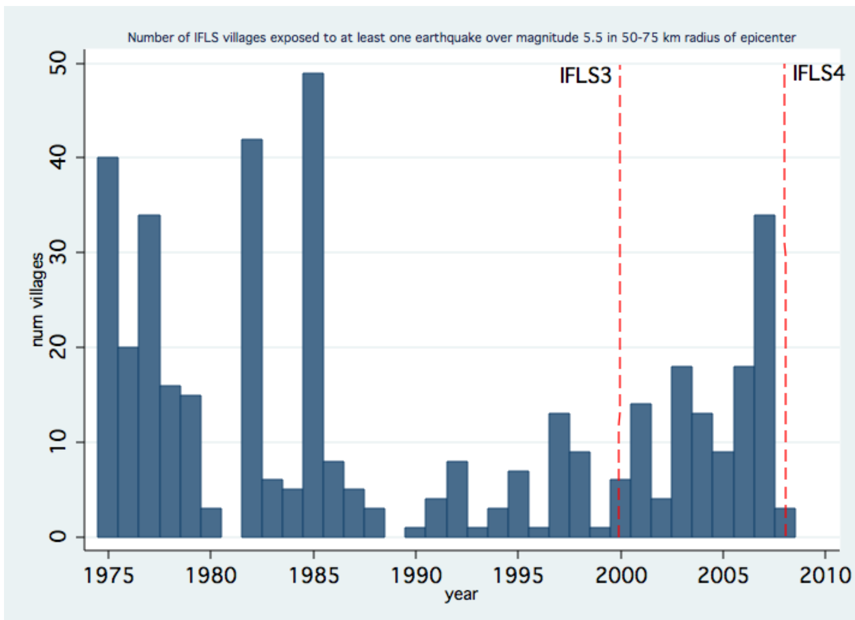


Figure A.2 Number of IFLS villages exposed to at least one earthquakes over magnitude 5.5 in 50 to 75 km radius of epicenter

A.4 : **Robustness checks** Difference-in-Difference estimation on household access to RASKIN, sample excludes households in periphery of treatment group, propensity score matched sample, using 2000 and 2008 balanced panel

	OPK/ RASKIN (Rice for poor program)					
	Purchased in last 12 months		Per Capita Quantity (KG)		Log Price per KG Real, 2007 Rupiah	
			(| Purchased rice in last 4 weeks)			
	(1)	(2)	(3)	(4)	(5)	(6)
	Treatment group: Households Exposed to Earthquake within 25 km of epicenter (EXCLUDING HH IN PERIPHERY, 25-50 km)					
Relative to No Earthquake Community						
2008*Earthquake	-0.115	-0.129*	0.591*	0.810*	-0.185*	-0.319**
	(0.082)	(0.073)	(0.331)	(0.396)	(0.094)	(0.149)
Relative to Poorest Households within village (at t-1)*						
2008*Earthquake*	0.193**	0.202*	0.17	0.178	0.09	0.122
Moderately Poor t-1	(0.094)	(0.104)	(0.418)	(0.430)	(0.103)	(0.173)
2008*Earthquake*	0.16	0.188*	-0.286	-0.294	-0.175*	-0.372**
Moderately Wealthy t-1	(0.099)	(0.104)	(0.307)	(0.407)	(0.098)	(0.156)
2008*Earthquake*	0.178	0.200*	-0.539	-0.547	0.047	0.046
Wealthiest t-1	(0.121)	(0.113)	(0.398)	(0.379)	(0.103)	(0.200)
Earthquake Area	0.077		0.015		0.11	
	-0.121		-0.26		-0.07	
t2008	0.339*	0.401**	0.129	0.647	-0.466**	-0.915***
	(0.177)	(0.182)	(0.341)	(0.447)	(0.192)	(0.301)
HH Controls	YES	YES	YES	YES	YES	YES
Village Controls	YES	YES	YES	YES	YES	YES
HH FE	NO	YES	NO	YES	NO	YES
Province*t FE	YES	YES	YES	YES	YES	YES
Village Clusters	YES	YES	YES	YES	YES	YES
Observations	2,930	2,930	1,056	1,056	1,056	1,056
Number of hhid		1,603		829		829
R-squared within	0.231	0.143	0.313	0.225	0.321	0.551

Notes: Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%
Interactions - post-year (2008)*wealth level, earthquake*wealth level, and wealth standing -are not shown in the table for clarity, but included in the regressions, and are available from author.

A.5 : **Robustness checks** Difference-in-Difference estimation on household access to RASKIN, sample excludes households in periphery of treatment group, propensity score matched sample, using 2000 and 2008 balanced panel

	ARTIFICAL TREATMENT GROUP: HH Exposed to Earthquakes within 25-50 km of epicenter (Excluding households within 25 km)					
	Purchased in last 12 months		Per Capita Quantity (KG)		Log Price per KG Real, 2007 Rupiah	
			(| Purchased rice in last 4 weeks)			
	(1)	(2)	(3)	(4)	(5)	(6)
Relative to No Earthquake Community						
2008*Earthquake	-0.069	-0.066	-0.28	-0.136	-0.089	-0.095
Relative to Poorest Households within village* (at t-1)						
2008*Earthquake*	0.069	0.051	-0.14	-0.224	-0.105	-0.365*
Moderately Poor t-1	-0.063	-0.094	-0.291	-0.171	-0.108	-0.205
2008*Earthquake*	0.06	0.033	0.219	0.192	-0.132	-0.454**
Moderately Wealthy t-1	-0.066	-0.091	-0.308	-0.32	-0.137	-0.239
2008*Earthquake*	0.123	0.125	0.417	0.452	-0170*	-0.39
Wealthiest t-1	-0.108	-0.124	-0.372	-0.353	-0.091	-0.296
Earthquake Area	-0.159		-0.188		-0.016	
	-0.163		-0.291		-0.16	
t2008	0.178	0.127	0.425*	0.744	-0.573***	-0.691***
	-0.119	-0.101	-0.218	-0.841	-0.196	-0.326
Observations	3,500	3,500	1,172	1,172	1,172	1,172
Number of hhid		1,876		916		916
R-squared within	0.202	0.183	0.277	0.169	0.282	0.505
	ARTIFICAL TREATMENT GROUP: HH Exposed to Earthquakes within 50-75 km of epicenter (Excluding households within 50 km)					
Relative to No Earthquake Community						
2008*Earthquake						
	-0.054	-0.061	-0.206	-0.148	-0.087	-0.122
Relative to Poorest Households within village (at t-1)*						
2008*Earthquake* 0.076	0.134	-0.172	-0.255	-0.249**	-0.254*	
Moderately Poor t-1	-0.052	-0.097	-0.242	-0.214	-0.096	-0.147
2008*Earthquake*	-0.117	-0.170*	0.18	0.075	-0.097	-0.077
Moderately Wealthy t-1	-0.085	-0.099	-0.295	-0.208	-0.098	-0.143
2008*Earthquake*	0.074	0.075	-0.185	-0.299	-0.136	-0.183
Wealthiest t-1	-0.095	-0.106	-0.21	-0.289	-0.111	-0.184
Earthquake Area	0.009		-0.257		-0.029	
	-0.07		-0.255		-0.058	
t2008	0.125	0.118	0.584*	1.280**	-0.580***	-1.123***
	-0.125	-0.119	-0.314	-0.489	-0.141	-0.305
Observations	4,159	4,159	1,364	1,364	1,364	1,364
Number of hhid		2,252		1,038		1,038
R-squared within	0.205	0.189	0.258	0.341	0.25	0.576
HH Controls	YES	YES	YES	YES	YES	YES
Village Controls	YES	YES	YES	YES	YES	YES
HH FE	NO	YES	NO	YES	NO	YES
Province*t FE	YES	YES	YES	YES	YES	YES
Village Clusters	YES	YES	YES	YES	YES	YES

Notes: Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%
Interactions - post-year (2008)*wealth level, earthquake*wealth level, and wealth standing -are not shown in the table for clarity, but included in the regressions, and are available from author.

1.10 Appendix B

In non-experimental causal studies, estimation of treatment effects usually requires an adjustment using pre-treatment variables to reduce selection bias due to systematic differences between the treated and control sample. Differences across any two treatment and control units are captured using observable pre-treatment characteristics, making the outcome orthogonal to treatment assignment conditional on observables. Such a match can yield an unbiased estimate of the treatment effect (Dehejia & Whaba, 2002). Several early studies on matching, including Rubin (1973), Cochran & Rubin (1973), Raynor (1983), proposed matching based on just one variable or weighting across a few selected variables. Subsequent work by Roesnbaum & Rubin (1983) suggested the use of a propensity score, which creates a conditional probability of assignment to a treatment group given a vector of observable covariates. Most studies focus on Propensity Score Matching (PSM) because in many situations, the dimensionality of observed characteristics is high (Dehejia & Whaba, 2002). Matching along a small number of characteristics such as a binary match is straightforward, however, when there are many variables PSM provides a means of weighting to yield a treatment effect that is more accurate than for an unmatched sample.

In the context of this study, the source of non-randomness comes from the assignment to earthquake treatment. Since I measure short term effects of high impact earthquakes, exposure is distributed over a few areas in the survey sample. These areas are likely to differ from unaffected areas making assignment to the treatment non-random. The sample contains significantly more untreated units relative to treated units. Hence, through PSM I narrow the untreated sample to units comparable to the treated units along observable characteristics. Suppose EQ_{hc} is an indicator variable equal to one if the community in which household h lives was affected by at least one earthquake over magnitude 5.5, within 25 km from 2006 to 2007. The EQ_{hc} variable is equal to zero if the household lives in a community that was not affected by an earthquake. Suppose Y_{hc1} is the value of the outcome of interest for household h in community c when exposed to the treatment ($EQ_{hc} = 1$), and Y_{hc0} is the outcome for the same household when not exposed ($EQ_{hc} = 0$) to the earthquake. Then the treatment effect for household h in community c is defined as γ_{hc}= Y_{hc0}- Y_{hc1}. The treatment effect of interest is the expected treatment effect for the treated population, earthquake affected in this case:

$$\begin{aligned}\gamma|_{EQ=1} &= E(\gamma_{hc}|EQ_{hc} = 1)\\ &= E(Y_{hc1}|EQ_{hc} = 1) - E(Y_{hc0}|EQ_{hc} = 1)\end{aligned}$$

As expected, $E(Y_{hc0}|EQ_{hc} = 1)$ is unobservable. While $\gamma^e_{hc} = E(Y_{hc1}|EQ_{hc} = 1) - E(Y_{hc0}|EQ_{hc} = 0)$ can be estimated, it yields a biased estimator of γ_{hc}. Matching along observable covariates can remove the associated bias in γ^e_{hc}.

The main identifying assumption required for matching is that of **Conditional Independence (CI)** (Rubin, 1977): If for each unit hc we observe a vector of covariates X_{hc}, Z_c, and $Y_{hc0} \parallel EQ_{hc}|X_{hc}, Z_c$, $\forall hc$, then the population treatment effect for the treated $\gamma_{hc}|EQ_{hc} = 1$ is identified and equal to the treatment effect conditional on observables X and Z, and assignment to treatment. Here, X_{hc} are observable household level characteristics and Z_c are observable community level characteristics. This condition intuitively states that conditional on observables X and Z, the distribution of the potential outcome of interest for the treated in the absence of the treatment is would have been the same as the untreated units. Thus under CI, assignment to treatment conditional on observable characteristics can be considered as random, similar to randomized experimental design.

One way to estimate $\gamma_{hc}|_{EQ,X,Z}$, is to stratify the sample into bins along observables of X_{hc} and Z_c. However, as the number of observables increase the number of cells increase exponentially and each cell may not contain both treatment and control units. Rosenbaum & Rubin (1983) introduce the propensity score to reduce this dimensionality problem. The propensity score, denoted here as $p(X_{hc}, Z_c)$= $Pr(EQ_{hc} = 1|X_{hc}, Z_c)$= $E(EQ_{hc}|X_{hc}, Z_c)$ is the probability of being treated (affected by an earthquake) given X_{hc} and Z_c . Conditional Independence immediately extends to the propensity score such that, $(Y_{hc0}, Y_{hc1},) \parallel EQ_{hc}|X_{hc}, Z_c, \Rightarrow (Y_{hc0}, Y_{hc1},) \parallel EQ_{hc}|p(X_{hc}, Z_c)$.

Data from 2000 (IFLS3) is used as the baseline year in which to Propensity Score Match (PSM) origin and split households from 2000 that remained within an IFLS community (non-attrited) in 2008. I create a propensity score using a logit model. IFLS3 is the preferred match survey as it allows a comparison of baseline survey households. Households affected by an earthquake in the two years prior to the 2008 survey are matched to unaffected ones along several dimensions. The matched data creates a balanced panel across the two survey years and allows me to establish a parallel trend across control and treatment groups in outcomes prior to the disaster using IFLS3 and IFLS2 data. In the sample, a household level propensity score is determined using both household and village level characteristics, including dependent variables at baseline. A match for a treatment household is derived using 4 nearest neighbors, with replacement, if within a 0.15 caliper, with the top 5 percent of the sample trimmed to avoid bad matches [23]. This is likely the closest way to simulate a randomized control trial using existing survey data. In the absence of earthquakes,

[23] Changing the matching technique does not affect the outcomes of the main analysis.

access to safety net resources across affected and unaffected households run parallel. Weighted difference-in-difference estimation is then carried out on the matched sample.

Chapter 2: Environmental Migration and Labor Markets in Nepal

Abstract

While an emerging literature cites weather shocks as migration determinants, scant evidence exists on how such migration impacts the markets of receiving communities in developing countries. We address this knowledge gap by investigating the impact of weather-driven internal migration on labor markets in Nepal. An increase of 1 percentage point in net migration reduces wages in the formal sector by 4.8 percentage points. The absence of wage effects in the informal sector is consistent with the exit of low-skilled native workers from the labor market. Understanding entrepreneurial constraints and drivers of labor market exits will inform pathways to resilience[1].

JEL Classification: J21, J61, O15
Keywords: Environmental Migration, Weather, Conflict, Labor Markets, Nepal

[1] This chapter is a version of a paper with Jean-Francois Maystadt and Valerie Mueller.

2.1 Introduction

Migration is understood to be a key mode of adaptation to extreme climatic events (IPCC (2014)). Rural workers search for employment elsewhere to mitigate income losses temporarily or move permanently if the damages are severe (Halliday (2006);Feng et al. (2010); Dillion et al. (2011); Gray and Mueller (2012b) Gray and Mueller (2012a); Marchiori et al. (2012); Gray and Bilsborrow. (2013); Bohra-Mishra et al. (2014); Mueller et al. (2014)). An emerging challenge in the climate change debate is to reconcile whether such adaptation bears additional consequences for human security and livelihoods (IPCC (2014)).

Studies of the consequences of migratory flows on the labor markets of hosting communities in industrialized countries are ubiquitous (Card (1990);Card (2005); Borjas (2005); Borjas (2006); Boustan et al. (2010); Ottaviano and Peri (2012); Pugatch and Yang (2011)). In developing countries, the issue has been investigated from the perspectives of either the migrants(Beegle et al. (2011); Grogger and Hanson (2011); De Brauw et al. (2013)), their countries of origin (Adams and Page (2005); Hanson (2009), for a review), or the households directly linked to migrants (Woodruff and Zenteno (2007); Yang (2008)). Scant evidence exists on how internal migration impacts the labor markets of receiving communities in developing countries, let alone the implications of disaster-driven migration (Kleemans and Magruder (2012); El Badaoui et al. (2014); Strobl and Valfort (2013)). We address this knowledge gap by investigating the impact of weather-driven migration on internal labor markets in a conflict-prone country, Nepal.

Standard models predict immigration is detrimental to workers that show high degree of substitutability with migrants (Johnson (1980a); Johnson (1980b); Altonji and Card (1991); Borjas (2003); Card and Lemieux (2001); Borjas and Katz (2007); Ottaviano and Peri (2012)). Migrants are implicitly assumed to be low skilled and to substitute natives with comparable skills. Recent work in Uganda supports these assertions (Strobl and Valfort (2013)). Elsewhere, migrants are characterized as highly skilled, yet displace low-skilled workers (Kleemans and Magruder (2012)). Kleemans and Magruder (2012) speculated that binding constraints (such as minimum wage laws) in the formal sector can create a wedge between formal- and informal-sector wages. These conditions further render substitution effects more pronounced among disadvantaged natives. Thus, immigration displaces low-skilled workers, causing a decline in the wages of (less educated) native workers predominantly employed in the informal sector (Kleemans and Magruder (2012)).

Exposure to civil war[2] and environmental degradation, and the linkages of these factors to rural-urban migration[3] render Nepal an interesting context in which to study the spillover effects of adaptation, with a direct focus on nearby labor markets. We apply the methodology of Boustan et al. (2010) to address biases inherent in the immigration literature: the self-selection of migrants at origin, the selection of migrant destinations, and native displacements. The methodology allows for the full exploitation of bilateral migration flows in order to identify plausibly exogenous push factors at origin and pull factors at destination. The instruments for the net migration rate (predicted in-migration and out-migration rates) in the wage regression are based on multiples of the predicted probability of moving bilaterally from one district to another and the predicted bilateral (in- and out-) migration flows. These two factors are predicted using models prior to the first stage. The first stage then uses two sets of instruments for net migration: the constructed in- and out-migration rates jointly and the net-migration rate derived from subtracting the first instrument from the first. This is in direct contrast to earlier work which uses spatially lagged weather shocks as instruments, raising concerns regarding the validity of the exclusion restriction due

[2] Urbanization and labor markets have been affected by conflicts in other settings (Kondylis (2010); Maystadt and Verwimp (2014); Alix-Garcia and Bartlett (2012); Alix-Garcia et al. (2013)).

[3] Environmental degradation and weather shocks have been argued to increase rural-urban migration in Nepal (Shrestha and Bhandari (2007); Massey et al. (2010)).

to spatial spillovers resulting from these shocks.[4]

We provide a few modifications to the Boustan et al. (2010) methodology to improve identification and adapt the methodology to the contextual setting of our study. First, we model out- and in- migration flows between districts in Nepal (which are later used to construct our instruments), accounting for lagged weather anomalies, *in addition to* conflict and historical migration flows, and their interactions with river density. Thus, we expand on the push-pull factors previously considered in the migration literature while introducing a dynamic estimation framework. Controlling for historical migration flows is crucial to decipher the relative importance of natural disasters and conflict events on immigration consequences. Second, we differentiate consequences on the labor market by native worker skills to interpret the empirical findings in relation to theoretical predictions in the literature (Altonji and Card (1991); Kleemans and Magruder (2012)).

Our dynamic model of out-migration (estimated prior to the first stage) indicates weather extremes are a prominent driver of out-migration in Nepal, corroborating earlier work on environmental migration patterns (Gray and Mueller (2012a), Mueller et al. (2014)). An increase by 1 standard deviation in the exposure to floods (droughts) reduces out-migration rates by approximately 18 percent (20 percent) in areas with mean river density. The effect of flooding is reversed for individuals in areas densely populated with rivers. Increasing the number of conflict events by 1 standard deviation also encourages out-migration to a lower degree, by 6 percent.

