Three Essays on Development and Resilience in Nepal

by

WILLIAM M. THOMPSON

(Under the Direction of Nicholas Magnan)

Abstract

This dissertation comprises three essays on economic development and resilience in rural Nepal. The essay summarized in chapter 3 uses an RCT to evaluate program impacts of Heifer International's flagship intervention in Nepal after 18 months. In the spring of 2015 central Nepal was rocked by a severe earthquake and a series of significant aftershocks; the second and third essays consider the household coping response to the earthquake. The essay in chapter 4 uses panel data collected before and after the earthquake, and combines a first-difference model with matching methods to test a version Townsend's (1995) full consumption insurance hypothesis. The essay in chapter 5 considers households in the earthquake area who did belong to Heifer programs, and uses propensity score matching to identify the effect of a special, one-off zero-interest loan directed at earthquake-affected households.

Index words: Asset transfers, Resilience, Earthquake, Livestock, Poverty

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WILLIAM M. THOMPSON

B.A., Davidson College, 2000

M.S., University of Georgia, 2014

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WILLIAM M. THOMPSON

Major Professor: Nicholas Magnan

Committee: Jeffrey Dorfman
Ellen McCullough

Electronic Version Approved:

Suzanne Barbour
Dean of the Graduate School
The University of Georgia
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Dedication

This dissertation is dedicated foremost to my wife and best friend Kalee Kirts Thompson. Without your love, devotion, and unwavering faith and support this document would not exist. Thank you for placidly tolerating sleepless nights, long separations for field work, and providing a refuge from the stress of graduate school. I love you, K, more than I can express.

I also dedicate this dissertation to our daughter Tallulah and the unborn, unnamed Baby Thompson arriving in a few short months. You inspire and drive me to do my best, and I'm honored to be your father.

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Chapter 1

Introduction

In the summer of 2014, and again in the summer of 2016, we collected data from nearly 3,300 rural women eligible to participate in an asset transfer program across three regions of Nepal. Nepal has made significant strides towards poverty alleviation in recent years, yet poverty persists, especially in the countryside; 55 percent of Nepalese survive on less than $1.25 a day, and that number climbs sharply in the rural mountain and hill districts where more than 70 percent of people rely on agriculture for their livelihoods.

The first essay in this dissertation deals with the short-term welfare effects of a productive asset transfer. It is often argued that the rural poor largely lack access to the productive assets and human capital necessary to be successful entrepreneurs. Productive asset transfer programs, which typically include a training component, are one approach by which non-governmental organizations (NGOs) and governments try to alleviate these constraints thereby facilitating a permanent transition out of poverty. These programs are popular among donors who subscribe to the well-known "teach a man to fish" maxim. In some cases, they also include a "pay it forward" component where recipients share what they have learned and even share some of their newly accumulated wealth, in the form of a productive asset, to other households in need. Despite their popularity, rigorous impact evaluations of combined asset transfer and training programs are few and far between. The first essay 3 uses an RCT to evaluate program impacts of Heifer International's (HI) flagship intervention in Nepal after 18 months.

The second and third essays deal with household coping responses to a major natural disaster that took place in Nepal during the same time frame described above. In the spring of

2015 central Nepal was rocked by a severe earthquake and a series of significant aftershocks. The event destroyed homes and infrastructure, killed livestock, interrupted access to water from natural sources and from irrigation, disrupted preparations for the monsoon rice season, cut off access to markets for agricultural inputs and outputs, and generally disrupted the economic lives of people in the earthquake zone. The essay in chapter 4 uses panel data collected before and after the earthquake, and combines a first-difference model with matching methods to test a version Townsend's (1995) full consumption insurance hypothesis.

The essay in chapter 5 considers households in the earthquake area who did belong to HI programs, and attempts uses propensity score matching to identify treatment effects of (i) the HI program on earthquake resilience , and (ii) a special, one-off zero-interest loan of NPR 15,000 (approximately $150 USD) offered to a subset Heifer beneficiaries belonging to earthquake-affected households. This loan sought to provide communities and individuals with the flexibility to identify their own most pressing needs and allow to them to invest accordingly, thus RF loans were unrestricted and could be used for any purpose.

Chapter 2

Background

2.1 Heifer International in Nepal

Heifer International (HI) is a global non-governmental organization often credited with originating the popular livestock-transfer programs that appear in the portfolios of many other NGOs and governments across the developing world. HI's flagship program in Nepal is the Smallholders in Livestock Value Chain (SLVC) program. The SLVC targets poor households in rural areas, and seeks to provide a sustainable livelihood and a pathway out of poverty for its primarily women beneficiaries. The standard HI intervention in Nepal provides a package of benefits that includes four main components:

i) *Self-help group formation*: HI facilitates formation of women's self-help groups (SHGs). Members meet regularly and are encouraged to contribute to group savings accounts.

ii) *Technical trainings*: HI trains beneficiaries on technical topics including nutrition, home gardening, fodder and forage development, and improved animal management. Beneficiaries are provided cash support for planting home gardens ($5) and fodder/forage production ($10).

iii) *Values-based trainings*: HI trains beneficiaries on their "cornerstone" principles.[1] The values-based training teaches and encourages beneficiaries to "pay it forward."

iv) *Livestock transfer*: Beneficiaries each receive two doe goats and cash support ($40) to help build a goat shelter. Each SHG receives one breeding buck.

[1]These principles include accountability; sharing and caring; sustainability and self-reliance; gender and focus on the family; genuine need and justice; improving the environment; full participation; training, education, and communication; and spirituality.

Nepal's 75 administrative districts are subdivided into village development committees (VDCs), each of which has nine wards. The SLVC operates at the ward level, targeting a pre-identified group of women to organize into a SHG. This group normally includes a member of all households in a neighborhood (*tole*).

Members of the directly recruited SHGs recruit a second generation of beneficiaries by forming up to five additional SHGs of other women in their ward. In most cases this covers nearly all remaining households in a ward. They then "pay forward" benefits by giving new SHG members technical trainings and their two first-born female goat offspring (while keeping and raising male kids for future sale). HI staff directly provide values-based trainings for second generation SHGs.

2.2 Earthquake

On April 25, 2015 the a major earthquake of magnitude $7.8M_w$ $(8.1M_s)$ and a maximum Mercalli intensity of VIII struck central Nepal, killing close to 9,000 people and injuring nearly 22,000. The event destroyed homes and infrastructure, killed livestock, interrupted access to water from natural sources and from irrigation, disrupted preparations for the monsoon rice season, cut off access to markets for agricultural inputs and outputs, and generally disrupted the economic lives of people in the earthquake zone. The cost of destroyed and damaged private property, infrastructure, and historic sites totalled approximately $7 billion USD, roughly one-third of Nepal's 2015 GDP.

The earthquakes strongly affected the agricultural households that are overwhelmingly represented in this study, many of whom did not resume farming for many months after the earthquake for fear of landslides, and because they were required to spend time and effort constructing temporary homes. In addition, the earthquake interrupted or diverted natural sources of water, and the monsoon in 2015 was weaker than average. Most of the households in the affected areas (and over 90% in our sample) are involved in agriculture. All of these factors combined to weaken the food economy in the rural hill districts of the heavily affected areas. In the months following the earthquake over 1.4 million people required food assistance, including 404,000 children already suffering from malnutrition and 200,000 breast-feeding mothers. International NGOs and the Nepali government moved reasonably quickly to deliver emergency food aid, but the response was incomplete and uneven, with less easily accesible districts remaining underserved. In addition to food aid, affected households typically received in-kind transfers of blankets, tarps, building materials for temporary shelters (corrugated tin, baling wire, etc.), clothing and basic medicines (Willitts-King and Bryant, 2017).

The Nepali government began distributing cash assistance one month after the first earthquake. This included NPR 30,000 (≈USD 300)[2] for funeral costs for those households who lost a member during the earthquake, NPR 15,000 (≈USD 150) for households assigned red cards (fully damaged houses) to build temporary shelters, and NPR 3,000 (≈USD 30) for households assigned yellow cards (partially damaged houses). Cash grant beneficiaries were identified on the basis of damage assessments conducted shortly after the earthquake, generally in an ad-hoc fashion with assistance of VDC leaders.[3] The initial cash grants were distributed throughout the 2015 monsoon, either through VDC-level Relief Distribution Committees (RDCs) or, in areas without RDCs, through VDC administrators. In our sample, households who received early cash assistance received NPR 21,000 (≈USD 210) on average. In several districts, non-governmental organizations were involved in the cash distribution process, working in coordination with or on behalf of the government. The early cash grants were followed late in 2015 by winter relief grants intended to assist victims in purchasing clothing, blankets, and fuel to withstand the cold during the first winter after the earthquakes (Asia Foundation, 2016b).

[2]NPR = Nepali rupees. Throughout this document we exchange rate NPR 100 ≈ 1 USD, a stable and approximate rate throughout the 2014-2016 time frame encompassed by these essays.

[3]Nepal comprises 75 administrative districts, which are further subdivided into village development committees (VDC), with each VDC consisting of nine wards. Wards most closely approximate the popular perception of a 'village'.

In addition to emergency relief the government of Nepal established the Rural Housing Reconstruction Program (RHRP) to assist affected households with the rebuilding their homes. The RHRP provides cash assistance to impacted households to promote 'owner-driven reconstruction', and is conditional on complying with building codes to make homes more earthquake resilient. The total size of the reconstruction grant is NPR 300,000 (≈USD 3,000), released in tranches of NPR 50,000 (≈USD 500), NPR 150,000 (≈USD 1,500), and NPR 100,000 (≈USD 1,000). Eligibility for the RHRP was determined based on a second round of damage assessments conducted in February 2016. In total 533,282 houses were deemed eligible for the grant. The government began disbursing RHRP grants in July 2016, the month after we collected the data used in this study; our survey instrument did not include questions related to RHRP eligibility.

Qualitative studies of household level coping responses to the earthquake suggest that households preferred taking on debt to other coping mechanisms (Asia Foundation, 2016). Generally speaking, borrowing is a preferred coping strategy and appears to be common across affected districts. Given that subsistence farming does not provide cash incomes, and that yields from non-subsistence staple-grain farming can only be sold at harvest times, households are accustomed to borrowing cash from relatives, neighbors, local moneylenders, or microfinance institutions and repaying that money only when crops are sold or remittances are sent by household members working elsewhere. The proliferation of saving and credit groups promoted by the government and NGOs for poverty alleviation and entrepreneurship development has promoted this practice. After the earthquakes, therefore, households under financial stress may have been more likely to borrow than sell assets or using other coping strategies. In general, rural households borrow for various purposes: routine expenses, to finance small businesses and to send migrants overseas. Borrowing from informal sources- particularly friends, family, and village money lenders- at high interest rates is more common than borrowing from banks. Borrowing locally from informal lenders is normally faster and easier than approaching banks that may require formal documents, and informal sources of lending generally offer more flexible repayment and ask for no or little collateral.

2.3 Data

The data used in the empirical analyses in this dissertation will come from three main sources:

i) A series of household surveys conducted before and after the earthquake to measure outcomes affected by the productive asset transfer program described above. Comprehensive household surveys were conducted in 2014 and 2016. Survey modules collected detailed data on income, expenditures, asset holdings, food security and dietary diversity, aspirations, women's empowerment, mental health, physical health, and time use.

ii) Various data sources measuring the severity of the Ghurka earthquake of 2015, measured at the household and community levels. We use the USGS ShakeMap to extract a modified Mercalli score, a method of measuring earthquake severity by grading the degree and extent of observed property damage sustained in a locality. In addition, the 2016 survey round contained a set of questions to capture earthquake damage and intensity.

iii) Administrative sources compiled by the Nepal Central Bureau of Statistics. Specifically, I draw on data from the Nepal census of 2011 for a range of village-level poverty indicators, and a nationally representative sample survey conducted in 2014 to cross-reference and validate data collected household surveys described above.

Specific variables, construction of new variables and indices, treatment of outliers and missing data, methods of imputation, methods for handling survey attrition, and other data-related issues are discussed within each essay.

Chapter 3

Essay One: Short-term impacts of a livestock transfer and training program in rural Nepal

3.1 Introduction

It is often argued the rural poor largely lack access to the productive assets and human capital necessary to be successful entrepreneurs. Productive asset transfer programs, which typically include a training component, are one way non-governmental organizations (NGOs) and governments try to alleviate these constraints thereby facilitating a permanent transition out of poverty. These programs are popular among donors who subscribe to the well-known "teach a man to fish" maxim. In some cases, they also include a "pay it forward" component where recipients share what they have learned and even share some of their newly accumulated wealth, in the form of a productive asset, to other households in need. Rigorous impact evaluations of combined asset transfer and training programs, particularly evaluations designed to measure impacts on indirect beneficiaries, are few and far between.[1]

In this paper we evaluate the short-term (1.5 year) welfare impacts of Heifer International's (HI) Smallholders in Livestock Value Chain Program in rural Nepal using a randomized controlled trial (RCT). Like similar programs, the program targets poor households in rural areas, particularly women, and seeks to provide a sustainable livelihood and a pathway out of poverty for its beneficiaries. The standard intervention in Nepal provides a package of benefits that includes group formation, livestock (in this case two female goats), technical trainings on improved animal management and entrepreneurship, and values-based

[1] Banerjee et al. (2015) test for spillover effects of a program without a "pay it forward" component and find none.

trainings. The values-based training encourages beneficiaries to "pay it forward" by providing technical training and giving the first-born female offspring of their received livestock to another poor individual in their community. As we will show, this program component substantially reduces per-beneficiary costs.

This paper contributes to the literature in three important ways. First, we add to a small but growing body of empirical evidence on the *overall* positive impact of livestock transfer programs worldwide (Bandiera et al., 2017; Banerjee et al., 2015; Darrouzet-Nardi et al., 2016; Jodlowski et al., 2016; Kafle, Winter-Nelson, and Goldsmith, 2016; Miller et al., 2014; Rawlins et al., 2014). Second, our evaluation is carefully designed to estimate the aforementioned "pay it forward" indirect effects on members of the same targeted community who were not initially targeted by the implementing partner. Measuring the strength and persistence of this element of the program design is crucial to understanding the overall program impacts. Third, our evaluation includes three unique treatments in order to unpack the welfare impacts of different program components. In the first treatment arm, beneficiaries received a complete package that included a livestock transfer, skills-based technical trainings and values-based non-technical trainings. In the second treatment arm, beneficiaries received skills-based technical trainings and values-based non-technical trainings, but *not* livestock. In the third treatment arm beneficiaries received a livestock transfer and skills-based technical trainings, but *not* values-based non-technical trainings. To our knowledge, previous studies in this area do not attempt to disaggregate the impacts of a bundled treatment.

Our hypotheses, along with detailed plans for handling the data and analysis, are documented in a registered pre-analysis plan available at `http://www.socialscienceregistry.org/trials/1504`. We estimate intent-to-treat (ITT) effects of the overall program on directly targeted beneficiaries as well as beneficiaries brought into the program through the "pay it forward" (PIF) process, whom we term second generation PIF beneficiaries. For direct beneficiaries, the observed short-term impacts are similar across the different program variations. Financial inclusion increases by between 0.32 and 0.36 standard deviations, and empowerment increases by between 0.21 and 0.25 standard deviations. Exploratory analysis suggests these findings stem from increased saving and membership in savings (or other) groups, increased ownership of productive assets, and increased control over use of income. Perhaps surprisingly, we do not observe major statistically significant differences across treatments for direct beneficiaries. We observe no impact on our summary measure of mental health, but direct beneficiaries have higher life satisfaction and self esteem, and the intervention may reduce worrying. We observe no impact on our summary measure of aspirations, but we do present evidence that direct beneficiaries positively adjust their aspirations for future income.

Unlike for direct beneficiaries, PIF effects differ across treatments in some cases. In particular, without values-based training, the effects on empowerment and financial inclusion are less than half as large and not statistically significant for second generation PIF beneficiaries. However, those who live in the same community as direct beneficiaries receiving values-based trainings, but who were not targeted as direct beneficiaries themselves, experience similar and statistically significant increases in financial inclusion and empowerment as those observed for directly targeted beneficiaries. This suggest the "pay it forward" encouragement (a critical component of the values-based training) helps successfully achieve a broader impact. These results are impressive given the relatively short time horizon over which to observe an impact on second generation PIF beneficiaries.

This paper focuses on short run impacts. We do not observe statistically significant changes in income, asset holdings, or expenditures in the short run. However, the timing of livestock growth and development implies no beneficiaries had additional marketable goats at the time of data collection. In this way, our results compliment those of Karlan et al. (2017), who observe improvements in financial inclusion and women's empowerment from participation in a group-savings program, but no effects on income, assets, consumption, or food security. Because livestock production is a slow process, it is unsurprising these short-term results mirror those from a group-savings intervention.

The rest of the paper is organized as follows: Section 3.2 provides a summary of the related literature and discusses the typical HI livestock transfer and training program. Section 3.3 describes in detail the experimental design and data. In Section 3.4 we present our empirical approach and our main findings of program impacts on our nine primary outcome indices. Section 3.5 provides additional analysis of short-term subindicators and a discussion of program costs. Section 5.6 concludes.

3.2 Background

3.2.1 Evidence on asset transfer and training programs

Asset transfers, particularly livestock, have been conducted in poor countries since at least 1944, when HI sent 17 cows from Arkansas to Puerto Rico. Since then HI has expanded its reach to over 125 countries. Numerous NGOs and even governments have embraced livestock transfer and training programs as a strategy for fighting poverty (World Vision, BRAC, Save the Children, Oxfam, and the Government of Rwanda are a few examples).

Despite the long history and prevalence of livestock transfer and training programs, until recently there was very little rigorous empirical evidence of their effectiveness (DFID 2014). Recent papers have found these programs increase income (Bandiera et al., 2017; Banerjee et al., 2015), expenditures (Bandiera et al., 2017; Banerjee et al., 2015; Jodlowski et al., 2016), savings (Bandiera et al., 2017; Banerjee et al., 2015), overall food consumption (Bandiera et al., 2017; Banerjee et al., 2015; Kafle, Winter-Nelson, and Goldsmith, 2016), dairy and meat consumption (Banerjee et al., 2015; Rawlins et al., 2014), dietary diversity (Darrouzet-Nardi et al., 2016; Jodlowski et al., 2016; Kafle, Winter-Nelson, and Goldsmith, 2016; Rawlins et al., 2014), food security (Bandiera et al., 2017; Banerjee et al., 2015), and anthropometrics (Miller et al., 2014; Rawlins et al., 2014). Evidence of impacts on emotional well-being and women's empowerment have been mixed (Bandiera et al., 2017; Banerjee et al., 2015; Roy et al., 2015).

Most notably, Banerjee et al. (2015) evaluate the impact of BRAC's graduation program, a large asset transfer and training program in six countries. Their study finds after three years the graduation program has significantly positive impacts on consumption, food security, assets, finance, time use, income and mental health, but no indirect effects for non-beneficiaries. They observe positive impacts on women's empowerment in the short but not long run.

Several major differences exist between the graduation program and the one evaluated here. First, beneficiaries of the graduation program chose an asset (or bundle of assets) from a list of productive assets. Although livestock was the most common choice, there were alternative options. The value of the productive asset transfer was always higher than the one evaluated here, and beneficiaries in their study also received a regular transfer of food or cash for a few months or even up to a year. In another significant deviation, beneficiaries of the livestock transfer and training program we study here were encouraged to "pay it forward," as described below. This encouragement is a central component of all livestock transfer and training programs implemented globally by Heifer International (HI). To our knowledge, no study has evaluated the impact of a program with this type of encouragement.

3.2.2 Heifer International's "pay-it-forward" livestock transfer and training program in Nepal

The intervention we evaluate replicates HI's Smallholders in Livestock Value Chain (SLVC) Program in rural Nepal described in section 2.1. Like similar programs, the program targets poor households in rural areas, and seeks to provide a sustainable livelihood and a pathway out of poverty for its primarily women beneficiaries. The standard HI intervention in Nepal provides a package of benefits that includes formation of women's self help and savings groups, technical trainings on improved animal management and entrepreneurship, values-based trainings, a productive asset transfer (in this case goats), and encouragement to "pay it forward".

The process is as follows: After identifying a location to receive the intervention, HI recruits an original group of direct beneficiaries. Direct beneficiary groups typically consist of close neighbors and often include most or all of the households in a given neighborhood. As a rule, HI considers all the households in a targeted area to be objectively poor and therefore eligible for the program, allowing for the possibility that a considerable range of relative wealth and poverty might exist within a group. Once selected, direct beneficiaries within a ward are organized into a self-help group (SHG). Over a period of several months all SHG members participate in a series of trainings. Trainings include (1) technical training on improved animal management, fodder/forage development, entrepreneurship, human and animal nutrition, and home gardening, and (2) HI's values-based training on topics of accountability, sharing and caring, sustainability, self-reliance, income management, environmental stewardship, spirituality, self-help group management, gender justice, and encouragement to pay it forward. The trainings culminate with the beneficiaries receiving a transfer of livestock which includes two doe goats for each beneficiary and a single buck of improved stock (to facilitate a breeding program) for the SHG.

A unique component of HI's model is that it encourages members to "pay it forward" by recruiting additional community members into the program, giving a gift of livestock (of equal value to what was received), and passing down all knowledge that was gained through participation in the programs. HI facilitates values-based empowerment training for both direct and PIF beneficiaries (albeit separately and at different points in time), while all other "pay it forward" trainings are implemented by direct beneficiaries with minimal support from HI. In this way, what might typically be deemed a spillover effect is actually an important program component. The program we evaluate follows an innovation to the basic HI pay-it-forward model, in which each direct beneficiary SHG is tasked with recruiting up to five PIF SHGs, with the goal of full saturation and complete adoption of improved practices and technologies within a community in a relatively short time frame.

3.3 Experimental design and data

3.3.1 Design

To establish a causal relationship between the program and changes in outcomes, this study uses a cluster randomized controlled trial (RCT). A cluster design was employed for two reasons. First, group membership is a key component of the program design. Second, PIF effects are anticipated among a second generation of beneficiaries. As described below, we will seek to estimate both direct and second generation PIF effects.

Nepal comprises 75 districts. Districts are further subdivided into village development committees (VDCs), which can be thought of as groupings of villages within a district. Every VDC is split into nine wards, and each ward might include multiple *toles*, or communities. A typical *tole* in the study area has approximately twenty to thirty households; a typical ward has roughly 150 households.

Nepal-based HI staff first identified 60 VDCs in which they had never worked, but that would be good candidates for an asset transfer and training program. Before assigning treatments, HI also identified a central ward and targeted *tole* within the selected central ward for each of the 60 selected VDCs. The expectation was that if assigned to treatment, everyone residing in the targeted *tole* would be targeted by the program, and therefore likely to enroll as a direct benificiary. Through this process, HI pre-identified all targeted beneficiaries (but not necessarily actual beneficiaries) who were later encouraged to form SHGs. Following treatment assignment, these SHGs formed in treated VDCs but not control VDCs. In this way, the individuals in the control arm are directly comparable with those in the treatment arms.

Although indirect PIF effects are expected, we do not anticipate contamination of the control. To an extent, the isolation of rural communities in Nepal provides a natural impediment to such contamination. This is especially true in the Middle Hills (home to about two-thirds of our sample), where lower population density, rugged terrain, poor roads, and inferior cellular connectivity cause communities to be especially cut off. Nevertheless, communities are linked by family and commercial ties. Fewer natural barriers against contamination exist in the Terai, the densely populated plain along the Indian border where about one third of our sample resides. Apart from naturally occurring geographic and social barriers to contamination, we also buffered treated wards from each other and from control VDCs by selecting the 'central' ward within a VDC to be the targeted ward. In this way, we ensure an additional degree of isolation and further reduce the prospect of unintentional spillovers that could bias results.

To improve balance across treatment and control VDCs (and between the various treatment VDCs) we stratified by geography and caste/ethnic composition. First we divided the sample of VDCs into four pools based on district groupings (Hills (2), Middle Hills (1), and Terai (1)). These clusters contained 15, 15, 10, and 20 VDCs respectively. Using administrative data, we then calculated the proportion of residents in each VDC from each of 39 caste/ethnic groups. Within each district grouping we ordered VDCs by the most prevalent caste/ethnic group, then second most prevalent caste/ethnic group, and so on through the ninth most prevalent caste/ethnic group.[2] This created new groups within the district groupings based on rank prevalence of caste/ethnicity. Within these groups, we ordered VDCs by the proportion of the most prevalent caste/ethnicity, then second most prevalent, and so on. From this ordering we established 16 bins.

Within each stratification bin, we then randomly assigned the 60 VDCs to one of three treatment arms or pure control. All three treatments share some common features. First, HI facilitates the formation of women's SHGs, so all beneficiaries are expected to acquire some level of social capital through group membership and participation. Group members are then encouraged to contribute to group savings accounts with a goal toward increasing financial inclusion. Finally, all beneficiaries are trained on a variety of technical topics including nutri-

[2]Only two of 60 VDCs had more than 9 caste/ethnic groups represented.

tion, home gardening, fodder and forage production, and improved animal management. In addition, all beneficiaries are provided a small amount of cash support for home gardens (approximately $5), fodder/forage production (approximately $10), and goat shed improvement (approximately $40). We'll call these common features the basic intervention.

In order to 'unpack' the benefits of various program components, two additional programmatic elements vary across across treatment arms: a productive asset transfer and additional values-based trainings. The productive asset transfer included two doe goats to each individual beneficiary, as well as a shared buck of improved breeding stock for the self-help group. The values-based trainings cover the HI Cornerstones not included in the basic intervention[3]: passing the gift; accountability; sharing and caring; sustainability and self-reliance; improving the environment; income; genuine need and justice; gender and focus on the family[4]; full participation; training, education, and communication; and spirituality. Notably, the values-based training encourages beneficiaries to "pay it forward" by providing technical training and giving the first-born female offspring of their received livestock to another poor individual in their community.

The treatment arms can be described as follows:

1. *Full Treatment* (FT): basic intervention, values-based training, and a productive asset transfer.

2. *No Goats* (NG): Identical to FT, but without the productive asset transfer.

3. *No Values-based Training* (NVT): Identical to FT, but without values-based training.

A fourth arm was randomly selected as pure control. Table 3.1 summarizes the elements of each treatment arm.

[3]improved animal management and nutrition are also HI Cornerstones, but are included as part of the basic intervention

[4]Notably, both men and women are encouraged to participate in gender and justice training.

Many of the welfare impacts we consider could be directly affected by either type of training or the asset transfer. For example, women's empowerment could increase as a result of interactions in the group, values-based trainings, technical skills trainings, and/or increased ownership over assets through the transfer. Similarly, income could increase as a direct result of any of these program components in concert or independently. Our experimental design allows us to differentiate between program components.

In addition to variation across treatments, we can also look at differential impacts over time to explore causal channels. Consider income: income could increase early on through improved knowledge about fodder development, livestock nutrition, vaccinations, and breeding, and improved shelters. Any of these factors - a direct result of animal management trainings - might lead to better livestock health of one's current livestock herd, such that the program could increase income in the short run. However, if initial herds are small or if improved practices are concentrated toward the transferred livestock only - then income effects would only be observed in the long run, specifically, after the transferred goat has given birth to a male kid, and once that kid is old enough to be sold.

With these inter-temporal effects in mind, Figure 3.1 illustrates a timeline of relevant activities. Project implementation began in mid to late 2014 (depending on location) and continued throughout 2015 and 2016. All direct beneficiaries first formed SHGs (shortly after the baseline survey, as described below) and were encouraged to begin saving at this time. Approximately six months later, between March and June 2015, these same direct beneficiaries received livestock if they were assigned to either the FT or NVT treatments. In late 2015 the second generation of beneficiaries, encouraged by direct beneficiaries in their area, enrolled through the "pay-it-forward" program, began to form groups, and participated in the various trainings. Notice that while we know when program activities for these beneficiaries began, it is difficult to know exactly when second generation "pay-it-forward" beneficiaries will receive livestock transfers, because such transfers depend on livestock fertility, which is inherently random. In fact, the program is designed in such a way that the "pay-it-forward" livestock transfer will be staggered, with some receiving livestock transfers within six months of enrolling in the program, while others will wait years before receiving a livestock transfer.

For establishing hypotheses regarding the anticipated timing of impacts, we must carefully consider livestock fertility cycles. We assume a doe can reasonably be impregnated within any given four month window, a five month gestation period, and offspring reach sexual maturity at around seven months (females) or an optimally marketable size at around ten months (males). Depending on breeding cycles and the availability of an improved buck, most direct beneficiaries might be expected to impregnate their does between June and October of 2015, implying the members of a second generation of program goats were typically born near the end of that year and the beginning of 2016. Goats normally experience single births (although multiples aren't uncommon), and the gender of the kid has important implications for impact. The program requires beneficiaries to donate their firstborn female offspring (once it has reached sexual maturity) to another beneficiary through the pay-it-forward mechanism. Alternatively, male kids are expected to be sold on the market.

Taken together these facts imply three noteworthy features of this study, all shown on Figure 3.1: (1) the earliest pay-it-forward beneficiaries could possibly have received goats was in mid 2016, (2) the earliest possible goat sales (of transferred goat kids) for direct beneficiaries will have taken place in late 2016, and (3) the earliest possible goat sales (of transferred goat kids) for second generation pay-it-forward beneficiaries will take place in early 2018. In section 3.4.4, we will return to these features to explain some of our findings. But first, we link the experimental design to the data collected and discuss our empirical approach.

We collected panel household survey data from rural women eligible to participate in the program across three regions of rural Nepal in June-September 2014 and 2016. The main data used for this analysis was collected in June-July 2016, approximately 1.5 years after initial enrollment in the program. Figure 3.1 shows how the survey timeline fits with program implementation, including surveys planned for future data analysis.

There are two types of respondents in the sample used for this analysis: targeted direct beneficiaries and prospective PIF beneficiaries. Specifically, our sample of targeted direct beneficiaries consists of all households in each of the targeted *toles* (around 25 per ward). In addition, after removing households from the targeted *tole*, we selected a random sample of 15 potential PIF beneficiaries from a complete roster of all households in the central ward. Because of the aggressive nature of the "pay-it-forward" encouragement, we expect that many (if not most) of these households will actually become PIF beneficiaries. Although no intervention took place in control VDCs, sampling in these VDCS occurred in exactly the same manner as in treatment VDCs: 25 individuals from pre-determined targeted *toles*, and 15 individuals from a complete roster of all households in the central ward.

Our total baseline sample is 2,376 women, including 1,286 targeted for direct treatment, and 1,089 households from the central ward likely to enter the program through the pay-it-forward mechanism. Shortly after HI delivered training and livestock to the original beneficiaries of the project, a devastating earthquake struck Nepal. The earthquake greatly affected the 10 VDCs belonging to the 'Middle Hills' stratification pool, and were therefore spread evenly across treatment groups and control. We made the decision to drop these from the RCT so that HI could provide earthquake relief in whatever manner they deemed appropriate. Following additional attrition not explicitly related to the earthquake, the remaining sample consists of 50 VDCs and 1,828 households, including 1,031 from targeted *toles* and 797 from the central ward more broadly.

3.3.2 Defining outcomes

We consider nine outcomes of interest, which we categorize as either short-term or long-term. Short-term outcomes are those we expect to change within the first 1.5 years of the program: women's empowerment, financial inclusion, aspirations, and mental health. Long-term outcomes are those less likely to change immediately given the reproductive and marketing cycle for goats: assets, income, expenditures, physical health and food security. Multiple subindicators for each outcome dimension exist. Subindicators are described and summarized alongside our results in section 3.4 (and in footnotes corresponding to the relevant results table). In almost all cases variables have been coded so that larger values are 'better', therefore positive regression coefficients represent improvements.

These subindicators are then aggregated into a primary summary index for each dimension of welfare. These summary indices will be employed as our nine primary outcomes of interest. For empowerment we utilize the Five Domains of Empowerment (5DE) of the Abbreviated Women's Empowerment in Agriculture Index (A-WEAI) modified to the local context (Alkire et al., 2012; Malapit et al., 2015). For other non-monetary measures we use a standardized weighted average as described by Anderson (2008). For income and expenditures we use logged Nepali rupee totals.

3.3.3 Baseline characteristics and balance

We test for balance on observable characteristics between all treated and control groups separately for each subpopulation of interest: direct and potential PIF beneficiaries. Specifically, we estimate:

$$y_{hv}^{t=0} = \beta_0 + \beta_1 T1_{hv} + \beta_2 T2_{hv} + \beta_3 T3_{hv} + \varepsilon_{hv} \tag{3.1}$$

Here, $y_{hv}^{t=0}$ is a demographic characteristic or outcome of interest for household h residing in village development committee (VDC) v as measured at baseline ($t = 0$). Three binary treatment variables take a value of 1 for households assigned to any of the three intervention packages, and a value of zero otherwise. $T1_{hv} = 1$ for households slated to receive the full package of benefits, $T2_{hv} = 1$ for households selected to receive the no-goats package, and $T3_{hv} = 1$ for households offered the no-values-based-training treatment. ε_{hv} is an idiosyncratic error term clustered at the VDC level. The omitted category is control households, therefore, β_1, β_2 and β_3 represent the average differences at baseline between FT-eligible households, NG-eligible households, NVT-eligible households (respectively) and controls.

We report balance on demographic characteristics in table 3.2 and on levels of outcome summary indices in table 3.3. In both tables column 1 presents the control means at baseline. Columns 2-4 contain the regression coefficients on the treatment variables from equation 3.1. Panel A presents results for the direct beneficiary subsample (DIR), while panel B presents analogous results for the potential pay-it-forward subsample (PIF). Overall these balance checks suggest a successful randomization. Table 3.2 shows only scattered and seemingly non-systematic differences across the various treatment arms and control. We observe some differences with respect to household size (DIR beneficiaries only), age (both DIR and PIF beneficiaries) and land holdings (PIF beneficiaries only). It's worth noting that none of these differences remain significant at the 10% level after adjusting for multiple inference. We do not expect the household size or age differences to have a material impact on outcomes, and therefore do not control for them in the main econometric specifications. Additional land holdings may impact outcomes, so we control for this in all PIF specifications.[5]

Table 3.3 presents the results of balance tests for each of the summary indices. We do find some evidence of systematically 'worse' baseline outcomes among FT households. The financial inclusion, aspirations, and physical health indices are all lower among DIR beneficiaries; aspirations and food security are lower among PIFs. To the extent that baseline levels of one indicator may affect treatment effects on another, it is appropriate to control for that variable in the estimation of treatment effects (e.g., if aspirations affect financial inclusion, one should control for imbalanced aspirations when financial inclusion is the outcome of interest).

[5] Appendix section A also presents balance on caste/ethnicity. The table confirms moderate balance of treatments across caste and ethnic lines. The sole exception appears to be that Chheriyas (a subset of high-caste Hindus) are somewhat under-represented in direct-target controls; we therefore include a dummy variable for membership in the Chheriya caste in all specifications for directly-targeted beneficiaries.

3.3.4 Attrition

Ignoring the sample purposefully removed following the earthquake, we observed 7.8% attrition (154/1982 households in the post-earthquake sample) between 2014 and 2016. To assess whether the observed attrition is systematic in a way that might bias our results, we employ an approach similar to Haushofer and Shapiro (2016), and adapted to our design. We first consider the full sample, but we also assess attrition separately for direct beneficiaries and those entering the program through the PIF mechanism. Specifically, we estimate whether attrition rates differ across treatment types and control households, and do not observe any significant treatment effects on attrition status (results reported in appendix table A.8). Next, we assess whether attrition rates differ across households with respect to a set of baseline characteristics. While we do find scattered individual cases where attrition status correlates with a baseline characteristic, these instances do not appear to be systematic or to threaten the integrity of our results (results reported in appendix tables A.9 and A.10).

3.4 Analysis

Our main research questions are: (i) what are the short-term welfare impacts of a productive asset transfer and training program? (ii) are all program components necessary for achieving impact? (iii) within a treated village, do treatment effects spillover to subsequent generations of beneficiaries? and (iv) which package of benefits results in the most cost-effective improvements to household and individual well-being in the short-term? We will address questions (i)-(iii) in this and the following section. Section 3.5.2 presents our preliminary analysis of question (iv).

Our specific hypotheses, along with detailed plans for handling the data and analysis, are documented in a registered pre-analysis plan prepared before any analysis took place. In some instances we deviate from this plan, and will specify when this is the case. Notable deviations are discussed in appendix A.

3.4.1 Recruitment rates

Before discussing welfare impacts, it is useful to look at participation rates. Table 3.4 presents the results of a regression of stated SHG membership on assigned treatment status. Among targeted direct beneficiaries, 90.1% of households offered the FT report belonging to a Heifer group. Directly targeted NG and NVT beneficiaries report lower (but still high) participation rates of 80.9% and 77.2%, respectively. As expected, potential PIFs participate at lower rates: 80.5% in the FT, 62.6% in the NG treatment arm, and 23.1% in the NVT treatment. All pairwise comparisons reject standard t-tests for equality of means at a significance level of $\alpha = 0.01$, with the exception of the the comparison of NG and NVT ($H_0 : NG = NVT$) for targeted direct beneficiaries.

These findings are helpful for interpreting our primary results. First, recruiting and retention of direct beneficiaries improves if goats and, or values-based training is included in the package of program benefits. One might hypothesize intuitively that goats help attract beneficiaries, while values-based training contributes to the formation of more cohesive groups with better retention. A similar pattern holds among potential pay-it-forward beneficiaries, but with more pronounced differences. Second, in the NVT treatment arm, PIF beneficiary recruitment is much lower at only 23%. This difference suggests that the encouragement to pay-it-forward, a key component of the values-based training, is vital to the self-propagating nature of the HI intervention. This may explain why studies of other similar programs have not observed spillover effects. Without incorporating spillovers into the program design, others are much less likely to participate.

3.4.2 Empirical approach

We now present our empirical approach for analyzing the welfare impacts of a productive asset transfer and training program. We estimate the intent to treat (ITT) impact for each of the three treatment groups relative to a common control, noting that the recruitment rates described by Table 3.4 suggest ITT effects estimated in Equation 3.2 are likely to be conservative.

To analyze whether treatment effects spillover to subsequent generations of beneficiaries, these effects are estimated separately for two subsamples: direct and PIF beneficiaries. In the analysis of direct treatment effects, the sample consists of those pre-selected for direct benefits (even those in control areas), so that the comparison group is those who would have been targeted for direct benefits had they been selected for treatment. In the analysis of PIF treatment effects, the sample consists of those pre-selected for possible second-generation benefits (even those in control areas), and the comparison group is those who would have been targeted only as second generation beneficiaries had they been selected for treatment. We note that PIF effects could arise through trainings, asset transfers, or through simple observation and replication. If households observe and replicate the behavior of direct beneficiaries, they may benefit indirectly from the HI trainings, even if they don't identify as a second generation program beneficiary. Because this second type of spillover effect is possible, estimation of local average treatment effects (LATE) is not preferable. We therefore estimate ITT effects, again noting they are likely conservative.

For each subpopulation we estimate:

$$y_{hv}^{t=1} = \beta_0 + \beta_1 T1_{hv} + \beta_2 T2_{hv} + \beta_3 T3_{hv} + \delta y_{hv}^{t=0} + \mathbf{X'_{hv}}\gamma + \mathbf{S'_{vb}}\rho + \varepsilon_{hv} \tag{3.2}$$

In equation 3.2, $y_{hv}^{t=1}$ is the outcome of interest for household h in village v, measured approximately one and a half years after the intervention ($t = 1$). As in equation 3.1, treatment indicator variables differ by the type of treatment: $T1_{hv}$, $T2_{hv}$, and $T3_{hv}$ each take a value of 1 for targeted households in wards selected to receive a particular type of treatment, and a value of 0 otherwise. Controls include the outcome of interest measured at baseline ($y_{hv}^{t=0}$), a vector of any relevant covariates imbalanced at baseline ($\mathbf{X'_{hv}}$), and a vector of stratification bin dummy variables ($\mathbf{S'_{vb}}$). For each subsample (direct or PIF) used in the estimation, β_1 represents the ITT (direct or PIF) treatment effect on households selected to receive the full treatment package (FT), β_2 identifies the ITT effect on households selected to receive the no-goats package (NG), and β_3 identifies the ITT effect on households selected to receive the no-values-based training treatment package (NVT). The counterfactual is targeted (direct or PIF) beneficiaries located in pure control areas. To test whether the treatments effects vary across treatment type for a given subsample, we conduct Wald tests for $\beta_1 = \beta_2$, $\beta_1 = \beta_3$, and $\beta_2 = \beta_3$.

We follow the emerging standard in the program evaluation literature by accounting for multiple hypotheses in two ways. First, the summary index for each welfare dimension consolidates several individual tests into a single test. Second, because we still have multiple outcome dimensions, we report naive p-values and adjusted q-values that control for the false discovery rate (FDR). Specifically, we calculate q-values for multiple hypothesis tests separately across short-run summary indices and long-term summary indices, but not across treatments, using the Benjamini and Hochberg (1995) step-up method outlined in Anderson (2008) and applied by Banerjee et al. (2015). We will report both naive p-values and q-values that control for FDR for all Wald tests of differences across treatments. When estimating treatment effects on sub-indicators (rather than summary indices) we will report naive p-values. We test for the impact on sub-indicators primarily to identify the mechanism behind impact (or lack thereof) observed for the summary indices. We therefore consider this analysis exploratory, and take a less stringent approach to hypothesis testing.

3.4.3 Short-term welfare impacts

Our main results are presented in table 3.5. Panel A shows ITT direct effects for our nine summary indices, arranged so that the four short-run outcomes are listed first. Column 1 contains control group means, and columns 2-4 contain ITT estimates for the each treatment (β_1, β_2, and β_3 in equation 3.2). Finally, columns 5-7 presents Wald tests for equal treatment effects. Panel B presents analogous pay-it-forward impacts.

For direct beneficiaries, the observed short-term impacts are similar across the different program variations. Financial inclusion increases by between 0.29 and 0.34 standard deviations, and empowerment increases by .043 and .048 points, which is equivalent to a 0.21-0.25 standard deviation increase. Accounting for the false discovery rate (FDR) across short-term outcomes, five of six estimates with p-values less than 0.1 also have q-values less than 0.1. In general we do not observe statistically significant differences in outcomes across treatments. This suggests either that our analysis could not capture small differences between treatments or that the combination of activities is not critical for increased empowerment or financial inclusion in the short run.

Panel B of table 3.5 summarizes ITT pay-it-forward impacts. As with the direct effects, we see significant increases in the financial inclusion index and the empowerment index. Among PIF beneficiaries in treatments with values-based training (FT and NG), we observe effects on women's empowerment at least as large as for direct beneficiaries. These results are impressive given the relatively short time horizon over which to observe pay-it-forward impacts. At the time of data collection the majority of PIF beneficiaries had formed SHGs and commenced training, but most had not yet received an asset transfer. However, without values-based training (NVT), the effects are less than half as large and not statistically significant. Treatments with the values-based training also have a positive impact on financial inclusion; with goats (FT) this effect is considerably smaller than for direct beneficiaries, and without goats (NG) it is the same size. As with empowerment, in the absence of values-based training (NVT) the financial inclusion effect is smaller and not significant. Accounting for the FDR, three of four estimates with p-values less than 0.1 have q-values less than 0.1.

3.4.4 Long-term welfare impacts

We do not observe changes in asset holdings, income or non-food expenditures for direct beneficiaries of the program. Recall that the size of transfer is much smaller than other similar programs (such as BRAC's graduation program evaluated by Banerjee et al. (2015)) and the time spent in the program is also shorter. It seems the asset transfer alone is not large enough to significantly alter total asset holdings (which includes livestock).[6] We note the timing of livestock growth and development implies no beneficiaries had additional marketable goats at the time of data collection. Income could increase in the short run through other mechanisms, but these results suggest this is not the case. A lack of impact on non-food expenditures is consistent with a finding that income has not increased during this time frame. We also do not observe changes in food security (as measured by a summary index of several food security and dietary diversity outcomes) or physical health.

In this way, our results compliment those of Karlan et al. (2017), who observe improvements in financial inclusion and women's empowerment from participation in a group-savings program, but no effects on income, assets, consumption, or food security. Because livestock production and subsequent impacts take time to develop, it is unsurprising that short-term impacts mirror those from a group-savings intervention.

3.5 Discussion

We find that a HI goat transfer and training program in Nepal significantly increases financial inclusion and women's empowerment after 1.5 years. To interpret the magnitude of this impact, we can consider what it means for a typical direct beneficiary with a median level of financial inclusion or empowerment as measured by our index at baseline. With respect to financial inclusion, access to the program moves this median individual to the 61st percentile in the distribution of financial inclusion indices. The impact on empowerment is the equivalent of moving her to the 65th percentile. These effects are similar for direct beneficiaries

[6]Table A.2 in the Appendix reports sensible (yet in some cases statistically insignificant) dynamics with respect to livestock. Direct and PIF beneficiaries who were eligible to receive two free goats under the FT and NVT treatments increase herds by 0.2-0.4 TLUs. Although statistically insignificant, a 0.2 increase in TLUs equals an addition of two goats. Therefore, these point estimates are broadly consistent with the magnitude of the transfers received by FT and NVT beneficiaries, plus a reasonable herd growth rate. Alternatively, among direct and PIF beneficiaries alike, we observe point estimates near zero for the NG treatment effect on livestock holdings. Despite extensive trainings on livestock management, these beneficiaries do not choose to invest more in livestock.

and those brought into the program via its PIF component with the one exception that the values-based training seems to be essential for the transmission of second generation PIF impacts.

These findings are consistent with a narrative that, a little more than one year after the intervention, program beneficiaries show improved levels of welfare outcomes that we might reasonably expect to respond to treatment in the short-term. Financial inclusion and empowerment may lay the groundwork for the ultimate intended program outcomes of improved asset holdings, and income, and subsequent analysis will test for whether these long run impacts are actually observed. Furthermore, the pay-it-forward program component seems to rapidly increase the number of households benefitting from the program.

3.5.1 Subindicators and mechanisms

Empowerment

An important short-term goal of the program is to increase women's empowerment. We employ indicators (modified to the local context) from the Five Domains of Empowerment (5DE) of the Abbreviated Women's Empowerment in Agriculture Index (A-WEAI) to calculate an empowerment score for all women in the sample (Alkire et al., 2012; Malapit et al., 2015). The index aggregates an empowerment score across decisions about production, access to and decision-making power over productive resources, control over credit and income, leadership in the community and time allocation. Each binary subindicator equals one if the respondent achieves adequate empowerment in that domain, and zero otherwise. Weights and definitions of adequacy are based on the A-WEAI, but adjusted to the local context.[7]

[7] Specifically, we use the following weights and definitions of adequacy: A respondent is adequately empowered in production decisions if she has at least some input into at least one production decision (weight = 1/5). Adequate ownership means the household owns at least one asset, and the respondent (individually) has at least some ownership of one asset (weight = 2/15). A respondent is adequately empowered in access to and control over credit if the household has at least some credit and the respondent participated to any extent in the decision to borrow (weight = 1/15).

Using this measure, we see an increase of 0.21 - 0.25 SD in empowerment among direct beneficiaries (table 3.6, panel A). Looking across the various indicators of the A-WEAI, we find a statistically significant average impact in two subindicators across all treatment types. Compared to women in the control group, direct beneficiaries are on average 4 percentage points more likely to own productive assets and 14-17 percentage points more likely to belong to a group. Although the majority of women (65 percent) are already in some kind of group at baseline (for example, a mother's group or savings group), this latter impact demonstrates the importance of group formation for a significant number of women (14-17%) for whom social capital gains have the most potential.

These results are fairly consistent across treatments. Notably, beneficiaries in the NG treatment arm did not receive livestock and yet they exhibit increased asset ownership of equal magnitude to other treatments, illustrating how this impact is not driven exclusively through the asset transfer. Interestingly, empowerment over production decisions does not increase as much (or significantly) in the NG treatment group, whereas control over income only increases significantly in the NG treatment group, and by a larger amount than other treatments.

Since livestock rearing is labor-intensive, we might be concerned the intervention increases time spent working in a way that harms welfare. The A-WEAI uses a detailed accounting of time use to calculate the number of hours spent working (as opposed to leisure or sleeping) on a typical day. Women are considered adequately empowered if they work less than 10.5 hours per day. Table 3.6 shows no impact (positive or negative) of the intervention in this domain. A more detailed analysis of impacts on time allocated to various activities (reported in Table A.1 of the Appendix) also reveals no impact. In the short-term, there is no evidence

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Adequacy in control over income means conditional on the household participating in an income-generating activity or expenditure, the respondent participates in decisions regarding at least one non-essential activity or expenditure (weight = 1/5). A respondent is adequately empowered in group membership if she is a member of any group (weight = 1/5). A respondent is adequately empowered for time use if she worked 10.5 hours or less in the previous 24 hours (weight = 1/5).

that women work more as a result of the intervention, or even that they spend more time on livestock-rearing.

With the exception of NVT beneficiaries (who show no increase using the summary index and weaker impacts across subindicators), the intervention increases empowerment among PIF beneficiaries in a similar manner as it does for direct beneficiaries (table 3.6, panel B). If anything, the results are larger and statistically stronger for those joining the program through the PIF program component. In particular, PIF beneficiaries demonstrate increases in input regarding production decisions, asset ownership, and group membership. There's also some evidence that PIF beneficiaries exhibit increased access to and control over decisions about income and credit.

The pre-specified empowerment index derived from the A-WEAI was selected for it's ability to measure empowerment, agency, and inclusion of women in the agriculture sector. The index was developed based on pilot surveys conducted in three countries through extensive collaboration between the United States Agency for International Development, the International Food Policy Research Institute and Oxford Poverty and Human Development Initiative. One criticism of the index is the use of pre-specified weights. Table A.7 of the appendix tests the sensitivity of our results to the use of an alternative index: a standardized weighted average as described by Anderson (2008), similar to the indices we employed for other non-monetary outcomes. The results are consistent, and even strengthened in the case of PIF beneficiaries. Notably, the PIF NVT impact is actually stronger (and statistically significant) using this measure, which could indicate important spillover effects based on observation and replication (a traditional spillover effect) that goes beyond the PIF program mechanism.

A second criticism of the empowerment index is that the results could be driven by group membership, which is an indicator the program is being successfully implemented, but does not necessarily indicate rising levels of empowerment. Table A.7 of the appendix also tests the sensitivity of our results to the use of an alternative Anderson (2008) index in which group membership is excluded as a subindicator. The estimates of direct impacts remain positive but are smaller across all three treatments, and no longer significant for direct NG and NVT beneficiaries. However, PIF impacts remain significant and are even stronger.

Financial inclusion

We observe a statistically significant 0.29 - 0.34 standard deviation impact on financial inclusion for direct beneficiaries. Table 3.7, panel A shows this effect is primarily driven by saving. Membership in a savings group is a requirement of the basic intervention, and we observe membership rates increasing by 14-19 percentage points on average across treatment types. But Table 3.7 shows this effect goes beyond simple membership. Those receiving the values-based training (FT and NG treatments) dramatically increase savings by 71-106%, whereas the effect is not significant for NVT direct beneficiaries. Although encouragement to save is included in the basic intervention, it seems the values-based training may have increased the effectiveness of the savings encouragement possibly through acquired social capital among group members or additional training on income management (one of the HI Cornerstones).

Although not significant, the signs on the coefficient for the amount owed to formal lenders (including a bank, development bank, cooperative, finance company, microfinance institution, or savings/credit group) and the amount owed to informal lenders (including a family member, friend, debt to a local shop, or village money lender) suggests a potential shift toward accessing formal credit markets. A statistically significant decrease of 2.3 percent in the discount rate of beneficiaries receiving the full treatment suggests an increased value on future well-being. The sign on the coefficient estimating the impact on the discount rate is consistent across treatments, but not statistically significant for NG or NVT beneficiaries.

Counter to this finding, however, we observe no increase in the length of time individuals plan ahead.[8]

Shifting to PIF impacts, panel B of table 3.7 reveals a statistically significant 0.17-0.38 standard deviation increase in financial inclusion for PIF beneficiaries receiving the values-based training. As previously noted, NVT direct beneficiaries fail to pay-it-forward, and this is reflected in the lack of PIF impact in the NVT treatment. Subindicator analysis tells a similar story to direct impacts, savings group membership rates increase by 14-18% while the amount saved increases by 9-64% among FT and NG PIF beneficiaries respectively (although insignificantly so for FT). No impact on savings-related outcomes are observed among NVT beneficiaries. The values-based training appears to be very important for savings group formation and functionality for second generation beneficiaries.