Incorporating historical migration rates in a dynamic model provides two interesting perspectives. First, including auxiliary controls is crucial in the environmental migration literature, as their omission can bias parameter estimates. Second, it sug-

[4] The problem of spatial spillovers is less of an issue when using approximations of shocks at origin to study international migration (Munshi (2003); Pugatch and Yang (2011)), since shocks occur outside the labor markets under investigation and the existence of spatial spillovers can be directly tested. In our study of internal migration in Nepal, we will nonetheless follow Pugatch and Yang (2011) to directly test the existence of spatial spillovers.

gests that weather extremes are of equal importance to these omitted factors. An increase of 1 standard deviation in the lagged out-migration rate increases future out-migration rates by about 22 percent. The corresponding increase for in-migration rates is even larger (at about 62 percent), reflecting strong network effects.

We find such prevailing factors push a more distinct group of individuals to migrate (Kleemans and Magruder (2012); Strobl and Valfort (2013)). Approximately half of the migrant population had completed 10 years of schooling, relative to 18 percent of natives, in 2010. These high-skilled migrants potentially saturate the formal sector, where one-fourth of natives are employed. These marked imbalances between the characteristics of the migrants and of the native population accentuate wage effects in the formal sector: an increase of 1 percentage point in net migration reduces wages in the formal sector by 4.8 percentage points. Kleemans and Magruder (2012) report an increase in the migrant share of the population by 1 percentage point reduces overall income by 1.9 percentage points in Indonesia. Similarly, Altonji and Card (1991) and Ottaviano and Peri (2012) find 1-2 percent declines in wages among low-skilled workers in the United States. Card (1990), finds that the Mariel boat lift from Cuba, which caused a 7% influx in Miami's labor force, had small insignificant impacts on wages and unemployment rates of low-skilled native workers. Borjas et al. (1997) shows that new arrivals to a city can cause existing workforce to relocate, spreading the costs of immigrations across a wider geographic area, thus minimizing labor impacts. Differences between these studies and the results found in our study could be driven by differences in the composition of the migrant population, where our study finds that environmental and conflict driven migrants in Nepal are more high-skilled.

Wage effects are concentrated in the formal sector, despite observed reductions in the employment of natives in the informal sector. The absence of wage effects in the informal sector is consistent with the exit of native workers from the informal labor market. We additionally show immigration largely leads to the unemployment of low-skilled natives. An increase of 1 percentage point in net migration leads to an increase of 1.5 percentage points in the unemployment of unskilled workers.

Our findings have implications for both the immigration and environmental migration literatures. First, migration is found to strongly affect labor outcomes in hosting districts in Nepal. While migrants bring skills to host economies, their presence depresses the wages of workers in the formal sector (in contrast to the findings of Kleemans and Magruder (2012) in Indonesia) and causes workers to exit the labor market altogether. Second, our results suggest vulnerability to weather extremes is not limited to those at the source of exposure. Conflict and flooding in areas populated by rivers displace people. The vulnerability of populations in external communities has spillover effects on migrant hubs. If the highly skilled workers are most affected, reductions in their purchasing power likely incur losses to providers of their services and goods. Understanding the constraints migrants face in starting their own enterprises and the drivers of labor market exits among the low-skilled natives will inform pathways to labor market resilience.

2.2 Vulnerability and Labor Market Conditions in Nepal

Flooding is not uncommon in Nepal and can potentially lead to an increase in migration, away from rivers and toward low-lying land (Banister and Thapa (1981); Shrestha (1999); Massey et al. (2010)). Our analysis covers periods of unprecedented increases in the frequency and severity of floods and landslides (Figure 2.1). Small-scale floods occurred (prior to 2002) followed by widespread exposure (in 47 districts), displacing hundreds of thousands by 2002 (UN report 2002). The 2007 floods displaced more than 19,000 households (Dartmouth Flood ObservatoryDartmouth Flood Observatory (2014) data and the International Disaster Database,CRED (Centre for Research on the Epidemiolo (2014)). A flood of an even larger magnitude occurred in eastern Nepal in 2008 as a result of a breach in an embankment at the Indo-Nepali border, displacing 42,000 households across several villages (UN Office for the coordination of Humanitarian Affairs (2008)). Flooding and landslides affected the far western and midwest regions during the heavy monsoon period of 2009: 4,000 households were displaced and the food stock of 25,000 families lost (UN Office for the coordination of Humanitarian Affairs (2009)).

Drought risk is rare and tends to occur during the winter, the regular monsoon period. Western and eastern Nepal have experienced episodes of consecutive droughts since 2000[5]. These culminated in a severe drought over the period November 2008 to February 2009, with precipitation 50 percent below the seasonal average (Wang et al. (2013)).

Civil conflict was also a major factor driving migration in Nepal from 1999 to 2006 (Bohra-Mishra (2011)). A Maoist insurgency began in the Rolpa district in western Nepal and much of the conflict was concentrated in mountainous and hilly terrain, and in poorer areas. The decade-long conflict led to the loss of more than 13,000 lives (Do and Iyer (2010)). There was considerable variation in the intensity of conflict across the country;[6] the Maoists controlled several districts in eastern and western Nepal by 2005 (Murshed and Gates (2005)). Violent outbreaks led to the movement of political refugees away from conflict-prone areas. The predicted probability of migration decreased for moderate levels of violence and increased as violence became more intense (Bohra-Mishra (2011)).

Local migration in Nepal driven by environmental and political factors is concentrated among more skilled and educated workers. Massey et al. (2010) found that environmental decay, as indicated by falling agricultural productivity, serves to increase the odds of local migration. Specifically, the odds of moving are significantly higher for individuals with more years of schooling and holding salaried occupations,

[5] See Figure A.1 in the appendix.
[6] See Figure A.2 in the appendix.

which is likely to indicate greater skill and therefore greater potential returns on human capital from migration. Among locally migrating adult males in Nepal compared with non migrants, the former are younger and more educated (Fafchamps and Shilpi (2013)). Similar to environmentally driven migration, within conflict areas, migrants who move both within and across districts tend to be younger and more educated, and to hold salaried jobs (Bohra-Mishra (2011)). These disparities across movers and nonmovers increase when migration is across districts.

The above migration trends suggest displacement associated with environmental disasters explains only a small portion of the mobility patterns in Nepal. Acknowledging additional push-pull factors, such as conflict and economic drivers, is crucial to provide an unbiased understanding of migration and its consequences on neighboring districts. This fact influences our decision to modify the Boustan et al. (2010) identification strategy to incorporate conflict and a dynamic component to proxy additional drivers of migration.

Previous work on environmental and conflict displacement suggests the relatively skilled will tend to move out of district. Our study focuses on between-district migration and classifying workers by sector in our LSMS data, we observe both migrants and non migrants tend to be employed in the informal sector (Table 2.1). However, the share of migrants employed in the formal sector is larger than the share of non migrants in this sector. A greater proportion engage in service-sector work; 39 percent of migrants compared to 17 percent of non migrants in 2003 (Table 2.1). Non migrants are also disproportionately employed in agriculture. While the agricultural sector remains an important contributor to Nepal′s economy, from 1965 to 2010, the share of gross domestic product accounted for by agriculture fell from 70 percent to 30 percent, while the share accounted for by services increased from 20 percent to more than 50 percent (International Labor Organization (2010)). These trends suggest that immigration is likely to affect services, the sector that employs the greatest share of migrants. Moreover, labor market adjustments following a shift in labor supply may be constrained given the declining role of agriculture in the economy.

2.3 Data

Our analysis draws from several data sources. First, migration and employment data are taken from two waves of the nationally representative Nepal Living Standards Survey (NLSS): 2003 and 2010. Second, we use the Armed Conflict Location and Event Dataset (ACLED), which documents georeferenced conflict events through 2010, to measure conflict exposure. Third, to create weather anomaly variables, we use 0.5×0.5 degree gridded satellite-based weather data provided by the POWER (Predicted of Worldwide Energy Resource) project of the National Aeronautics and Space Administration (NASA) of the United States for the years 1981 to 2013 (US National Aeronautics and Space Administration (2014)). Fourth, gridded population data are extrapolated from the Center for International Earth Science Information Network at Columbia University. Fifth, river networks and geographic characteristics (such as distance) are extracted from the United States Geological Survey HydroSHEDS (Hydrological Data and Maps Based on Shuttle Elevation Derivatives at Multiple Scales dataset). [7] Below we elaborate on how our outcomes and explanatory variables are constructed from the aforementioned datasets.

2.3.1 Definition of Variables

2.3.1.1 Migration

We create migration flows using the migration information of 7,000 and 14,000 individuals (residing in 3,954 and 5,556 households in 69 districts[8]) in 2003 and 2010,

[7] The data source is: *http : //hydrosheds.cr.usgs.gov/index.php.*

[8] In total, six districts are excluded from our panel because they were omitted from the 2003 and 2010 surveys. In 2003, Accham, Mustang, and Rasuwa districts were unreachable due to conflict.

respectively. Inflows are based on individuals who reported moving to district k from district j in year t using NLSS sampling weights for population-based inferences. Bilateral migration outflows are similarly defined. We restrict our focus to inflows and outflows for four years preceding the 2003 and 2010 surveys to minimize the impact of recall bias and ensure sufficient coverage of conflict and weather events in the period observed.[9] Population figures derived from the 1995 NLSS are then used to further convert the migration flows into shares of migrants moving into and out of each district k from each district j for each year. This procedure creates two 69×69 matrices of bilateral in- and out-migration rates at the district level, which are used to predict net migration rates, the key variable for the identification of the impact of migration in the labor regressions.

2.3.1.2 Conflict

A conflict event is defined as a single altercation in which one or more groups use force for a political end (Raleigh et al. (2010)). Following this definition, the number of conflict events per square kilometer is defined by district-year for the four years prior to 2003 and 2010. Between 1996 and 2006, the end of the civil war, about 3,030 conflict events were reported in the ACLED dataset for Nepal.

2.3.1.3 Weather Anomalies

We create seasonal flood and drought indicator variables, for the same period covering migration flows, for each 1×1 degree grid that overlaps a district in a given year. Heavy monsoon is from June to September. Regular monsoon is from November in the previous year through February of the current year. A flood shock indicator, for each grid in a given year, is set to 1 if cumulative rainfall over the heavy monsoon

Dolpa, Ilam, and Manang districts were omitted in 2010.

[9] Modifying the number of years over which migration is observed has little impact on the estimation of predicted migration rates.

season exceeds the 90th percentile of the time-series distribution. Similarly, a drought shock indicator, for each grid in a given year, is set to 1 if cumulative rainfall over the regular monsoon season falls below the 10th percentile of the distribution.

Annual district-level flood and drought indicators are set to 1 if a flood or drought occurs in any grid overlapping the district. The flood and drought variables are interacted with river density data to capture an additional dimension of district exposure to the weather anomalies. River density is calculated as the length of the river segments in kilometers divided by each district area.

2.3.1.4 Labor Market Outcomes

Our labor supply variables focus on the employment status of the individual. An individual is considered employed if he reported working in the last 12 months prior to the survey interview. Otherwise, the individual is categorized as unemployed (did not work nor engage in domestic activities in the last 12 months) or inactive (did engage in domestic activities in the last 12 months).

Two stratifications are made in the analysis to facilitate the interpretation of results. The first stratification is based on the sector of employment, which relies on the NLSS definition. We also stratify the sample by skill, whereby individuals having more than 10 years of schooling are characterized as highly skilled and others are considered low skilled.

Individual and household earnings over a 12-month period are used to construct monthly formal- and informal-sector wages, respectively. We use the national consumer price index to convert 2003 wages into 2010 real terms. Monthly wages for formal-sector workers are taken directly from the survey. For the majority of workers employed in the informal sector, we proxy for earnings with revenues from own farms and enterprises. To construct individual monthly earnings, we divide monthly revenues by the number of members in the household reported to be employed in the enterprise.

Our measure proxy for informal earnings may under- or overestimate true individual earnings in the informal sector. We might systematically overestimate revenues per capita by omitting hired employees from the denominator (because they were missing from the agricultural module). On the other hand, we may underestimate individual earnings because we are unable to clarify which household members were employed by the enterprise on a permanent basis.

Because household enterprises are more the rule than the exception, we restrict the analysis of migration impacts to the sample of household heads. Particularly for the informal sector, adding members from larger households may attenuate the effect of immigration inasmuch as their employment status may depend on their relative position in the household and other joint household decisions. Since restricting the focus to household heads sufficiently reduces the initial sample size, we detail how heads differ from the rest of the natives in the Summary Statistics section.

2.3.2 Summary Statistics

Table 2.1 compares the characteristics of migrants, nonmigrants, and household heads of both groups in our sample. Migrants tend to be younger and more educated than nonmigrants, and a greater percentage are women. The proportion of migrants that completed 10 or more years of schooling is 29 percent, compared with 14 percent of non-migrants in 2003. These differences widen by 2010, when 46 percent of migrants are considered skilled according to our definition, compared with 18 percent of non-migrants. Given the skill differentials, it is not surprising that a greater percentage of migrants work in the formal sector.

Restricting the nonmigrant sample to household heads changes the distribution of gender and age characteristics with negligible effects on educational endowment. Focusing on the heads produces a sample closer to full employment. As expected, household heads obtain greater formal- and informal-sector wages on average (than the complete sample of nonmigrants), and the difference is persistent over time.

2.4 Methodology

We employ the Boustan et al. (2010) methodology to account for changes in native labor market outcomes attributable to immigration, using the following empirical model:

$$Y_{ijt} = \alpha_1 + \beta M_{jt} + \lambda X_{ijt} + \gamma Q_{jt} + \delta_j + \delta_t + \epsilon_{ijt}, t = [2003, 2010] \tag{2.1}$$

The dependent variable Y represents the non-migrant labor outcomes (employed, unemployed, and log monthly wages) for individual level i, living in area j at time t. Labor supply and wage variables are a function of several factors: the net labor migration rates M to area j over the last four years, a vector of demographic controls X that reflect one′s earning potential (age, gender, education), a location variable Q (urban destination), a location fixed effect δ_j to reflect labor market differences at the regional level, and a time fixed effect δ_t to account for time trends. Errors are clustered at the district level, for the 69 districts, to allow for correlation between individuals within district-level labor markets.

To deal with the endogeneity of the net migration rate M, predicted in- and out-migration rates are used as instruments for the observed net migration rates (Boustan et al. (2010))[10]. We also subtract the predicted out-migration rate from the predicted in-migration rate to create the predicted net migration rate and use this one instrument for the net-migration rate. Thus we have two sets of instruments,

[10] We follow Boustan et al. (2010) in how we compute the standard errors in the first- and second-stage regressions. The first-stage regressions use block-bootstrapped standard errors (clustering at the district level) to account for the fact that the predicted in- and out-migration rates are generated regressors.

predicted in- and out-migration rates together, or the predicted net migration rate as an instrument for the net migration rate in a just identified model.

Equations (2) through (4) delineate how the predicted in-migration rate is computed. Out-migration rates are calculated in a similar fashion to compute net migration rates (equations (5) through (7)). To compute the in-migration rate for location j, we must first predict the in-migration flows, IM_{jt}, of migrants to location j. This is the product of the number of migrants leaving location k and the probability that these migrants move from location k to location j, $\widehat{P_{kjt}}$, where $\widehat{O_{kt}}$ denotes the out-migration rate. The instrument for the in-migration rate is the predicted inflow in equation (2) divided by district j's population in 1995. Predicted in-migration flows (equation (2)) are affected only by outmigration in all j states excluding own state k itself[11]. Predicted out-migration flows (equation (5)) is similar.

$$IM_{jt} = \sum_{k \neq j} \left(\widehat{O_{kt}} \times pop_{k1995} \right) \times \widehat{P_{kjt}}, \text{ with } t = [2003, 2010] \tag{2.2}$$

$$O_{kt} = \alpha_2 + \theta_1 Z_{kt-1} + \theta_2 M_{kt-1} + \delta_k + \delta_t + \epsilon_{kt}, \tag{2.3}$$

$$\text{with} \quad t = [2000, 2001, 2002, 2003, 2007, 2008, 2009, 2010]$$

$$P_{kjt} = \alpha_3 + \phi f(d_{kj}) + \delta_t + \epsilon_{kt}, \text{ with } t = [2003, 2010] \tag{2.4}$$

In (3), we modify the out-migration rate, O_{kt}, equation from Boustan et al. (2010) and later Strobl and Valfort (2013) in three ways. First, the out-migration rate is influenced by origin weather shocks (floods, droughts and their interaction with river density), as well as by past conflict events (Z_{kt-1})[12]. Although the consistency of our results does not depend on the addition of these interaction terms and the conflict

[11]The use of migration out of (into) other states excluding own state helps to avoid the issue of endogeneity as discussed. In addition, excluding own state automatically implies excluding own state lagged weather and conflict variables used in equation (3) and (6) to predict out(in) migration flows which could indirectly affect the main dependent variables of the analysis.

[12]Weather and conflict variables are not used directly as instruments, only to construct predicted in and out migration rates which are the excluded instruments used in the analysis

variables, such modifications are motivated by the vulnerability of Nepali households to floods, as described in Section 2.2. Second, we estimate out-migration flows using a linear probability model with district and time fixed effects. Third, we improve the predictive power of out-migration rates by estimating a dynamic model, incorporating lagged migration rates. A standard system generalized method of moments (GMM) dynamic model (Blundell and Bond (1998)) is applied with robust standard errors.[13] The predictive power of the dynamic model is assessed against an alternative model, ordinary least squares (OLS) with standard errors robust to time and spatial correlation (Conley (1999)). We assume that spatial dependency disappears beyond a cutoff point of 64 kilometers, which corresponds to the maximum distance between the centroids of any pair of neighboring districts. We also allow for time dependency of up to two years, which is larger than the minimum time lag (T powered 0.25) recommended by Green (2003) and Hsiang (2010).

For each source location k, the probability of moving from location k to location j is then estimated by a dyadic model in equation (4), which depends on the proximity between locations k and j, d_{jk}. We define the proximity as a Euclidian distance between locations and allow for a nonmonotonic relationship with the introduction of a quadratic term. We estimate (4) using a linear probability model with time fixed effects δ_t to account for unobserved time-specific variables that influence migration. Standard errors are clustered at the origin level.