Similar to the empowerment index, a criticism could be made that the financial inclusion results could be driven by group membership, which may reflect successful program implementation but does not necessarily indicate greater financial inclusion. Table A.7 of the appendix tests the sensitivity of our financial inclusion results to the use of an alternative Anderson (2008) index in which savings group membership is excluded as a subindicator. The results suggest the estimated impacts on financial inclusion are robust to the exclusion of the subindicator.

Aspirations

A stated goal of the intervention is to alter individual aspirations and increase hope of a better future. We observe no change in the summary aspirations index (table 3.8). To construct the index, the survey follows Bernard and Taffesse (2014) and asks about aspirations across income, assets, social status and education, and then (in a deviation from

[8]The "planning horizon" variable is an ordered categorical variable following Laajaj (2017) indicating how far individuals plan ahead where (0) = Do not plan ahead, (1) = plan ahead one week, (2) = plan ahead one month, and (3) = plan ahead 6 months.

Bernard and Taffesse (2014) which asks respondents to assign weights) each category is assigned a weight using (Anderson, 2008).[9]

Notably, we do observe an increase in (logged) income aspirations, the category of aspirations most directly targeted by the intervention, for direct beneficiaries (panel A). The impact is not statistically distinguishable across treatments (although it is not statistically significant for NVT beneficiaries), and dampened for PIF households. In a separate analysis focused on the formation and failure of aspirations (Janzen et al., 2017), we argue that income aspirations are a better measure of financially-related aspirations than asset aspirations in this context, given the way the survey questions were written. The latter asks only about the "value of home and land" which is an incomplete measure of assets and wealth in this context, and likely leads to substantial measurement error. We do not observe an impact on aspirations regarding social status, although the coefficient estimates are consistently positive. Here again, measurement error is a concern; the survey question used to quantify aspirations in this category asks, "In the future, how many women in this ward do you think might actually seek your advice?" Feedback from the survey team raises concerns that this is not an appropriate or comprehensible measure for the local context. We also do not observe any impact on aspirations for education (measured by the response to "What level of education would you like your children to achieve" in general, and disaggregated by gender) of one's children, but we note the program does not directly target improvements in child well-being or education.

Mental health

[9]Following Bernard and Taffesse (2014), the questions related to income aspirations are: (1) "What is the maximum level of income that a person in your community might expect to earn in a year?" (2) "What is the minimum level of income that a person in your community might expect to earn in a year?" (3) "What is your present personal level of income?" and (4) 'What level of yearly income do you personally think you might be able to achieve in the future?" The first two questions are intended to help respondents delineate a realistic range before stating their own current status and their aspirations. The third question records the personal status for that dimension. The fourth question is interpreted as the individual's aspiration. A similar series of questions are asked to assess aspirations in the other three categories.

We also see no increase in the mental health index, and point estimates are close to zero. However, we do find some significant effects on individual components of the index (table 3.9), which includes a measure of depression, locus of control, optimism, life satisfaction, self esteem, happiness, worries, and trust.[10] The point estimate for the life satisfaction effect is positive for all treatments, but only significant (and larger, 0.5 points on a ten point scale) for those receiving the NG package. Self-esteem increases by 0.2-0.3 points on an 18 point scale for FT and NVT treatments. We also find an increase in the worry score (which indicates *less* worrying) for the FT group (0.5 points on a 16 point scale). Given the treatment and the effects we find in other areas, we are surprised at the lack of overall effect (and inconsistency across treatments) on mental health.

As was the case for directly targeted households, we see no significant change in the mental health summary index for potential pay-it-forward households (table 3.9, panel B). There's some evidence that FT PIF beneficiaries are less worried, however the observed direct impacts on life satisfaction and self esteem do not exist among PIF beneficiaries.

3.5.2 Program costs

A common criticism leveled against asset transfer and training programs is that they may not be cost-effective when compared with alternatives like unconditional cash transfers. Successfully implemented, the PIF model may mitigate such concerns. We collected detailed cost data on all program activities in each treatment arm, and these amounts can be apportioned to direct and pay-it-forward beneficiaries with a few reasonable assumptions. Cost per

[10]Depression is an abbreviated version of the CES-D scale Radloff (1977) with a high value indicating high levels of depression. Locus of control is an abbreviated Rotter (1966) scale where a high value indicates a stronger internal locus of control. Worries questions employ the Penn. State worries questionnaire, and a high value indicates *less* worried. Remaining subindicators are based on aggregated responses to 3-4 questions per subindicator from the 2009 World Values Survey wor (2009), where high values indicate positive welfare (high optimism, more satisfied, high self esteem, happy, and more trusting). A more detailed description of how subindicators are calculated is provided in the pre-analysis plan with a list of specific questions available upon request.

beneficiary in our sample varies by treatment arm as well as direct/pay-it-forward status. Some program costs are common or shared across all treatments. All three treatment types receive the same human capital and technical trainings, for instance, and much of the NGO overhead and administrative expenses are spread evenly across all SHGs regardless of treatment status. Other costs are not incurred at all in certain treatment arms: the NG treatment arm incurs no costs for livestock, while NVT incurs no costs for values-based trainings. In addition to differential costs across treatment arms, recall that direct beneficiaries take on the responsibility for paying it forward - sharing livestock and knowledge to second generation beneficiaries. While HI does conduct some trainings directly, and does provide a limited amount of backstopping to ensure the quality and completeness of the pay-it-forward process, direct beneficiaries shoulder much of the costs associated with pay-it-forward beneficiaries. Therefore, any analysis of the costs of the treatment effects presented here must take into account both these dimensions of heterogeneous costs.

We present costs per beneficiary broken down by treatment arm and direct/pay-it-forward status in table 3.10. We tabulate costs associated exclusively with direct beneficiaries in the panel A. Panel B includes HI's costs for provision of benefits to pay-it-forward beneficiaries. With no discernible monetary benefits associated with the short-run welfare gains from the program we cannot make meaningful cost-benefit calculations at this point, but we do note that cost per beneficiary for this program is low: direct beneficiary costs were approximately \$392 USD per household in the full treatment, whereas PIF costs were \$82 USD per household. The average across all fully-treated beneficiaries was \$120. These per-beneficiary costs are considerably lower than those studied in larger productive asset (Banerjee et al., 2015) and cash transfer (Haushofer and Shapiro, 2016) programs.

3.6 Conclusions

In this study we evaluate the short-term impacts of HI's livestock transfer and training program in Nepal using an RCT. We find that in just over one year women beneficiaries are more empowered and connected to financial markets. Our analysis suggests these findings stem from increased saving and membership in savings (or other) groups, increased ownership of productive assets, and increased control over use of income. These impacts are observed not only among households who received livestock and/or training directly from the program, but also for those brought into the program through encouragement to "pay it forward," where other women in the same village are recruited, trained and eventually given livestock by initial Heifer beneficiaries. These findings, combined with the cost analysis presented in section 3.5.2, demonstrate how encouragement to "pay-it-forward" can help achieve a broader impact, at least in terms of women's empowerment and improved access to finance, at low cost.

Our subindicator analysis presented in section 3.5 reveals additional positive impacts that are worth highlighting. We observe no impact on our summary measure of mental health, but direct beneficiaries have higher life satisfaction and self esteem, and there is some evidence the complete multifaceted intervention reduces the time beneficiaries spend worrying. We also present evidence of increased aspirations for future income among direct beneficiaries.

We do not observe statistically significant changes to the longer-run outcomes of income, assets (although herds do increase by approximately 2 goats as expected), and expenditures, but it may be too early to observe these effects. In future work we will measure the strength and persistence of program impacts and the cost effectiveness of the program disaggregated across program components. Measuring the strength and persistence of effects is crucial to understanding the full program impacts.

The reported ITT effects depend on both the magnitude of the impact for those who take up the program and the recruitment rate. We observe substantial differences in program uptake across treatment arms, as well as between directly targeted and potential PIF beneficiaries. Among those directly targeted 90.1% (FT), 80.9% (NG), and 77.2% (NVT) reported participation. These rates reflect effective recruitment and retention, and suggest goats contribute to the program's appeal. Among potential PIFs 80.1% (FT), 62.6% (NG), and 23.1% (NVT) report program participation. The relatively low recruitment rate among potential PIFs in the NVT treatment without the PIF component demonstrate the importance of this aspect of the intervention for its self-propagating nature.

Although this planned future work is important, we think the current analysis makes an important contribution of its own. Evaluating short-term impact contributes to an understanding of causal mechanisms. This paper demonstrates how a multifaceted social protection program that combines trainings with an asset transfer has immediate effects. However, the paper also provides suggestive evidence that not all program components are necessary for achieving short-term impacts. Similar impacts are observed among direct beneficiaries who were not allocated an asset transfer or who didn't receive the program's "values-based trainings." Although we cannot yet say if either program component is necessary for achieving long-run impacts (including the important long-run outcomes of income, assets and expenditures), we can say confidently that the basic intervention is sufficient for achieving financial inclusion or empowerment in the short-run. This finding supports evidence provided by Karlan et al. (2017) demonstrating women savings groups in Ghana, Malawi and Uganda increase women's empowerment and financial inclusion over a similar time period, even if they don't affect income, assets, or expenditures.

That being said, these supplemental program components (beyond the basic intervention) may be important for achieving a broader impact through "paying-it-forward," as both components increase recruitment of second generation beneficiaries. The HI values-based training, which explicitly encourages beneficiaries to "pay-it-forward," seems particularly critical for the transmission of PIF impacts. Although "pay-it-forward" is a well-known concept - particularly popular during the holiday season in developed countries with widely publicized examples of paying for a stranger's coffee or leaving an unfathomably large tip at a restaurant - it is rarely incorporated into the design of social protection programs. Yet it could be, and our analysis suggests this unique program component could be an important and cost-effective tool for achieving program goals.

Table 3.1: Description of program components by treatment arm

Description of Program Components	T1 (FT)	T2 (NG)	T3 (NVT)
Basic intervention	x	x	x
SHG formation			
SHG savings encouragement			
training on nutrition			
training on improved animal management			
training and cash support ($5) for home gardening			
training and cash support ($10) for fodder & forage production			
cash support ($40) for goat shed improvement			
access to community animal health worker			
Productive asset transfer	x		x
2 doe goats			
1 shared buck of improved breeding stock (per SHG)			
Values-based trainings	x	x	
encouragement to "pay-it-forward"			
training on SHG management			
training on gender and justice			
*training on remaining HI Cornerstones**			

*The remaining HI Cornerstones not noted elsewhere in this table include: accountability; sharing and caring; sustainability and self-reliance; improving the environment; income; full participation; training, education, and communication; and spirituality.

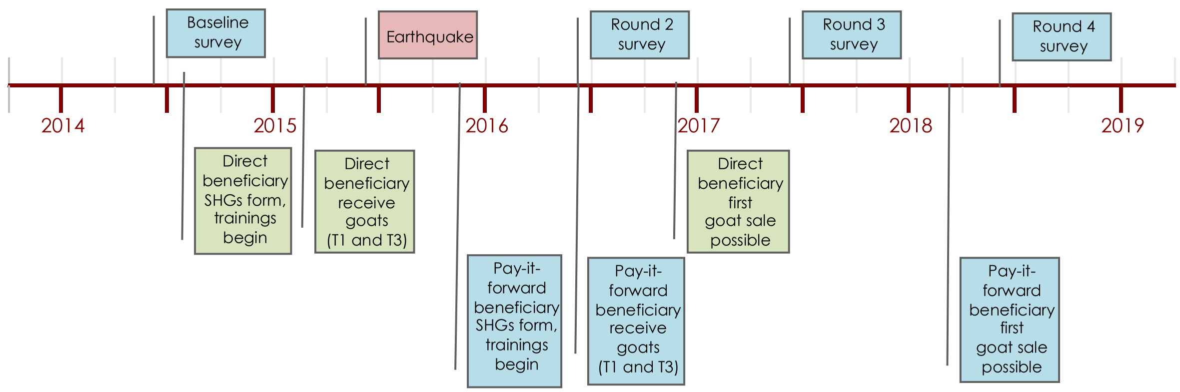

Figure 3.1: Study timeline

Table 3.2: Baseline balance: demographic

	Control mean	FT	NG	NVT	FT=NG	FT=NVT	NG=NVT	N
Panel A: Direct Beneficiaries								
HH Size	5.945	-0.414*	-0.198	-0.029	-0.216	-0.385**	-0.169	1,108
	(2.744)	(0.237)	(0.319)	(0.222)	(0.469)	(0.045)	(0.555)	
Average Age	31.138	-0.004	0.026	-2.523*	-0.030	2.519**	2.549	1,108
	(10.875)	(1.436)	(1.845)	(1.379)	(0.986)	(0.040)	(0.132)	
Resp. Age	41.759	-0.661	-1.196	-2.683	0.535	2.022	1.486	1,108
	(13.498)	(1.440)	(1.971)	(1.729)	(0.783)	(0.235)	(0.493)	
Resp. Edu	2.887	-0.406	0.550	-0.128	-0.957*	-0.278	0.678	1,108
	(4.029)	(0.603)	(0.689)	(0.653)	(0.067)	(0.549)	(0.239)	
Resp. Lit.	0.533	-0.025	0.070	0.020	-0.095	-0.045	0.050	1,108
	(0.500)	(0.094)	(0.079)	(0.100)	(0.172)	(0.629)	(0.514)	
Income	11.064	-0.031	0.258	-0.003	-0.289	-0.028	0.261	1,109
	(1.371)	(0.215)	(0.224)	(0.190)	(0.201)	(0.882)	(0.196)	
Land	0.494	-0.045	-0.030	-0.069	-0.015	0.024	0.039	1,107
	(0.664)	(0.083)	(0.079)	(0.088)	(0.838)	(0.770)	(0.618)	
TLU	2.465	-0.108	-0.237	0.298	0.129	-0.406	-0.535	1,109
	(2.179)	(0.402)	(0.358)	(0.389)	(0.734)	(0.323)	(0.148)	
Has Migrant	0.588	0.027	0.059	0.063	-0.032	-0.036	-0.004	1,108
	(0.493)	(0.065)	(0.050)	(0.064)	(0.584)	(0.609)	(0.942)	
Panel B: Pay-it-forward								
HH Size	5.968	-0.529	0.119	0.194	-0.648*	-0.723**	-0.075	873
	(2.713)	(0.320)	(0.368)	(0.339)	(0.054)	(0.018)	(0.829)	
Average Age	29.381	3.509**	1.627	1.073	1.882	2.436*	0.554	873
	(10.131)	(1.537)	(1.611)	(1.194)	(0.275)	(0.071)	(0.695)	
Resp. Age	40.272	2.596	0.175	0.728	2.421	1.868	-0.553	872
	(12.569)	(1.846)	(1.556)	(1.680)	(0.237)	(0.382)	(0.769)	
Resp. Edu	2.806	-0.561	-0.278	-0.364	-0.284	-0.197	0.086	872
	(3.947)	(0.537)	(0.510)	(0.540)	(0.554)	(0.699)	(0.858)	
Resp. Lit.	0.516	-0.049	0.003	-0.072	-0.052	0.023	0.075	871
	(0.501)	(0.082)	(0.083)	(0.078)	(0.534)	(0.776)	(0.355)	
Income	11.165	-0.202	0.142	-0.115	-0.343**	-0.086	0.257*	873
	(1.321)	(0.182)	(0.179)	(0.172)	(0.031)	(0.556)	(0.076)	
Land	0.379	0.059	0.112	0.151**	-0.053	-0.092	-0.039	870
	(0.605)	(0.072)	(0.077)	(0.074)	(0.526)	(0.262)	(0.653)	
TLU	2.096	0.315	0.363	0.283	-0.048	0.032	0.080	873
	(1.941)	(0.313)	(0.424)	(0.377)	(0.905)	(0.930)	(0.862)	
Has Migrant	0.624	-0.053	0.016	0.027	-0.069	-0.080	-0.012	873
	(0.486)	(0.065)	(0.057)	(0.083)	(0.186)	(0.315)	(0.873)	

OLS estimates of baseline differences between treatment and control groups. For each outcome, we report the coefficient of interest and clustered (VDC) standard errors in parentheses; Column (1) reports the mean and standard deviation of the outcome variable. Column (2) compares the effect of belonging to any treatment group to the control group. Column (3) measures the effect of belonging to the FT group at baseline. Column (4) measures the effect of belonging to the NG group at baseline. Column (5) measures the effect of belonging to the NVT group at baseline. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3.3: Baseline balance: outcome indices

	Control mean	FT	NG	NVT	FT=NG	FT=NVT	NG=NVT	N
Panel A: Direct Beneficiaries								
Empowerment	0.776	-0.024	-0.008	-0.022	-0.017	-0.002	0.015	1,101
	(0.187)	(0.024)	(0.023)	(0.027)	(0.472)	(0.942)	(0.577)	
Finance	0.078	-0.242*	0.022	-0.188	-0.264	-0.054	0.210	1,109
	(1.031)	(0.124)	(0.160)	(0.140)	(0.103)	(0.701)	(0.226)	
Aspirations	0.088	-0.373**	-0.055	-0.267*	-0.318**	-0.107	0.212	1,109
	(1.530)	(0.154)	(0.178)	(0.159)	(0.011)	(0.237)	(0.101)	
Mental health	-0.052	-0.147	-0.002	-0.055	-0.145	-0.091	0.053	1,109
	(1.013)	(0.147)	(0.114)	(0.139)	(0.340)	(0.594)	(0.709)	
Assets	0.181	-0.175	-0.067	-0.109	-0.108	-0.066	0.042	1,109
	(0.889)	(0.199)	(0.173)	(0.200)	(0.586)	(0.767)	(0.833)	
Income (Rs.)	11.064	-0.031	0.258	-0.003	-0.289	-0.028	0.261	1,109
	(1.371)	(0.215)	(0.224)	(0.190)	(0.201)	(0.882)	(0.196)	
Expenditures	9.607	-0.322*	-0.031	-0.297	-0.291*	-0.024	0.266	1,108
	(1.135)	(0.192)	(0.205)	(0.220)	(0.063)	(0.889)	(0.161)	
Physical health	0.041	-0.205**	0.129	0.042	-0.334***	-0.248**	0.087	1,109
	(0.914)	(0.090)	(0.079)	(0.098)	(0.001)	(0.033)	(0.409)	
Food Security	-0.022	-0.104	0.007	-0.036	-0.111	-0.068	0.043	1,109
	(0.885)	(0.125)	(0.136)	(0.143)	(0.410)	(0.630)	(0.778)	
Panel B: Pay-it-forward								
Empowerment	0.743	0.011	0.000	0.030	0.010	-0.019	-0.030	867
	(0.183)	(0.022)	(0.028)	(0.025)	(0.675)	(0.383)	(0.281)	
Finance	-0.010	-0.045	-0.001	-0.221	-0.044	0.176	0.220	873
	(1.025)	(0.123)	(0.153)	(0.135)	(0.742)	(0.125)	(0.134)	
Aspirations	0.004	-0.302**	-0.114	-0.153	-0.188*	-0.149	0.039	873
	(0.932)	(0.141)	(0.149)	(0.159)	(0.056)	(0.184)	(0.751)	
Mental health	-0.013	-0.175	-0.038	-0.119	-0.138	-0.056	0.082	873
	(1.032)	(0.163)	(0.156)	(0.190)	(0.335)	(0.755)	(0.638)	
Assets	-0.041	0.063	0.223	-0.018	-0.160	0.081	0.241	873
	(0.919)	(0.170)	(0.165)	(0.200)	(0.424)	(0.723)	(0.287)	
Income (Rs.)	11.165	-0.202	0.142	-0.115	-0.343**	-0.086	0.257*	873
	(1.321)	(0.182)	(0.179)	(0.172)	(0.031)	(0.556)	(0.076)	
Expenditures	9.654	-0.304*	-0.177	-0.251	-0.126	-0.053	0.074	871
	(1.369)	(0.171)	(0.188)	(0.174)	(0.394)	(0.685)	(0.625)	
Physical health	-0.096	-0.032	0.188	-0.004	-0.220***	-0.028	0.192*	873
	(0.995)	(0.116)	(0.119)	(0.141)	(0.006)	(0.797)	(0.088)	
Food Security	-0.007	-0.305***	-0.018	-0.115	-0.287**	-0.190*	0.097	873
	(0.975)	(0.097)	(0.102)	(0.104)	(0.011)	(0.091)	(0.399)	

OLS estimates of baseline differences between treatment and control groups. For each outcome, we report the coefficient of interest and clustered (VDC) standard errors in parentheses; Column (1) reports the mean and standard deviation of the outcome variable. Column (2) compares the effect of belonging to any treatment group to the control group. Column (3) measures the effect of belonging to the FT group at baseline. Column (4) measures the effect of belonging to the TNG group at baseline. Column (5) measures the effect of belonging to the NVT group at baseline. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3.4: Treatment compliance by arm and direct/pay-it-forward

	Control mean	FT	NG	NVT	N
Targeted direct beneficiaries	0.122	0.779***	0.687***	0.650***	1,031
	(0.328)	(0.076)	(0.088)	(0.086)	
Potential PIF	0.066	0.739***	0.560***	0.165**	797
	(0.249)	(0.064)	(0.108)	(0.076)	

OLS regressions, clustered (VDC) standard errors in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3.5: ITT effects on summary indices

	Control mean	FT	NG	NVT	FT=NG	FT=NVT	NG=NVT	N
Panel A: Direct impacts								
Short-term								
Empowerment	0.777	0.043**	0.044*	0.048**	-0.000	-0.005	-0.004	1,020
	(0.189)	(0.020)†	(0.023)	(0.022)†	(0.989)	(0.813)	(0.836)	
Financial Inclusion	0.077	0.291**	0.341**	0.329***	-0.050	-0.038	0.012	1,033
	(0.981)	(0.130)†	(0.140)†	(0.122)†	(0.743)	(0.739)	(0.931)	
Aspirations	-0.017	0.094	0.308	0.091	-0.213	0.004	0.217	1,032
	(0.996)	(0.113)	(0.191)	(0.148)	(0.230)	(0.977)	(0.262)	
Mental health	0.009	0.174	-0.065	0.089	0.238**	0.085	-0.153	1,032
	(0.970)	(0.107)	(0.105)	(0.090)	(0.050)	(0.410)	(0.145)	
Long-term								
Assets	0.080	-0.001	0.012	0.019	-0.013	-0.020	-0.007	1,032
	(1.031)	(0.124)	(0.152)	(0.115)	(0.924)	(0.847)	(0.959)	
Income	11.503	-0.123	0.095	-0.060	-0.218*	-0.063	0.155	1,031
	(1.228)	(0.172)	(0.144)	(0.162)	(0.090)	(0.681)	(0.217)	
Expenditures	9.641	-0.157	-0.069	-0.084	-0.088	-0.073	0.015	1,033
	(1.397)	(0.142)	(0.144)	(0.140)	(0.543)	(0.575)	(0.909)	
Physical health	0.041	-0.002	0.058	0.083	-0.059	-0.085	-0.026	1,031
	(0.813)	(0.075)	(0.103)	(0.088)	(0.571)	(0.353)	(0.819)	
Food Security	0.004	0.027	0.150	-0.104	-0.123	0.130	0.253**	1,032
	(0.953)	(0.092)	(0.104)	(0.099)	(0.297)	(0.218)	(0.044)	
Panel B: Pay-it-forward impacts								
Short-term								
Empowerment	0.749	0.067***	0.078***	0.031	-0.011	0.037	0.048**	786
	(0.195)	(0.023)†	(0.021)†	(0.021)	(0.649)	(0.140)	(0.032)	
Financial Inclusion	-0.041	0.171*	0.377***	0.145	-0.205*	0.026	0.232**	796
	(1.032)	(0.098)	(0.121)†	(0.111)	(0.098)	(0.805)	(0.049)	
Aspirations	-0.029	-0.058	0.117	-0.004	-0.175	-0.054	0.121	795
	(1.047)	(0.156)	(0.101)	(0.112)	(0.239)	(0.741)	(0.246)	
Mental health	0.023	0.070	-0.162	0.190*	0.233***	-0.119	-0.352***	795
	(0.899)	(0.093)	(0.115)	(0.111)	(0.005)	(0.111)	(0.001)	
Long-term								
Assets	-0.037	0.094	-0.099	-0.073	0.193*	0.167	-0.026	795
	(1.064)	(0.127)	(0.099)	(0.109)	(0.070)	(0.155)	(0.765)	
Income	11.506	-0.168	-0.230	-0.061	0.062	-0.106	-0.168	795
	(1.134)	(0.186)	(0.153)	(0.148)	(0.701)	(0.552)	(0.231)	
Expenditures	9.537	0.133	-0.033	-0.030	0.166	0.163	-0.003	796
	(1.449)	(0.173)	(0.183)	(0.147)	(0.307)	(0.230)	(0.983)	
Physical health	0.064	-0.123	0.158	-0.069	-0.281**	-0.054	0.226**	794
	(0.936)	(0.106)	(0.100)	(0.091)	(0.017)	(0.628)	(0.026)	
Food Security	-0.059	0.125	0.013	-0.009	0.112	0.133	0.022	795
	(0.874)	(0.089)	(0.097)	(0.098)	(0.296)	(0.206)	(0.845)	

OLS regressions, clustered (VDC) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. N.B. that FT=NG, FT=NVT, NG=NVT represent Wald tests of a null of equal treatment effects, p-values are reported as the sub-statistic. Control variables include baseline dependent variable, stratification bin dummies, and imbalanced variables at baseline. FT: full treatment, NG: no-goats treatment, NVT: no-values-based-training treatment. Indices for aspirations, mental health, assets, financial inclusion, food security, and physical health are calculated as a standardized weighted average of subindicators following Anderson (2008). Income is the logged sum of total household income. Expenditures is the logged sum of total non-food expenditures. The empowerment index is a modified 5DE index of the A-WEAI (Alkire et al., 2012; Malapit et al., 2015).

Table 3.6: ITT effects on women's empowerment

	Control mean	FT	NG	NVT	FT=NG	FT=NVT	NG=NVT	N
Panel A: Direct impacts								
Empowerment index	0.777	0.043**	0.044*	0.048**	-0.000	-0.005	-0.004	1,020
	(0.189)	(0.020)	(0.023)	(0.022)	(0.989)	(0.813)	(0.836)	
Production decisions	0.903	0.040*	0.006	0.041	0.034	-0.001	-0.035	1,030
	(0.296)	(0.021)	(0.032)	(0.025)	(0.163)	(0.955)	(0.236)	
Asset ownership	0.926	0.043**	0.046**	0.041**	-0.003	0.002	0.005	1,030
	(0.262)	(0.017)	(0.019)	(0.020)	(0.835)	(0.917)	(0.787)	
Credit access/control	0.416	-0.043	-0.022	-0.018	-0.022	-0.025	-0.003	1,025
	(0.494)	(0.051)	(0.065)	(0.055)	(0.751)	(0.668)	(0.963)	
Control over income	0.892	0.021	0.078***	0.038	-0.057**	-0.018	0.039	1,026
	(0.311)	(0.025)	(0.025)	(0.029)	(0.026)	(0.546)	(0.148)	
Group membership	0.651	0.139***	0.167***	0.158***	-0.028	-0.019	0.009	1,027
	(0.478)	(0.051)	(0.045)	(0.040)	(0.616)	(0.699)	(0.836)	
Works $\leq$ 10.5 hours	0.685	0.020	-0.055	0.012	0.074	0.007	-0.067	1,031
	(0.465)	(0.039)	(0.036)	(0.041)	(0.117)	(0.887)	(0.166)	
Panel B: Pay-it-forward impacts								
Empowerment index	0.749	0.067***	0.078***	0.031	-0.011	0.037	0.048**	786
	(0.195)	(0.023)	(0.021)	(0.021)	(0.649)	(0.140)	(0.032)	
Production decisions	0.846	0.115***	0.101***	0.096***	0.014	0.019	0.005	794
	(0.362)	(0.033)	(0.029)	(0.032)	(0.510)	(0.475)	(0.809)	
Asset ownership	0.917	0.058***	0.062***	0.038*	-0.004	0.020	0.024	793
	(0.276)	(0.021)	(0.022)	(0.020)	(0.825)	(0.181)	(0.111)	
Credit access/control	0.323	0.028	0.097*	0.059	-0.069	-0.032	0.038	791
	(0.469)	(0.046)	(0.053)	(0.050)	(0.182)	(0.514)	(0.491)	
Control over income	0.892	0.033	0.084***	0.049*	-0.051**	-0.016	0.035	793
	(0.311)	(0.031)	(0.027)	(0.026)	(0.032)	(0.513)	(0.105)	
Group membership	0.579	0.133**	0.186***	0.032	-0.054	0.100	0.154**	793
	(0.495)	(0.056)	(0.064)	(0.059)	(0.415)	(0.118)	(0.020)	
Works $\leq$ 10.5 hours	0.703	0.005	-0.056	-0.042	0.061	0.047	-0.014	795
	(0.458)	(0.033)	(0.055)	(0.050)	(0.257)	(0.339)	(0.829)	

OLS regressions, clustered (VDC) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. N.B. that FT=NG, FT=NVT, NG=NVT represent Wald tests of a null of equal treatment effects, p-values are reported as the sub-statistic. Control variables include baseline dependent variable, stratification bin dummies, and imbalanced variables at baseline. FT: full treatment, NG: no-goats treatment, NVT: no-values-based-training treatment. The empowerment index is a modified 5DE index of the A-WEAI (Alkire et al., 2012; Malapit et al., 2015). The index aggregates an empowerment score across decisions about production, access to and decision-making power over productive resources, control over credit and income, leadership in the community and time allocation. Each binary subindicator equals one if the respondent achieves empowerment, and zero otherwise. Weights and definitions of adequacy are defined in the main text.

Table 3.7: ITT effects on financial inclusion

	Control mean	FT	NG	NVT	FT=NG	FT=NVT	NG=NVT	N
Panel A: Direct impacts								
Financial index	0.077	0.291**	0.341**	0.329***	-0.050	-0.038	0.012	1,033
	(0.981)	(0.130)	(0.140)	(0.122)	(0.743)	(0.739)	(0.931)	
Amount saved	3.824	0.709**	1.056***	0.379	-0.347	0.330	0.677**	1,032
	(2.930)	(0.319)	(0.311)	(0.334)	(0.269)	(0.284)	(0.036)	
Savings group	0.539	0.192***	0.183***	0.139**	0.008	0.053	0.044	1,026
	(0.499)	(0.060)	(0.064)	(0.063)	(0.900)	(0.374)	(0.488)	
Owe formal lender	0.937	0.121	0.316	0.550	-0.195	-0.429	-0.234	1,032
	(3.242)	(0.468)	(0.505)	(0.430)	(0.699)	(0.346)	(0.643)	
Owe informal lender	3.352	-0.366	-0.897	-0.293	0.531	-0.073	-0.604	1,032
	(5.129)	(0.480)	(0.556)	(0.482)	(0.340)	(0.878)	(0.260)	
Discount rate	0.053	-0.024**	-0.003	-0.012	-0.021*	-0.012	0.009	749
	(0.079)	(0.011)	(0.014)	(0.013)	(0.099)	(0.235)	(0.486)	
Planning horizon	1.829	0.055	-0.066	0.196	0.121	-0.141	-0.262*	1,029
	(0.943)	(0.144)	(0.154)	(0.144)	(0.446)	(0.309)	(0.092)	
Panel B: Pay-it-forward impacts								
Financial index	-0.041	0.171*	0.377***	0.145	-0.205*	0.026	0.232**	796
	(1.032)	(0.098)	(0.121)	(0.111)	(0.098)	(0.805)	(0.049)	
Amount saved	3.545	0.092	0.628**	-0.238	-0.535	0.330	0.866**	795
	(2.981)	(0.308)	(0.310)	(0.340)	(0.117)	(0.387)	(0.021)	
Savings group	0.492	0.136**	0.181***	0.002	-0.046	0.134*	0.180***	793
	(0.501)	(0.062)	(0.058)	(0.061)	(0.474)	(0.058)	(0.004)	
Owe formal lender	0.646	-0.325	0.594	0.474	-0.920**	-0.799**	0.120	795
	(2.802)	(0.346)	(0.456)	(0.405)	(0.035)	(0.025)	(0.806)	
Owe informal lender	3.316	-0.288	-0.702	0.019	0.414	-0.307	-0.721*	795
	(5.183)	(0.423)	(0.474)	(0.396)	(0.305)	(0.405)	(0.079)	
Discount rate	0.043	-0.012	0.011	0.005	-0.023**	-0.017	0.006	585
	(0.064)	(0.009)	(0.012)	(0.013)	(0.032)	(0.124)	(0.629)	
Planning horizon	1.759	0.164	0.080	0.234	0.084	-0.069	-0.153	794
	(0.962)	(0.111)	(0.126)	(0.140)	(0.516)	(0.586)	(0.260)	

OLS regressions, clustered (VDC) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. N.B. that FT=NG, FT=NVT, NG=NVT represent Wald tests of a null of equal treatment effects, p-values are reported as the sub-statistic. Control variables include baseline dependent variable, stratification bin dummies, and imbalanced variables at baseline. FT: full treatment, NG: no-goats treatment, NVT: no-values-based-training treatment. The financial inclusion index is a weighted standardized average of each subindicator presented in this table. Subindicators include the logged amount saved in the last month, a dummy variable equal to one if the respondent belongs to a savings group, the logged amount owed to formal/informal lenders, the calculated discount rate following Ashraf, Karlan, and Yin (2006), and an ordered categorical variable indicating how far individuals plan ahead following Laajaj (2017).

Table 3.8: ITT effects on aspirations

	Control mean	FT	NG	NVT	FT=NG	FT=NVT	NG=NVT	N
Panel A: Direct impacts								
Aspirations index	-0.017	0.094	0.308	0.091	-0.213	0.004	0.217	1,032
	(0.996)	(0.113)	(0.191)	(0.148)	(0.230)	(0.977)	(0.262)	
Income aspirations	11.043	0.424*	0.808**	0.440	-0.384	-0.017	0.367	1,031
	(3.038)	(0.239)	(0.348)	(0.279)	(0.217)	(0.946)	(0.276)	
Asset aspirations	13.794	-0.124	0.336	-0.100	-0.460	-0.024	0.436	1,031
	(2.754)	(0.275)	(0.396)	(0.361)	(0.265)	(0.948)	(0.350)	
Children's educ.	14.885	-0.574	0.157	0.234	-0.731	-0.809*	-0.077	1,031
	(3.581)	(0.514)	(0.620)	(0.549)	(0.229)	(0.074)	(0.901)	
Daughters' educ.	14.185	-0.979	0.118	0.014	-1.097	-0.993*	0.104	1,031
	(4.086)	(0.602)	(0.755)	(0.609)	(0.133)	(0.066)	(0.886)	
Sons' educ.	14.581	-0.377	0.245	-0.104	-0.622	-0.273	0.349	1,031
	(3.810)	(0.447)	(0.594)	(0.548)	(0.308)	(0.590)	(0.603)	
Status aspirations	15.567	3.757	4.377	0.235	-0.620	3.522	4.142	1,031
	(19.168)	(2.521)	(2.915)	(2.473)	(0.830)	(0.142)	(0.136)	
Panel B: Pay-it-forward impacts								
Aspirations index	-0.029	-0.058	0.117	-0.004	-0.175	-0.054	0.121	795
	(1.047)	(0.156)	(0.101)	(0.112)	(0.239)	(0.741)	(0.246)	
Income aspirations	11.045	0.044	0.532	0.021	-0.488	0.023	0.510	795
	(3.131)	(0.397)	(0.356)	(0.355)	(0.131)	(0.950)	(0.103)	
Asset aspirations	13.814	-0.443	0.081	-0.260	-0.524	-0.183	0.340	795
	(2.478)	(0.378)	(0.234)	(0.294)	(0.163)	(0.654)	(0.237)	
Children's educ.	14.692	-0.059	-0.450	0.411	0.391	-0.470	-0.861**	795
	(3.612)	(0.499)	(0.415)	(0.422)	(0.373)	(0.338)	(0.031)	
Daughters' educ.	13.626	-0.253	-0.307	0.244	0.054	-0.497	-0.551	794
	(4.330)	(0.492)	(0.483)	(0.457)	(0.915)	(0.317)	(0.257)	
Sons' educ.	14.574	-0.441	-0.488	-0.009	0.047	-0.432	-0.478	794
	(3.507)	(0.474)	(0.338)	(0.373)	(0.919)	(0.391)	(0.214)	
Status aspirations	15.641	-0.203	1.422	0.469	-1.625	-0.672	0.953	795
	(20.629)	(2.637)	(2.610)	(2.575)	(0.535)	(0.788)	(0.677)	

OLS regressions, clustered (VDC) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. N.B. that FT=NG, FT=NVT, NG=NVT represent Wald tests of a null of equal treatment effects, p-values are reported as the sub-statistic. Control variables include baseline dependent variable, stratification bin dummies, and imbalanced variables at baseline. FT: full treatment, NG: no-goats treatment, NVT: no-values-based-training treatment. The aspirations index is a weighted standardized average of income, asset, status, and educational aspirations. Subindicators include aspirations for future income, aspirations for value of home and land, aspirations for social status, and aspirations of children's education. Subindicators omitted from the index include aspirations for childrens education disaggregated by gender.

Table 3.9: ITT effects on mental health

	Control mean	FT	NG	NVT	FT=NG	FT=NVT	NG=NVT	N
Panel A: Direct impacts								
Mental health index	0.009	0.174	-0.065	0.089	0.238**	0.085	-0.153	1,032
	(0.970)	(0.107)	(0.105)	(0.090)	(0.050)	(0.410)	(0.145)	
Depression score	6.541	0.278	-0.099	0.141	0.377*	0.137	-0.240	1,032
	(1.906)	(0.238)	(0.226)	(0.230)	(0.059)	(0.518)	(0.206)	
Locus of control	2.915	-0.212	0.050	-0.055	-0.263	-0.157	0.105	1,032
	(1.482)	(0.156)	(0.162)	(0.167)	(0.112)	(0.351)	(0.570)	
Optimism	6.404	0.091	-0.047	-0.029	0.138	0.120	-0.018	1,032
	(1.184)	(0.160)	(0.175)	(0.139)	(0.477)	(0.425)	(0.913)	
Life Satisfaction	6.361	0.247	0.487**	0.189	-0.240	0.058	0.298	1,030
	(1.904)	(0.226)	(0.222)	(0.195)	(0.293)	(0.785)	(0.148)	
Self-esteem	9.604	0.313*	0.200	0.235*	0.113	0.078	-0.036	1,032
	(1.655)	(0.162)	(0.140)	(0.134)	(0.534)	(0.660)	(0.811)	
Happiness	2.070	-0.011	-0.086	-0.037	0.076	0.027	-0.049	1,032
	(0.530)	(0.064)	(0.059)	(0.064)	(0.175)	(0.668)	(0.389)	
Worry score	9.075	0.514**	-0.043	0.251	0.557***	0.263	-0.294	1,017
	(2.091)	(0.200)	(0.231)	(0.224)	(0.004)	(0.160)	(0.163)	
Trust score	1.528	0.086	-0.259	0.120	0.346*	-0.034	-0.379*	1,029
	(1.334)	(0.168)	(0.223)	(0.181)	(0.085)	(0.847)	(0.079)	
Panel B: Pay-it-forward impacts								
Mental health index	0.023	0.070	-0.162	0.190*	0.233***	-0.119	-0.352***	795
	(0.899)	(0.093)	(0.115)	(0.111)	(0.005)	(0.111)	(0.001)	
Depression score	6.610	0.268	-0.220	-0.071	0.488*	0.339*	-0.150	795
	(1.889)	(0.173)	(0.261)	(0.201)	(0.051)	(0.078)	(0.560)	
Locus of control	2.990	-0.094	-0.257*	-0.090	0.163	-0.004	-0.167	795
	(1.388)	(0.121)	(0.149)	(0.165)	(0.227)	(0.977)	(0.350)	
Optimism	6.354	0.018	-0.085	0.117	0.103	-0.099	-0.201	795
	(1.185)	(0.145)	(0.158)	(0.172)	(0.437)	(0.424)	(0.167)	
Life Satisfaction	6.431	-0.046	0.232	0.051	-0.279	-0.098	0.181	794
	(1.940)	(0.221)	(0.206)	(0.185)	(0.162)	(0.610)	(0.297)	
Self-esteem	9.841	-0.007	-0.086	-0.050	0.079	0.043	-0.036	795
	(1.819)	(0.198)	(0.199)	(0.230)	(0.725)	(0.872)	(0.887)	
Happiness	2.051	-0.049	-0.043	0.058	-0.007	-0.107**	-0.101*	795
	(0.525)	(0.056)	(0.066)	(0.060)	(0.890)	(0.021)	(0.060)	
Worry score	8.917	0.534**	0.050	0.381	0.484	0.153	-0.331	787
	(1.951)	(0.230)	(0.366)	(0.238)	(0.180)	(0.474)	(0.339)	
Trust score	1.528	0.327	-0.294	0.196	0.621**	0.131	-0.489*	794
	(1.352)	(0.210)	(0.267)	(0.243)	(0.017)	(0.575)	(0.070)	

OLS regressions, clustered (VDC) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. N.B. that FT=NG, FT=NVT, NG=NVT represent Wald tests of a null of equal treatment effects, p-values are reported as the sub-statistic. Control variables include baseline dependent variable, stratification bin dummies, and imbalanced variables at baseline. FT: full treatment, NG: no-goats treatment, NVT: no-values-based-training treatment. The mental health index is a weighted standardized average of each subindicator presented in this table. Depression is an abbreviated version of the CES-D scale Radloff (1977) with a high value indicating high levels of depression. Locus of control is an abbreviated Rotter (1966) scale where a high value indicates a stronger internal locus of control. Remaining subindicators are based on aggregated responses to 3-4 questions per subindicator from the 2009 World Values Survey, where high values indicate positive welfare.

Table 3.10: Costs by treatment arm and direct/pay-it-forward

Panel A: Costs for direct beneficiaries	**T1 (FT)**	**T2 (NG)**	**T3 (NVT)**
Basic intervention			
Trainings (technical)	24	24	24
Cash support (home garden, fodder, forage)	15	15	15
Cash support for shed improvement	40	40	40
Community animal health worker	10	10	10
Equipment & Supply	90	90	90
Administrative	52	52	52
Productive asset transfer			
2 doe goats	120		120
Shared buck of improved breeding stock	10		10
Values-based trainings			
Trainings	31	38	
Per-beneficiary total	**$392**	**$269**	**$361**

Panel B: Costs for PIF beneficiaries			
Basic intervention			
Trainings (technical)			
Cash support (home garden, fodder, forage)			
Cash support for shed improvement			
Community animal health worker	10	10	
Administrative	52	52	
Productive asset transfer			
2 doe goats			
Shared buck of improved breeding stock	10		
Values-based trainings			
Trainings	10	10	
Per-beneficiary total	**$82**	**$72**	**$0**

Chapter 4

Essay Two: Consumption Smoothing and Coping

4.1 Introduction

Households in less developed countries face income volatility from exposure to risk and myriad shocks. Such risk can be idiosyncratic, affecting individual households: the illness or death of a bread-winner, loss of employment, or plot-level crop failure. Alternatively, some risks and shocks covary across a community: droughts and severe weather, geopolitical or macroeconomic shocks, and widespread crop failures are examples. The problem of low and highly variable income is exacerbated by the fact that poor households in less developed countries often lack access to the formal financial sector, and the insurance, credit, and savings instruments that might allow them to efficiently insure against income risk.

This essay examines the effect of a natural disaster on food consumption, non-food consumption expenditures, and household income. On April 25, 2015 a major earthquake of magnitude $7.8M_w$ ($8.1M_s$) struck central Nepal, killing close to 9,000 people and injuring nearly 22,000. The event destroyed homes and infrastructure, killed livestock, interrupted access to water from natural sources and from irrigation, disrupted preparations for the monsoon rice season, cut off access to markets for agricultural inputs and outputs, and generally disrupted the economic lives of people in the earthquake zone. The cost of destroyed and damaged private property, infrastructure, and historic sites totaled approximately $7 billion USD, roughly one-third of Nepal's 2015 GDP. Our sample frame includes qualitatively similar regions that differ in the degree of earthquake shock experienced, and includes data from one year before and one year after the earthquake. This presents a unique opportunity to study the coping response of households in the earthquake zone. We ask two research questions: to what extent were households able to smooth consumption in the wake of income shocks induced by the earthquake, and what strategies were employed to accomplish this smoothing?

Reducing consumption may result in far-reaching negative inter-temporal welfare effects. Chen and Zhou (2007), for example, finds that the Chinese famine of 1959-1961 caused individuals born in the 1959 birth cohort to grow to smaller adult stature than they otherwise would have, and that the famine also greatly impacted lifetime earning and labor supply. Such effects may even be inter-generational: Lindeboom, Portrait, and van den Berg (2010) find that individuals exposed *in utero* to the Dutch potato famine of 1846-1847 experienced worse lifetime health and economic outcomes. On the other hand, strategies employed to protect consumption, including *ex ante* strategies such as precautionary saving (Paxson, 1992) and income diversification, and *ex post* coping mechanisms like selling livestock or other productive assets Kazianga and Udry (2006), may also negatively affect welfare. Coping mechanisms that rely on liquidating buffer stocks of livestock or selling other capital may put households at risk of falling below a threshold level of productive assets required to stay out of persistent poverty (Lybbert et al., 2004; Carter and Lybbert, 2012). Other coping mechanisms may inhibit the long-run accumulation of human capital (Jacoby and Skoufias, 1997) or investment in one's health (Frankenberg, Smith, and Thomas, 2003).

A substantial literature in development economics deals with the question of how households manage this dilemma. In physically and socially isolated villages that lack access to formal financial markets, households may develop informal mechanisms to share risk among themselves. Shocks may reduce aggregate (average) consumption, but to the extent that such risk-sharing arrangements exist and are effective, idiosyncratic changes to income will not affect changes in individual consumption (Townsend, 1994; Ravallion and Chaudhuri, 1997).

Empirical evidence of the existence and success of consumption smoothing and risk-sharing arrangements is mixed and context dependent. Udry (1994) shows that in Nigeria risk-sharing does not fully insure households against idiosyncratic income shocks despite evidence of extensive borrowing and lending between households. On the other hand, Skoufias and Quisumbing (2005) synthesizes the results for five studies using panel data from Bangladesh,Ethiopia, Mali, Mexico, and Russia, examining the extent to which households are able to insure consumption against a broad range of economic shocks. While the degree of consumption insurance varies, each study shows that food consumption is better insured than non-food consumption from idiosyncratic shocks. Porter (2012) provides evidence that households in Ethiopia are able to smooth consumption against idiosyncratic agricultural shocks like illness and pest infestations, but are unable to protect themselves from covariate rainfall failure. Similarly, Harrower and Hoddinott (2005) use panel data from Mali to show that households are well protected from specific shocks, but reject the hypothesis of complete consumption smoothing in the case of covariate shocks. Finally, Islam and Maitra (2012) and Genoni (2012) use panel data from Bangladesh and Indonesia (respectively) to show that households protect consumption from idiosyncratic health shocks. Islam shows that households sell livestock in response to health shocks, and short-term insurance may therefore come at long-term cost, but that this effect may be mitigated by access to microcredit. Genoni, however, finds that health shocks do induce transfers from non-coresident kin, and no evidence of asset depletion.

We show that- in the present context- while households experienced the earthquake as a covariate shock, income risk presented by the earthquake was actually idiosyncratic. The data suggest that sources of income are diversified within households and within villages, and that some sources of income were more vulnerable to the earthquake than others. To the extent that this is true, we can apply empirical models that control for covariant shocks and test for the effect of idiosyncratic changes in income on changes in consumption as in Townsend (1994), Skoufias (2003), Islam and Maitra (2012).

This paper offers three main contributions to the existing literature. First, we present evidence of substantial- but not perfect- consumption smoothing over non-food consumption expenditures with respect to idiosyncratic changes to income, achieved by a combination of borrowing in the informal sector and cash aid extended by the Nepali government. Further, while the earthquake itself reduced a household food consumption score (FCS), idiosyncratic income shocks induced by the earthquake had no additional effect. Second, we explore the mechanisms by which local risk-sharing likely operated, demonstrating that households coped with the effects of the earthquake primarily by borrowing from local creditors to supplement cash aid received from the Nepali government. Finally, the context is distinct in the literature. To our knowledge, we present the first empirical analysis of consumption smoothing and risk-sharing following an earthquake. Related, this paper contributes to a growing body of work that specifically addresses the effects of earthquakes in less-developed countries (Yang, 2008; Filipski et al., 2015; Gignoux and Menéndez, 2015; Kirchberger, 2015).

The essay proceeds as follows. Section 4.2 describes the earthquake, with a qualitative summary of the effects and the policy response; section 4.3 summarizes theoretical models of risk-sharing; section 4.4 describes the data and sample frame; section 4.5 reviews our identification strategy; section 4.6 presents findings; section 4.7 includes a discussion of mechanisms; section 4.8 offers a brief discussion of policy relevance and concludes.

4.2 2015 Nepal earthquake

The earthquake and a series of strong aftershocks affected the agricultural households that are overwhelmingly represented in this study, many of whom did not resume farming for many months after the earthquake for fear of landslides and because they were required to spend time and effort constructing temporary homes. In addition, the earthquake interrupted or diverted natural sources of water, and the 2015 monsoon was weaker than average. Most of the households in the affected areas (and over 90% in our sample) are involved in agriculture. All of these factors combined to weaken the food economy in the rural hill districts of the heavily affected areas. In the months following the earthquake over 1.4 million people required food assistance, including 404,000 children already suffering from malnutrition and 200,000 breastfeeding mothers. International NGOs and the Nepali government moved reasonably quickly to deliver emergency food aid, but the response was incomplete and uneven, with less easily accessible districts remaining underserved. In addition to food aid, affected households typically received in-kind transfers of blankets, tarps, building materials for temporary shelters (corrugated tin, baling wire, etc.), clothing and basic medicines (Willitts-King and Bryant, 2017).

The Nepali government began distributing cash assistance one month after the first earthquake. This included NPR 30,000 (≈USD 300) for funeral costs for those households who lost a member during the earthquake, NPR 15,000 (≈USD 150) for households assigned red cards (fully damaged houses) to build temporary shelters, and NPR 3,000 (≈USD 30) for households assigned yellow cards (partially damaged houses). Cash grant beneficiaries were identified on the basis of damage assessments conducted shortly after the earthquake, generally in an ad-hoc fashion with assistance of VDC leaders.[1] The initial cash grants were distributed throughout the 2015 monsoon, either through VDC-level Relief Distribution Committees (RDCs) or, in areas without RDCs, through VDC administrators. In our sample, households who received early cash assistance received an average of NPR 21,000 (≈USD 210). In several districts, non-governmental organizations were involved in the cash distribution process, working in coordination with the government. The early cash grants were followed in late 2015 by winter relief grants of intended to assist victims in purchasing clothing, blankets, and fuel to withstand the cold during the first winter after the earthquakes (Asia Foundation, 2016b).

In addition to emergency relief the government of Nepal established the Rural Housing Reconstruction Program (RHRP) to assist affected households with the rebuilding their

[1] Nepal comprises 75 administrative districts, which are further subdivided into village development committees (VDC), with each VDC consisting of nine wards. Wards most closely approximate the popular perception of a 'village'. Note that as of 2017, the old administrative units were dissolved, and VDCs have been replaced by *gaunpalika*.

homes. The RHRP provides cash assistance to impacted households to promote 'owner-driven reconstruction', and is conditional on complying with building codes to make homes more earthquake resilient. The total size of the reconstruction grant is NPR 300,000 (≈USD 3,000), released in tranches of NPR 50,000 (≈USD 500), NPR 150,000 (≈USD 1,500), and NPR 100,000 (≈USD 1,000). Eligibility for the RHRP was determined based on a second round of damage assessments conducted in February 2016. In total 533,282 houses were deemed eligible for the grant. The government began disbursing RHRP grants in July 2016, the month after we collected the data used in this study; our survey instrument did not include questions related to RHRP eligibility.

Qualitative studies suggest that households preferred taking on debt to other coping mechanisms (Asia Foundation, 2016). Generally speaking, borrowing is a preferred coping strategy and appears to be common across affected districts. Given that subsistence farming does not provide cash income, and that yields from non-subsistence staple-grain farming can only be sold at harvest times, households are accustomed to borrowing cash from relatives, neighbors, local moneylenders, or microfinance institutions and repaying that money only when crops are sold or remittances are sent by household members working elsewhere. The proliferation of saving and credit groups promoted by the government and NGOs for poverty alleviation and entrepreneurship development has promoted this practice. After the earthquakes, therefore, households with financial challenges may have been more likely to borrow than sell assets or using other coping strategies. Rural households borrow for various purposes: routine expenses, to finance small businesses and to send migrants overseas. Borrowing locally from informal sources- particularly friends, family, and village money lenders- at high interest rates is more common than borrowing from banks. Such loans are normally faster and easier than approaching banks that may require formal documents, and informal sources of lending often offer more flexible repayment and ask for no or little collateral.