Thus far, we have explained how we predict in-migration rates. We must also predict out-migration rates to have the complete set of variables used as excluded instruments in equation (1) . Out-migration rates are computed in a similar fashion

[13] The method provides more efficient estimates than difference GMM estimations (Arellano and Bond (1991)) but requires an additional assumption with respect to stationarity. We apply Fisher′ test for panel unit root using an augmented Dickey-Fuller test (Maddala and Wu (1999)). For our main variables reported in Table 5.2, we can reject the null hypothesis of nonstationarity in all variables at any reasonable confidence level. One exception is the number of conflicts per square kilometer, but note that that our results do not depend on the inclusion of the conflict variables (Table 5.1).

from equations (5)-(7) below:

$$\begin{aligned} OM_{jt} &= \sum_{k \neq j} \left(\widehat{I_{kt}} \times pop_{k1995} \right) \times \widehat{P_{jkt}}, \text{ with } t = [2003, 2010] && (2.5) \\ I_{kt} &= \alpha_2 + \theta_1 Z_{kt-1} + \theta_2 M_{kt-1} + \delta_k + \delta_t + \epsilon_{kt}, && (2.6) \\ \text{with} & \quad t = [2000, 2001, 2002, 2003, 2007, 2008, 2009, 2010] \\ P_{jkt} &= \alpha_3 + \phi f(d_{jk}) + \delta_t + \epsilon_{kt}, \text{ with } t = [2003, 2010] && (2.7) \end{aligned}$$

Equation (5) denotes the predicted out-migration flow OM_{jt} of migrants from location j. The predicted out-migration flow from j is estimated as the sum over all destination districts k ($k \neq j$) of the number of migrants settling in destination district k who are estimated to come from source district j. Equation (6) provides the predicted in-migration rate for districts estimated in a similar form to equation (3). From (7), a function of distance across districts is used to estimate the likelihood of individuals leaving source region j to move to region k. Predicted district level observations of P_{jkt} and I_{kt} from equations (6) and (7) are used to create predicted out-migration flows in (5). The predicted out-migration flow from location j is divided by district j's population in 1995 to create the predicted out-migration rate used as an instrument, along with the predicted in-migration rate in the empirical estimation.

Our identification strategy hinges on the assumption that the predicted out-migration rates, predicted in-migration rates and predicted net migration rate affect individual labor market outcomes at the destination only through their effect on net migration.[14] By focusing on district-level migration rates, we essentially reduce the potential for the exclusion restriction to be violated due to the spatial correlation of shocks across cities and villages within the same district. Furthermore, by including district fixed effects, we control for unobserved factors at the destination that might

[14] The average net migration rate (Table 5.2) is slightly lower than rates observed in the US literature but within the realm for internal migration in developing countries (Strobl and Valfort (2013)).

be correlated with net migration and affect labor market outcomes.

The only credible threat to identification would come from spatial correlation between the variables used to predict net-migration rates from sending districts and unobserved local labor market conditions at the district level (Boustan et al. (2010); Pugatch and Yang (2011)). This is certainly one rationale for lagging these variables when predicting in- and out-migration. Yet we cannot rule out that (lagged) political and environmental shocks are correlated across districts and feature enough persistency to threaten the validity of the exclusion restriction. We will therefore test the robustness of our analysis in Section 2.5.3 by augmenting the regressions in equation (1) with spatially lagged political and environmental shocks that explicitly control for spatial correlation across districts.

2.5 Results

2.5.1 Results from the Regressions Used to Predict Net Migration Rates

We first present the parameter and standard error estimates from the OLS version of (3) (column 3, Table 5.1). An increase of 1 standard deviation (that is, by 0.387) in flood incidence during the heavy monsoon (i.e. 0.387) reduces the out-migration rate by 0.0009 (at mean river density).[15] Given the mean value of the out-migration rate (0.005), the impact corresponds to a reduction of 18 percent. However, flood exposure, particularly in areas with dense river networks (floods*river density), can push individuals out of their locations of origin. For example, consider individuals living in areas where the river density is 2 standard deviations above the mean. An increase of 1 standard deviation in flood incidence elevates their chance of out-

[15] Descriptive statistics for district-level variables, which are used to compute the average partial effects, are given in Table 5.2.

migration by 3 percent.

Inferences on the flooding parameters are similar when based on the dynamic model (column 6, Table 5.1). At the cost of imposing an additional assumption with respect to the exogenous nature of past migration,[16], the dynamic model is found to offer a better specification fit. The F-test of joint significance in the first-stage equation is slightly higher for the instruments resulting from the dynamic model. Our instrumental variables (predicted migration rates) and the interpretation of the remaining parameters are therefore based on our preferred specification, the dynamic model.

A major advantage of the dynamic model is the ability to control for auxiliary factors that affect historical migration rates. To give perspective on the relative importance of flooding on out-migration rates, auxiliary factors, as proxied through the lagged out-migration rate, influence out-migration rates by a similar order of magnitude. An increase of 1 standard deviation in historical out-migration rate augments out-migration rates by 22 percent compared with an 18 percent reduction from an equivalent increase in flooding exposure. While the number of conflicts also has a consistently positive effect on out-migration rates, the effects are smaller with an increase of 1 standard deviation, leading to a 6 percent increase in out-migration rates.

We briefly remark on the in-migration rate regression (column 12, Table 5.1). Lagged migration is the only statistically significant determinant. An increase of 1 standard deviation in historical in-migration rates is predicted to increase in-migration by 62 percent, reflecting strong network effects.

We next turn to the models used to predict the probabilities of moving from district k to j and vice versa (4). Both specifications suggest a convex relationship between the probability of moving and distance: the probability is almost always

[16] To validate the consistency of the GMM estimator, the test for the first-order serial correlation rejects the null hypothesis of no correlation, while the hypothesis for second-order serial correlation cannot be rejected. The Sargan test for over identification does not reject the null hypothesis of zero correlation between the instrumental variables and the error term.

negatively correlated with the linear term (for 124 and 127 of the 138 estimated pairs in P_{kj} and P_{jk}, respectively) and positively correlated with the squared term (for 132 and 136 of the 138 estimated pairs in the same two specifications). The small sample of district pairs, however, influences the precision of our estimates. About 25 percent of the coefficients on the linear and squared distance variables are statistically significant at the 10 percent critical level in both probability specifications.

Table 5.3 presents the results from the first-stage regressions. Predicted migration rates calculated from formula (2) for in-migration (and a similar formula for out-migration) are used as instruments for actual net migration rates. We also provide a just-identified version of the first stage, using the predicted net migration rate as one instrument subtracting the aforementioned two formulas.

Figure 5.1 maps the predicted and observed net migration rates. Although strongly correlated in areas with major cities, the two maps substantially differ in that the predicted figures capture a subsample of the observed net migration rates. For Kathmandu, actual and predicted net-migration rates are strongly correlated. Actual net migration rates were 0.020 and 0.117, while predicted net migration rates were 0.023 and 0.064 in 2003 and 2010, respectively. In other cities, such as Nepalganj in the southwestern Banke district (Figure 5.1), the distinction between actual and predicted migration is much larger. The actual net migration rate is 0.046 and 0.010 in contrast to the predicted net migration rate of -0.003 and -0.004 in 2003 and 2010, respectively. The striking differences across predicted and observed net migration rates highlight that the interpretation of our results is not generalizable to any type of migrants in Nepal.

2.5.2 Impact of Migration on Hosting Labor Markets

We now present our estimates of the impact of net migration rates on labor markets outcomes. In Table 5.4, our dependent variable is the logarithm of monthly real wage, distinguishing between the formal and informal sectors. The two-stage least-squares estimates under just-identified (column 5) or over identified (column 6) equations indicate a strong negative impact in the formal sector. A 1 percent increase in net migration rates would translate into a fall in real wages by about 5 percent. Contrary to the findings of Kleemans and Magruder (2012), the negative impact is found only in the formal sector. These effects are consistent with migrants' being engaged in activities in the formal sector more than nonmigrants.

The formal-sector wage effects for each district are extrapolated from the regression results and presented in Figure 5.2. A 1 percent increase in net migration rates from increased frequency of droughts, floods, and conflict in this part of the world is expected to have profound effects on the economic geography of Nepal. There is quite a bit of variation in the wage effects across space which corresponds to district migration hot spots depicted in Figure 5.1, which suffers the most negative consequences.

Our descriptive statistics also reveal that the difference between migrants and nonmigrants may be driven by distinctions in skills: in 2010, 46 percent of migrants were considered skilled compared with 18 percent of nonmigrants. It is therefore not surprising to observe that net migration negatively affects the real wages of high-skilled nonmigrants (columns 1-3, Panel A, Table 5.5), in particular in the formal sector where most (relatively) high-skilled migrants are competing (columns 7-9, Panel B, Table 5.5). The magnitude of the wage effect resembles wage losses in the context of labor substitutability among low-skilled workers in the United States (for example, 1-2 percent declines found by Altonji and Card (1991) or Ottaviano and Peri (2012)). Nonetheless, the negative impact found in the formal sector for the low-skilled workers (columns 10-12, Panel B, Table 5.5) sheds doubt on a mechanism exclusively based on labor substitutability.

Tables 5.6 and 5.7 point to another source of vulnerability for low-skilled workers. Low-skilled workers face a lower probability of employment (columns 14 and 15, Table 5.6) and a higher probability of unemployment (columns 8 and 9, Table 5.7). Raising net migration by 1 percentage point increases the unemployment of unskilled workers by 1.5 percentage points. A slightly lower (reverse) elasticity is found for employment probability. Similarly, employment and unemployment probabilities have the expected sign for skilled workers, although statistically significant for the probability to be unemployed (columns 5 and 6, Table 5.7). Such contrasting results are consistent with a displacement of low-skilled workers out of the labor market.

2.5.3 Validity of the Instruments

The identification strategy hinges on two main identifying assumptions: the strength and the exogenous nature of the predicted net migration rates used as instruments. First, the individual t- and F-tests, assuming weak instruments, indicate the instruments are strong predictors of the actual net migration rate (Table 5.3). The Kleibergen Paap rk Wald F statistics range between 12 and 14 for our preferred dynamic specification, which exceeds the Stock and Yogo (2005) critical values with 15 percent absolute bias.[17] We also note that the predicted net migration rates positively affect observed net migration rates, which is reassuring given that just-identified estimates are median-unbiased.

Second, it is intuitively plausible that the predicted migration rates affect labor market outcomes *only* through observed migration rates. In Section 2.4, we rationalize the focus of the analysis at the district level and the use of lagged environmental and political shocks in predicting migration rates to satisfy the exclusion restriction. One possible violation of the exclusion restriction would nonetheless result if (weather and political) shocks in neighboring districts have direct impacts on labor

[17] The F statistics on excluded IV is also above the rule-of-thumb of 10 provided by Stock and Yogo (2005). We also note that when using the predicted out-migration and in-migration rates as separate instruments, the Hansen J test features a p-value above 0.100. It should be noted that the two instruments are similar in nature and the test assumes that at least one instrument is valid.

market outcomes.[18] We therefore test the stability of our coefficients of interest in the second-stage regressions to the inclusion of spatially lagged variables. The spatially lagged variables are obtained by multiplying the variables used to predict migration in equation (3) with a distance-based spatial matrix that weights the value of each variable for one district by the inverses of the Euclidean distances to the geographical centers of all other districts (Anselin (2002)). The inclusion of these spatially lagged variables does not alter substantially the magnitude of the impact of migration on labor market outcomes.[19] We can therefore rule out the possible threat to our identification strategy that would result from spatial spillovers from environmental and political shocks.

2.5.4 Reflections on the Role of the Informal Labor Market in Absorbing Displaced Workers

The seemingly contrasting results between employment and wage outcomes deserve further investigation. The displacement of low-skilled workers out of the labor market cannot be explained by the labor substitution mechanism. First, immigration may change demand in ways differentially affecting formal- and informal-sector workers (Altonji and Card (1991)). For example, a growing literature demonstrates immigration influences prices and consumption composition (Saiz (2003)Saiz (2007); Lach (2007); Cortes (2008)). Second, although our findings are somewhat consistent with the predictions of Kleemans and Magruder (2012), our informal-sector results suggest binding constraints preclude the absorption of workers (for example, registration

[18] Past migration in equation (3) may also be endogenous. Our results are similar when past migration is omitted and the instruments are constructed using an OLS estimation (as shown in columns 1-3 and 7-9 in Table 5.1). The robustness of the two-stage estimates is provided in Tables A.1 and A.2 in the appendix.

[19] Results are provided in Table A.3. There is only one exception : the impact on wages for the low-skilled workers appears to be positive when spatially lagged variables are included. However, when restricted to the formal sector, we found a negative impact, similar to the one found in Table 5.5 (columns 11-12).

requirements may prevent the entry of new enterprises, or credit constraints prevent enterprise expansion). We reflect on the plausibility of these hypotheses descriptively.[20]

We first examine whether native workers change their consumption patterns in response to migrant flows. It is important to note that the general equilibrium framework developed by Altonji and Card (1991) accounts for the increase in the demand for goods caused by the shift in the population from migration. We explore an additional effect on labor demand, which is through shifts in preferences for goods. If the purchasing power parity of workers declines with immigration, then we might expect to observe changes in consumption patterns. While total consumption remains unaffected by migration, native workers reduce the share of service goods consumed in exchange for other nonfood essentials (Table 5.8). These compositional changes in demand do not explain labor market exits in the informal sector, but they do offer one explanation for why formal-sector workers are at most risk. A greater share of formal-sector workers are engaged in the service sector, in which services are likely to have a higher elasticity of demand.

We next assess how constraints on the creation and expansion of enterprises may affect the ability of the informal sector to absorb displaced workers. Descriptive statistics indicate that the majority of enterprises are financed through households' own savings (approximately 40 percent) (Table 5.9). Only a small percentage of enterprises tried to obtain a loan to operate or expand their business (23 percent in 2010) and fewer complained of unsuccessful attempts (3 percent). Overall, the environment for hired labor is low (for example, only 17 percent in 2010). Informal

[20] These hypotheses are by no means exhaustive. The skilled may be differentially affected if migration affects innovation and technology boosting their marginal productivity (Kerr (2013)). Additionally, from a worker's perspective, the returns to his skills or education in the informal sector may be lower than his reservation wage, rendering unemployment more desirable than employment in the informal sector. Although testing the role of migration in innovation is beyond the scope of the paper, we find no descriptive evidence to support the reservation wage argument when comparing the returns on education across sectors in simple Mincerian wage regressions (Table A.4 in the appendix).

enterprises are more inclined to hire workers and a significantly greater number of workers per enterprise. The absence of financial capital may discourage enterprises in the informal sector from expanding or entrepreneurs from creating start-ups.

2.6 Conclusion

We employ the Boustan et al. (2010) multi-stage procedure to identify the effects of environmental migration on the labor markets of hosting communities. We modify these authors' procedure for constructing the instrumental variables to incorporate additional variables relevant to our setting (such as conflict exposure), district and time fixed effects, and a dynamic component. We show the dynamic model is preferred to the standard OLS accounting for spatial and time correlation (Conley (1999)). Inferences based on the dynamic model suggest droughts and floods are equally crucial determinants of migration as auxiliary factors, proxied by lagged migration. Predictions from the dynamic model are used to construct instruments for net migration rates in the second stage.

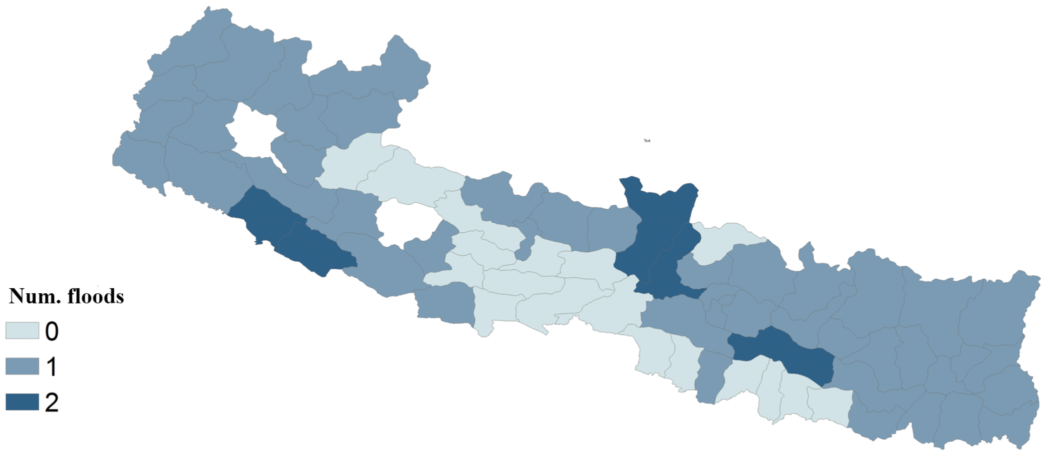
Our second-stage regressions indicate wage losses are slightly larger than those observed in the United States and elsewhere (4.8 percent). Labor substitution is imperfect in the Nepal case inasmuch as migrants appear more skilled than the average native worker in hosting communities. The demand for labor in the formal sector also appears binding in the short term following earlier work in Indonesia (Kleemans and Magruder (2012)). Imperfect substitution coupled with fixed labor demand in the formal sector may partially explain why wage losses are more pronounced here than in other settings.

Although migrants are positively selected, as in Indonesia, we find informal-sector employment (not wages) is negatively affected. The wages of the informal sector adjust due to the exit of workers from the labor market. Migration appears to change consumption patterns by reducing the share of service goods consumed. Service goods may have a higher elasticity of demand. Furthermore, formal-sector workers are at greater risk than informal-sector workers since a greater share are employed in the service sector. The informal sector's ability to absorb excess labor may also be limited by opportunities to access financial capital in Nepal to support new enterprises or encourage older enterprises to grow. Such descriptive evidence suggests the provision of grants to support enterprises following periods of disasters may foster resilience in hosting economies to forced migration (de Mel et al. (2012)).