4.3 Risk-sharing and consumption smoothing

In this essay, we present a formal test for consumption smoothing in the wake of a shock. As noted in Townsend (1995), tests of consumption smoothing rely on the insight that households will attempt to spread consumption of lifetime earnings evenly across time, either through mechanisms that mitigate income shocks, or through coping strategies that attenuate their effects. Such mechanisms may operate across space, as is the case with informal risk-sharing between households within a village or families within a kinship group. Alternatively, they may operate across time, as is the case with credit transactions.

Here we briefly summarize the theoretical underpinnings of the test used in the empirical analysis that follows. Full derivations of the test can be found in Mace (1991), Cochrane (1991), and Townsend (1994). This summary closely follows Mace (1991) in particular. Complete risk-sharing implies that individual consumption varies positively with aggregate consumption and not with idiosyncratic variables, including individual income shocks. The social planner maximizes the weighted sum of the expected lifetime utilities of the H households in the economy given by the objective function (4.1), by allocating consumption across households subject to the aggregate constraint given by equation (4.2):

$$\sum_{h=1}^{H} \omega_h \sum_{t=0}^{\infty} \pi(s_t) U[C_{ht}(s_t); b_{ht}(s_t)], \tag{4.1}$$

$$\sum_{h=1}^{H} C_{ht}(s_t) = \sum_{h=1}^{H} y_{ht}(s_t) \tag{4.2}$$

where ω_h is a time-invariant Pareto weight assigned to household h, the term $\pi(s_t) \in [0, 1]$ denotes the probability of state of the world s occurs in time period t. Consumption for household h at time t is denoted by $C_{ht}(s_t)$, and $b_{ht}(s_t)$ incorporates taste-shifters. Household income at time t is given by $y_{ht}(s_t)$. Dropping the state notation for expositional clarity, we assume an exponential utility function:

$$U[C_{ht}; b_{ht}] = -\frac{1}{\sigma}\exp[-\sigma(C_{ht} - b_{ht})], \qquad \sigma > 0 \tag{4.3}$$

We further assume that individuals have the same constant of absolute risk aversion σ. Manipulating the first-order conditions, it follows that consumption for household h at time t is given by

$$C_{ht} = C_{at} + \frac{1}{\sigma}(\ln \omega_h - \omega_a) + (b_{ht} - b_{at}) \tag{4.4}$$

where ω_a, C_{at}, and b_{at} are village averages at t as given by

$$\omega_a = \frac{1}{H}\sum_{h=1}^{H} \ln \omega_h, \qquad C_{at} = \frac{1}{H}\sum_{h=1}^{H} C_{ht}, \qquad b_{at} = \frac{1}{H}\sum_{h=1}^{H} b_{ht}.$$

The major implication of risk sharing is reflected in equation (4.4): individual consumption varies positively with aggegate consumption, which varies by state and over time. Taking the first difference of equation (4.4) we arrive at

$$\Delta C_{ht} = \Delta C_{at} + (\Delta b_{ht} - \Delta b_{at}) \tag{4.5}$$

Equation (4.5) implies that under the assumption of full risk-sharing, individual consumption C_{ht} depends only on village-level average consumption net of changes in tastes/preferences $(\Delta b_{ht} - \Delta b_{at})$, which occur slowly and infrequently and are arguably negligible. Note also that the additive fixed effect of the household's Pareto weight relative to the village average $(\ln \omega_h - \omega_a)$ falls out with the first difference. Therefore, equation (4.5) presents a straightforward test for full consumption insurance: simply regress changes in individual consumption on changes in aggregate consumption and other explanatory variables, including time-invariant demographic controls and idiosyncratic changes to income or employment. Formally:

$$\Delta C_{ht} = \alpha + \theta \Delta C_{at} + \gamma \Delta y_{ht} + \epsilon_{ht} \tag{4.6}$$

where Δy_{ht} is change in income. The risk-sharing model predicts that $\theta = 1$, that $\gamma = 0$. Additionally, according to the full risk-sharing model, individual consumption responds only to aggregate risk and not idiosyncratic risk: all variables other than aggregate consumption should enter insignificantly. In short, temporary income shocks should not affect current levels of consumption in a context where sufficient smoothing mechanisms exist. Econometrically, note that the FD model embedded in 4.6 addresses potential bias from omitted variables, and implicitly removes the household's Pareto weight.

Assuming power utility rather than exponential utility, equation (4.6) it can be shown that:

$$\Delta \ln C_{ht} = \alpha + \theta \Delta \ln C_{at} + \gamma \Delta \ln y_{ht} + \epsilon_{ht} \tag{4.7}$$

as before, the risk-sharing model predicts that $\theta = 1$, $\gamma = 0$.[2]

In the analysis that follows, we prefer the log formulation of the empirical specification for three reasons. First, even after topcoding income and consumption at the 99^{th} percentile, the distribution of the outcomes of interest exhibit long right tails that may bias the parameter estimates in a regression on levels. Second, because the difference in logs approximates a growth rate, the log formulation offers the attractively simple interpretation that an x percentage points change in income causes a y percentage points change in consumption, or an elasticity of consumption with respect to the income. Third, common specifications like Cobb-Douglas and logarithmic utility are special cases of the power utility assumption that underlies the log formulation.

[2]See appendix B to this chapter for the derivation

4.4 Data

4.4.1 Sample frame

The earthquake's epicenter lies in the Ghorka district, and because of the topography and geologic features of the region the strongest shockwaves traveled along fault lines to the east. The geographic area encompassed by this study includes four districts in the middle Hills, two of which (Dhading and Nuwakot) lie immediately east of the epicenter and were among the most devastated areas, and two of which are west of the epicenter and survived the earthquake relatively unscathed. The affected and unaffected areas bear a strong qualitative resemblance to each other: they share a similar cultural heritage and mix of caste/ethnicity, they engage in similar agricultural practices, and display comparable topography and geographic features. Such similarity combined with a differential earthquake shock suggest the possibility of an effective natural experiment.

The data are from two rounds of a household survey conducted in central Nepal in June, 2014 and June, 2016. In addition to the demographic features of the household, both rounds of the survey collected data on income, expenditures, credit use, food security and dietary diversity, and a complete household roster. The June 2016 survey round included modules to measure the impact of the earthquake at the household level and a battery of questions to capture coping responses to the earthquake. The full baseline sample consists of 874 households in 70 wards.

4.4.2 Descriptive statistics

Table 4.1 presents descriptive statistics of demographics, key outcomes (household expenditures, income, and debt) as measured at baseline, a set of variables capturing the earthquake shock, and a basic set of coping strategies. In this section we focus on columns (1)-(3), which feature descriptive statistics of the full dataset before undertaking the matching procedure described in section 4.5.[3] The identification strategy described in section 4.5 relies on a good measurement of the degree of earthquake intensity experienced by a household. We use the modified Mercalli intensity (MMI) scale (figure 4.1), which uses a rating of earthquake intensity based on locally observed effects, composed of increasing levels of intensity that range from imperceptible shaking to catastrophic destruction.[4] We rely mainly on an MMI imputed from recorded ground motions captured by the United States Geological Survey (USGS) ShakeMap and measured at the VDC level. Figure 4.2 displays a histogram of VDC average MMI. The data display pronounced groupings around MMI≈5.5, MMI≈6.0, at just under MMI≈7.0, and at MMI≈8.0. We divide the sample into a "destructive" earthquake group that experienced MMI>7 ($N = 259$, 19 ward clusters) and a "sub-destructive" group that experienced MMI≤7 ($N = 615$, 51 ward clusters).[5] Our rationale for dividing the sample at MMI≤7 is twofold. First, this threshold creates a greater difference between the average MMI shock in the destructive and sub-destructive subsamples compared to the next-most intuitive threshold of MMI≤6.5. Second, setting the cutoff at MMI≤7 results in a larger, richer pool of counterfacutal observations that facilitates the matching exercise described below.

Table 4.1A summarizes the two measurements of earthquake intensity described above: the MMI experienced by respondents at the VDC level, extracted from ShakeMap GIS layers, and a binary variable indicating whether the respondent holds a "red" earthquake card. In the MMI>7 subsample, households experienced mean MMI of 8.02, indicating 'destructive' earthquake likely to cause great damage in poorly built structures. This description applies to most homes in the area, which are normally constructed from stone or soft bricks and timber. Unsurprisingly 89% of households in the MMI>7 subsample report holding a a red earthquake card. In the MMI≤7 subsample, households experienced a mean MMI of 5.99,

[3]Columns (4)-(6) are discussed along with the matching procedure.
[4]https://earthquake.usgs.gov/learn/topics/mercalli.php
[5]Note that dividing the sample at MMI=7 is the same as dividing it by more/less affected district.

indicating a "strong" earthquake less likely to cause extensive property damage. Only 7% of households in this subsample hold a red card.

Table 4.1B reports the means of several demographic characteristics of the sampled households at baseline.[6] As noted above, virtually all of the respondents were female, had a mean age of little over 40 years, and had completed a little under two years of formal education. Approximately 33% of the full sample belong to one of the two highest Hindu castes (in this area, Brahmins and Chhetris, while about 45% belong to the Janajati, a collection of non-casted indigenous ethnicities that reside in the middle Hills. The remaining sample consists mainly of Dalits (so-called "untouchables") and low-caste Hindus. In general, membership in a higher caste implies higher literacy rates, a greater likelihood to own one's home and land, and may imply better access to the formal financial sector and social services. Around half of the sample reported having a migrant member either in Nepal or abroad, indicative of a groundswell of out-migration and the growing importance of remittances that has greatly influenced life in rural Nepal in recent years.

Table 4.1C condenses and summarizes three key categories of coping responses. The "reduced consumption" variable is binary and indicates whether the respondent household cut meals, reduced meal size, or ate less expensive and/or preferred foods at any point in the year after the earthquake and prior to follow-up. In hard-hit areas, approximately 29% of households indicated having pursued some sort of consumption-reduction coping strategy compared to 7% in less-affected areas. "Borrow" indicates the percentage of respondent households that said they took out a loan as a coping strategy, or that they bought food on credit as a coping strategy. In the MMI>7 subsample 47% of households reported using some kind of credit transaction as a coping mechanism, compared to only 9% of households belonging to the MMI≤7 subsample. Finally "share" denotes the percentage of households (27% of strongly affected families, 2% of weakly affected) that said they shared food stockpiles

[6]This is an abbreviated list. Appendix section B reports all 29 variables used in the empirical analysis described below.

or other resources with friends or family members. Taken together, these descriptive statistics suggest partially- but not not fully- insured consumption, and that consumption smoothing may have occurred through a combination of credit transactions and informal risk-sharing.

Finally, table 4.1D summarizes pre-earthquake (baseline) levels of the main outcomes of interest: household food consumption score (FCS), household non-food expenditures, household income (excluding home production), and total household debt. All monetary values are measured in Nepali rupees (100 NPR≈\$1 USD) and topcoded at the 99^{th} percentile.

Household income consists of an aggregate of revenue from the sales of crops, revenue from the sale of livestock and livestock products, income from salaries, revenue from off-farm enterprises and small businesses, income from day labor, and income from remittances and transfers. Cash aid related to the earthquake is not is not included in income. We did not collect data on the value of agricultural output for home consumption, nor did we collect the data on the cost of agricultural inputs. Household income and expenditures appear to be slightly lower in the MMI≤7 group at baseline, but not significantly so. We also note that total household income is broadly consistent with other data sets collected from similar populations, after accounting for the value of home-production.

The food consumption score (FCS) serves as a proxy indicator for food consumption expenditures. We calculate FCS by determining the number of meals where members of the household consumed a food group during the three days that preceded the survey, multiply the frequencies by a quality weight, and then sum the resultant frequency and quality weighted sub-indices into a single indicator (WFP, 2008).[7] We calculate FCS using the following food groups and weights as recommended by the WFP: staple grains (wt=2), pulses (wt=3), meat and fish (wt=4), dairy and eggs (wt=4), vegetables (wt=1), fruits (wt=1), and oils (wt=0.5). Typical Nepali households consume two main meals a day (*khana*), therefore

[7]Due to the design of the survey food consumption module, the FCS calculation employed here differs from the standard calculation as defined by the WFP. The WFP standard tracks the past seven days, not the past six meals. Further, we omit some minor food groups/categories (such as condiments and sweets) for which we lack data.

the sample mean FCS of ≈36 corresponds to a household that eats six typical meals of rice and lentils plus a vegetable over a three day period. The 25^{th} percentile (FCS≈27) may have skipped a meal or omitted vegetables, for instance, while the 75^{th} percentile (FCS≈49) may have supplemented a basic meal with meat or dairy on two or three occasions. The survey instrument did not collect data on the monetary value of food consumption, but we believe that FCS proxies well for food expenditures for a number of reasons. First, the FCS has been shown to reflect diet quality in terms of caloric energy and diversity (Wiesmann et al., 2009), so it stands to reason that it would also correlate closely to the cost of a market-basket of food items. Second, using FCS avoids measurement error from attempting to assign a monetary value to home-produced agricultural products. Finally, food prices may reflect locally heterogeneous inflation induced by the earthquake and other shocks; indeed it is easy to conceive of a scenario where food expenditures increase even as dietary quality deteriorates.

Total annual household non-food expenditures consists of an aggregate of several categories of household consumption, including amounts spent on clothing, school-related expenses, transportation, medical expenses, kitchenware and furniture, home improvements, and ceremonies/celebrations. Note that the time frame over which we measure these expenses varies by expenditure category, with the window ranging from the one-month to one-year period preceding the survey, depending on the type of expenditure; we annualize all expenditures prior to aggregating. Because the reference point is the date of the survey, the annual aggregate may overstate actual expenditures if the earthquake caused an initial decrease in spending, followed by an increase later on.

The "outstanding debt" variable is the sum of the outstanding principal balances on all loans for which a member of the household is responsible. We also collected detailed information on the type of lender and purpose of each loan. Average total indebtedness was substantially (but not significantly) higher in the MMI>7 group at baseline. Note that the survey instrument did not collect data on interest rates.

4.5 Identification Strategy

Ravallion and Chaudhuri (1997) assert that the test given by equation (4.7) gives biased parameter estimates whenever there is a common village-level component in household income changes, and show that controlling for village-year rather than ΔC_{at} attenuates the bias. In the village context, we expect that livelihoods are interdependent to some degree. We therefore estimate a version of the empirical model suggested by Ravallion and Chaudhuri (1997).[8]

$$\Delta \ln C_{hv} = \alpha + \gamma_1 \Delta \ln y_{hv}^{neg} + \gamma_2 \Delta \ln y_{hv}^{pos} + \lambda X_{hv} + \mu_v + \epsilon_{hv} \tag{4.8}$$

$\Delta \ln C_{hv}$ is our main outcome of interest: the growth rate of consumption for household h residing in ward v. Equation 4.7 treats positive and negative income shocks symmetrically, implying that the growth rates of consumption are equivalent between those who experienced income gains and losses of similar magnitude. Because this assumption may not be valid, we follow (Harrower and Hoddinott, 2005) and allow negative and positive income growth to enter the model separately as $\Delta \ln y_{hv}^{neg}$ and $\Delta \ln y_{hv}^{pos}$. Note that because the panel includes only two time periods, the growth rates of consumption and income approximate percent changes, and ward-year collapses to a ward fixed effect (μ_v). We include a vector X_{hv} of demographic controls and baseline levels of time-variant household characteristics that may affect the dependent variable and ϵ_{hw} is an idiosyncratic error term clustered at the ward level.

We estimate equation (4.8) by OLS, noting that the model takes the first difference of outcomes across periods for the same household. Unobserved time invariant factors are

[8] We draw on the empirical strategies of Skoufias (2003), Harrower and Hoddinott (2005), and Islam and Maitra (2012), notable papers in the literature that use a version of the Ravallion and Chaudhuri (1997) test.

removed by the FD model. Recall that the theoretical model explicitly removes a household's idiosyncratic Pareto weight, which summarizes a range of unobservables that partially explain household deviations in consumption from the village average. In addition, if levels of consumption are reported with error, first differencing removes the component of the measurement error that is constant across time. For instance, it eases concerns that a respondent's position in her household does not allow her to observe all spending with complete accuracy.

The ward fixed effect μ_v captures the effect of local variation in earthquake intensity and all other covariant shocks including, perhaps critically, local rates of inflation. This includes risk related to the earthquake shock, but encompasses other sources of risk as well. In this context one other known (but less-easily quantified) covariant shocks occurred. During the year that passed between the earthquake and second-round data collection, an unofficial trade embargo and blockade at the Indian border disrupted local markets for petrol, cooking fuel, and many other essential goods. The embargo certainly affected food and fuel prices nationwide; embargo-induced local heterogeneity in prices across the sample frame may partially account for variation in consumption outcomes.

Although specification (4.8) implicitly controls for the earthquake via ward fixed effects, we would like to quantify the effect of the earthquake itself on consumption, therefore we also estimate:

$$\Delta \ln C_{hv} = \alpha + \gamma_1 \Delta \ln y_{hv}^{neg} + \gamma_2 \Delta \ln y_{hv}^{pos} + \theta_v MMI_v + \lambda X_{hv} + \epsilon_{hv} \tag{4.9}$$

where MMI_v is the intensity of earthquake shock as measured on the modified Mercalli scale.[9] The main causal treatment effect of interest in both specifications are the income changes $\Delta \ln y_{hv}^{neg}$ and $\Delta \ln y_{hv}^{pos}$ *conditional on the effects of covariate shocks* captured by μ_v or MMI_v. Equation 4.9 is a weaker version of the test because it omits potentially significant

[9] In equation 4.9 MMI_v enters linearly. The results below are robust to non-linear specifications, specifically a quadratic, cubic, and letting MMI enter as discrete dummies.

covariant shocks, especially the blockade. While we are primarily interested in the effect of income shocks, MMI_v also has an interpretation as the effect on $\Delta \ln C_{hv}$ through non-income channels.

The earthquake shock may seem credibly exogenous *prima facie*, but there are several reasons to question whether exposure to the earthquake was as-if randomly assigned. First, as other authors have noted (Kirchberger, 2015; Filipski et al., 2015), within a community households may self-select into farm and house sites that are more vulnerable to earthquakes based on both observable and unobservable characteristics. To the extent that such characteristics are time invariant and affect our outcomes of interest, they will be differenced out in the FD model. Even so, to lessen the risk of endogeneity from self-selection into earthquake-prone areas, we favor a VDC-level measurement of earthquake intensity to one measured at the household level, such as the actual damage to one's home.

Next, despite the qualitative similarities between these districts, we did not purposefully construct the sample to ensure ex-ante balance of demographic characteristics and outcomes of interest. Baseline checks reveal significant imbalance between the MMI>7 and MMI≤7 groups. Therefore, we augment the natural experiment by using propensity scoring methods to improve on the counterfactual, an approach adopted by Deryugina et. al (2014), Kirchberger (2015), and Keswell and Carter (2014), among others. Specifically, we run a logistic regression of a dummy variable representing membership in the MMI>7 group on a full set of demographic variables and baseline levels of outcomes of interest, and use the results to predict the probability that MMI>7: [10]

$$Pr(MMI > 7|X) = \frac{e^{\{\alpha + X\beta\}}}{1 + e^{\{\alpha + X\beta\}}} \tag{4.10}$$

[10]In appendix B we conduct sensitivity analyses without balancing weights and with balancing weights based on the alternative threshold of MMI=6.5, discussed in 4.4.2 . Results under both alternatives are generally robust to the preferred specification in terms of the signs and magnitudes of the point estimates.

For each household h equation (4.10) gives the propensity score, or $P_h = \Pr(MMI > 7|X)$. Restricting the sample to common support, each observation in the MMI<7 is assigned a weight using a kernel matching (KM) algorithm, employing the Epanechnikov kernel with a bandwidth of 0.06. We bottom and topcode the weights at the 1^{st} and 99^{th} percentiles to limit influence of outliers.

Table 4.1 reports the means and standard deviations of each MMI group for the matched and the unmatched sample, and the t-statistic from a regression of the variable on a dummy variable taking a value of unity if MMI>7. Balance for the unmatched sample is reported in columns (1)-(3), and for the matched sample in columns (4)-(6).[11] Note that imposing common support reduces the sample size from $N = 874$ to $N = 691$. *Ex-ante* demographic characteristics and baseline outcomes generally fail to reject a null hypothesis of equality of means after applying balancing weights. In total, 12 out of 30 matching variables in X were out of balance prior to matching; earthquake intensity and coping responses display marked and statistically significant differences even after weighting.

Table 4.1E presents additional balance test statistics. First, as Sianesi (2004) suggests, we examine the pseudo-R^2 of the propensity score regression reestimated on the matched sample. Prior to matching the pseudo-R^2 is 0.295; after matching the pseudo-R^2 is reduced to 0.014. Furthermore, an LR test for the joint significance of the matching covariates in the unmatched sample easily rejects the null ($p = 0.000$), while the same test fails to reject in the matched sample ($p = 1.000$). A final metric of matching quality is also given in panel E: the mean and median values of the standardized bias (SB) of the covariates in X in the matching regression. (Rosenbaum and Rubin, 1985). For each covariate in X SB is defined as the difference in sample means in the treated and control subsamples as a percentage of the square root of the average sample variances in both groups. Unlike t-tests, SB is not sensitive to sample size and may therefore offer a better test of balance. Generally SB for

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[11] Table 4.1 is a select list of key demographic variables and the baseline levels of outcomes of interest; the complete list is presented in appendix table B.1.

any given covariate belonging to X should be under 5%. In the matched sample mean and median SB are well below the heuristic at 3.7% and 3.1%, respectively.

To further motivate the analysis below, figures 4.3, 4.4, and 4.5 show the distributions of the three main outcomes of interest over common support in the matched sample. Figure 4.3 plots the distribution of change in log total income ($\Delta \ln y$) and clearly shows greater density in the negative region for MMI>7 compared to MMI<7. However, many households in the MMI>7 still experienced positive income growth, implying the possibility of risk-sharing in the harder-hit areas. Figure 4.4 shows the distribution of change in the log of FCS ($\Delta \ln FCS$) in the MMI> 7 and MMI≤7 groups. The figure appears to show less density in the positive region for MMI>7 compared to MMI< 7, indicating the possibility of attenuated FCS in the earthquake zone. Finally, figure 4.5 plots the distribution of change in the log of non-food consumption expenditures ($\Delta \ln NFC$) in each MMI group, and shows greater density in the positive region for MMI>7 compared to MMI<7, suggesting increased spending on non-food items in the affected areas.

4.6 Results

4.6.1 Income and labor effects

As a precursor to our tests for consumption smoothing, we test for earthquake effects on income and labor provision. Poor agricultural households may either diversify their portfolio of productive activities *ex ante* in an effort to reduce income variablity, or they divert labor into off-farm employment or establish small enterprises to offset losses resulting from a productivity shock. A considerable empirical literature explores the role of non-farm income in the developing world. Reardon et al. (2007) reviews the importance of the diversification of rural household income into nonfarm activities. Kochar (1999) examines the extent to which households smooth consumption in the face of idiosyncratic income shocks by supplying more hours of labor. Jayachanran (2006) examines the labor market response to covariate rainfall shocks, and finds that they induce individuals to sell their labor at a lower wage rate.

In a paper especially relevant to our context, Kirchberger (2015) presents an analysis of the labor market response to the Yokyakarta (Indonesia) earthquake of 2006, finding that the earthquake actually had a positive effect on wage growth for individuals who were employed in the agricultural sector prior to the earthquake.

The vast majority of households in the sample frame grow food and raise livestock for home consumption, and most others have at least one additional source of cash income. In the context of risk-sharing, a diverse portfolio of income generating activities can contribute to household resilience in two ways. First, if different sectors are more or less affected by covariant shocks, then some households will exhibit greater income resilience than others. Second, if a community comprises households with diversified income (or alternatively, the village comprises households with heterogeneous secondary income sources), then less affected households can help insure against income shocks for more affected households, consistent with theoretical models of idiosyncratic risk and insurance in village economies. Therefore, insofar as households and communities have diverse sources of income, local risk-sharing arrangements may have insulated households against earthquake-induced negative income shocks more efficiently. Qualitative studies state- and our data confirms- that households in earthquake affected areas have diversified income. Respondents in Dhading and Nuwakot (MMI>7) report having earned cash income from 1.72 and 1.60 distinct sectors on average; village-level means are similar and generally do not deviate significantly from the district average.

We estimate earthquake treatment effects on $\Delta \ln y$, as well as changes in log income from specific sources:

$$\Delta \ln y_{hv} = \theta_{tv} MMI_v + \lambda X_{hv} + \epsilon_{hv} \tag{4.11}$$

where $\Delta \ln y_{hv}$ is change in log income, MMI_v is average MMI at the VDC level, and X_{hv} is a vector of time invariant demographic and household characteristics. In this case the earthquake shock is the treatment effect of interest (not income shocks as above) and is measured at the VDC level, therefore ϵ_{hv} is idiosyncratic error clustered at the VDC level. We use the matched sample as described in section 4.5. Results from equation (4.11) are reported in table 4.2. Overall we find that, as expected, the earthquake negatively affected incomes. The marginal effect of a unit increase in MMI on the growth in total income (column (1)) is roughly -25 percentage points.

While the point estimates are negative across the board, they are only significant for livestock income. Prior work has found that distress sales of livestock in the event of natural disasters can flood the market, resulting in lower prices (Maystadt and Ecker, 2014). Effects on remittance income are not significant at conventional levels, but are- contra to expectations- negative and relatively large in magnitude. One possibility is that households in the MMI>7 group, which corresponds to the two districts that are directly adjacent to Kathmandu, have a higher probability of having a domestic migrant in the capital (25% vs. 18%). At the same time, households in the MMI≤7 group have a higher probability of having a migrant overseas (48% vs. 26%). Presumably overseas remittances are more stable, i.e. less likely to have been affected by the earthquake.

Sector entry and exit indicate a coping response characterized by changes in labor market participation. Table 4.3 estimates an OLS model of earthquake effects on the number of sectors in which a household was active after the earthquake:

$$Q_{hv}^{sec} = \theta_{tv} MMI_v + \lambda X_{hv} + \epsilon_{hv} \tag{4.12}$$

as well as linear probability models predicting the likelihood that a household entered or exited a sector in the post-earthquake period:

$$\Delta S_{hv} = \theta_{tv} MMI_v + \lambda X_{hv} + \epsilon_{hv} \tag{4.13}$$

Where Q_{hv}^{s} is a count of sectors from which the household reported earning income, ΔSEC_{hv} is a binary variable indicating entry or exit into a sector after the earthquake (entry and exit models are estimated separately), all other variables are as defined above, standard errors are clustered at the VDC level, and the matching model is applied.

The results show no evidence of economically significant shifts into new sectors as a result of the earthquake. In fact, the earthquake seems to have reduced the number of overall active sectors (-0.80 active sectors per unit of MMI). Similarly, table 4.4 presents estimates of the probability that a household exited a previously active sector in response to the earthquake. The earthquake increased the probability that a household previously active in the crop and livestock sectors would exit. On the other hand, earthquake implied a lower probability of exiting the day labor sector. Focus group discussions and qualitative reports both stated that after the earthquake demand for labor in the construction sector increased, driving up wages. To the extent that these reports are accurate, individuals already earning income from day labor would have been disinclined to exit the sector. Curiously, this effect does not seem to extend to new entrants.

4.6.2 Consumption

Results of the econometric models specified in equations (4.8) and (4.9) are reported in table 4.5. Columns (1) and (3) control for the earthquake via ward fixed effects (equation (4.8), while columns (2) and (4) present the results from the weaker version of the test that controls for the earthquake with MMI (equation 4.9). Note that we code a negative income shocks as an absolute value.

Columns (1) and (2) present results for the food consumption score ($C_{hv} = FCS_{hv}$). Consistent with the predictions of the risk-sharing model, the results show null effects of idiosyncratic income shocks on FCS for both positive and negative income shocks. MMI has a highly significant, but small, effect on the FCS: a unit change in MMI results in a -6 percentage point decrease. Converting to levels, the MMI effect size through non-income channels is approximately -2 units of FCS. For the average household in the affected sample such an effect is the rough equivalent of skipping meat or dairy at one meal over a three day period. While this effect is small, we reiterate that the food consumption data was collected over a year after the earthquake, which implies a lingering effect that might have been much stronger in the weeks and months shortly after the earthquake.

Columns (3) and (4) present results for non-food consumption expenditures ($C_{hv} = NFC_{hv}$). Under conditions of complete consumption smoothing, we would fail to reject the null hypothesis that $\Delta \ln y_{hv}$ equals zero. For the case of positive income shocks we do fail to reject the null. However, we find a negative and significant coefficient on the $\Delta \ln y_{hv} < 0$ variable in both columns (3) and (4): a 10% change in income implies a ≈1.15% change in non-food expenditures. Somewhat counter-intuitively, each unit of MMI intensity implies an increase in non-food consumption expenditure of approximately +16.5 percentage points (we look more carefully at this effect below). This is opposite of our prior expectation with respect to a covariant shock. Earthquakes may differ from other types of covariant shocks because they destroy homes, belongings, and productive assets. These items require repair/replacement over a relatively short time frame. Also, as is the case here, an acute localized covariant shock may be met with a relatively rapid policy response. Such a response may include cash aid and aid in-kind, either of which could appear in the model as an increase in consumption. Clearly, some types of spending may increase and others may decrease, and the increasing budget line-items may obscure a negative effect on other line-items.

Therefore, to gain a greater understanding of the dynamics driving the unexpected positive MMI effect on non-food expenditures we examine the results of specification 4.9 for disaggregated non-food expenditures. Figure 4.5 presents coefficient plots of the MMI, $\Delta \ln y_{hw} > 0$, and $\Delta \ln y_{hw} < 0$ for seven disaggregated categories of non-food expenditures: home improvement, donations, celebrations, donations, educational expenses, medical expenses, clothing, and ceremonial spending. Notably, the donations category has a positive MMI point estimate and a very wide confidence interval; some households seem to have made sizeable charitable contributions, and given the context it seems plausible that these donations remained in their communities. If so, this finding may support a hypothesis of local risk-sharing. Surprisingly, these findings include a null MMI effect on home improvements, which may be explained by in-kind donations of building materials and labor. On the opposite end, educational spending and clothing display negative point estimates with respect to MMI.

4.7 Smoothing Mechanisms

While the main specifications presented above establish the extent to which households were able to smooth consumption, they shed little light on the means by which consumption smoothing was achieved. Economic theory states that the Pareto efficient allocation of consumption within a village explored by the social planner's problem presented in section 4.3 can be supported by a competitive equilibrium with complete contingent markets. While complete contingent markets represent a strong assumption that normally fails to hold, certain features of the village economy may allow for institutional arrangements that approximate the Pareto efficient allocation of risk. Flow of information in a community may be sufficiently strong that the incidence of idiosyncratic shock to households' income is well-known, permitting community-level institutions to insure members against variable income without the problems of moral hazard and adverse selection that may affect an outside insurer.

In practice, the set of state-contingent markets has been understood to comprise the full range of formal and informal coping strategies available to the household to manage risk across time and space. Households may insure against consumption shocks through a diverse set of *ex ante* risk management strategies and *ex post* coping mechanisms. It seems unlikely that individuals adopt earthquake-specific *ex ante* risk management strategies. While substantial earthquake risk exists throughout Nepal due its orientation along the boundary of the Indo-Australian and Asian tectonic plates, devastating earthquakes occur infrequently; prior to 2015 the most recent serious earthquake occurred in 1934.

Because earthquake specific risk management *ex ante* seems unlikely, we focus our discussion of mechanisms on coping strategies after the shock. Qualitative data collected in focus group discussions as well as the descriptive statistics presented in table 4.1B suggest a coping response that included the following components:

1. Some degree of reduced consumption

2. Cash aid received from the government and/or NGOs

3. Borrowing

4. Sharing food stockpiles within one's community or network

Before moving on, we also acknowledge and rule out the possibility that households engaged extensively in other common coping strategies. In our own sample, less than 5% of households in the MMI>7 districts say they sold livestock as a coping strategy, sold other productive or non-productive assets, removed children from school, called a migrant home, or sent a migrant away.

With respect to cash aid, recall that the Ghorkha earthquake was met by the policy response from the Nepali government described in section 4.2 (although whether the scale or scope was sufficient remains a matter of debate), and with humanitarian aid from foreign governments and NGOs. Households in MMI>7 areas reported average cash aid of NPR 20,995 (roughly USD $210). Returning to table 4.2, column (2) presents results of the regression of the growth rate of total income *including* aid received in cash on the MMI measurement of income shock. After accounting for earthquake-related cash transfers, MMI appears to have a null effect on income. Therefore, the level of cash aid may have been sufficient to offset income losses induced by the earthquake on average. Nevertheless, cash aid may have been insufficient to support the additional non-food consumption expenditures induced by the earthquake as reported in table 4.5, which appears to have been achieved through extensive credit use.

4.7.1 Credit

As we noted in section 4.4, the descriptive statistics related to coping strategies suggest that many households accessed credit or bought food on account. In this section, we estimate earthquake treatment effects on change in log total borrowing, as well as change in log borrowing from different types of lenders. We are interested local risk-sharing arrangements. Therefore for the purpose of this analysis we group creditors into "outside source" lenders (banks, development banks, finance companies, microfinance), "local source" lenders (friends and family, cooperatives, savings and credit organizations, village money lenders, shopkeepers). Again, we use an empirical strategy similar to the above, replacing the outcome variable from specification 4.9 with the growth rate in borrowing:

$$\Delta \ln B_{hv} = \alpha + \gamma_1 \Delta \ln y_{hv}^{neg} + \gamma_2 \Delta \ln y_{hv}^{pos} + \theta_v MMI_v + \lambda X_{hv} + \epsilon_{hv} \tag{4.14}$$

where $\Delta \ln B_{hv}$ is the change in household log total loan balances, and all other variables are defined as above.

Results from equation (4.14) are reported in table 4.6. Columns (1) and (2) of table 4.6 examine the effect of changes in idiosyncratic income shocks, controlling for MMI and ward fixed effects, respectively. After controlling for MMI, negative idiosyncratic income shocks ($\Delta \ln y_{hv} < 0$) display strongly positive point estimates with respect to growth in total borrowing, significant at $\alpha = .05$ in the ward fixed effects specification. Positive idiosyncratic changes to income growth ($\Delta \ln y_{hv} > 0$) have the opposite sign and roughly equivalent magnitude, but fail to achieve statistical significance at conventional levels. Note well, however, that a Wald test of the null hypothesis of the equivalence of the postive and negative growth rate parameters easily rejects at $\alpha = 0.01$: overall, a negative income shock seems to be associated with increases in overall credit use, and vice-versa.

Columns (3) and (4) examine change in outside-source debt, while columns (5) and (6) consider changes to local debt. Overall, negative income shocks sharply reduce outside debt while dramatically increasing local borrowing. For local-source debt, the parameter estimates for $\Delta \ln y_{hv} > 0$ are 0.728 (controlling for MMI, significant at $\alpha = 0.01$) and 0.818 (ward fixed effects, $\alpha = 0.01$), hence a 100% loss of income would have been met with a roughly 73-82% increase in local borrowing. The parameter estimates for a negative income shock on outside-source borrowing range from -0.422 to -0.479 depending on the specification (significant at the 5% and 10% levels, respectively). A reasonable hypothesis consistent with the theory of risk-sharing is that households that suffered a negative income shock were less able to access outside (mostly formal) credit markets, and resorted to the local, informal credit market instead. We observe no statistically significant effect of a positive income shock on disaggregated outside/local borrowing.

Holding idiosyncratic shocks to income constant, the earthquake appears to have strongly and significantly increased overall borrowing (each unit of MMI increases the difference in log borrowing by 1.2) and with respect to disaggregated outside-source (an increase of 0.291 per unit of MMI) and local-source credit use (an increase of 0.875 per unit of MMI).

To the extent that formal credit markets are incomplete, local risk-sharing mechanisms may incorporate informal borrowing and lending. Table 4.7 further disaggregates the local-source lenders listed above into network (borrowing from friends and family, cooperatives) and non-network (village money lenders, shopkeepers, and savings and credit institutions) components. For network sources, the additive effect of an income shock is similar for households that experienced positive and negative income growth rates: point estimates are of the expected size and magnitude, ranging from 0.190-0.427 for network borrowing, and 0.235-0.360 for non-network borrowing depending on specification. Turning to non-network local borrowing, positive income growth may offset increases in non-network borrowing induced by the earthquake, while a negative income change increases it. In the ward FE specification a 100% increase in income implies a 39.7% decrease in non-network informal borrowing, while a decrease of the same magnitude implies an increase of 19.6%.

4.7.2 Causal mediation analysis

The analysis to this point is certainly suggestive of a mechanism: cash aid and informal borrowing seem to form the causal pathway by which households that experienced a negative income shock due to the earthquake smoothed non-food consumption. We test this hypothesis more formally using causal mediation analysis (CMA) as outlined in Imai, Keele, and Tingley (2010) and Imai, Keele, and Yamamoto (2010). The goal of CMA is to investigate causal mechanisms by examining the role of intermediate variables (mediators) that lie on the causal path between treatments and outcomes. Note carefully that in the analysis that follows, we define treatment R as the negative income shock $\Delta \ln y_{hv}$ and *not* the earthquake shock itself.

Imai, Keele, and Tingley (2010) define the causal mediation effect $\delta_h(r)$ for each household h using the following potential outcomes framework:

$$\delta_h(r) \equiv Y_h(r, M_h(1)) - Y_h(r, M_h(0)) \tag{4.15}$$

for a generic outcome Y_h, a binary treatment $R \in \{0, 1\}$ (where r is the realized value of the treatment)[12] and a binary mediator $M \in \{0, 1\}$. The causal mediation effect represents the indirect effect of the treatment on the outcome via the mediating variable. The following counterfactual question helps explain equation 4.15: "What change would occur to the outcome if one changes the mediator from the value that would be realized under the control condition, $M_h(0)$, to the treatment condition, $M_h(1)$, while holding the treatment status at r?" Similarly, define the direct effect of the treatment for each household h:

$$\zeta_h(r) \equiv Y_h(1, M_h(r)) - Y_h(0, M_h(r)) \tag{4.16}$$

which is the causal effect of R_h on Y_h, holding the mediator constant at the potential value that would occur at r. The parameter $\zeta_h(r)$ represents all mechanisms that affect the outcome

[12]We adopt the notation R and r rather than T and t (which is the standard in the CMA literature) to avoid confusion with the time-period notation in section 4.3.

other than the mediator M. Finally, it can be shown that the total effect of treatment can be decomposed into the causal mediation and direct effects:

$$\tau_h \equiv Y_h(1, M_h(1)) - Y_h(0, M_h(0)) = \frac{1}{2}\sum_{r=0}^{1}\{\delta_h(r) + \zeta_h(r)\} \tag{4.17}$$

Under the additional assumption that the causal mediation and and direct effects do not vary as functions of treatment status (the so-called no-interaction assumption), the mediation and direct effects sum to the total effect, yielding the simple decomposition $\tau_h = \delta_h + \zeta_h$. The percentage of the total effect transmitted to the outcome by the mediator can be calculated as $\frac{\delta_h}{\tau_h}$.

The average causal mediation effect (ACME, $\bar{\delta}(r)$) is given by the expected value of δ_h over relevant population. ACME is identified under the assumption of sequential ignorability (SI), which states (i) that R_h is as-if randomly assigned conditional on pre-treatment confounders X_i, and (ii) that $M_h(r)$ is as-if randomly assigned conditional on pre-treatment confounders X_i and treatment R_h.

The simple ACME is estimated by the following linear structural equation model (LSEM):

$$\begin{aligned} M_h &= \alpha_1 + \beta_1 R_h + \xi_1' X_h + \epsilon_{i1}, \\ Y_h &= \alpha_2 + \beta_2 R_h + +\gamma M_h + \xi_2' X_h + \epsilon_{i2} \end{aligned} \tag{4.18}$$

For this exercise, we designate a negative income shock as the treatment R, the change in log non-food consumption expenditures ($\Delta \ln NFC$) as the outcome Y_h, and examine the causal effect of two potential mediators M_h: informal borrowing ($\Delta \ln B_{hv}$) and cash aid ($\Delta \ln A_{hv}$). The vector X_h remains the same as above (with the addition of earthquake intensity, MMI). We estimate the LSEM 4.18 on the matched sample, clustering standard errors at the ward level.[13] Under the assumptions of sequential ignorablity and no-interaction the ACME ($\bar{\delta}$) is given by $\hat{\beta}_1\hat{\gamma}$, and direct effect ($\bar{\zeta}$) is given by $\hat{\beta}_2$. SI corresponds closely

[13]We use the *-medeff-* Stata package, which can accommodate a continuous treatment.

to the conditional independence assumption in propensity score matching and selection on observables models, and like the CIA cannot be tested directly. Note that first equation in the LSEM is identical to equation 4.14; we therefore advance the same arguments for the first stage of SI that we advanced for the identification of the treatment effect of $\Delta \ln y_{hv}$ on $\Delta \ln B_{hv}$. The second stage of SI is less clearly identified, and requires us to accept the assumption that changes to the mediator M are conditionally independent of income shocks and pre-earthquake confounders. This less clear, therefore we consider the results presented below instructive, if not strictly identified.

Table 4.8 presents the average direct and total effects of a negative shock to income on non-food consumption expenditures, and the ACME of the two proposed mediators. As expected, the total effects τ are comparable to the analysis above, with a point estimate of $\approx$ -0.081 to -0.084. In the absence of an informal borrowing mediator, the treatment effect ζ is -0.103; in the absence of cash aid, the treatment effect ζ is -0.098. The ACME for informal borrowing is estimated at 0.022, suggesting that informal borrowing attenuates the direct effect of a negative income shock by 25.2%. Likewise, the ACME for cash aid is estimated at 0.014, suggesting that cash aid attenuated the direct effect of a negative income shock by 16.2%. All parameters with respect to both mediators are statistically significant at $\alpha = 0.1$, with the exception of the ACME for cash aid.[14]

4.8 Policy implications and conclusion

The results described above present a clear set of policy implications. Earthquake victims seem to prefer local, informal debt to other coping strategies. At this point, we do not make any normative claims about the long-run welfare implications of a coping strategy that relies more heavily on borrowing and less extensively on reducing consumption, dissaving, smoothing assets, or other costly coping mechanisms. Assuming that earthquake victims are

[14] The ACME for cash aid only covers zero by $\frac{5}{1000}$, or approximately 1/3 of the mean effect.

rational, risk averse, and have a well-defined intertemporal utility function they will choose the least costly arrangement of *ex ante* coping mechanisms. Even so, informal debt may carry usurious interest rates, and therefore carry negative intertemporal welfare implications.

One possible interpretation of the results presented above is that cash aid, while important for consumption smoothing, was insufficient to smooth perfectly. The clearest policy recommendation we can make would be to increase the amount of cash grants. The problem with this is that not all victims needed a larger grant; those whose incomes were unaffected by the earthquake would receive the larger transfer even if it's not necessary to restore them to their *ex ante* utility. A better solution may be to offer low interest or zero interest loans.

The final essay in this dissertation presents an illustrative example from the same context and a related data set. In chapter 5, a subset beneficiaries of the HI SLVC program described in section 2.1, all of whom had been enrolled in the program for at least one year prior to the earthquake, received a one-time interest free loan of NPR 15,000. We find that recipients of the loan did not take on debt to the same extent that the earthquake victims in this analysis did. Further, they were shown not to reduce consumption by meal skipping, reducing meal size, or eating less expensive or preferred foods.

I. Instrumental	Not felt by many people unless in favourable conditions.
II. Weak	Felt only by a few people at best, especially on the upper floors of buildings. Delicately suspended objects may swing.
III. Slight	Felt quite noticeably by people indoors, especially on the upper floors of buildings. Many to do not recognise it as an earthquake. Standing motor cars may rock slightly. Vibration similar to the passing of a truck. Duration estimated.
IV. Moderate	Felt indoors by many people, outdoors by a few people during the day. At night, some awakened.
V. Rather Strong	Felt outside by most, may not be felt by some people in non-favourable conditions. Dishes and windows may break and large bells will ring. Vibrations like train passing close to house.
VI. Strong	Felt by all; many frightened and run outdoors, walk unsteadily. Windows, dishes, glassware broken; books fall off shelves; some heavy furniture moved or overturned; a few instances of fallen plaster. Damage slight.
VII. Very Strong	Difficult to stand; furniture broken; damage negligible in building of good design and construction; slight to moderate in well-built ordinary structures; considerable damage in poorly built or badly designed structures; some chimneys broken. Noticed by people driving motor cars.
VIII. Destructive	Damage slight in specially designed structures; considerable in ordinary substantial buildings with partial collapse. Damage great in poorly built structures. Fall of chimneys, factory stacks, columns, monuments, walls. Heavy furniture moved.
IX. Violent	General panic; damage considerable in poorly designed structures, well designed frame structures thrown out of plumb. Damage great in substantial buildings, with partial collapse. Buildings shifted off foundations.
X. Intense	Some well build wooden structures destroyed; most masonry and frame structures destroyed with foundation. Rails bent.
XI. Extreme	Few, if any masonry structures remain standing. Bridges destroyed. Rails bent greatly.
XII. Cataclysmic	Total destruction – everything is destroyed. Lines of sight and level distorted. Objects thrown into the air. The ground moves in waves or ripples. Large amounts of rock move position. Landscape altered, or leveled by several meters. In some cases, even the routes of rivers are changed.

Figure 4.1: Modified Mercalli intensity

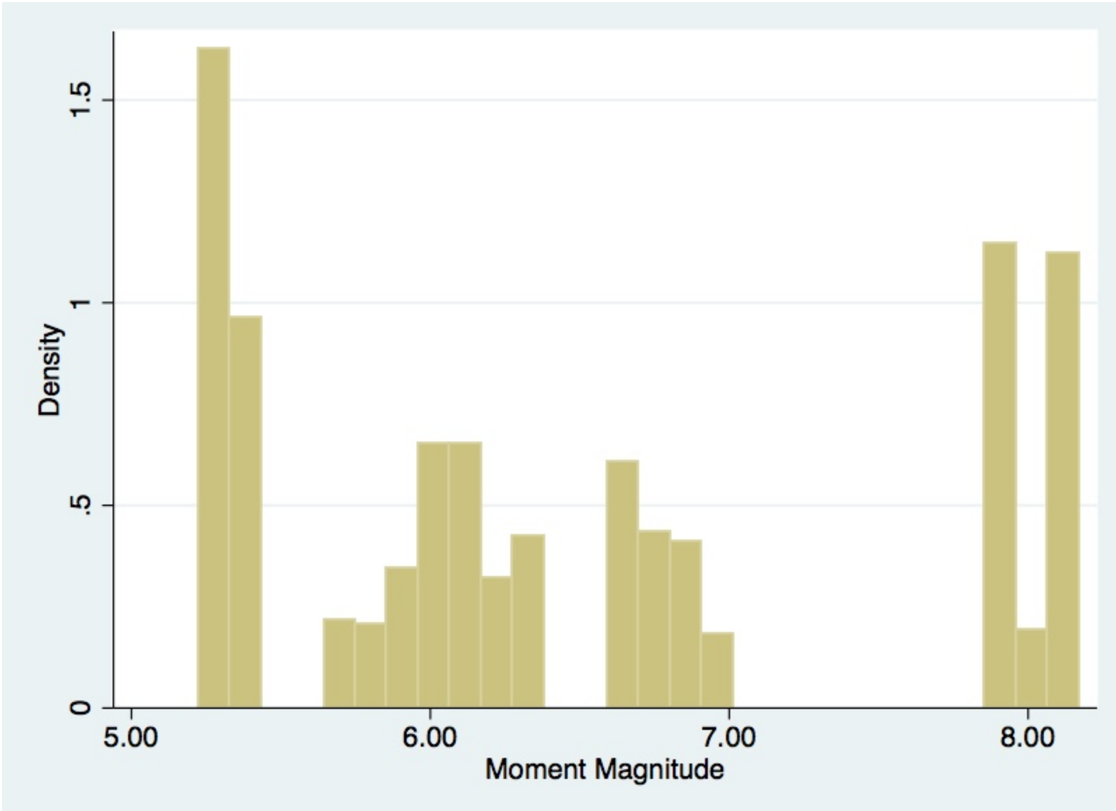

Figure 4.2: Distribution of moment magnitudes

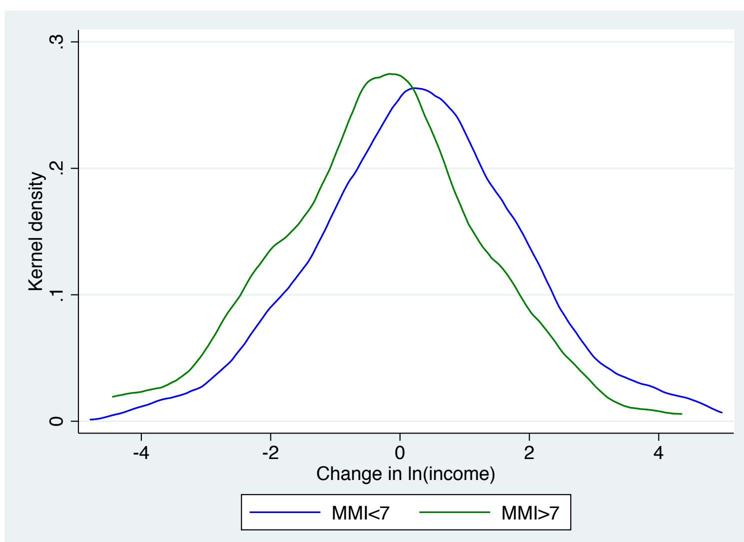

Figure 4.3: Distribution of $\Delta \ln y$

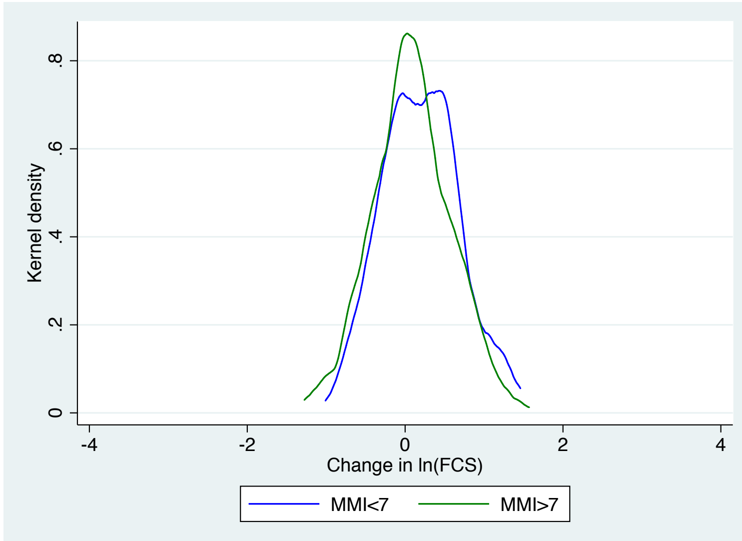

Figure 4.4: Distribution of $\Delta \ln FCS$

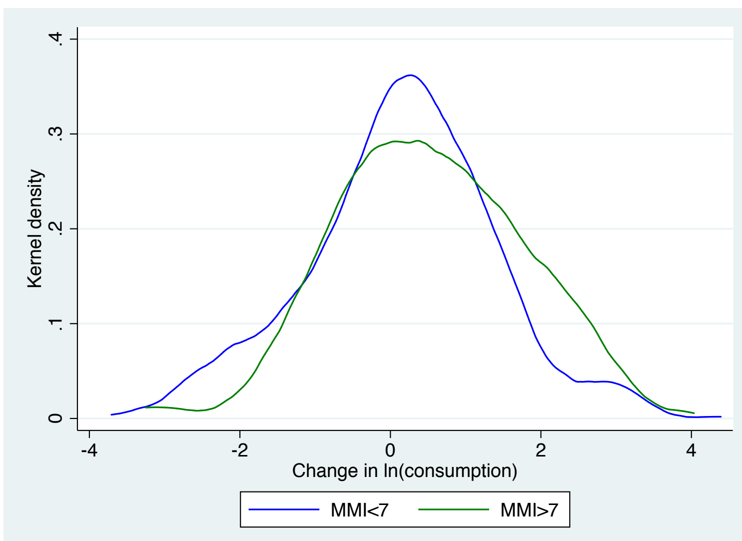

Figure 4.5: Distribution of $\Delta \ln NFC$

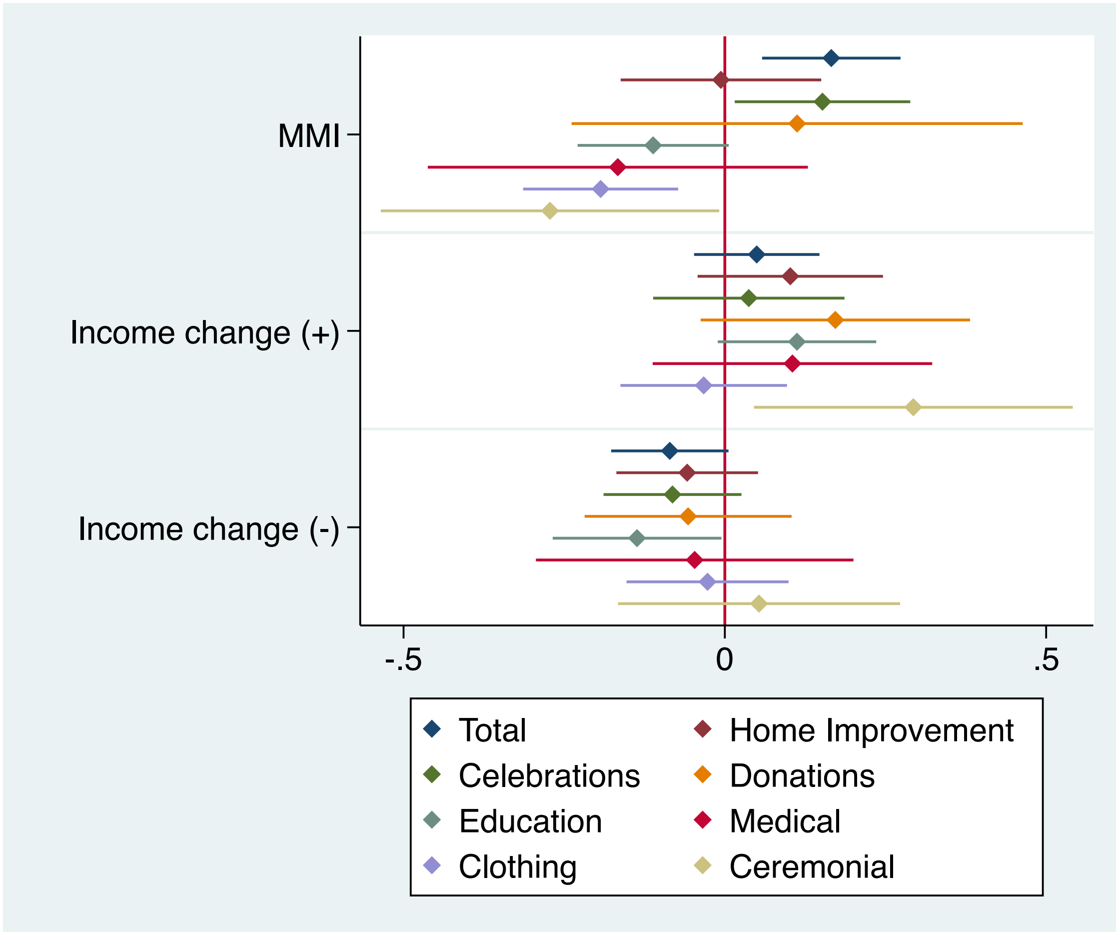

Figure 4.6: Disaggregated effects on consumption (90% CI)