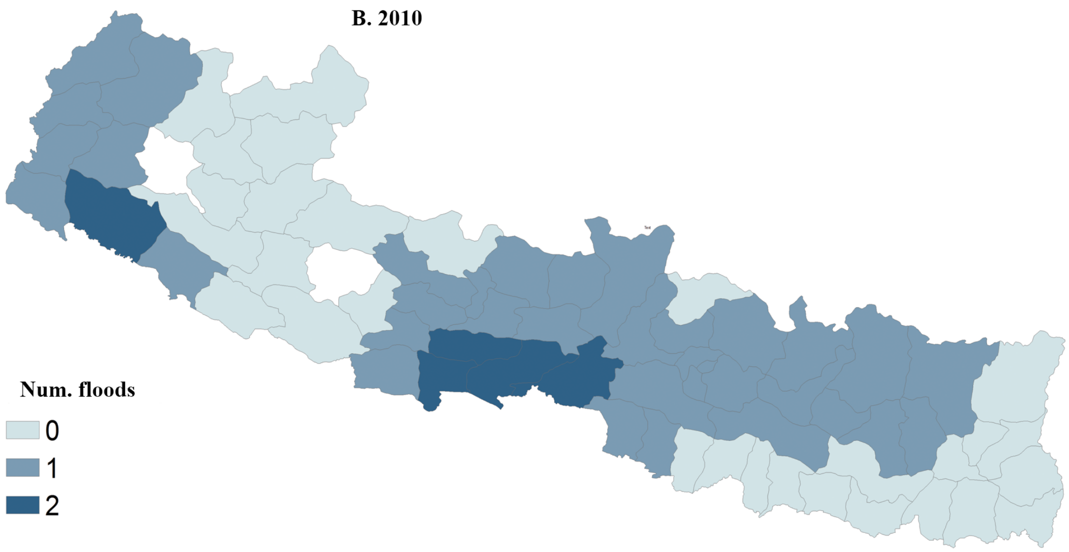
.1 Tables & Figures

Figure 2.1—Floods in Nepal, cumulative over previous four years, 2003 versus 2010

A. 2003

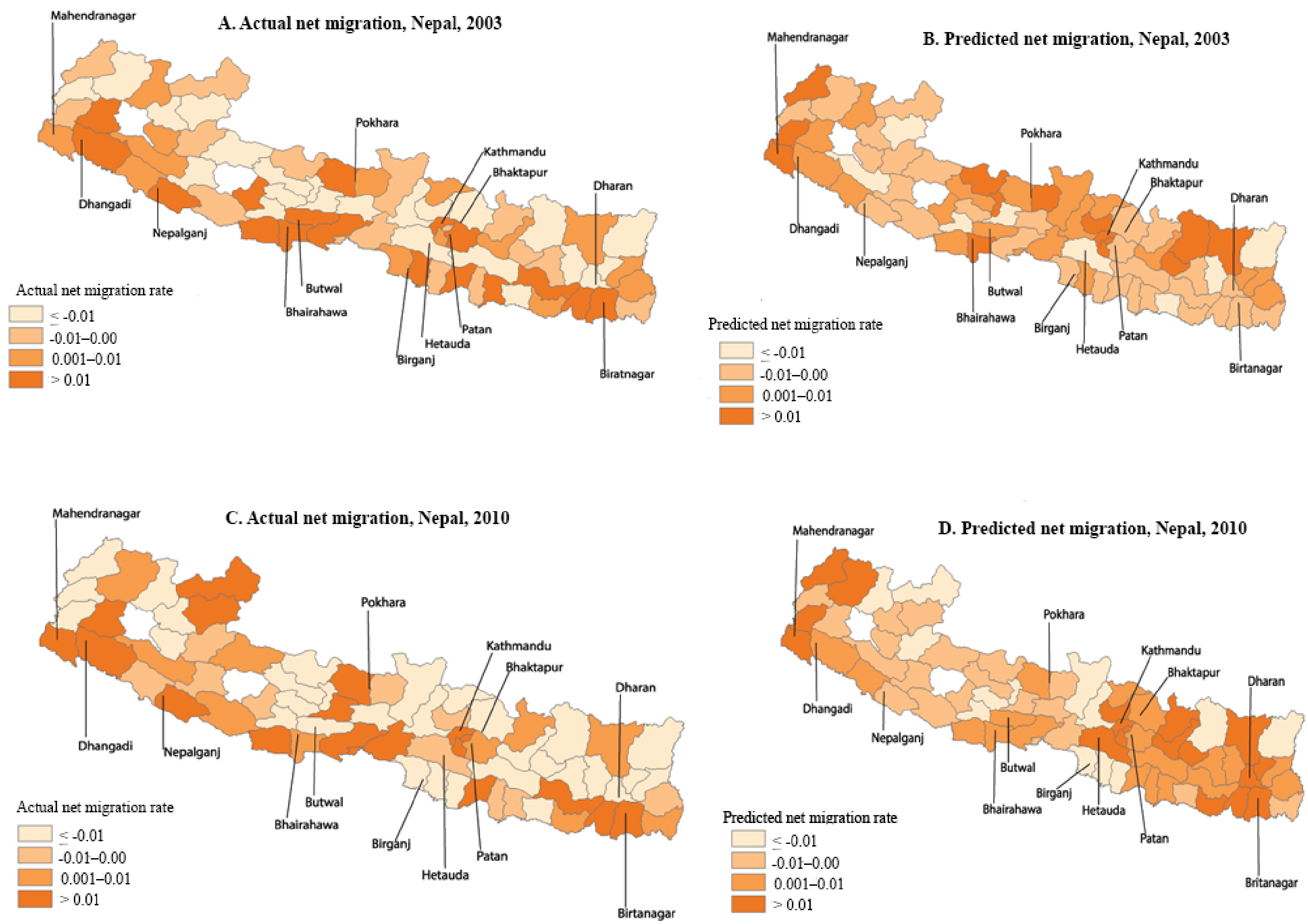


B. 2010



Source: Authors' representation based on data from NASA (2014).
Note: Districts not used in analysis are omitted from maps.

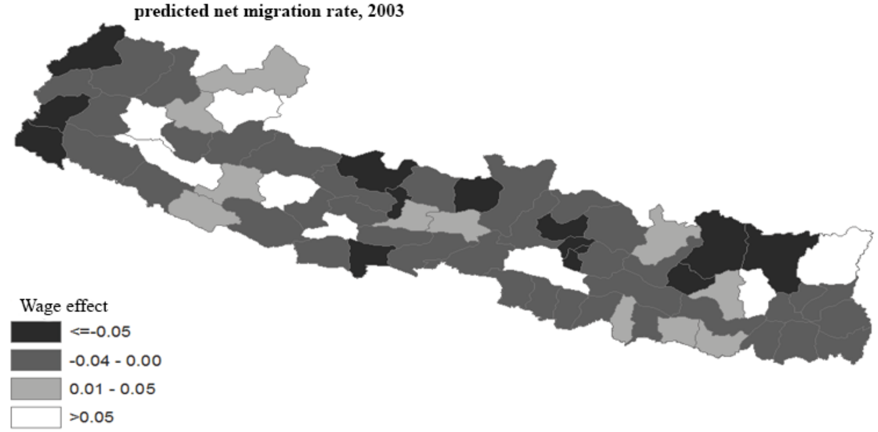
Figure 5.1 —Actual and Predicted Net Migration, Nepal, 2003 and 2010



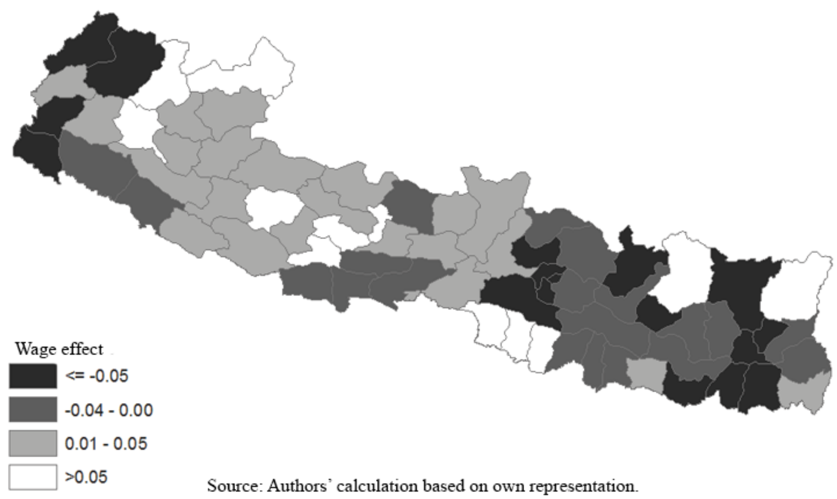
Source: Authors' representation based on own calculations.
Note: Districts not used in analysis are omitted from maps.

Figure 5.2—Estimated Effects of predicted net migration on formal-sector wages, Nepal, 2003 and 2010

A. Estimated effect on formal-sector wages of a 1 percent increase in within-district predicted net migration rate, 2003



B. Estimated effect on formal-sector wages of a 1 percent increase in within-district predicted net migration rate, 2010



Source: Authors' calculation based on own representation.
Note:Districts not used in analysis are omitted from map.

Table 2.1—Summary statistics, individual characteristics of migrants and natives aged 18-65, weighted, 2003 and 2010

	2003			2010			2003	2010
	Non-migrant	Migrant	Diff. (p-val)	Non-migrant	Migrant	Diff. (p-val)	Non-migrant HH head	Non-migrant HH head
	(n = 7,303)	(n = 241)		(n = 14,367)	(n = 401)		(n = 2,742)	(n = 5,230)
Age	36.70	28.50	0.000	37.80	25.70	0.000	43.40	43.70
	(13.60)	(11.60)		(13.60)	(10.10)		(11.60)	(11.50)
Male	0.53	0.43	0.000	0.43	0.24	0.000	0.85	0.72
	(0.50)	(0.50)		(0.50)	(0.43)		(0.36)	(0.45)
Schooling	3.69	6.52	0.000	4.25	8.24	0.000	3.36	3.98
	(4.57)	(4.71)		(4.81)	(4.58)		(4.36)	(4.51)
Highly skilled	0.14	0.29	0.174	0.18	0.46	0.000	0.12	0.14
	(0.34)	(0.46)		(0.39)	(0.50)		(0.32)	(0.35)
Labor Variables								
Employed	0.90	0.75	0.358	0.84	0.58	0.152	0.97	0.94
(last 12 months)	(0.30)	(0.43)		(0.37)	(0.50)		(0.17)	(0.24)
Unemployed	0.03	0.07	0.000	0.13	0.26	0.000	0.01	0.06
(last 12 months)	(0.18)	(0.25)		(0.34)	(0.44)		(0.12)	(0.23)
Inactive	0.07	0.18	0.000	0.03	0.16	0.375	0.02	0.004
(last 12 months)	(0.25)	(0.39)		(0.17)	(0.37)		(0.13)	(0.06)
	(n = 6,572)	(n = 180)		(n = 12,068)	(n = 233)		(n = 2,660)	(n = 4,707)
Work primary job	0.26	0.32	0.084	0.20	0.27	0.027	0.31	0.23
(|empl. in formal)	(0.44)	(0.47)		(0.40)	(0.44)		(0.46)	(0.42)
	(n = 1708)	(n = 57)		(n = 2,413)	(n = 63)		(n = 798)	(n = 1,080)
Real wage	10,276	10,221	0.996	13,445	8,653	0.569	14,765	17,582
(|empl. & formal)	(80,981)	(18,267)		(63,605)	(8,107)		(114,300)	(89,454)
	(n = 2,713)	(n = 84)		(n = 5,700)	(n = 75)		(n = 1,323)	(n = 2,034)
Real wage[1]	1,566	1,584	0.912	3,245	4,049	0.783	1,890	3,676
(|empl. & informal)	(5,561)	(2,919)		(24,501)	(10,973)		(7,301)	(27,204)
Share of Migrants by Industry								
	(n = 5,960)	(n = 151)		(n = 9,901)	(n = 173)		(n = 2,484)	(n = 4,264)
Agriculture,	0.70	0.52		0.71	0.53		0.70	0.67
Forestry & Fishery	(0.46)	(0.50)		(0.46)	(0.50)		(0.46)	(0.47)
Services	0.17	0.39		0.20	0.35		0.18	0.22
	(0.38)	(0.49)		(0.40)	(0.48)		(0.38)	(0.41)
Manufacturing	0.08	0.08		0.05	0.06		0.06	0.05
	(0.26)	(0.27)		(0.22)	(0.25)		(0.24)	(0.23)
Construction	0.05	0.02		0.04	0.05		0.07	0.06
	(0.21)	(0.13)		(0.21)	(0.23)		(0.25)	(0.24)

Notes: Real wages expressed at the monthly level in 2010 rupees. *Highly skilled* is defined as having 10 or more years of schooling. HH = Household. [1] Real monthly wage for individual in informal sector constructed using agricultural or enterprise revenues per worker. [2] Real monthly wage for Household in the informal sector is household agricultural or enterprise revenue.

Table 5.1—Determinants of in- and out-migration rates

Dependent variable	Out-migration rate						In-migration rate					
	OLS			Dynamic model			OLS			Dynamic model		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Flood in heavy	-0.002***	-0.002***	-0.014***	-0.002***	-0.002**	-0.008**	-0.000	-0.000	0.002	0.000	0.000	0.000
monsoon at $t-1$	(0.001)	(0.001)	(0.005)	[0.001]	[0.001]	[0.004]	(0.000)	(0.000)	(0.004)	[0.000]	[0.000]	[0.004]
Drought in regular	-0.001	-0.001	-0.000	-0.003**	-0.003**	0.005	-0.001	-0.001*	-0.001	0.001	0.001	-0.001
monsoon at $t-1$	(0.001)	(0.001)	(0.004)	[0.001]	[0.001]	[0.005]	(0.000)	(0.000)	(0.003)	[0.001]	[0.001]	[0.003]
No. of conflicts		-0.041	-0.041		0.028	0.031*		-0.100***	-0.100***		0.035	0.018
per sq km at $t-1$		(0.031)	(0.031)		[0.018]	[0.019]		(0.022)	(0.022)		[0.045]	[0.019]
Out-migration				0.171***	0.169***	0.159**				0.277***	0.356***	0.370***
rate at $t-1$				[0.055]	[0.058]	[0.062]				[0.090]	[0.094]	[0.100]
Flood in HM at $t-1\times$			0.068***			0.033*			-0.017			-0.001
River density			(0.025)			[0.019]			(0.023)			[0.021]
Drought in RM at $t-1\times$			-0.003			-0.043**			0.003			0.009
River density			(0.021)			[0.022]			(0.015)			[0.014]
Observations	552	552	552	552	552	552	552	552	552	552	552	552
R-squared	0.013	0.016	0.021				0.004	0.045	0.046			
AB test for AR(1) (p-val)				0.000	0.000	0.000				0.000	0.000	0.000
AB test for AR(2) (p-val)				0.627	0.576	0.737				0.701	0.731	0.708
Sargan test (p-val)				0.643	0.155	0.962				0.132	0.107	0.122
Hansen test (p-val)				0.160	0.307	0.331				0.371	0.152	0.332

Notes: Time and district - origin for specifications (1)–(6) and destination for specification (7)–(12)—fixed effects are included.
Robust standard errors in parentheses. Based on Conley (1999) a correction for spatial dependency with a cutoff point of 64 kilometers is applied for OLS specifications.
* significant at 10%, ** at 5%, *** at 1%. AB = Arellano and Bond(1991); HM =heavy monsoon; RM = Regular monsoon;
AR(1) = first-order autocovariance in residuals of order 1; AR(2) = first-order autocovariance in residuals of order 2

Table 5.2—Descriptive statistics for district-level variables, periods 2000 to 2003 and 2007 to 2010 (districts = 69, n = 552)

	Mean	St. dev.	Fisher's test
Flood during heavy monsoon (unweighted)	0.183	(0.387)	329***
Drought during heavy monsoon (unweighted)	0.308	(0.462)	443***
Total conflicts per square km	0.002	(0.009)	120
River density (length of river per square km)	0.171	(0.023)	343***
Actual migration outflow rate from district	0.005	(0.007)	358***
Actual migration inflow rate to district	0.003	(0.005)	329***
Aggregate actual net migration rate (cum. 4-year) (weighted by sample size in each district)	0.005	(0.031)	

Note: *** significant at 1%

Table 5.3—Relationship between predicted and actual migration rates (first stage)

Dependent variable	Actual net migration rate			
	Dynamic model		OLS model	
	IV(1)	IV(2)	IV(1)	IV(2)
Predicted net migration rate	1.459***		2.107***	
(cumulative 4-yr)	(0.533)		(0.668)	
Predicted out migration rate		-0.580**		-4.829
(cumulative 4-yr)		(0.241)		(5.123)
Predicted in migration rate		1.918***		2.165**
(cumulative 4-yr)		(0.672)		(0.862)
Individual age	-0.00000	-0.00001	-0.00001	-0.00001
	(0.000)	(0.000)	(0.000)	(0.000)
Individual male	0.00008	0.0002	0.0002	0.0002
	(0.000)	(0.000)	(0.000)	(0.000)
Individual education years	-0.0000	-0.00002	-0.00003	-0.00002
	(0.000)	(0.000)	(0.000)	(0.000)
Urban	0.00015	0.00017	0.00025	0.00034
	(0.000)	(0.000)	(0.000)	(0.000)
Observations	24.235	24.235	24.235	24.235
R-Squared	0.598	0.652	0.646	0.652
Number of districts	69	69	69	69
F-stat	58.28***	63.92***	61.67***	64.5***
F-stat on excl. IV	13.86***	12.53***	23.003***	13.34***
Weak identification test[a]	13.784	12.464	22.861	13.223
Stock-Yogo critical values				
10 percent maximal IV size	16.380	19.930	16.380	19.930
15 percent maximal IV size	8.960	11.590	8.960	11.590

Notes: Time and district fixed effects are included. [a] The weak identification test provides the Kleibergen-Paap rk Wald F statistic.
Standard errors in parentheses are bootstrapped and clustered at the district level.
* significant at 10%, ** at 5%, *** at 1%.

Table 5.4—Effect of net migration rate on wages for nonmigrant household heads aged 18-65 (second stage)

Dependent Variable	Log monthly real wages (2010 Nepal rupees)								
	OLS	IV(1)	IV(2)	OLS	IV(1)	IV(2)	OLS	IV(1)	IV(2)
	All			Formal sector			Informal sector		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Net migration rate	-1.6014	1.745	0.992	-5.072***	-4.753***	-5.066***	1.162	6.700	5.791
(cumulative 4-yr)	(0.962)	(3.298)	(2.808)	(0.560)	(0.855)	(0.671)	(1.554)	(5.129)	(4.597)
Individual control	Y	Y	Y	Y	Y	Y	Y	Y	Y
Occupation dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	5,234	5,234	5,234	4,119	4,119	4,119	3,113	3,113	3,113
R-squared (within)	0.510	0.508	0.509	0.285	0.285	0.285	0.365	0.362	0.363
Districts	69	69	69	67	67	67	69	69	69

Notes: Time and district fixed effects included. Standard errors, clustered at the district level, in parentheses. * significant at 10%, ** at 5%, *** at 1%. In all subsequent specifications, IV(1) and IV(2) use predicted net migration rates to instrument actual net migration rate, and predicted in- and out-migration rates as separate instruments for actual in- and out-migration rates, respectively.

Table 5.5—Effect of net migration rate on wages for nonmigrant household heads aged 18-65, by skill (second stage)

Dependent Variable	Log monthly real wages (2010 Nepal rupees)					
	OLS	IV(1)	IV(2)	OLS	IV(1)	IV(2)
Panel A	All sectors					
	High skill			Low skill		
	(1)	(2)	(3)	(4)	(5)	(6)
Net migration rate	-1.940*	-1.253	-1.202	-0.6378	4.615	3.431
(cumulative 4-yr)	(1.068)	(1.453)	(1.438)	(1.133)	(4.638)	(3.961)
Individual control	Y	Y	Y	Y	Y	Y
Occupation	Y	Y	Y	Y	Y	Y
Observations	1,075	1,075	1,075	4,154	4,154	4,154
R-squared (within)	0.464	0.464	0.464	0.480	0.478	0.479
Panel B	Formal sector					
	High skill			Low skill		
	(7)	(8)	(9)	(10)	(11)	(12)
Net migration rate	-1.675**	-1.518*	-1.593**	-5.397***	-4.655***	-5.376***
(cumulative 4-yr)	(0.705)	(0.818)	(0.790)	(0.745)	(1.326)	(0.939)
Individual controls	Y	Y	Y	Y	Y	Y
Occupation	Y	Y	Y	Y	Y	Y
Observations	573	573	573	1,530	1,530	1,530
R-squared (within)	0.171	0.171	0.171	0.250	0.250	0.250
Number of districts	45	45	45	66	66	66

Notes: Time and district fixed effects included. Standard errors, clustered at the district level, in parentheses. * significant at 10%, ** at 5%, *** at 1%. High skill refers to those individuals with at least 10 years of education.

Table 5.6—Effect of net migration rate on employment for nonmigrant household heads aged 18–65 (second stage)

Dependent variable	Employment probability (worked in last 12 months)								
	OLS	IV(1)	IV(2)	OLS	IV(1)	IV(2)	OLS	IV(1)	IV(2)
Panel A	All			Formal sector			Informal sector		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Net migration rate	-0.721***	-0.934***	-0.981***	0.459*	0.594	0.725	-1.132***	-1.466***	-1.630***
(cumulative 4-yr)	(0.110)	(0.154)	(0.161)	(0.241)	(0.381)	(0.485)	(0.209)	(0.434)	(0.556)
Individual control	Y	Y	Y	Y	Y	Y	Y	Y	Y
Occupation dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	7,965	7,965	7,965	7,965	7,965	7,965	7,965	7,965	7,965
R-squared (within)	0.055	0.055	0.055	0.055	0.055	0.055	0.040	0.040	0.040
Districts	69	69	69	69	69	69	69	69	69
Panel B	High skill			Low skill					
	(10)	(11)	(12)	(13)	(14)	(15)			
Net migration rate	-0.113	-0.073	-0.098	-0.710***	-1.031***	-1.096***			
(cumulative 4-yr)	(0.170)	(0.189)	(0.173)	(0.163)	(0.212)	(0.217)			
Individual control	Y	Y	Y	Y	Y	Y			
Occupation dummies	Y	Y	Y	Y	Y	Y			
Observations	1,358	1,358	1,358	6,604	6,604	6,604			
R-squared (within)	0.182	0.182	0.182	0.111	0.111	0.111			
Districts	64	64	64	69	69	69			

Notes: Time and district fixed effects included. Standard errors, clustered at the district level, in parentheses.
* significant at 10%, ** at 5%, *** at 1%.

Table 5.7—Effect of net migration rate on unemployment for nonmigrant household heads aged 18–65

Dependent variable	Unemployment probability (worked in last 12 months)								
	OLS	IV(1)	IV(2)	OLS	IV(1)	IV(2)	OLS	IV(1)	IV(2)
	All			High skill			Low skill		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Net migration rate	1.011***	1.295***	1.372***	0.552***	0.570***	0.574***	1.147***	1.542***	1.675***
(cumulative 4-yr)	(0.211)	(0.172)	(0.163)	(0.163)	(0.182)	(0.173)	(0.329)	(0.257)	(0.215)
Individual control	Y	Y	Y	Y	Y	Y	Y	Y	Y
Occupation dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	7,965	7,965	7,965	1,358	1,358	1,358	6,604	6,604	6,604
R-squared (within)	0.100	0.099	0.099	0.153	0.153	1,358	0.095	0.094	0.093
Districts	69	69	69	64	64	64	69	69	69

Notes: Time and district fixed effects included. Standard errors, clustered at the district level, in parentheses.
* significant at 10%, ** at 5%, *** at 1%.

.1 Appendix

Table 5.8—Effect of net migration rate on nonmigrant household expenditure patterns

	OLS	IV(1)	IV(2)	OLS	IV(1)	IV(2)
Panel A	Log per capita total Expenditures (real 2010 rupees)			Share food expenditures (real 2010 rupees)		
	(1)	(2)	(3)	(4)	(5)	(6)
Net migration rate	-0.549	1.133	1.105	0.003	0.031	0.016
(cumulative 4-yr)	(0.436)	(1.504)	(1.539)	(0.146)	(0.163)	(0.167)
Individual control	Y	Y	Y	Y	Y	Y
Occupation dummies	Y	Y	Y	Y	Y	Y
Observations	7,965	7,965	7,965	7,965	7,965	7,965
R-squared (within)	0.449	0.447	0.447	0.242	0.242	0.242
Number of districts	69	69	69	69	69	69
	OLS	IV(1)	IV(2)	OLS	IV(1)	IV(2)
Panel B	Share nonfood expenditures excl. services (real 2010 rupees)			Share services expenditure (real 2010 rupees)		
	(7)	(8)	(9)	(10)	(11)	(12)
Net migration rate	0.555***	0.855***	0.879***	-0.558**	-0.886***	-0.895***
(cumulative 4-yr)	(0.117)	(0.188)	(0.191)	(0.225)	(0.147)	(0.126)
Individual control	Y	Y	Y	Y	Y	Y
Occupation dummies	Y	Y	Y	Y	Y	Y
Observations	7,965	7,965	7,965	7,965	7,965	7,965
R-squared (within)	0.356	0.355	0.354	0.065	0.064	0.063
Number of districts	69	69	69	69	69	69

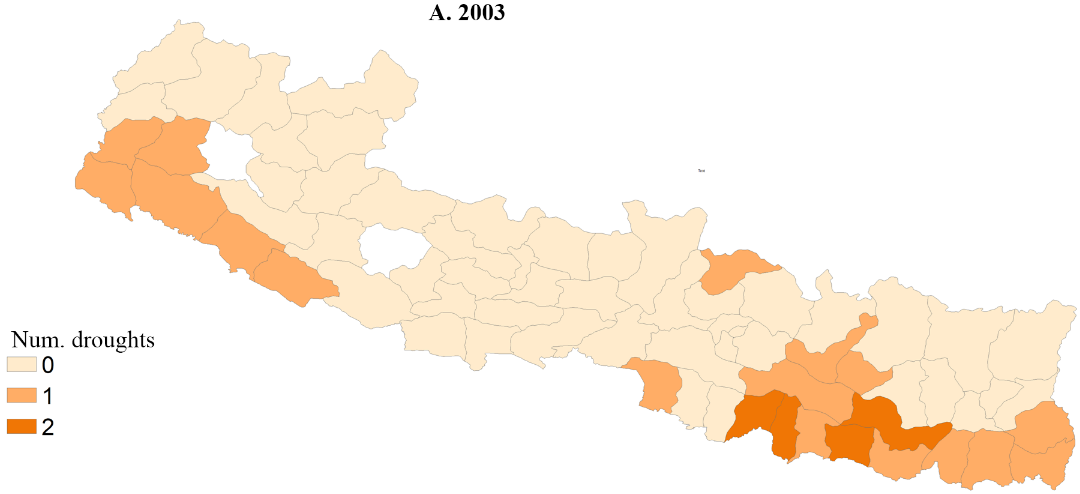
Notes: Time and district fixed effects included. Standard errors, clustered at the district level, in parentheses. * significant at 10%, ** at 5%, *** at 1%.

Table 5.9—Nonmigrant household financial and capacity constraints of enterprises (| own enterprise), weighted, 2003 and 2010

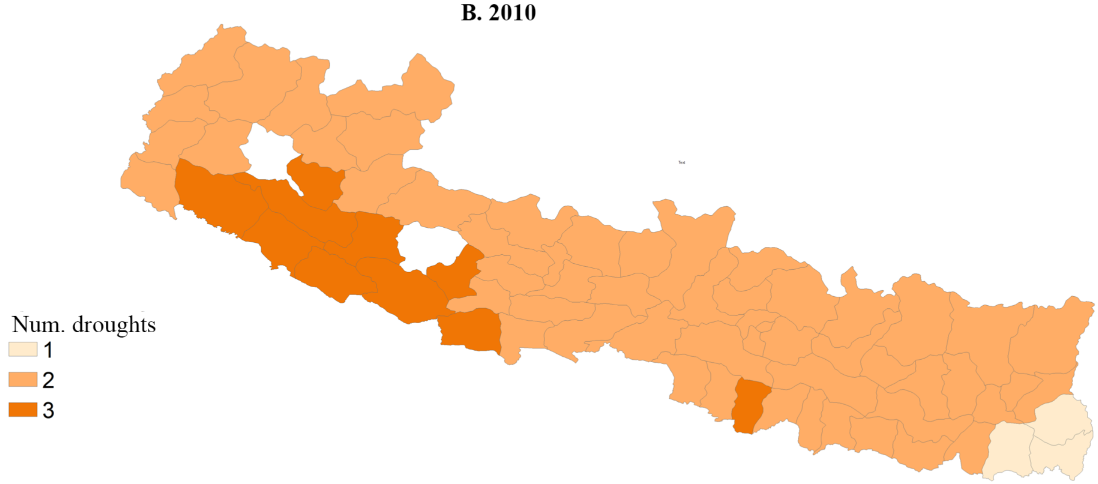
	2003 All	2010 All	2003 Low skill	2003 High skill	2010 Low skill	2010 High skill
	(n=865)	(n = 1,854)	(n = 695)	(n = 170)	(n = 1,469)	(n = 385)
Is the enterprise registered with the govermnent? (yes)	0.20	0.21	0.15	0.54	0.15	0.48
	(0.40)	(0.41)	(0.35)	(0.50)	(0.36)	(0.50)
What was the main source of money for setting up the enterprise?						
Didn't need any money	0.30	0.33	0.31	0.21	0.35	0.20
	(0.46)	(0.47)	(0.46)	(0.41)	(0.48)	(0.40)
Own savings	0.41	0.37	0.39	0.53	0.37	0.41
	(0.49)	(0.48)	(0.49)	(0.50)	(0.48)	(0.49)
Relatives or friends	0.14	0.13	0.15	0.10	0.13	0.16
	(0.35)	(0.34)	(0.36)	(0.30)	(0.34)	(0.37)
Bank (agricultural, commercial, Grameen type)	0.07	0.06	0.07	0.07	0.05	0.11
	(0.26)	(0.25)	(0.26)	(0.26)	(0.23)	(0.31)
Other financial institution	0.01	0.04	0.01	0.01	0.03	0.08
	(0.12)	(0.20)	(0.12)	(0.12)	(0.18)	(0.27)
Other	0.07	0.06	0.07	0.07	0.07	0.05
	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)	(0.21)
Have you tried to borrow money to operate or expand your business in the past 12 months? (relative to no)						
Yes, successfully	0.20	0.23	0.20	0.18	0.22	0.31
	(0.40)	(0.42)	(0.40)	(0.39)	(0.41)	(0.47)
Yes, unsuccessfully	0.04	0.03	0.04	0.03	0.03	0.04
	(0.19)	(0.17)	(0.19)	(0.17)	(0.17)	(0.20)
Did you hire anyone over the past 12 months? (yes)	0.13	0.17	0.11	0.30	0.14	0.35
	(0.34)	(0.38)	(0.31)	(0.46)	(0.34)	(0.49)
How many workers do you normally hire during a month when the enterprise is operating?						
(| hired in last 12 months)	8.88	9.98	4.99	17.80	11.00	7.84
	(32.10)	(38.60)	(20.60)	(48.20)	(42.80)	(28.40)
What problems, if any, do you have in running your business?						
No major problem	0.35	0.49	0.36	0.30	0.51	0.38
	(0.48)	(0.50)	(0.48)	(0.46)	(0.50)	(0.49)
Caiptal or credit problem	0.15	0.13	0.15	0.22	0.13	0.16
	(0.36)	(0.34)	(0.35)	(0.41)	(0.33)	(0.36)
Lack of customers	0.31	0.14	0.32	0.24	0.13	0.17
	(0.46)	(0.34)	(0.47)	(0.43)	(0.34)	(0.37)
Other	0.18	0.25	0.17	0.25	0.23	0.30
	(0.39)	(0.43)	(0.38)	(0.44)	(0.42)	(0.46)

Figure A.1—Droughts in Nepal, cumulative over previous four years, 2003 versus 2010

A. 2003



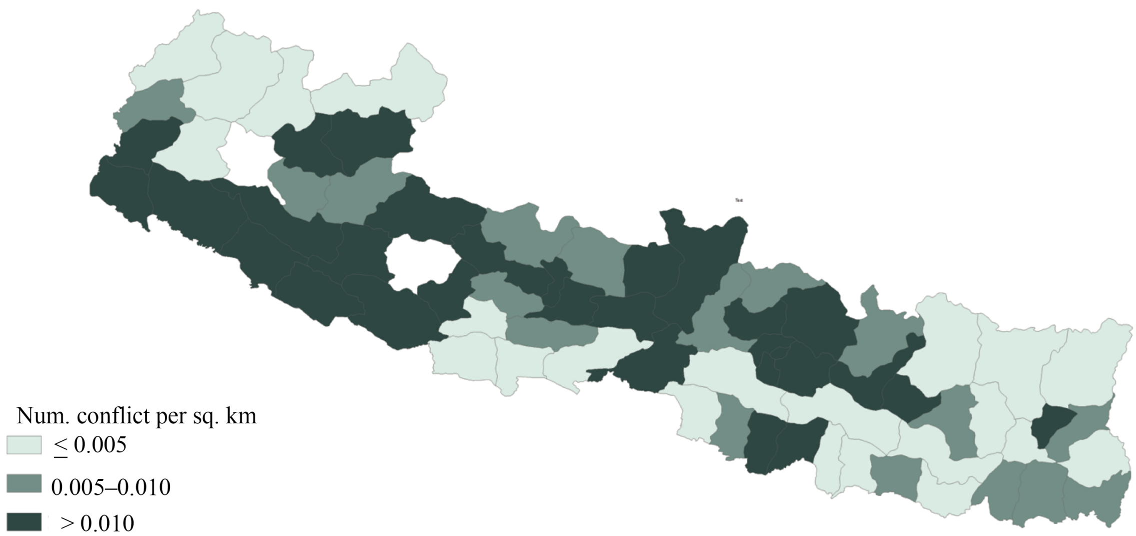
B. 2010



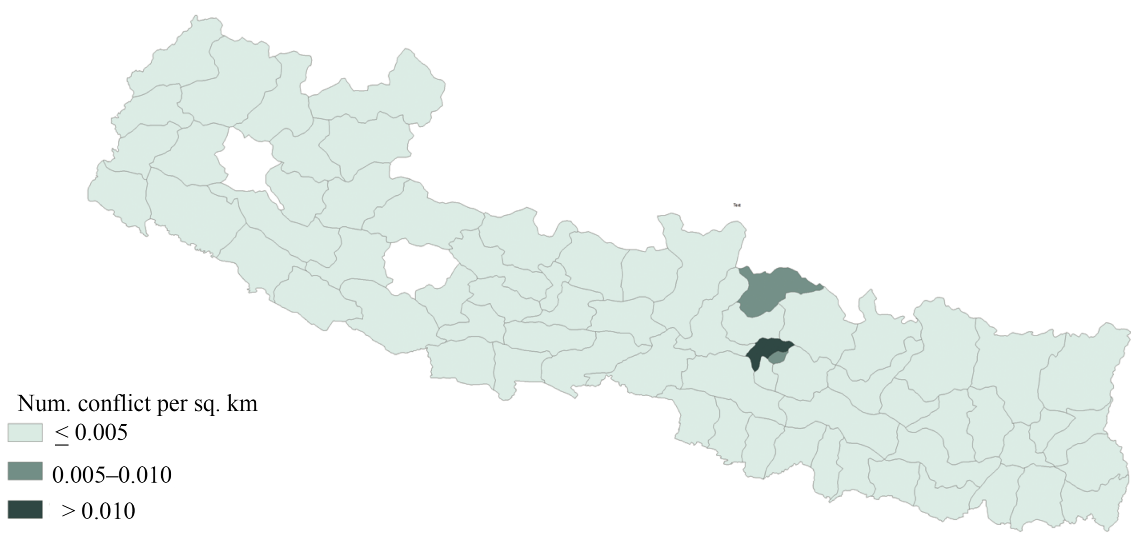
Source: Authors' representation based on NASA (2014).
Note: Districts not used in analysis are omitted in maps.

Figure A.2—Conflicts in Nepal, cumulative over previous four years, 2003 versus 2010

A. 2003



B. 2010



Source: Authors' representation based on ACLED (20104).
Note: Districts not used in analysis are omitted from Maps.

Table A.1—Effect of net migration rate on wages using alternate instruments derived from adjusted OLS method for nonmigrant household heads aged 18-65 (second stage)

Dependent variable	Log monthly real wages (2010 Nepal rupees)					
	IV(1)	IV(2)	IV(1)	IV(2)	IV(1)	IV(2)
	All		Formal sector		Informal sector	
	(1)	(2)	(3)	(4)	(5)	(6)
Net migration rate	0.290	0.126	-5.2797***	-5.3236***	4.6988	4.5644
(cumulative 4-yr)	(2.395)	(2.303)	(0.603)	(0.583)	(4.021)	(3.945)
Individual control	Y	Y	Y	Y	Y	Y
Occupation dummies	Y	Y	Y	Y	Y	Y
Observations	5,234	5,234	2,119	2,119	3,113	3,113
R-squared (within)	0.509	0.509	0.285	0.285	0.364	0.364
Number of districts	69	69	67	67	69	69

Notes: Time and district fixed effects included. Standard errors, clustered at the district level, in parentheses. * significant at 10%, ** at 5%, *** at 1%.

Table A.2—Effect of net migration rate on wages using alternate instruments derived from adjusted OLS method for nonmigrant household heads aged 18-65, by skill (second stage)

Dependent Variable	Log monthly real wages (2010 Nepal rupees)			
	IV(1)	IV(2)	IV(1)	IV(2)
Panel A	All sectors			
	High skill		Low skill	
	(1)	(2)	(3)	(4)
Net migration rate	-1.444	-1.794	2.403	2.309
(cumulative 4-yr)	(1.368)	(1.229)	(3.405)	(3.359)
Individual control	Y	Y	Y	Y
Occupation dummies	Y	Y	Y	Y
Observations	1,075	1,075	4,154	4,154
R-squared (within)	0.464	0.464	0.479	0.480
Number of districts	60	60	69	69
Panel B	Formal sector			
	High skill		Low skill	
	(5)	(6)	(7)	(8)
Net migration rate	-1.7355**	-1.7758**	-5.8440***	-5.9253***
(cumulative 4-yr)	(0.807)	(0.808)	(0.828)	(0.803)
Individual control	Y	Y	Y	Y
Occupation dummies	Y	Y	Y	Y
Observations	573	573	1,530	1,530
R-squared (within)	0.171	0.171	0.250	0.250
Number of districts	45	45	66	66

Notes: Time and district fixed effects included. Standard errors , clustered at the district level, in parentheses. * significant at 10%, ** at 5%, *** at 1%. High skill refers to those individuals with at least 10 years of education.