Table 4.1: Baseline descriptives and balance

	Unmatched Sample (N=874)			Matched Sample (N=689)		
	MMI>7	MMI<7	Diff	MMI>7	MMI<7	Diff
Panel A: Earthquake						
MMI	8.02	5.95	2.07***	8.02	5.94	2.08***
	(0.10)	(0.55)	(0.04)	(0.10)	(0.57)	(0.05)
Red Card	0.89	0.07	0.83***	0.89	0.10	0.79***
	(0.31)	(0.25)	(0.02)	(0.32)	(0.30)	(0.04)
Panel B: Demographics						
HH Size	5.84	5.89	-0.05	5.88	6.08	-0.19
	(2.66)	(2.73)	(0.21)	(2.66)	(2.96)	(0.32)
Max Edu	8.63	9.61	-0.99***	8.72	8.57	0.15
	(3.80)	(3.26)	(0.27)	(3.81)	(3.72)	(0.38)
High Caste	0.40	0.25	0.15***	0.40	0.37	0.03
	(0.49)	(0.43)	(0.04)	(0.49)	(0.48)	(0.05)
Has Migrant	0.44	0.60	-0.16***	0.46	0.47	-0.02
	(0.50)	(0.49)	(0.04)	(0.50)	(0.50)	(0.05)
Farm HH	0.86	0.81	0.04	0.86	0.86	-0.00
	(0.35)	(0.39)	(0.03)	(0.35)	(0.35)	(0.03)
Panel C: Coping						
Reduce	0.29	0.07	0.22***	0.29	0.06	0.23***
	(0.45)	(0.26)	(0.02)	(0.45)	(0.24)	(0.04)
Borrow	0.47	0.09	0.38***	0.46	0.11	0.34***
	(0.50)	(0.29)	(0.03)	(0.50)	(0.32)	(0.05)
Share	0.27	0.02	0.24***	0.27	0.05	0.22***
	(0.44)	(0.15)	(0.02)	(0.44)	(0.22)	(0.04)
Cash Aid	25,158.14	1,456.62	23,701.52***	24,992.72	2,338.74	22,653.98***
	(9,232.36)	(5,777.14)	(533.28)	(9,368.80)	(7,022.41)	(979.74)
Panel D: Outcomes						
FCS	35.76	39.28	-3.52***	35.92	35.47	0.45
	(15.19)	(14.89)	(1.18)	(15.25)	(14.65)	(1.63)
Expenditures	19,315.63	24,408.19	-5,092.56*	19,468.81	19,867.67	-398.86
	(28,944.20)	(40,953.50)	(3,018.55)	(29,344.43)	(34,742.28)	(3,059.30)
HH Income	137,395.82	170,439.23	-33,043.41	133,874.28	133,521.39	352.89
	(260,836.15)	(303,395.14)	(23,053.41)	(259,090.53)	(230,731.40)	(24,121.08)
HH Debt	95,878.00	77,111.53	18,766.47	98,270.73	105,223.62	-6,952.89
	(231,607.12)	(188,308.17)	(15,692.79)	(235,542.90)	(242,029.92)	(24,269.47)
Panel E: Balance Tests						
	Pseudo R^2	$p> \chi^2$	Mean Bias	Med. Bias	Rubin's R	Rubin's B
Unmatched	0.295	0.000	18.8	15.6	147.08	0.90
Matched	0.014	1.000	3.7	3.1	27.97	0.79

Table 4.2: Earthquake effects on change in log income

	(1) Total	(2) Total+Aid	(3) Livestock	(4) Crop Sales	(5) Salaried	(6) Day Labor	(7) Remitttances
MMI	-0.254***	-0.024	-1.478***	-0.277	-0.226	-0.023	-0.351
	(0.087)	(0.087)	(0.427)	(0.319)	(0.297)	(0.190)	(0.223)
Constant	0.501	-0.485	9.326**	5.573	2.564	-3.313	0.912
	(1.261)	(1.133)	(4.483)	(4.115)	(3.647)	(3.294)	(3.119)
N	689	689	689	689	689	689	689
R^2	0.11	0.10	0.10	0.08	0.07	0.01	0.06

$*p < 0.1, **p < 0.05, ***p < 0.01$. Cluster robust (VDC) standard errors in parentheses.

Note: The table presents earthquake effects on total household income and income disaggregated by sector as specified by equation (4.11) in the text. All regressions are OLS with balancing weights as described in section 4.4, and include demographic controls.

Table 4.3: Earthquake effects on sector entry

	(1) Δ Q Sec.	(2) Livestock	(3) Crop	(4) Salary	(5) Labor
MMI	-0.106*	-0.088***	0.008	-0.029*	-0.020
	(0.052)	(0.022)	(0.012)	(0.015)	(0.014)
Constant	2.270***	0.788**	0.077	0.109	0.131
	(0.370)	(0.308)	(0.173)	(0.173)	(0.210)
N	689	689	689	689	689
R^2	0.02	0.11	0.03	0.06	0.05

$*p < 0.1, **p < 0.05, ***p < 0.01$. Cluster robust (VDC) standard errors in parentheses.

Note: The table presents earthquake effects on the probability that earthquake shocks induced a household to enter a sector. All regressions are OLS/LPM with balancing weights as described in section 4.4, and include demographic controls.

Table 4.4: Earthquake effects on sector exit

	(1) Livestock	(2) Crop	(3) Salary	(4) Labor
MMI	0.059**	0.040*	-0.009	-0.025*
	(0.024)	(0.023)	(0.015)	(0.014)
Constant	-0.148	-0.548*	-0.129	0.441**
	(0.242)	(0.323)	(0.208)	(0.218)
N	689	689	689	689
R^2	0.06	0.11	0.10	0.06

$*p < 0.1, **p < 0.05, ***p < 0.01$. Cluster robust (VDC) standard errors in parentheses.

Note: The table presents earthquake effects on the probability that earthquake shocks induced a household to exit a sector. All regressions are OLS/LPM with balancing weights as described in section 4.4, and include demographic controls.

Table 4.5: Impact of idiosyncratic income change on consumption

	FCS	FCS	NFC	NFC
$\Delta \ln y_{hv} < 0$	-0.008	-0.021	-0.112*	-0.115**
	(0.022)	(0.019)	(0.063)	(0.053)
$\Delta \ln y_{hv} > 0$	-0.003	-0.015	0.001	0.044
	(0.021)	(0.020)	(0.062)	(0.058)
MMI		-0.059***		0.164**
		(0.019)		(0.062)
N	689	689	688	688
R^2	0.58	0.48	0.28	0.14
MMI/FE	FE	MMI	FE	MMI

$*p < 0.1, **p < 0.05, ***p < 0.01$. Cluster robust (ward) standard errors in parentheses.

Note: The tables present parameter estimates based on specifications (4.8) and (4.9) from the text. Columns (1)-(2) present results for FCS; (3)-(4) present non-food consumption expenditures. Columns (1) and (3) model the earthquake as part of a ward fixed effect. Columns (2) and (4) control for the earthquake directly. Income shocks $\Delta \ln y_{hv}$ are continuous variables; ($\Delta \ln y_{hv} < 0$) is coded as an absolute value. Demographic controls and baseline levels of time-variant household characteristics are included in the regressions but not in these results.

Table 4.6: Impact of idiosyncratic income change on borrowing (all credit sources)

	Total	Total	Outside	Outside	Local	Local
$\Delta \ln y_{hv} < 0$	0.549**	0.489*	-0.479**	-0.422**	0.818***	0.728***
	(0.273)	(0.266)	(0.209)	(0.204)	(0.278)	(0.271)
$\Delta \ln y_{hv} > 0$	-0.476	-0.330	-0.126	-0.046	-0.008	0.115
	(0.324)	(0.316)	(0.159)	(0.161)	(0.351)	(0.322)
MMI		1.205***		0.291**		0.875***
		(0.278)		(0.135)		(0.265)
N	689	689	689	689	689	689
R^2	0.23	0.10	0.20	0.04	0.22	0.10
MMI/FE	FE	MMI	FE	MMI	FE	MMI

Table 4.7: Impact of idiosyncratic income change on borrowing (disaggregated local sources)

	Local	Local	Network	Network	Non-network	Non-network
$\Delta \ln y_{hv} < 0$	0.818***	0.728***	0.373	0.427*	0.360	0.235
	(0.278)	(0.271)	(0.293)	(0.245)	(0.316)	(0.262)
$\Delta \ln y_{hv} > 0$	-0.008	0.115	0.190	0.345	-0.250	-0.238
	(0.351)	(0.322)	(0.394)	(0.340)	(0.212)	(0.230)
MMI		0.875***		0.426		0.354
		(0.265)		(0.280)		(0.320)
N	689	689	689	689	689	689
R^2	0.22	0.10	0.26	0.07	0.24	0.07
MMI/FE	FE	MMI	FE	MMI	FE	MMI

$*p < 0.1, **p < 0.05, ***p < 0.01$. Cluster robust (ward) standard errors in parentheses.

Note: The tables present parameter estimates based on specification (4.14) from the text. Columns (1), (3), and (5) model the earthquake as part of a ward fixed effect. Columns (2), (4), and (6) control for the earthquake directly. Income shocks ($\Delta \ln y_{hv}$) are continuous variables; $\Delta \ln y_{hv} < 0$ is coded as an absolute value. Demographic controls and baseline levels of time-variant household characteristics are included in the regressions but not in these results.

Table 4.8: Causal mediation analysis

	M=Inf. Borrowing			M=Cash Aid		
		90% C.I.			90% C.I.	
Effect	Mean	*Lower*	*Upper*	Mean	*Lower*	*Upper*
ACME (δ)	0.022	0.009	0.038	0.014	-0.005	0.037
Direct (ζ)	-0.103	-0.183	-0.026	-0.098	-0.175	-0.023
Total (τ)	-0.081	-0.162	-0.002	-0.084	-0.164	-0.006
% of τ mediated	-0.252	-1.282	-0.105	-0.162	-0.717	-0.074

Note: Average causal mediation effects (δ) of informal borrowing and cash aid, following (Imai, Keele, and Tingley, 2010) .

Chapter 5

Essay Three: Effects of a Low-interest Loan on Resilience and Coping

5.1 Introduction

This essay builds on the findings of chapters 3 and 4. In addition to the more general welfare benefits that we have already considered in chapter 3, the earthquake offers the opportunity to test whether participating in the HI program enhanced ex-ante resilience to the earthquake. Further, in chapter 4 we showed that the coping response to the Nepal earthquake for untreated households included some reduction to household consumption, but also an increase in informal borrowing by households that experienced a loss of income. Recall that the households examined in the empirical exercise in chapter 4 were not beneficiaries of the Heifer program evaluated in chapter 3. In addition to the normal HI programming, a subset of HI beneficiaries received a one-time, flexible zero interest "revolving fund" (RF) loan of NPR 15,000 (USD 150) to address their most pressing needs after the earthquake. We therefore have the ability to assess the main policy recommendation arising from the findings of chapter 4: that an effective policy response to the earthquake might include easing liquidity constraints through cash transfers or low-interest disaster loans.

While the RF is officially a loan, it exhibits features of a cash transfer and may act as one. Cash transfers in the context of humanitarian aid are frequently justified on grounds that that they allow stricken households to direct the funds where they're most needed. In addition, it is often cheaper to get money to people than in-kind assistance because aid agencies face lower fixed costs and fewer logistical constraints. Studies have shown that food and debt repayment are common uses for humanitarian cash transfers, as is shelter following natural disasters that destroy homes, businesses, and infrastructure. In addtion, cash may be used to purchase a range of goods and services. In the Philippines following Typhoon Haiyan, for example, people reported using the cash for food, shelter, agricultural inputs, medicine, school fees, sharing, debt repayment, clothing, hygiene, fishing equipment and transport (Venton, Bailey, and Pongracz, 2015). When cash is received in installments over time, how the additional funds are spent may change over time. In response to the 2011 Somalia famine, for instance, initial cash transfers mainly went to food purchases and paying off debt, but the fraction spent on other items increased in subsequent transfers (Longley, Dunn, and Brewin, 2012).

This flexbilty of use implies that cash transfers may affect a wide range of outcomes including improving food security, helping households meet basic daily needs, ensuring access to shelter, and reestablishing liveliehoods in the wake of a natural disaster. With respect to food security, Schwab, Margolies, and Hoddinott (2013) find that cash transfers outperform other modalities as a food security safety net in Yemen. Hidrobo et al. (2014) find similar outcomes in Ecuador, and Bailey and Hedlund (2013) find that recipients of a cash transfer tended to increase the amount and diversity of foods that they eat. Going beyond food security, cash can also reduce the extent to which households employ negative coping strategies, such as dietary modifications, putting children to work and taking on hazardous jobs (Lehmann and Masterson, 2014). In Uganda, Gilligan and Hoddinott (2007) show that emergency cash transfers reduce children's anemia. Finally, impacts have also been observed on social capital, as people are able to repay debts, host others and contribute to ceremonies (Slater and Mphale, 2008).

In this essay, therefore, we ask two main research questions: To what extent did an asset transfer and training program enhance *ex ante* resilience to a natural disaster? And, to what extent did an interest-free loan (arguably a pseudo-grant/transfer) affect coping strategy selection *ex post* of a natural disaster? The results presented here suggest that both interventions improved food security outcomes. All HI beneficiaries score lower (better) on a modified version of the Coping Strategies Indicator for Food Security (CSI-FS). In addition, the sub-indicator components of the CSI-FS display statistically and economically significant improvements to meal-skipping, eating smaller meals, eating cheaper or less preferred foods, and to the probability of buying food on credit. Furthermore, while all beneficiaries enjoyed improved food security, beneficiaries who received the loan also took on less (potentially costly) debt from other sources.

The findings in this essay with respect to the treatment effects of the HI intervention contribute to the literature on ex-ante coping and livelihood strategies. The findings with respect to the RF loan contribute to the literature cited above dealing with cash transfers for disaster relief. To the extent that these findings are externally valid, they may help craft policy responses to other disasters.

5.2 Program

All of the communities considered in this empirical exercise are located in areas affected by the earthquake as described in chapter 2. In addition to operating the SLVC as described in chapter 2.1, Heifer also engaged in various relief activities. In the days immediately after the earthquake Heifer partners distributed food aid (mainly sacks of rice and lentils) and other in-kind aid (tarps, blankets, building materials) to earthquake victims via pre-existing self-help groups. Shortly after, HI offered a program called "Helping Earthquake Affected Livestock" (HEAL), which supported the reconstruction of destroyed livestock sheds and feeders. Both of these measures were offered to all Heifer beneficiaries (i.e. those who were enrolled in the RCT described in chapter 3, and pre-existing HI members participating in the SLVC but who were not enrolled in the RCT).

The cornerstone of their earthquake response was the Revolving Fund (RF), a zero-interest loan of NPR 15,000 (approximately $150 USD). The RF sought to provide communities and individuals with the flexibility to identify their own most pressing needs and allow to them to invest accordingly, thus RF loans were unrestricted and could be used for any purpose. RF loans were repayable to the borrower's SHG after 24-36 months, and SHGs were to retain all remitted funds for reinvestment in a group-level project chosen by the membership. Unlike the immediate in-kind aid and HEAL, the RF was not offered to RCT enrollees, but only to HI members *not enrolled under the RCT*.

Our sample included 554 households that were eligible to receive the RF, of whom 486 acknowledged actually receiving the RF and provided data on how they used the funds. The most common uses were food purchases (189 occurrences), small livestock purchases (149), and home repairs; less-frequently occurring uses included building or repairing livestock sheds (63), clothing (48), temporary shelters (45), medicines (43), and purchases of seed (36) and fertilizer (47).[1] About half the recipients (243 households) devoted their funds to a single use. Single-use beneficiaries most frequently used their RF funds to purchase small livestock (68 occurrences), repair their homes (39), purchase large livestock (36), build or repair livestock sheds (30), and purchase food (21).

5.3 Data and sample frame

5.3.1 Sample frame

The geogpraphic area encompassed by this study includes Dhading and Nuwakot, two districts[2] in the middle Hills that lie immediately east of the epicenter and were among the most devastated areas.

As described above, there are three types of households represented in this data set: "no-cash" beneficiary households (households that received the SLVC *but not* the RF), "cash" beneficiaries (households that received the SLVC *and* the RF), and comparison households. The no-cash subset of the sample comprises 10 VDCs and 203 treated households, originally selected to be part of the RCT described in chapter 3, but removed from that study after

[1]Note that these total to greater than 486 because funds could apply to more than a single use

[2]Nepal comprises 75 administrative districts, which are further subdivided into village development committees (VDC), with each VDC consisting of nine wards. Wards most closely approximate the popular perception of a 'village'. In this paper the unit of analysis is the household; VDC is the unit of treatment and the unit at which covariant earthquake shock is measured.

the earthquake to allow HI to provide necessary relief without biasing the RCT. For the no-cash sample, the intervention commenced for direct beneficiaries in treated wards in the fall of 2014, allowing for technical trainings, values-based training, and formation of self-help groups prior to the earthquake in April 2015. At the time of the earthquake, goats had not been delivered and indirect SHGs remained unformed. At the time of data collection (June 2016) goats had been delivered to direct beneficiaries, and indirect SHGs had been formed and trained but had not yet received any goats.

The "cash" subset comprises 16 VDCs and 585 treated households. At the time of the earthquake in April 2015 the earlier entrants should have had to time to establish goat herds capable of generating a regular and sustainable stream of income. Later beneficiaries (late 2013 and 2014) are less likely to have established sustainable herds, are more likely to resemble no-cash beneficiaries in terms of their accumulated benefits. This implies a challenge to identifying the "cash" effect.

The sample included 11 untreated comparison VDCs with 514 households. The structure of the sample in the untreated VDCs does not correspond perfectly to the structure of the sample in treated VDCs. In untreated VDCs, households were sampled at random from a randomly selected ward, while in treated VDCs we sampled the entirety of a tole directly targeted for the HI program, and then randomly sampled the remaining households in the ward outside the targeted *tole*.[3] To account for this disparity, we apply sampling weights to directly targeted HI beneficiaries that reflect the probability of having been randomly selected.[4]

5.3.2 Data

The data are from a household survey conducted in June, 2016. The survey instrument collected demographic features of the household, income, expenditures, credit use, food security

[3] See the discussion of sampling in chapter 3 for details.

[4] Details of the weights are discussed below in 5.5.

and dietary diversity, and a complete household roster. It also included modules to measure the impact of the earthquake at the household level and a battery of questions to capture coping responses to the earthquake.

Descriptive statistics

The leftmost "unmatched" columns of tables 5.1, 5.2, and 5.3 present the means and standard deviations of key demographic and descriptive statistics for each of the sub-groups noted above, and equality of means tests for possible comparison. The most basic demographic characteristics appear to be mostly balanced across treatments: the average beneficiary age is approximately 42 years old, belongs to a household of about 5.5 people with an average age around 30. The average household is split roughly 50/50 between men and women and has a 55% chance of having at least one migrant.

Within each comparison non-trivial imbalance exists for several variables that may affect welfare outcomes. Non-cash beneficiaries are less likely to be high-caste Hindus than the untreated comparisons, and cash beneficiaries appear to be more likely to be high-caste Hindu. Further, based on 2011 census data, both non-cash and cash VDCs were more populous than controls, but were also slightly less densely populated. Based on Foster-Greer-Thorbeck (Foster, Greer, and Thorbecke, 1984) poverty measurements non-cash VDCs appear to be slightly more impoverished in terms of headcount poverty rates, and cash VDCs slightly less, than control VDCs. The identification strategy outlined below attempts to mitigate this imbalance.

To quantify the effect of the earthquake we use the modified Mercalli intensity (MMI) scale as described in chapter 4, which uses a rating of earthquake intensity based on locally observed effects, composed of increasing levels of intensity that range from imperceptible shaking to catastrophic destruction. We present two estimates of earthquake intensity as measured by MMI: a MMI imputed from recorded ground motions captured by the United States Geological Survey and measured at the household level, and an unofficial, ward level estimate based on the recall (one year ex-post, at the time of data collection) of a community leader. A MMI of 8 corresponds to a severe earthquake, with "great damage in poorly built structures," a description likely to apply to most homes in the study area. In addition to MMI we report the percentage of in-sample families that received a red earthquake card. In the weeks following the earthquake the Nepali government assessed earthquake damage and assigned a red or yellow card to eligible families; a red card indicated "complete damage" and entitled the homeowner to RS 15,000 in rebuilding funds, while a yellow card indicated "partial damage" and entitled the holder to RS 3,000. Over 90% of the respondents reported receiving a red card.

Outcomes

Qualitative studies (Asia Foundation, 2016) and the findings of chapter 4 suggest that in the absence of an NGO intervention households coped with the effects of the earthquake by adopting various food security coping tactics, by borrowing informally from village money lenders, friends and family, and by purchasing food and other goods on account from local shopkeepers. These findings align with our own quantitative results presented in chapter 4.

The elements of the SLVC intervention and the patterns of use of RF funds detailed above most clearly suggest that the two HI programs should have improved food security outcomes, attenuated reductions to livestock herd sizes, and protected income from livestock. The ability to maintain savings and smooth consumption are also important indicators of resilience. Outcomes of interest therefore include whether or not households adopted various coping strategies at the time of the earthquake (described below), levels of livestock and crop income in the year after the earthquake, savings behavior one year after the earthquake, and accumulated debt one year after the earthquake.

We examine three main categories of outcomes:

- General welfare indicators including assets and income one year post-earthquake
- Food security coping strategy selection (as summarized by the CSI-FS)

- Borrowing and saving

Coping strategy indicators include whether a household stated that they sold livestock specifically in order to cope with the earthquake (yes/no), whether a household stated that took out loans specifically to cope with the earthquake (yes/no), revenue from the sale of livestock and the sale of crops in the year after the earthquake, and a locally-adapted Coping Strategies Index for Food Security (CSI-FS) (Maxwell, 1996). The CSI-FS is calculated by collecting data on a household's specific food security related coping strategies, assigning those activities weights to reflect their severity, and assigning a frequency weight that indicates how often a household used the coping mechanism. The index is a household's severity and frequency weighted sum of strategies employed:

$$CSI = \sum_{c}^{C} w_c \times frequency_c \tag{5.1}$$

The components of the index include whether a household cut meals ($w_c = 3$), whether a household ate smaller meals ($w_c = 2$), whether a household consumed less expensive or less preferred foods ($w_c = 1$), and whether a household bought food on account ($w_c = 2$). Maxwell shows that the CSI-FS correlates well with more complex measurements of food security (Maxwell et al., 2003). We also report each of the components as a binary outcome. Note that the CSI-FS data is based on recall; the survey instrument asked respondents to describe their use of coping strategies in the (roughly) six month period after the earthquake.