Table A.3—Testing exclusion restrictions, including spatially lagged weather shock and climate variables in own district

	IV(1)	IV(2)	IV(1)	IV(2)	IV(1)	IV(2)
Panel A	Log monthly real wage (2010 Nepal rupees)					
	Formal		High skill		Low skill	
	(1)	(2)	(3)	(4)	(5)	(6)
Net migration rate	-4.005*	-4.107**	-7.070	-4.136	18.839***	19.976***
(cumulative 4-yr)	(2.209)	(2.041)	(8.189)	(9.021)	(6.931)	(7.142)
Observations	2,120	2,120	1,075	1,075	4,154	4,154
R-squared	0.219	0.219	0.112	0.112	0.114	0.113
Number of districts	67	67	60	60	69	69
Panel B	Employed (worked in last 12 months)					
	Formal sector		High skill		Low skill	
	(7)	(8)	(9)	(10)	(11)	(12)
Net migration rate	1.240*	1.497*	-1.668*	-1.551*	-0.956**	-1.008***
(cumulative 4-yr)	(0.739)	(0.829)	(0.916)	(0.890)	(0.377)	(0.380)
Observations	7,967	7,967	1,358	1,358	6,604	6,604
R-squared	0.055	0.055	0.088	0.088	0.090	0.090
Number of districts	69	69	64	64	69	69
Panel C	Unemployed (worked in last 12 months)					
	All		High skill		Low skill	
	(13)	(14)	(15)	(16)	(17)	(18)
Net migration rate	1.319***	1.383***	1.950**	1.860**	1.305***	1.381***
(cumulative 4-yr)	(0.363)	(0.378)	(0.850)	(0.872)	(0.395)	(0.406)
Observations	7,965	7,965	1,358	1,358	6,604	6,604
R-squared	0.077	0.077	0.103	0.103	0.079	0.078
Number of districts	69	69	64	64	69	69
	Included in Panels A, B, and C					
Spatially lagged variables	Y	Y	Y	Y	Y	Y
HH head controls	Y	Y	Y	Y	Y	Y

Notes: Time and district fixed effects included. Standard errors, clustered at the district level, in parentheses. * significant at 10%, ** at 5%, *** at 1%. Spatially lagged variables include the spatially lagged versions of all weather and conflict variables used in Table 2. HH = Household.

Table A.4—Relationship between skill and wages for nonmigrant household heads, aged 18-65

Dependent Variable	Log real wage per month (2010 Nepal rupees)					
	All		Formal sector		Informal sector	
	(1)	(2)	(3)	(4)	(5)	(6)
Yrs of schooling	0.044***		0.051***		0.057***	
	(0.006)		(0.005)		(0.008)	
Schooling by category (relative to less than primary, 0–4 yrs of school) :						
Completed primary to		0.103**		0.166***		0.280***
less than secondary (5–9 yrs of school)		(0.050)		(0.046)		(0.058)
Completed secondary to less than		0.340***		0.354***		0.602***
higher secondary (10–11 yrs of school)		(0.065)		(0.067)		(0.079)
Completed higher secondary or more		0.730***		0.663***		0.653***
(12 or more years of school)		(0.112)		(0.058)		(0.154)
Individual controls	Y	Y	Y	Y	Y	Y
Observations	5,234	5,234	2,121	2,121	3,113	3,113
R-squared (within)	0.401	0.405	0.275	0.276	0.365	0.365
Number of districts	69	69	69	69	69	69

Notes: Time and district fixed effects included. Standard errors, clustered at the district level, in parentheses.
* significant at 10%, ** at 5%, *** at 1%.

Chapter 3: Crop Loss and Youth Labor and Schooling Outcomes in Tanzania

Abstract

I investigate the relationship between transitory income shocks and youth labor and schooling outcomes. I find that crop shocks increase labor significantly among youth aged 14 to 19 and increase the probability that youth enrolled in school miss school by 13 –18%. The observed impacts on youth predominantly affect female youth. Labor across female and male youth is approximately equivalent among unaffected households. I find that youth not enrolled in school are more likely to take up paid employment as a result of crop shocks. Youth enrolled in school on the other hand, substitute in for unpaid labor and participation in household chores. Further, comparing youth and children, I find that while youth schooling outcomes are affected, I find no similar effects among children aged 7 to 13.
JEL Classification:J22; J82; I25
Keywords: Crop loss, Youth labor, Youth schooling

3.1 Introduction

This paper examines the impact of crop loss [1] on labor and schooling outcomes for youth. I define youth as secondary school aged individuals between 14 and 19 years, and children as primary aged between 7 and 13 years. I subsequently compare differences in household choices of labor allocation between youth and children, given such shocks. While a growing body of literature examines the causes and consequences of child labor (Jensen (2000b), Ranjan (2001), Beegle et al. (2006), de Janvry et al. (2006), Edmonds (2006), Kruger (2007), Hazarika and Sarangi (2008)), very little is known about the economic impact of transitory shocks on secondary school aged youth in developing countries.

Transitory shocks could increase youth labor supply and reduce participation and performance in school, contributing to lower long-term human capital accumulation similar to childhood exposure (Beegle et al. (2007), Maccini and Yang (2009), Psacharopoulos (1997)). Schooling enrollment among youth in Sub-Saharan African countries like Tanzania remain particularly low. In Tanzania, the net enrollment ratio (NER)[2] for youth aged 14-17 in Ordinary Levels was 36.6% in 2011, and just 2.7% for youth aged 18-19 in Advanced Levels (Education Sector Development Committee (2011)), while the dropout rate for youth enrolled in secondary schools was 9.3%.

Using panel data from the Tanzania Living Standards Measurement Survey (LSMS) I find that for youth enrolled in school, exposure to crop shocks in the prior rainy season reduces current school attendance and increases labor hours in unpaid farm and non-farm activities. For youth not enrolled in school, such shocks increase hours spent on wage employment dramatically, while decreasing participation in household chores. Youth affected by crop shocks spend nearly double the number of hours en-

[1] Crop loss is measured if a household is affected by floods, droughts or diseases and pests, and loses over 10% of the value of crops during the last growing season.

[2] NER = Enrolled children in the official school age group / Total number of children in the official school age group

gaged in labor relative to children and are also significantly likely to miss school as a result of such shocks, an impact not seen for children. Unlike primary schooling, secondary schooling in Tanzania is not free. Fees and additional costs associated with attending school are likely to cause households affected by crop shocks to keep school attending youth out of school. School attending youth may also substitute for household and farm labor of adults and youth not in school, who are more likely to take up wage employment after such shocks. The value of youth labor after a crop shock is expected to be higher than for households that do not face such shocks.

Results observed for children are not as dramatic as some recent papers on the topic (Beegle et al. (2006)), potentially because our sample is nationally representative of agricultural households in Tanzania and does not focus on a particularly poor region (Kagera)[3]. In addition, the difference could stem from the fact that a larger share of households in our sample are unaffected by crop shocks compared to Beegle et al. (2006) and a slightly smaller share of households lose over 50% of total crop value. Nonetheless I find that crop shocks are a significant determinant of youth outcomes, particularly youth in school, and it is important to find mechanisms that could reduce this burden for agricultural households.

Youth and child labor are detrimental to the accumulation of formal academic education, which may lead to lower long-term earning potential. However, for older children or youth, access to vocational training or on-the-job training may increase long-term earnings by providing more direct knowledge or 'education' of the job and increasing job networks (Emerson and Souza (2011)). Emerson and Souza (2011) investigate whether child labor is harmful, and find that the impact of entering the labor market on adult earnings is negative for young children but this negative effect becomes positive for children aged 12 to 14.

[3]Our definition of shocks (see footnote 1) is very similar to Beegle et al. (2006). While Beegle et al. (2006) take any magnitude of loss as an indicator of crop loss, I create a crop loss indicator equal to one if the share of crop loss was over 10% of the total crop value, in order to make our measure comparable to Beegle et al..

Focusing specifically on youth in Tanzania, Kahyarara and Teal (2008) study which forms of educational investments - vocational schooling, academic education or on-the-job training - are profitable for increasing incomes. The authors find that returns to education increase with the level of education for secondary school aged youth, and are higher than that of vocational training or on-the-job training. Interruptions to youth education and increased labor due to crop shocks could reduce educational attainment of youth by affecting performance. Though likely to happen rarely, they could also reduce the likelihood that youth not in school return to school if financial burdens were keeping youth out of school. Although this paper does not examine the long-term implications of crop loss on youth, it shows that youth schooling and labor outcomes are both affected.

To understand whether crop loss affecting youth labor and schooling reduce human capital formation in Tanzania, I refer to Akabayashi and Psacharopoulos (1999). Many youth and children in the Tanzania LSMS agricultural household sample who go to school also work. Youth or child labor can reduce time devoted to study negatively affecting schooling performance. If can also reduce time spent in school or lead to dropouts. However, these impacts are only harmful from an economic perspective if they contribute to a reduction in human capital formation. Akabayashi and Psacharopoulos (1999) using time-log data from the Human Resource Development Survey (HRDS) in Tanzania, find that hours of work for children tend to be negatively correlated with development of reading and math skills both through hours worked and the indirectly through the reduction of investment in human capital building activities. A study by Beegle et al. (2007) considers the long-run consequences of child labor in Tanzania's Kagera region. Instrumenting for child labor with crop and rainfall shocks, the paper causally associates labor with reduced educational attainment. As existing studies focus on the impact of child labor, work on the long run impact of youth labor is needed.

In this paper I address the question of whether crop shocks affect labor and schooling. This question has already been examined for children but not for youth. In addition, prior literature has not compared outcomes across youth and children.

The remainder of the paper is organized as follows. Appendix 3.2 describes our empirical approach. Appendix 3.3 presents our main empirical results, while Appendix 3.4 concludes.

3.2 Empirical Strategy

I investigate the effect of crop shocks for agricultural households on youth labor and schooling outcomes, and compare youth outcomes to child outcomes. Specifically, I use individual and household panel data from the Tanzania Living Standard Measurement Survey (LSMS). There are two waves of the LSMS for 2008/09 and 2010/11 and the LSMS is a nationally representative survey[4]. I focus on agricultural households in the panel, with a positive plot area, comprising of 2284 households in the fist wave and 2755 in the second wave[5]. Around 70% of agricultural households participate in some agriculture. Attrition for the agricultural household sample at 1.7%. Although attrition for most longitudinal surveys is a common problem, in this case attrition for the LSMS sample is very low most likely as the gap between survey years very small. There is no differential attrition between households affected by crop loss and the unaffected.

If crop loss shocks are measured accurately and are not correlated with time-variant factors that may also affect the outcomes of interest, I can recover causal estimates of the effect of transitory crop shocks on youth labor by estimating the

[4]More technical information about the survey can be found in National Bureau of Statistics (2009), National Bureau of Statistics (2011)

[5]Four hundred and seventy one original households' were matched to more than one household in the second wave

following equation:

$$y_{it} = \beta_0 + \beta_1 \mathbf{X}_{ijt} + \beta_2 \mathbf{cropshock}_{ijt} + \gamma_t + \gamma_t * \alpha_d + \eta_j + \epsilon_{ijt} \tag{3.1}$$

where i indexes individual, j household, d district and survey year ($t = 2008, 2009, 2010, 2011$). Dependent variable y is youth labor hours (or youth schooling - currently in school, missed school in last 2 weeks - and labor hours broken down by activity), *cropshock* is a indicator measure of income shocks. I denote by $\mathbf{X}_{ijt}$ a vector of control variables including time-varying individual, household and community controls, and by η_j household fixed effects. By including district-time fixed effects I purge any time-district specific difference from the estimation. Household fixed effects are used to control for time-invariant household and community characteristics, thereby making the coefficient on $\mathbf{cropshock}_{ijt}$ a measure of an idiosyncratic shock.

To examine differences in effects of crop loss on youth versus children in the full sample of youth and children, I follow the specification:

$$\begin{aligned} y_{it} = \beta_0 + \beta_1 \mathbf{X}_{ijt} + \beta_2 \mathbf{cropshock}_{ijt} + \beta_3 \mathbf{cropshock}_{ijt} * \mathbf{child}_{ijt} + \beta_4 \mathbf{child}_{ijt} \\ + \gamma_t + \alpha_d + \gamma_t * \alpha_d + \eta_j + \epsilon_{ijt} \end{aligned} \tag{3.2}$$

where child is an index variable that is one if the individual is aged 7–13 and and zero if aged 14–19. For this specification I am interested in observing coefficients β_2 and β_3, to compare differences between youth and children.

I investigate threats to identification, specifically plausibility of the exogeneity and the transitory assumptions on nature of crop shocks. This follows closely the work of Beegle et al. (2006) on child labor and shocks. Crop loss shocks could be systematically correlated with characteristics of households' such as wealth, land area and several other factors, thereby violating the exogeneity assumption. To minimize such concerns, I use household fixed effects, survey year dummies, and some individual and household controls, including mother and father in household, child age, and household size. Exogeneity may also be violated if youth or child labor predicted shocks. Using the second wave of the LSMS I examine the relationship between lagged mean youth and child labor at the household level and crop shocks.

Shocks can be viewed as transitory if a household (not) experiencing a shock in one period is no (less) more likely than other households to experience a shock in a future period. I explore the exogeneity and transitory assumptions further in the next section. In addition, the use of district, district-time and household fixed effects, allow us to purge the data of time invariant district and household effects,in essence reverting shocks to the household mean, and control for differences across district in a given year. I do not examine this empirically through a regression specification, like Beegle et al. (2006) because a large sample of the households experience no shock in both periods making any correlation between past and current shocks automatically significant.

3.2.1 Variable Measurement

3.2.1.1 Seasonal crop shocks

I measure crop loss values using the responses to questions in the household agricultural module in each survey round. Losses are measured for the last long rainy season - in which the majority of crops are planted - prior to the survey. I calculate the total crop area planted and total crop area harvested. If the total crop area planted was larger than the crop area harvested, I treat the value of crops for the harvested area, given in the survey, as a proportion of the total *potential value* of planted crops. For example, if 80% of planted land is harvested, I treat the value of harvested crop as 80% of the potential value. From this, I derive the value of crop loss and the share of total crop value lost. This is calculated if the area harvested was less than the area planted reportedly due to droughts, rains, insects or disease. I consider this estimate of the value of crop loss as more accurate than studies using respondent accounts of crop loss value. I measure the effect of crop loss for households' with any positive plot area and for plot areas between 1 and 25.5 hectares following Beegle et al. (2006).

Tanzania is still highly agrarian and many agricultural households' consistently face risks to their income process Dercon (1996). For the purpose of this paper, I create a seasonal crop shock dummy taking on a value of 1 if households' face lose more than 10% of the value of planted crops. I assume that losing over 10% of crop value can be considered to have a significant impact on household income[6]. Values lower than 10% may also be more subject to measurement error of the area planted and harvested as these values would be reasonably close together. I demonstrate that such shocks are both exogenous and transitory.

3.2.1.2 Labor and Schooling

I define child labor as total hours spent working on economic activities in the week prior to the survey. Economic activities consist of unpaid farm labor, unpaid non-farm labor on household businesses and wage employment. I also measure the disaggregated impact of crop loss on participation in the different activities. Most youth aged 14-19 and children aged 7-13[7] allocate the majority of their labor time to unpaid labor. A larger proportion of youth work in wage employment. I measure hours spent on household chores, collecting firewood and fetching water, as a separate form of labor, as responses are collected for participation in chores yesterday. Schooling outcomes measured are currently enrolled in school and missed school in the last 2 weeks. Conditional missed school is defined for only youth or children currently enrolled in

[6] Varying this cut-off at 15%, 20% and 25% does not change the results significantly.

[7] While many studies consider children as aged 7-15, I differentiate child and youth groups by primary and secondary school age

school. The unconditional measure of not being present in school in last 2 weeks is denoted by 0 if in school and did not miss school, 1 if in school and missed school, and 1 if not in school. I examine whether crop loss affects schooling enrollment, and also look at its impact on labor allocation patterns for youth, separately by schooling enrollment status.

3.2.1.3 Additional Controls

Other controls included in the estimation are youth or child age, father present in household , mother present in household, household wealth quartiles, and household size. Additionally I include district, year and household fixed effects and district-time controls. Ownership of various durable assets, and other characteristics such as material used to build the household are used to create a wealth score from Principal Component Analysis (PCA). Households are categorized into quartiles as poor, moderately poor, moderately wealthy and wealthiest, for the nationally representative sample. The inclusion of these controls absorbs their direct impact on youth or child labor and schooling, and ensures that the impact of crop shocks is not attenuated by commonly associated time-varying omitted variables. I find that the inclusion of these controls has very little effect on the significance, or size of the coefficients on crop loss.

3.2.2 Summary Statistics

Appendix 3.5 and Table A.1 in Appendix 3.6 present summary statistics for youth and children across sub-samples. This first sample considers all agricultural households with plot ownership. The second sample is a sub sample of the first constrained to households with plots 1–25.5 acres. Focus on this subsample follows Beegle et al. (2006) by excluding households with small crop areas and very large (commercial) farms because the marginal impact of crop loss for these outlier groups may be different from the remaining sample. The subsequent four columns present summary statistics for the two groups, separately by individuals exposed to crop shocks and those not exposed. For the full sample, on average youth worked close to 18 hours in the prior week, with over 60% participating in some form of economic activity. Youth spent a little over half an hour yesterday on household chores and roughly around 45% of youth engage in household chores. From Appendix 3.6, children worked approximately 7 hours in the prior week and 45% of children engaged in at least 1 hour of economic activity in the last week. Children spent a little over 0.4 hrs on household chores yesterday, with around 40% of children participating in some form of chores in the prior day. From the two tables, youth work well over double the hours worked by children, as expected. 85% of children in the sample are currently in school while only around 50% of youth are enrolled.

From both tables, with the exception of work in economic activities and participation in household chores, mean distribution across most covariates is similar for households affected and unaffected by crop shocks. Over 85% of households in the youth and child samples are rural. Household size for the youth and child sample of agricultural households' is large, with a mean close to 8. Parental education for both the child and youth samples are low. For both groups, approximately 10% of fathers' and 5% of mothers' have more than primary school education. Agricultural household wealth, on average, is below the national representative survey mean.

3.2.2.1 Exogeneity and Transitory Assumptions

I next examine plausibility of the transitory and exogenous assumptions of the crop shock variable for agricultural households. Appendix 3.5 Panel A shows the frequency with which shocks occurred for the longitudinal LSMS panel. Over 20% of households in the sample are affected by at least one shock over the two survey periods. In the sample of 3846 households only 2.6% of households are affected by a shock in both periods. However, as shown in Appendix 3.5 panel B, the conditional probability of experiencing a shock in the second period, having experienced one in the first is 20%. On the other hand, the probability of experiencing a shock in the second period, without experiencing one in the first period is 10%. The difference between the two probabilities is statistically significant. However, the conditional probability in either case is quite low. I conclude that the use of household and district-time fixed effects should control for any time-invariant or location-time specific differences such that shocks can be viewed as transitory in the LSMS sample.

Panel C, in Appendix 3.5, shows the share of the value of crop loss by number of shocks experienced over the 2 year LSMS panel. For households experiencing one shock during both surveys just over 30% of households lose over 50% of total crop value, this suggests crop shocks have a large impact on income for a large proportion of households. Of the 100 households facing shocks in both survey periods, over 20% experienced losses over 50% in the first LSMS wave and over 30% lose over 50% of total crop value in the second LSMS wave.

Appendix 3.5 presents the results from a simple OLS regression of crop loss shocks in the second wave of the LSMS on lagged child and youth labor in the previous period. Additional household controls are also included. Columns (1) and (2) provide some evidence that lagged youth labor or lagged child labor from LSMS wave 1 do not predict crop loss shocks in wave 2. Similarly, in column (3) it is clear that households with a higher youth or child labor intensity are not significantly more likely to experience shocks. Characteristics of the household head are not more likely to predict shock occurrence, with the exception of household head age in column (2). However, head age is only significant at the 90% level.

3.3 Results

3.3.1 Crop Loss Impact on Youth Labor and Schooling

Appendix 3.5 presents the results from OLS regressions of crop loss shocks on youth labor allocation. All specifications include time-varying child and household controls, year dummies and household, district, district-year fixed effects. I find evidence of a statistically significant positive relationship between crop shocks observed in the last long rainy season prior to the survey and total labor hours spent on economic activities for youth in the week prior to the survey. This suggests that the impacts of crop shocks last beyond the growing season. Youth in agricultural households facing crop loss spend on average 4 hours more on labor. However, crop loss in the prior growing season has no impact on time spent on household chores for youth. From columns (5) and (6) I observe that a crop loss shock is associated with 2.2 – 2.7 additional hours spent on unpaid farm labor, and from columns (7) and (8) 1.5 to 1.9 extra hours spent on unpaid non-farm labor. Crop shocks are not associated with increased labor in wage employment for the full sample of youth (column (9) and (10)). Other factors like youth age and gender affect the allocation of time to various labor activities. On average female youth allocate less time to labor in economic activities but more time on household chores, as expected. Differentiating by type of economic activities, females are significantly more likely to spend time in unpaid non-farm labor activities while males are more likely to engage in farm and wage labor.

If as shown crop loss affects youth participation in household labor in the post growing season, the underlying concern is that it would also affect youth schooling outcomes. From Appendix 3.5 columns (1) and (2) I find that seasonal crop shocks have no significant impact on the likelihood of schooling enrollment. However, the unconditional likelihood of not being present in school (regardless of enrollment status) increases by 6–9% (columns (3) and (4)). On average, nearly 15% percent of all youth report missing school in the last 2 weeks, while 26% of youth enrolled in school report missing school. Estimates conditional on school enrollment in columns (5) and (6) indicate that youth in school whose households are affected by crop loss are more likely to have missed school in the last two weeks by 12.5–17.6% over youth in unaffected households. The coefficient on crop loss when regressed on missed school is larger for the sample with a plot between 1 and 25.5 acres. The results from Appendix 3.5 and Appendix 3.5 are suggestive of a substitution effect between time spent on labor activities and missed school for youth as a result of crop shocks, which persists in the period following the growing season. The results on youth schooling may also be related to the costs of secondary schooling. In addition to school fees, students may be required to pay for uniforms, food and other materials. Hence, if crop shocks increase household budget constraints youth school attendance for future periods may be compromised.

3.3.2 Crop Loss Impact on Youth: By School Enrollment

Appendix 3.5 presents estimates on the effect of crop loss for youth by school enrollment status. A large proportion of secondary school aged youth in Tanzania are not enrolled in school. In the sample of agricultural households approximately 50% of youth are enrolled. Crop loss could have very different implications for youths' activities depending on whether they are currently in school. For youth already already participating in the labor force, a significant rise in the participation in labor activities as a result of crop loss may not raise as much concern as for youth who are currently enrolled in school.

In Appendix 3.5 I find a larger coefficient on the effect of crop loss on total labor for youth not enrolled in school compared to those enrolled. However, the difference across the two groups is not statistically significant. As a result of crop loss those not enrolled observe a per week increase in labor of 6.187 (not statistically significant) hours while those enrolled increase labor by 5.381 hours (significant at the 99% level).

When labor is broken into categories, both groups experience a significant increase in participation in at least one type of labor, but the types of labor differ dramatically. From the table, columns (4) and (6) indicate that youth in school increase unpaid farm and non-farm labor by approximately 2.5 hours per week in each activity as a result of crop loss. Furthermore, youth in school are predicted to spend nearly 1 additional hour in the last day on household chores (column (10)). I do not observe similar increases for these types of labor for youth not in school. However, youth not in school increase wage employment by 4.6 hours, (column (6)) while reducing time spent on household chores in the last day by 0.7 hours. These results indicate that when affected by a crop shock in the prior season, youth in school may substitute for the paid labor taken up by others in the household. I find a statistically significant increase in total household labor, and adult labor, with an increase in adult labor in wage employment. Results for adults are not shown in tables here, but can be provided. Because the impact of a crop shock may persist at least in the short term after the shock, youth in school compensate by taking up unpaid labor and household chores. Youth not in school may be more likely to seek out wage employment as a result of shocks, similar to other adults, if the impact of crop shocks from the growing season persist in the short to medium horizon.

3.3.3 Crop Loss Impact on Youth: By Gender

I assess the effects of crop loss on female and male youth independently to observe gender differences in the distribution of youth labor. Female youth from households not affected by crop loss work 16.9 hours per week, while male youth work 16.1 hours per week. The difference is not statistically significant. Appendix 3.5 presents results on youth labor allocation and schooling outcomes stratified by gender. The impact of crop loss on labor hours and schooling outcomes vary dramatically by gender. Appendix 3.5 column (1) and (2) show that female youth in households affected by crop shocks increase labor by a significant 7.6 hours per week, whereas under the same circumstances, males increase labor by 1 hr per week and the increase is not statistically significant. The coefficients on crop loss across males and females are significantly different.

The difference in labor hours supplied by male and female youth in households exposed to crop loss stems from a significant difference across the two groups in participation in unpaid farm labor. I find that female youth spend nearly 6 hours extra per week (column (4)) working on the farm if the household faces crop loss. On the other hand, the effect of crop loss on unpaid farm labor is -0.36 hours per week and insignificant for male youth, from column (2). The observed results for increased farm labor across female youth occurs in the period following crop shocks in the last long rainy season. This could indicate an increase in female farm labor in preparation for the following growing season substituting for adult labor which is more likely to be employed in wage labor as a result of the crop shock.

I find the results on differences in schooling across male and female youth in households impacted by crop loss align with the results on labor. In particular, although no impact is found on either male or female youth enrollment in schooling, I find a significant impact of crop loss on female youth missing school in the last 2 weeks. In Appendix 3.5 (columns (7) to (10)) I present results on the conditional probability of missing school in the last 2 weeks and the unconditional likelihood of not being present in school, disaggregated by gender. Conditional on enrollment, female youth are 35% more likely to miss school when affected by crop shocks, unconditionally this is 12%. Male youth are 11.7% more likely to miss school conditional on being enrolled, however, the increase for males is not statistically significant and it is significantly lower than the increase for female youth. Our results show a gender bias in the household allocation of labor and subsequent schooling outcomes for youth. That is, I find that although female youth in unaffected areas tend to spend approximately the same amount of time as male youth on labor, when agricultural households are affected by crop loss females work disproportionately more. As a result, female youth in school are less likely to attend school if the household face a crop shock in the prior season.

3.3.4 Comparing Youth Outcomes to Child Outcomes

Existing literature shows a strong association between crop shocks and an increase in child labor and a decline in educational enrollment for children. Our analysis concurs with the first finding, though not the second (Appendix 3.6). Results from Table A.2 Appendix 3.6 indicate that primary school aged children spend 1.4 to 1.8 hours extra on labor activities as a result of crop shocks. This is nearly 3 hours less than the increase observed for youth due to crop loss. With one exception, I do not find significant differences in hours worked across children and youth for labor broken down by type of activity. Column (3) measures the impact of crop loss on hours worked per week in farm labor. In households affected by crop loss children work 1.3 hours significantly less than youth on farm labor. Table A.3 Appendix 3.6 does not show any significant impact of crop loss on the likelihood of school attendance for primary school aged children.

Appendix 3.5 compares the effect of crop shocks on children to that on youth. From column (1) I find that children work 1 hour less than youth as a result of crop loss, but the difference is not significant. This result is unexpected because if I assume youth are more productive, particularly in agricultural activities, then I would expect the marginal increase in labor as a result of crop loss to be significantly larger for youth. Similarly, I do not find a difference in hours spent on household chores across youth and children in households affected by crop shocks.

Finally, I consider the differences in schooling outcomes across children and youth, as a result of crop shocks. From Appendix 3.5 I do not find any differential impact on school enrollment for youth or children experiencing crop shocks. Columns (7) and (8) suggest however, that there is a significant difference in likelihood of missing school or not being present in school between youth and children in households exposed to crop shocks. The unconditional likelihood of not being present in school and the conditional likelihood of missing school in the past 2 weeks is respectively 4% and 9% lower for children relative to youth. Comparing child outcomes to youth outcomes suggest that youth schooling may be more affected as a result of crop shocks. Youth many be more likely to miss or be absent from school than children as a result of past crop shocks because of the cost of education for youth. While primary school is free in Tanzania, secondary school is not. Further, other costs associated with school attendance like uniforms, and supplies are likely to be higher for youth than children. After crop shocks, credit constrained households are more likely to lead to reduced participation in school by youth potentially due to the higher opportunity cost of attendance for this group.

3.4 Conclusion

The International Labor Organization (ILO) defines child labor as work that interferes with schooling by depriving children of the opportunity to attend school, or obliging children to leave school prematurely or requiring them to attempt to combine school attendance with excessively long and heavy work. Youth labor could potentially have similar implications as child labor on human capital accumulation, especially for youth in school.

In this work, I have used plausibly exogenous and transitory crop shocks, caused by droughts, flood, disease and pests, to estimate the causal impact of seasonal crop loss on outcomes of youth in agricultural households. In addition I have compared youth outcomes to child outcomes. I find that youth labor allocation towards various economic activities increases significantly when households are constrained by crop loss in the prior growing season. I also find that youth are significantly more likely to have missed school in the last 2 weeks if affected by crop loss in the last season. I further find that there are differences in labor and schooling outcomes for youth exposed to crop loss by gender. Female youth are disproportionately more likely to engage in unpaid labor and miss school compared to male youth despite engaging in similar levels of labor if not affected by crop loss. Further, I find marked differences in the types of labor in which youth enrolled in school and those not enrolled in school participate.

Prior literature has focused on the impact of crop loss and other shocks on child labor. Hence, I compare differences across child and youth labor and schooling outcomes as a result of crop loss. I find that while both children and youth increase labor participation, relative to children youth increase labor by more as a result of the shock. However, differences are only significant for unpaid farm labor. More importantly, I find that while youth schooling outcomes are affected by shocks, child schooling is not affected. These findings imply that crop loss is important in determining youth labor and schooling outcomes, and that youth schooling attendance is more vulnerable to shocks than child schooling. Crop shocks are measured in the last rainy season prior to the survey, while labor and schooling outcomes are observed for the week before the survey. Hence, crop shocks may alter youth labor and schooling patterns in the short to medium term, as budget constrained households face the post-harvest period. Further, youth schooling in Tanzania is not free and child schooling is free, and additional costs associated with youth schooling are likely to be higher than child schooling, creating a barrier to youth remaining in school. The opportunity cost of labor for youth relative to children of attending school as opposed to working is likely to increase after crop shocks.

This paper demonstrates that while child labor and its causes and consequences are clearly important from a policy perspective, we need to pay more attention to youth outcomes as well. I cite earlier work by Kahyarara and Teal (2008) suggesting that for youth in Tanzania returns to education are larger than returns to on-the-job investments or vocational training. Further research is needed to establish the impact of crop loss and other shocks on long-term outcomes, including earnings and labor choices for youth in developing countries. This is of interest from a policy perspective because youth schooling enrollment in many developing countries, including Tanzania, tends to be very low. If shocks contribute to poor schooling outcomes for youth, ways of insuring against such shocks to prevent negative outcomes for youth must be developed

3.5 Tables & Figures

Table 1: Summary statistics: youth

	(1)		(2)		(3)		(4)	
	Youth aged 14–19							
Sample restriction: land acres (Ha)	> 0		1– 25.5		1– 25.5		1– 25.5	
Sample restriction: shock					With shock		No shock	
Hours worked in economic activities –last week[a]								
Mean	17.80	(21.60)	17.10	(21.50)	19.10	(21.60)	16.80	(21.40)
Proportion>0	0.64	(0.48)	0.62	(0.49)	0.68	(0.47)	0.60	(0.49)
Hours on household chores–yesterday								
Mean	0.51	(1.38)	0.53	(1.49)	0.58	(2.24)	0.52	(1.28)
Proportion>0	0.45	(0.50)	0.45	(0.50)	0.48	(0.50)	0.44	(0.50)
Log value of crop loss								
Mean	11.30	(1.73)	11.40	(1.55)	11.80	(1.44)	10.10	(1.16)
Proportion>0	0.16	(0.37)	0.17	(0.38)	1.00	(0.00)	0.00	(0.00)
Household wealth factor score	-1.08	(2.37)	-1.20	(2.28)	-1.68	(1.76)	-1.10	(2.36)
Household size	7.85	(4.40)	7.85	(3.81)	7.36	(3.37)	7.95	(3.90)
Rural	0.84	(0.37)	0.86	(0.35)	0.88	(0.32)	0.85	(0.35)
Individual currently in school	0.53	(0.50)	0.52	(0.50)	0.50	(0.50)	0.53	(0.50)
Father's schooling								
No school	0.21	(0.41)	0.21	(0.41)	0.22	(0.41)	0.21	(0.41)
Some primary, 1–7 years of schooling	0.62	(0.49)	0.65	(0.48)	0.65	(0.48)	0.65	(0.48)
Some secondary, 8–13 years of schooling	0.10	(0.30)	0.09	(0.28)	0.07	(0.25)	0.09	(0.29)
Higher than secondary	0.01	(0.09)	0.01	(0.07)	0.00	(0.05)	0.01	(0.08)
Don't know	0.05	(0.23)	0.05	(0.21)	0.07	(0.25)	0.04	(0.21)
Mother's schooling								
No school	0.33	(0.47)	0.32	(0.47)	0.29	(0.45)	0.32	(0.47)
Some primary, 1–7 years of schooling	0.59	(0.49)	0.61	(0.49)	0.67	(0.47)	0.60	(0.49)
Some secondary, 8–13 years of schooling	0.06	(0.24)	0.05	(0.22)	0.02	(0.15)	0.05	(0.23)
Higher than secondary	0.00	(0.03)	0.00	(0.03)	0.00	(0.00)	0.00	(0.03)
Don't know	0.03	(0.16)	0.02	(0.15)	0.03	(0.16)	0.02	(0.15)
Observations	3858		2804		488		2316	

Notes: a– Economic activities refers to unpaid farm labor, unpaid non-farm labor and wage employment.
Standard deviations are in parenthesis. *** indicates p<.01; ** indicates p<.05; * indicates p<.10

Table 2: Frequency and magnitude of shocks, agricultural households in LSMS survey

Panel A: Frequency of shocks		
Number of shocks across two survey rounds	Number of households	%
0	3016	78.42
1	730	18.98
2	100	2.60
Total	3846	100

Panel B: Conditional probability of shock occurrence	
	Probability
Pr. of shock in period 2— no shock in period 1	0.10
Pr. of shock in period 2— shock in period 1	0.20
Pr. of no shock in period 2 — no shock period 1	0.90
Pr. of no shock in period 2 — no shock period 1	0.80

Panel B: Magnitude of shock by number of shocks across survey rounds			
Share of the value of crop loss to total crop value	1 shock	2 shocks, survey round 1*	2 shocks, survey round 2*
10–25%	31.23	30.00	25.00
26–50%	37.39	47.00	42.00
51–75%	20.00	12.00	21.00
76–100%	11.37	11.00	12.00
Observations	730	100	100

Notes: * 2 shocks, survey round 1 denotes share of crop losses in 1st period if shocks in both survey rounds. Similarly, 2 shocks, survey round 2 denotes share of crop losses in 2nd period if shocks in both survey rounds.

Table 3: Predicting occurrence of crop loss in LSMS wave 2

	Crop loss in long-rainy period LSMS 2010/11		
	(1)	(2)	(3)
Lagged youth labor LSMS 2008/09	-0.0011		-0.0011
	(0.001)		(0.001)
Lagged child labor LSMS 2008/09		-0.0001	0.0004
		(0.001)	(0.001)
Household head age	0.0015	0.0016*	0.0015
	(0.000)	(0.000)	(0.000)
Household head gender	0.0058	0.0059	0.0058
	(0.016)	(0.016)	(0.016)
Household head education	-0.0007	-0.0007	-0.0007
	(0.000)	(0.000)	(0.000)
Observations	2,754	2,754	2,754
R-squared	0.009	0.006	0.010

Notes: Standard deviations are in parenthesis. *** indicates p<.01; ** indicates p<.05; * indicates p<.10

Table 4: Crop loss impacts on youth labor allocation

	Youth aged 14 to 19									
	Hours worked in economic activities (last week)		Hours on household chores (yesterday		Hours on unpaid farm labor (last week)		Hours on unpaid non-farm labor (last week)		Hours worked wage activities (last week)	
Sample restriction: land acres	>0	1–25.5	>0	1–25.5	>0	1–25.5	>0	1–25.5	>0	1–25.5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Shock: any crop loss	4.108***	3.929**	0.026	0.011	2.717***	2.246*	1.485**	1.934**	-0.093	-0.252
	(1.312)	(1.733)	(0.097)	(0.143)	(0.917)	(1.236)	(0.670)	(0.814)	(0.635)	(0.893)
Gender: female	-1.422*	-2.538**	0.273***	0.301***	-2.944***	-3.473***	3.639***	3.452***	-2.118***	-2.518***
	(0.846)	(1.021)	(0.063)	(0.084)	(0.592)	(0.728)	(0.432)	(0.479)	(0.410)	(0.526)
Age	1.822***	1.802***	-0.000	0.007	1.044***	0.977***	0.222**	0.166	0.556***	0.659***
	(0.219)	(0.267)	(0.016)	(0.022)	(0.153)	(0.190)	(0.112)	(0.125)	(0.106)	(0.137)
Household size	-0.095	-0.631	0.003	-0.029	-0.110	-0.755***	-0.070	-0.143	0.085	0.267
	(0.254)	(0.397)	(0.019)	(0.033)	(0.178)	(0.283)	(0.130)	(0.186)	(0.123)	(0.205)
Household wealth (relative to poorest)										
Moderately poor	2.442	1.783	-0.035	0.021	0.054	-0.443	2.094***	2.214**	0.293	0.012
	(1.497)	(2.000)	(0.111)	(0.166)	(1.047)	(1.427)	(0.765)	(0.939)	(0.725)	(1.031)
Moderately wealthy	4.295**	5.471**	-0.037	-0.030	0.752	2.275	2.393***	2.693**	1.150	0.503
	(1.814)	(2.412)	(0.135)	(0.200)	(1.268)	(1.721)	(0.927)	(1.133)	(0.879)	(1.243)
Wealthiest	4.969**	5.403	0.092	0.378	-0.344	0.371	3.720***	4.121**	1.593	0.910
	(2.517)	(3.542)	(0.187)	(0.293)	(1.760)	(2.527)	(1.286)	(1.664)	(1.219)	(1.825)
Time dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
District and district-time dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Household Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	3,743	2,723	3,743	2,723	3,743	2,723	3,743	2,723	3,743	2,723
R-squared	0.168	0.143	0.060	0.068	0.060	0.068	0.417	0.425	0.054	0.070

Notes: Other controls include mother and father schooling, and mother and father in household.
Standard deviations are in parenthesis. *** indicates p<.01; ** indicates p<.05; * indicates p<.10

Table 5: Crop loss impacts on youth schooling outcomes

	Youth aged 14 to 19					
	Currently in school		No present in school in last 2 weeks (unconditional)		Missed school in last 2 weeks (conditional on in school)	
Sample restriction: land acres	>0	1–25.5	>0	1–25.5	>0	1–25.5
	(1)	(2)	(3)	(4)	(5)	(6)
Shock: any crop loss	0.023	0.010	0.060**	0.091**	0.125***	0.176***
	(0.030)	(0.040)	(0.030)	(0.040)	(0.046)	(0.059)
Gender: female	-0.020	-0.026	0.009	0.013	0.013	0.006
	(0.020)	(0.024)	(0.020)	(0.024)	(0.027)	(0.033)
Age	-0.091***	-0.094***	0.071***	0.071***	0.005	-0.001
	(0.005)	(0.006)	(0.005)	(0.006)	(0.007)	(0.009)
Household size	0.005	0.005	-0.002	-0.005	-0.002	-0.017
	(0.006)	(0.009)	(0.006)	(0.009)	(0.012)	(0.018)
Household wealth (relative to poorest)						
Moderately poor	-0.028	-0.035	0.051	0.019	0.049	0.051
	(0.035)	(0.046)	(0.035)	(0.047)	(0.055)	(0.073)
Moderately wealthy	-0.007	0.015	0.058	0.015	0.014	0.029
	(0.042)	(0.056)	(0.042)	(0.056)	(0.064)	(0.084)
Wealthiest	-0.071	0.023	0.071	0.002	-0.010	0.141
	(0.058)	(0.082)	(0.058)	(0.082)	(0.083)	(0.118)
Time dummies	Y	Y	Y	Y	Y	Y
District and district-time dummies	Y	Y	Y	Y	Y	Y
Household Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	3,742	2,722	3,741	2,722	1,994	1,427
R-squared	0.168	0.176	0.119	0.120	0.099	0.139

Notes: Other controls include mother and father schooling, and mother and father in household.
Standard deviations are in parenthesis. *** indicates p<.01; ** indicates p<.05; * indicates p<.10

Table 6 : Crop loss impacts on labor outcomes of youth by schooling status

Sample restriction: land acres	Hours worked in economic activities (last week)		Hours on unpaid farm labor (last week)		Hours on unpaid non-farm labor (last week) 1–25.5		Hours worked wage activities (last week)		Hours on household chores (yesterday)	
	Not in school (1)	In school (2)	Not in school (3)	In school (4)	Not in school (5)	In school (6)	Not in school (7)	In school (8)	Not in school (9)	In school (10)
Shock: any crop loss	6.187	5.381***	3.172	2.698*	-1.598	2.546*	4.613**	0.137	-0.764***	0.911***
	(3.853)	(2.056)	(2.774)	(1.391)	(1.698)	(1.306)	(2.276)	(0.617)	(0.232)	(0.291)
Age	0.499	-0.156	-0.011	-0.093	-0.104	-0.113	0.614*	0.050	-0.013	0.016
	(0.560)	(0.296)	(0.403)	(0.200)	(0.247)	(0.188)	(0.331)	(0.089)	(0.034)	(0.042)
Household size	-6.775***	-1.247	-6.567***	-3.304***	3.025***	2.268***	-3.232***	-0.211	0.474***	0.222
	(2.077)	(1.107)	(1.496)	(0.749)	(0.915)	(0.703)	(1.227)	(0.332)	(0.125)	(0.157)
Household wealth (relative to poorest)										
Moderately poor	-2.424	4.171*	-3.666	1.354	3.541**	2.257	-2.300	0.559	0.164	0.305
	(3.876)	(2.441)	(2.790)	(1.651)	(1.708)	(1.550)	(2.289)	(0.732)	(0.235)	(0.345)
Moderately wealthy	4.664	6.016**	0.643	3.296*	4.080**	1.551	-0.059	1.169	-0.039	0.165
	(4.548)	(2.827)	(3.274)	(1.912)	(2.004)	(1.796)	(2.686)	(0.848)	(0.274)	(0.400)
Wealthiest	11.184	5.378	7.106	1.271	3.844	4.033	0.235	0.074	1.769***	0.484
	(7.496)	(4.046)	(5.396)	(2.737)	(3.303)	(2.570)	(4.428)	(1.214)	(0.450)	(0.573)
Time dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
District and district-time dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Household Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,290	1,420	1,290	1,420	1,290	1,420	1,290	1,420	1,285	1,417
R-squared	0.230	0.270	0.140	0.199	0.516	0.465	0.122	0.072	0.207	0.121

Notes: Other controls include mother and father schooling, and mother and father in household.
Standard deviations are in parenthesis. *** indicates p<.01; ** indicates p<.05; * indicates p<.10

Table 7 : Crop loss impacts on labor and schooling outcomes of youth by gender

	Hours worked in economic activities (last week)		Hours on unpaid farm labor (last week)		Currently in school		Not present in school in last 2 weeks (unconditional)		Missed school in last 2 weeks (conditional on in school)	
Sample restriction: land acres						1–25.5				
	Male	Female	Male	Female	Male	Female	Male	Female	Male	Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Shock: any crop loss	0.964	7.600***	-0.364	5.870***	0.039	-0.062	0.030	0.187***	0.117	0.349***
	(2.828)	(2.578)	(1.999)	(1.835)	(0.057)	(0.065)	(0.058)	(0.066)	(0.084)	(0.118)
Age	-0.036	-1.001*	-0.312	-0.981**	-0.000	-0.002	-0.000	0.004	-0.001	0.014
	(0.782)	(0.552)	(0.552)	(0.393)	(0.016)	(0.014)	(0.016)	(0.014)	(0.030)	(0.035)
Household size	-4.598	4.805	-5.209**	3.226	0.101	-0.129*	-0.085	0.136*	0.040	0.195
	(3.273)	(3.045)	(2.314)	(2.168)	(0.066)	(0.077)	(0.067)	(0.078)	(0.110)	(0.152)
Household wealth (relative to poorest)										
Moderately poor	-3.984	9.381***	-2.730	3.823	0.135	-0.022	-0.075	0.113	-0.018	0.220
	(4.216)	(3.347)	(2.980)	(2.383)	(0.085)	(0.085)	(0.086)	(0.085)	(0.133)	(0.151)
Moderately wealthy	-4.526	14.912***	-5.108	7.471*	0.111	-0.069	-0.016	0.009	0.232	0.053
	(5.875)	(5.407)	(4.152)	(3.850)	(0.119)	(0.137)	(0.120)	(0.138)	(0.172)	(0.226)
Wealthiest	0.271	26.127**	-9.889	3.928	0.213	0.093	0.036	-0.091	0.389	0.279
	(11.193)	(10.493)	(7.911)	(7.471)	(0.226)	(0.265)	(0.229)	(0.268)	(0.300)	(0.381)
Time dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
District and district-time dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Household Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,425	1,298	1,425	1,298	1,425	1,297	1,425	1,297	787	640
R-squared	0.119	0.252	0.089	0.128	0.224	0.249	0.123	0.120	0.223	0.257

Notes: Other controls include mother and father schooling, and mother and father in household.
Standard deviations are in parenthesis. *** indicates p<.01; ** indicates p<.05; * indicates p<.10

Table 8 : Comparing crop loss impacts on youth vs child

	Hours worked in economic activities (last week)	Hours on household chores (yesterday)	Hours worked on unpaid farm labor (last week)	Hours on unpaid non-farm labor	Hours worked wage (last week)	Currently in school activities	Missed school in last 2 weeks (unconditional)	Missed school in last 2 weeks (conditional on in school)
Sample restriction: land acres	1–25.5							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Shock: any crop loss	3.075***	0.141	2.575***	1.114**	-0.684	-0.006	0.047**	0.097***
	(1.015)	(0.108)	(0.591)	(0.433)	(0.443)	(0.017)	(0.023)	(0.033)
Any crop loss*Child	-1.006	0.167	-1.283*	-0.524	0.751	-0.009	-0.042*	-0.092***
	(1.069)	(0.114)	(0.658)	(0.482)	(0.466)	(0.018)	(0.024)	(0.033)
Child	-1.263	0.001	-0.848*	-0.172	-0.171	-0.115***	0.079***	0.015
	(0.794)	(0.084)	(0.481)	(0.352)	(0.346)	(0.013)	(0.018)	(0.022)
Gender: female	-1.078**	0.225***	-2.074***	2.231***	-0.848***	0.011	-0.013	-0.025**
	(0.428)	(0.035)	(0.317)	(0.192)	(0.186)	(0.007)	(0.010)	(0.012)
Age	1.463***	0.030***	0.825***	0.450***	0.264***	0.001	-0.000	0.003
	(0.108)	(0.009)	(0.080)	(0.048)	(0.047)	(0.002)	(0.002)	(0.003)
Household size	-0.156	-0.014	-0.142	-0.093	0.082	0.002	-0.003	-0.011*
	(0.183)	(0.012)	(0.135)	(0.067)	(0.080)	(0.003)	(0.004)	(0.006)
Household wealth (relative to poorest)								
Moderately poor	1.121	0.052	0.486	0.608	0.235	0.010	-0.002	0.006
	(0.929)	(0.069)	(0.688)	(0.372)	(0.405)	(0.015)	(0.021)	(0.028)
Moderately wealthy	3.258***	0.040	1.371	1.361***	0.271	0.013	0.035	0.019
	(1.172)	(0.087)	(0.868)	(0.468)	(0.511)	(0.019)	(0.027)	(0.034)
Wealthiest	3.689**	0.040	0.796	2.335***	0.391	-0.058**	0.014	0.013
	(1.781)	(0.125)	(1.319)	(0.675)	(0.777)	(0.029)	(0.040)	(0.051)
Time dummies	Y	Y	Y	Y	Y	Y	Y	Y
District and district-time dummies	Y	Y	Y	Y	Y	Y	Y	Y
Household Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Observations	6,567	6,567	6,567	6,567	6,567	6,563	6,563	4,680
R-squared	0.205	0.040	0.115	0.362	0.044	0.072	0.049	0.064

Notes: Other controls include mother and father schooling, and mother and father in household.
Standard deviations are in parenthesis. *** indicates p<.01; ** indicates p<.05; * indicates p<.10

3.6 Appendix

Table A.1: Summary statistics: child

	(1)		(2)		(3)		(4)	
	Child aged 7–13							
Sample restriction: land acres (Ha)	> 0		1– 25.5		1– 25.5		1– 25.5	
Sample restriction: shock					With shock		No shock	
Hours worked in economic activities –last week[a]								
Mean	7.22	(12.40)	7.06	(12.60)	7.63	(12.90)	6.93	(12.60)
Proportion>0	0.46	(0.50)	0.45	(0.50)	0.50	(0.50)	0.42	(0.49)
Hours on household chores–yesterday								
Mean	0.41	(1.69)	0.46	(1.79)	0.52	(3.05)	0.41	(1.37)
Proportion>0	0.39	(0.49)	0.42	(0.49)	0.46	(0.48)	0.41	(0.49)
Log value of crop loss								
Mean	11.30	(1.67)	11.40	(1.59)	11.70	(1.53)	10.10	(1.12)
Proportion>0	0.17	(0.38)	0.18	(0.38)	1.00	(0.00)	0.00	(0.00)
Household wealth factor score	-1.40	(2.05)	-1.55	(1.89)	-1.86	(1.62)	-1.48	(1.94)
Household size	7.71	(4.31)	7.67	(3.59)	7.23	(3.19)	7.76	(3.66)
Rural	0.86	(0.35)	0.89	(0.31)	0.91	(0.28)	0.88	(0.32)
Individual currently in school	0.85	(0.36)	0.85	(0.36)	0.85	(0.36)	0.85	(0.36)
Father's schooling								
No school	0.19	(0.39)	0.18	(0.39)	0.19	(0.39)	0.18	(0.38)
Some primary, 1–7 years of schooling	0.67	(0.47)	0.69	(0.46)	0.70	(0.46)	0.69	(0.46)
Some secondary, 8–13 years of schooling	0.10	(0.30)	0.08	(0.28)	0.06	(0.24)	0.09	(0.29)
Higher than secondary	0.00	(0.06)	0.00	(0.04)	0.00	(0.04)	0.00	(0.04)
Don't know	0.04	(0.20)	0.04	(0.19)	0.05	(0.22)	0.04	(0.19)
Mother's schooling								
No school	0.312	(0.46)	0.31	(0.46)	0.33	(0.47)	0.30	(0.46)
Some primary, 1–7 years of schooling	0.606	(0.49)	0.63	(0.48)	0.63	(0.48)	0.63	(0.48)
Some secondary, 8–13 years of schooling	0.0575	(0.23)	0.04	(0.20)	0.02	(0.14)	0.05	(0.21)
Higher than secondary	0.00188	(0.04)	0.00	(0.04)	0.00	(0.00)	0.00	(0.05)
Don't know	0.0224	(0.15)	0.02	(0.15)	0.03	(0.16)	0.02	(0.15)
Observations	5362		3977		714		3263	

Notes: a –Economic activities refers to unpaid farm labor, unpaid non-farm labor and wage employment. Standard deviations are in parenthesis. *** indicates p<.01; ** indicates p<.05; * indicates p<.10

Table A.2: Crop loss impacts on child labor allocation

	Child aged 7 to 13									
	Hours worked in economic activities (last week)		Hours on household chores (yesterday		Hours on unpaid farm labor (last week)		Hours on unpaid non-farm labor (last week)		Hours worked wage activities (last week)	
Sample restriction: land acres	>0	1–25.5	>0	1–25.5	>0	1–25.5	>0	1–25.5	>0	1–25.5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Shock: any crop loss	1.359**	1.853**	0.217**	0.406***	0.910**	1.527***	0.574*	0.367	-0.125	-0.041
	(0.561)	(0.732)	(0.090)	(0.126)	(0.423)	(0.559)	(0.332)	(0.411)	(0.117)	(0.161)
Gender: female	-0.211	-0.586	0.148**	0.140*	-1.307***	-1.344***	1.196***	0.874***	-0.101	-0.116
	(0.361)	(0.435)	(0.058)	(0.075)	(0.272)	(0.332)	(0.214)	(0.244)	(0.075)	(0.096)
Age	1.229***	1.184***	0.045***	0.056***	0.608***	0.678***	0.576***	0.454***	0.045***	0.051**
	(0.083)	(0.100)	(0.013)	(0.017)	(0.062)	(0.076)	(0.049)	(0.056)	(0.017)	(0.022)
Household size	-0.103	-0.059	-0.030	-0.044	0.058	0.104	-0.153**	-0.141	-0.008	-0.022
	(0.122)	(0.174)	(0.020)	(0.030)	(0.092)	(0.133)	(0.072)	(0.098)	(0.025)	(0.038)
Household wealth (relative to poorest)										
Moderately poor	0.358	0.675	0.106	0.261*	0.761	1.118	-0.518	-0.566	0.115	0.123
	(0.650)	(0.890)	(0.104)	(0.153)	(0.490)	(0.680)	(0.384)	(0.500)	(0.135)	(0.196)
Moderately wealthy	1.684**	2.647**	0.119	0.346*	1.526**	2.147**	0.299	0.545	-0.142	-0.045
	(0.834)	(1.153)	(0.134)	(0.198)	(0.629)	(0.880)	(0.493)	(0.648)	(0.174)	(0.253)
Wealthiest	2.816**	3.581**	0.029	0.049	1.535	2.390*	1.541**	1.652	-0.260	-0.461
	(1.242)	(1.791)	(0.200)	(0.308)	(0.936)	(1.368)	(0.735)	(1.006)	(0.259)	(0.394)
Time dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
District and district-time dummies	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Household Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	5,199	3,844	5,199	3,844	5,199	3,844	5,199	3,844	5,199	3,844
R-squared	0.214	0.195	0.024	0.043	0.067	0.078	0.401	0.379	0.017	0.030

Notes: Other controls include mother and father schooling, and mother and father in household.
Standard deviations are in parenthesis. *** indicates p<.01; ** indicates p<.05; * indicates p<.10

Table A.3: Crop loss impacts on child schooling outcomes

	Child aged 7 to 13					
	Currently in school		Not present in school in last 2 weeks (unconditional)		Missed school in last 2 weeks (conditional on in school)	
Sample restriction: land acres	>0	1–25.5	>0	1–25.5	>0	1–25.5
	(1)	(2)	(3)	(4)	(5)	(6)
Shock: any crop loss	-0.004	0.000	-0.013	-0.023	-0.017	-0.013
	(0.018)	(0.022)	(0.022)	(0.028)	(0.022)	(0.027)
Gender: female	0.026**	0.028**	-0.033**	-0.040**	-0.027*	-0.029*
	(0.011)	(0.013)	(0.014)	(0.017)	(0.014)	(0.016)
Age	0.031***	0.031***	-0.020***	-0.020***	0.004	0.004
	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
Household size	0.000	0.001	-0.010**	-0.009	-0.018***	-0.014*
	(0.004)	(0.005)	(0.005)	(0.007)	(0.005)	(0.007)
Household wealth (relative to poorest)						
Moderately poor	-0.016	-0.027	0.073***	0.040	0.082***	0.018
	(0.020)	(0.027)	(0.026)	(0.034)	(0.026)	(0.034)
Moderately wealthy	-0.024	-0.032	0.107***	0.074*	0.110***	0.044
	(0.026)	(0.035)	(0.033)	(0.044)	(0.033)	(0.043)
Wealthiest	0.050	0.031	0.016	-0.013	0.089*	0.013
	(0.039)	(0.055)	(0.050)	(0.069)	(0.047)	(0.065)
Time dummies	Y	Y	Y	Y	Y	Y
District and district-time dummies	Y	Y	Y	Y	Y	Y
Household Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	5,194	3,841	5,192	3,839	4,396	3,253
R-squared	0.055	0.063	0.052	0.054	0.067	0.072

Notes: Other controls include mother and father schooling, and mother and father in household.
Standard deviations are in parenthesis. *** indicates p<.01; ** indicates p<.05; * indicates p<.10

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