Financial outcomes deal with saving and borrowing behavior. Unlike the food security coping data, these outcomes reflect timing of the survey, or one year after the earthquake. These include a binary variable indicating whether or not the respondent put any money in savings in the preceding month, the amount placed in savings in the preceding month, total outstanding household debt, the total amount of debt owed to formal lenders, and the total amount of debt owed to informal lenders. The levels of debt reported here are *exclusive* of the RF loan itself. To measure household debt, we asked the respondent to list all of the outstanding loans and credit accounts for which any member of the household was responsible. For each loan the survey instrument also collected the type of lender and purpose of the loan. For the purposes of this analysis we consider formal lenders to be banks, development banks, finance companies, savings and credit groups, and savings cooperatives. Informal sources include friends and family members, village money lenders, and shopkeepers who extend credit or allow purchases on account.

In addition to food security outcomes and financial outcomes, we also consider additional well-being indicators that may be affected by membership in a Heifer SHG or receipt of the RF. Specifically, these are the probability that the household sold livestock to cope with the shock, goat herd size, revenue from livestock related sources, and revenue from the sale of crops.

5.4 Empirical strategy

Our identification strategy is conceptually simple. We have a multilevel treatment:

I. Households that received the HI intervention ("*No-cash*")

II. Households that received the HI + RF intervention ("*Cash*")

III. Households that received neither ("*Controls*")

i. Comparing (I) to (III) gives base intervention effect.

ii. Comparing (II) to (III) gives base intervention + RF effect.

iii. Comparing (I) to (II) gives the RF effect.

The no-cash sample identified potential direct beneficiaries *ex ante*. The cash sample comes from the roster of actual self-help group members. We therefore drop non-compliers from no-cash sample to ensure comparability to the cash sample, and define the causal parameter of interest to be the average treatment effect on the treated (ATT):

$$\tau_{ATT} = E(\tau|T=1) = E[Y(1)|T=1] - E[Y(0)|T=1] \tag{5.2}$$

or the difference between the expected value of outcome Y with and without treatment for those who actually took treatment.

Identification strategy

Neither cash nor no-cash households were randomly assigned to treatment. In this section, we formalize the causal inference problem and propose a propensity score matching (PSM) estimator. We make the following identifying assumptions, which are sufficient to define ATT using propensity score matching (PSM):

$$Y(0) \perp T|X \tag{5.3}$$

and

$$P(T=1|X) < 1 \tag{5.4}$$

which correspond to the canonical conditions established in Rosenbaum and Rubin (1983). By selecting and appropriately weighting observations on common support (condition 5.4), matching methods eliminate two of the three potential sources of bias outlined in Heckman, Ichimura, and Todd (1998): bias arising due to difference in the supports of X between the treated and comparison groups, and bias attributable to the differences between the two groups within the distribution of X over common support. Just as OLS does, however, matching relies on the conditional independence assumption set forth under condition (5.3), which assumes away selection on observables, the potential third source of bias. Balance tests on the matched sample can demonstrate that the CIA fails to hold, but not affirmatively prove that it does. In this application, conditional independence holds if, conditional on X, there remains no unobserved heterogeneity that affects participation in the loan and no-loan treatment groups.

Defending the CIA requires knowledge of the factors that account for selection into each program. HI intervenes on the VDC level and excludes no one, and they expect near universal program uptake in these VDCs. Heifer's decision to intervene, then, is a primary factor driving program participation. This decision is based largely on need, so we reason that headcount poverty is correlated with unobservables and adequately controls for VDC level factors that influence the decision to intervene. In chapter 3 we showed that household-level selection into the HI program is very high, if not quite universal in treated VDCs; close to 90% of household that have the opportunity choose to join SHGs. To account for the relatively small number of households who decline to participate, we include time-invariant demographic and household-level characteristics in the matching model.

The framework above can be seen as a multi-level treatment consisting of two treatments, each sharing a common comparison group. Propensity score matching in the multinomial case is possible[5], but is computationally burdensome and raises the possbility that a misspecification of one of the comparisons will compromise all the others. A practical alternative that we adopt is to estimate a series of binomial models. Lechner (2001) suggests such a binomial-series approach and compares it to the multinomial probit, finding little difference in performance.

Therefore, to begin we estimate separate binomial logit models for each comparison (i-iii) above:

$$P(T = 1|X) = \frac{e^{\{\alpha+X\beta\}}}{1 + e^{\{\alpha+X\beta\}}} \tag{5.5}$$

Where X is a vector of time invariant VDC, demographic, and household level characteristics thought to affect program participation and outcomes. The matching model used here included VDC/ward level variables (population density of the VDC in 2011, headcount poverty in VDC in 2011)[6], earthquake-related variables (modified Mercalli intensity,

[5] See Lechner (2001) for discussions of PSM with multinomial logit.

[6] N.B. The 2011 census predates any HI intervention in the sample, and is therefore appropriate for matching.

percentage of households in the individuals home ward displaced by the earthquake), and demographic controls (caste/ethnicity, literacy, household size, whether the household has a migrant, average age in the household, permanent (i.e. salaried) income). We arrived at the precise composition of X found to balance the matched sample iteratively, and it varied only slightly between the three comparisons.

For each household h equation (5.5) gives the propensity score, or $P_h = \Pr(T = 1|X)$. Next we apply two commonly used matching algorithms to our data set: kernel matching (KM) and nearest neighbor matching (NNM). As sample size grows all PSM estimators approach a comparison of exact matches, and therefore yield the same results asymptotically (Smith, 2000). In small samples, however, the choice of matching algorithm may imply a trade off between bias and variance and therefore becomes important (Heckman, Ichimura, and Todd, 1997).

For this application, we prefer the KM matching algorithm, a nonparametric matching estimator that uses all of the observations on common support from the comparision group to construct the counterfactual outcome for each treated household. This differs from NNM, which may use far fewer observations from the comparision group. The major advantage of KM comes from the lower variance achieved by using more information, which comes at the cost of (potentially) increased bias from including poor matches. Heckman, Ichimura, and Todd (1998) derives the asymptotic properties of the KM estimator. Smith and Todd (2005) notes that KM can be thought of as a weighted regression of the counterfactual outcome on an intercept with the weights given by the kernel weights. Formally, the weights are defined as follows:

$$w_{ij} = \frac{K(\frac{P_h - P_j}{h})}{\sum_{j \in C_h^0} K(\frac{P_h - P_j}{h})} \tag{5.6}$$

Where $K\{\cdot\}$ is the kernel function and h is a bandwidth parameter. So long as one uses weights from a symmetric, nonnegative, unimodal kernel, then the KM estimator will apply greater weights to observations with P_j close to the treated household and lower weights to observations with more distant P_j. We employ the Epanechnikov distribution with a bandwidth of 0.06, but note that exploratory analysis indicated that neither the selection of kernel function or nor bandwidth had a material effect on balance or outcomes.

The main panel of tables 5.1 and 5.2 present standard t-tests for covariate balance of X on the unmatched and matched samples for the no-loan and loan treatment groups to untreated comparison, respectively. Such tests are a useful first-pass assessment on matching quality, but should be supplemented by other tests such as the standardized bias of covariates and/or joint significance of X on the reestimated propensity score in the matched sample. Turning first to column (3) of 5.1A, note that the unmatched sample displays imbalance in three variables that may plausibly affect selection into the treatment group: whether the household is high-caste, the headcount poverty rate in the household's home VDC in 2011, and the population density in the household's home VDC in 2011. Additionally, three earthquake related variables are out of balance, each of which may reflect underlying features of the locality that may have affected HI's decision to intervene there. Matching the sample as described above and checking again for balance, we find that the most problematic variables reject the null in a t-test for equality of means. Turning next to 5.2A, we find a similar pattern: caste, headcount poverty (2011), and population density (2011) are imbalanced in the unmatched sample, but balanced after we apply matching.

The bottom panel of tables 5.1 and 5.2 present additional balance tests. First, as Sianesi (2004) suggests, we examine the pseudo-R^2 of the propensity score equation (5.5) reestimated on the matched data. Prior to matching the pseudo-R^2 for the no-loan comparison is 0.45 (table 5.1); after matching the pseudo-R^2 is reduced to 0.105. Furthermore, an LR test for the joint significance of X in the unmatched sample easily rejects the null ($p = 0.000$), while the same test fails to reject in the matched sample ($p = 0.841$). Once again a similar pattern holds for the loan-group comparison: original pseudo-R^2 of 0.145 is greatly attenuated in the reestimation of of equation (5.5) on the matched data, and LR goes from easily rejecting ($p = 0.000$) to failing to reject ($p = 0.872$).

A final metric of matching quality is also given by the mean and median values of the standardized bias (SB) of the covariates in X (Rosenbaum and Rubin, 1985). For each covariate in X SB is defined as the difference in sample means in the treated and control subsamples as a percentage of the square root of the average sample variances in both groups:

$$SB = 100 \cdot \frac{\bar{X}_1 - \bar{X}_0}{\sqrt[2]{0.5 \cdot (V_{1M}(X) + V_{0M}(X))}} \tag{5.7}$$

Unlike t-tests, SB is not sensitive to sample size and may therefore offer a better test of balance. Generally SB for any given covariate belonging to X should be under 5%. In the matched no-loan comparison group, mean SB is 8.6% (median 7.0%), which is above the heuristic but still sharply reduced from the unmatched sample. In the matched loan comparison group, mean SB is 3.1% (median 1.9%). Taken as a whole, the diagnostics presented in tables 5.1 and 5.2 suggest the matching procedure substantially improved balance across the support of X.

Table 5.3 reports balance with respect to the cash to no-cash comparison. Matching reduces common support much more than the other two comparisons (N=870 to N=537), and leaves several important variables out of balance. Further, while the pseudo-R^2 is much lower in the the matched sample and the χ^2 statistic is much lower, it still easily rejects the null hypotheses that the matching covariates jointly equal zero ($p = 0.000$). While attenuated in the matched sample, mean and median SB are both well above the rule-of-thumb levels. For these reasons, although the results of this comparison are broadly consistent with the pattern of outcomes established in comparison (i) and (ii), we interpret the results of this comparison (iii) with caution.

Nearest-neighbor robustness check

NNM is conceptually simpler than KM, and offers the the advantage of using only the closest matches from the comparison group. This reduces bias, at the cost of a smaller effective sample size and increased variance. In post-matching diagnostics we show the NNM estimator to balance the sample slightly less effectively than KM, we therefore report it as a robustness check. In NNM estimation every treated unit is compared with one or more untreated unit that is similar in terms of the propensity score. Define the set of M closest matches to h among the untreated observations ($C_h^0(M)$) with replacement as

$$C_h^0(M) = \{l = 1, \cdots, N | T_l = 0, |P_h - P_l| \leq d_i(M)\} \tag{5.8}$$

where M is the number of matches, P_h indicates the propensity score for household h. Define $d_i(M)$ as the distance from household h to the M^{th} closest match in the untreated group:

$$\sum_{l:T_l=0} 1\{|P_h - P_l| < d_i(M)\} < M$$

and

$$\sum_{l:T_l=0} 1\{|P_h - P_l| \leq d_i(M)\} \geq M$$

where $1\{\cdot\}$ is the indicator function that returns 1 when the value in the brackets is true, and zero other wise. We construct the matched datset by implementing this method using the five nearest neighbors, imposing common support and weighting each control observation by number of times it is used as a match.

5.5 Estimation and results

We implement the PSM model by conducting a WLS regression on the matched sample. We do this mainly to facilitate the calculation of wild-cluster bootstrap standard errors, which are appropriate due to the small number of treatment clusters.[7] We note, however, that this

[7]The standard method of generating the wild-cluster bootstrap t-statistic requires imposing a sharp-null hypothesis on the treatment effects estimator, which is computationally most straightforward in regression framework.

practice is consistent with the advice of Ho et al. (2007), who suggest that matching may be most effective when used as a pre-processing technique.

The empirical specification is:

$$y_{hv} = \beta_0 + \beta_1 T^g_{hv} + \mathbf{X'_{hv}}\gamma + D_h\rho + \varepsilon_{hv} \tag{5.9}$$

Where y_{hv} is outcome y for household h in VDC v, T^g_{hv} is a binary treatment indicator, and g indicates one of the three comparisons outlined above, X'_{hv} is the same vector of household characteristics used to match the comparison sample, D_{hv} is a district fixed effect, and β_0, β_1, γ, and ρ are parameters to be estimated. When the comparison is between cash and no-cash (g =iii), we also include a dummy variable in X that takes a value of one for "early" beneficiaries, defined as those who belong to wards where SHG formation took place prior 2014, to control for duration in the program.

In addition to the propensity score derived weights described above, we also apply survey weights to a subset of the data to reflect their probablity of having been included in the sample. Recall from 5.3.1 that comparison households were drawn at random from a selected ward, with a average probablity of selection of approximately 0.20. Direct beneficiaries, however, were all taken from a pre-specified neighborhood with a probablity of selection of unity; we therefore apply a sampling weight of 0.20 to direct beneficiaries to impart an equal weight to the comparison.[8] Following DuGoff, Schuler, and Stuart (2014), this enters a simple multiplicative factor of the PSM weight. We cluster standard errors at the level of treatment (VDC) level using the wild-cluster bootstrap t procedure as proposed by Cameron, Gelbach, and Miller (2008).

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[8]Note that his adjusment only applies to comparisons i. and ii., for comparison iii. all members of the sample were selected with equal probability.

5.5.1 General coping and outcomes

Table 5.4 presents treatment effects on a range of coping strategy indicators and welfare outcomes for the three comparisons specified above: cash vs. untreated (columns (1) and (2)), no-cash vs. untreated (columns (3) and (4)), and cash vs. no-cash (columns (5) and (6)). For each comparison, the left column presents the mean and standard deviation for the outcome in the matched counterfactual.

Turning first to the cash/untreated comparison (i), we find large and highly significant decreases in the CSI-FS, and in the probablity that a household took out loans as a coping strategy. The treatment effect on CSI-FS is -4.030 from a counterfactual mean of 6.045. The treatment effect on the probability of taking out a loan is -21.4 percentage points, compared to a counterfactual mean of 44.9%. We also find that cash-treated households maintain larger goat herds (+1.338 goats) but, counter intuitively, do not own more TLUs overall.

The no-cash vs. untreated comparison (ii) presents a similar profile of outcomes. No-cash beneficiaries show lower CSI-FS compared to their matched counterparts (-3.338, counterfactual mean 4.008), have bigger goat herds (+3.095 goats). Most importantly, the no-cash group was no less likely to have taken out loans to cope with the effects of the earthquake.

Finally, comparison (iii) considers cash vs. no-cash, and broadly confirms the overall dynamic suggested by comparisons (i) and (ii): there is no statistically significant difference in the CSI-FS between the groups.

5.5.2 Food Security

Table 5.5 disaggregates the components of the CSI-FS. Both cash and non-cash households boast a significantly lower CSI-FS than the comparison group (table 5.4). The point estimate is lower for cash households than it is for non-cash, but the difference between the two treated groups is not significant. From a food security standpoint, both types of Heifer households appear to have coped more effectively with the earthquake than non-Heifer households.

Treatment effects on components of the CSI-FS are reported in table 5.5. Cash and non-cash households were less likely to have cut meals as a coping strategy (-20.2 to -17.3 percentage points compared to their counterfactual). In addition, cash beneficiaries were less likely to have eaten smaller meals, less expensive/preferred foods, or to have bought food on account. Non-cash beneficiaries are similarly less likely to have eaten less expensive/preferred foods, and there is slightly weaker evidence that they ate smaller meals when compared to controls.

Finally, cash recipients were much less likely than controls to have bought food on account (-16.6 percentage points, comparison (i)), while non-cash beneficiaries were equally likely (comparison (ii)). This is consistent with the broader finding, described below, that RF households borrowed less money to cope with the earthquake, and that they relied less on informal credit in particular.

5.5.3 Credit and Savings

In table 5.6 we look at treatment effects on borrowing and savings behavior. Both non-cash and cash beneficiaries were significantly more likely to have saved money in the month prior to the survey than control households. However, the amounts saved in the preceding month do not differ significantly from controls among cash beneficiaries, and are actually lower among non-cash.

Consistent with the results of chapter 4, earthquake affected households seem to have primarily coped with the shock by taking on debt. Based on the results presented here, cash beneficiaries seem to have been much less reliant on debt to cope with the earthquake, with a total outstanding debt lower than controls by $600 USD. This is especially striking considering that the size of the transfer was only $150 USD. On the other hand, non-cash debt loads do no differ from controls in estimated magnitude or statistical significance. Further, cash beneficiaries exhibit significantly lower levels of informal debt (-$285) when compared to the control group, while non-cash beneficiaries actually show higher levels of informal debt (+\$361). The direct cash vs. no-cash comparison (iii) seems to confirm the relationship implied by (i) and (ii): cash recipients carried \$403 less debt overall compared to non-cash HI beneficiaries. This disaggregates to a slightly higher levels of formal debt (+\$102.935), and lower levels of informal debt (-\$473.67).

5.6 Conclusion

Qualitative studies and the quantitative results presented in chapter 4 strongly suggest that additional grants or loans may have improved coping and resilience outcomes for household in the earthquake area.

We evaluated program outcomes for two groups of HI beneficiaries: those who received a zero-interest loan of NPR 15,000 and those who did not. The results presented here suggest that both interventions improved food security outcomes. All HI beneficiaries score lower (better) on a modified version of the Coping Strategies Indicator for Food Security (CSI-FS). In addition, the sub-indicator components of the CSI-FS display statistically and economically significant improvements to meal-skipping, eating smaller meals, eating cheaper or less preferred foods, and to the probability of buying food on credit. Furthermore, while all beneficiaries enjoyed improved food security, beneficiaries who received the loan also took on less (potentially costly) debt from other sources.

Table 5.1: Descriptives and balance: cash/untreated

	Unmatched Sample ($N = 1034$)			Matched Sample ($N = 954$)		
	Treated	Comparison	Diff	Treated	Comparison	Diff
HH Size	5.47	5.32	0.15	5.39	5.576	-0.181
	(2.44)	(2.49)	(0.15)	(2.43)	(2.344)	(0.230)
Resp. Age	42.26	42.88	-0.61	41.67	42.930	-1.261
	(13.23)	(13.55)	(0.84)	(13.53)	(13.665)	(1.381)
Average Age	30.74	30.88	-0.14	30.64	31.408	-0.764
	(11.28)	(12.21)	(0.73)	(11.62)	(11.710)	(1.059)
High Caste	0.47	0.38	0.08***	0.42	0.393	0.027
	(0.50)	(0.49)	(0.03)	(0.49)	(0.489)	(0.047)
Hindu	0.86	0.86	-0.01	0.83	0.844	-0.017
	(0.35)	(0.35)	(0.02)	(0.38)	(0.363)	(0.037)
Resp. Edu	2.16	1.90	0.26	2.16	2.317	-0.161
	(3.64)	(3.52)	(0.23)	(3.58)	(3.986)	(0.400)
Has Migrant	0.54	0.50	0.04	0.51	0.528	-0.017
	(0.50)	(0.50)	(0.03)	(0.50)	(0.500)	(0.049)
Farm HH	0.95	0.93	0.03*	0.96	0.918	0.046*
	(0.21)	(0.26)	(0.01)	(0.18)	(0.274)	(0.025)
Pp. Den. (2011)	272.67	284.25	-11.57**	272.10	277.347	-5.250
	(46.50)	(102.92)	(4.78)	(49.47)	(89.622)	(7.549)
HC Pov. (2011)	16.21	18.17	-1.97***	17.10	16.828	0.269
	(4.77)	(3.43)	(0.27)	(4.43)	(3.855)	(0.402)
Road Access	0.51	0.45	0.06*	0.45	0.399	0.053
	(0.50)	(0.50)	(0.03)	(0.50)	(0.490)	(0.047)
MMI Max	7.89	7.90	-0.01	7.92	7.896	0.025*
	(0.17)	(0.11)	(0.01)	(0.16)	(0.117)	(0.013)
Ward MMI	6.43	7.73	-1.31***	6.56	7.848	-1.286***
	(2.07)	(1.47)	(0.12)	(2.12)	(1.370)	(0.160)
%Displaced	0.49	0.63	-0.15***	0.49	0.491	-0.003
	(0.50)	(0.48)	(0.03)	(0.50)	(0.500)	(0.049)
Red Card	0.96	0.91	0.05***	0.96	0.903	0.055**
	(0.20)	(0.29)	(0.02)	(0.20)	(0.297)	(0.027)

Balance Diagnostics

	Pseudo R^2	p> χ^2	Mean Bias	Med. Bias	Rubin's R	Rubin's B
Unmatched	0.145	0.000	17.9	11.2	92.07	0.70
Matched	0.005	0.872	3.1	1.9	17.14	0.78

Means, standard deviations, and difference in means and the t-statistic from the regression of the variable on a dummy variable taking a value of 1 if MMI>7. Columns (1)-(3) use unweighted data; columns (4)-(6) balancing weights as described in section 5.4

Table 5.2: Descriptives and balance: no-cash/untreated

	Unmatched Sample (N = 730)			Matched Sample (634)		
	Treated	Comparison	Diff	Treated	Comparison	Diff
HH Size	5.92	5.32	0.60***	5.96	6.470	-0.513
	(2.51)	(2.49)	(0.19)	(2.67)	(3.202)	(0.499)
Resp. Age	42.86	42.88	-0.02	42.19	40.915	1.277
	(14.07)	(13.55)	(1.05)	(14.11)	(12.088)	(1.942)
Average Age	30.02	30.88	-0.87	29.82	27.715	2.106*
	(10.64)	(12.21)	(0.88)	(9.52)	(9.051)	(1.179)
High Caste	0.15	0.38	-0.23***	0.27	0.277	-0.008
	(0.36)	(0.49)	(0.03)	(0.45)	(0.448)	(0.071)
Hindu	0.85	0.86	-0.01	0.93	0.712	0.219***
	(0.36)	(0.35)	(0.03)	(0.25)	(0.453)	(0.070)
Resp. Edu	1.80	1.90	-0.10	1.89	2.057	-0.171
	(3.60)	(3.52)	(0.27)	(3.93)	(4.035)	(0.746)
Has Migrant	0.48	0.50	-0.02	0.46	0.601	-0.142*
	(0.50)	(0.50)	(0.04)	(0.50)	(0.490)	(0.077)
Farm HH	0.94	0.93	0.02	0.96	0.922	0.038
	(0.23)	(0.26)	(0.02)	(0.20)	(0.269)	(0.055)
Pp. Den. (2011)	200.23	284.25	-84.01***	234.27	241.645	-7.377
	(66.78)	(102.92)	(6.89)	(56.77)	(48.269)	(5.947)
HC Pov. (2011)	26.00	18.17	7.83***	20.83	20.595	0.239
	(11.00)	(3.43)	(0.56)	(2.14)	(1.834)	(0.280)
Road Access	0.49	0.45	0.04	0.39	0.285	0.106
	(0.50)	(0.50)	(0.04)	(0.49)	(0.452)	(0.071)
MMI Max	8.06	7.90	0.15***	8.02	7.958	0.063***
	(0.10)	(0.11)	(0.01)	(0.11)	(0.050)	(0.011)
Ward MMI	8.40	7.73	0.67***	8.26	8.242	0.016
	(0.70)	(1.47)	(0.09)	(0.74)	(1.137)	(0.137)
%Displaced	0.75	0.63	0.12***	0.80	0.783	0.015
	(0.43)	(0.48)	(0.04)	(0.40)	(0.413)	(0.067)
Red Card	0.90	0.91	-0.01	0.92	0.962	-0.038
	(0.30)	(0.29)	(0.02)	(0.27)	(0.191)	(0.028)

Balance Diagnostics

	Pseudo R^2	p> χ^2	Mean Bias	Med. Bias	Rubin's R	Rubin's B
Unmatched	0.450	0.000	31.4	18.7	132.51	5.95
Matched	0.015	0.841	8.6	7.0	29.30	0.70

Means, standard deviations, and difference in means and the t-statistic from the regression of the variable on a dummy variable taking a value of 1 if MMI>7. Columns (1)-(3) use unweighted data; columns (4)-(6) balancing weights as described in section 5.4.

Table 5.3: Descriptives and balance: cash/no-cash

	Unmatched Sample ($N = 870$)			Matched Sample ($N = 517$)		
	Treated	Comparison	Diff	Treated	Comparison	Diff
HH Size	5.47	5.92	-0.45**	5.35	6.532	-1.179**
	(2.44)	(2.51)	(0.18)	(2.56)	(3.366)	(0.561)
Resp. Age	42.26	42.86	-0.60	41.38	44.866	-3.485
	(13.23)	(14.07)	(0.98)	(13.79)	(16.129)	(2.801)
Average Age	30.74	30.02	0.73	30.77	30.742	0.025
	(11.28)	(10.64)	(0.80)	(12.02)	(8.860)	(1.648)
High Caste	0.47	0.15	0.31***	0.38	0.577	-0.195**
	(0.50)	(0.36)	(0.03)	(0.49)	(0.495)	(0.076)
Hindu	0.86	0.85	0.00	0.81	0.895	-0.082
	(0.35)	(0.36)	(0.03)	(0.39)	(0.307)	(0.052)
Resp. Edu	2.16	1.80	0.36	2.15	2.571	-0.420
	(3.64)	(3.60)	(0.26)	(3.55)	(4.724)	(0.774)
Has Migrant	0.54	0.48	0.06*	0.47	0.590	-0.120
	(0.50)	(0.50)	(0.04)	(0.50)	(0.493)	(0.082)
Farm HH	0.95	0.94	0.01	0.97	0.984	-0.014
	(0.21)	(0.23)	(0.02)	(0.17)	(0.124)	(0.014)
Pp. Den. (2011)	272.67	200.23	72.44***	257.33	262.620	-5.289
	(46.50)	(66.78)	(3.90)	(54.61)	(50.967)	(8.006)
HC Pov. (2011)	16.21	26.00	-9.80***	19.37	20.905	-1.539***
	(4.77)	(11.00)	(0.53)	(3.94)	(1.936)	(0.377)
Road Access	0.51	0.49	0.02	0.42	0.236	0.184***
	(0.50)	(0.50)	(0.04)	(0.49)	(0.426)	(0.068)
MMI Max	7.89	8.06	-0.16***	7.85	7.970	-0.116***
	(0.17)	(0.10)	(0.01)	(0.16)	(0.101)	(0.018)
Ward MMI	6.43	8.40	-1.97***	6.92	7.968	-1.045***
	(2.07)	(0.70)	(0.13)	(2.18)	(0.663)	(0.171)
%Displaced	0.49	0.75	-0.27***	0.47	0.869	-0.402***
	(0.50)	(0.43)	(0.03)	(0.50)	(0.339)	(0.057)
Red Card	0.96	0.90	0.06***	0.94	0.922	0.023
	(0.20)	(0.30)	(0.02)	(0.23)	(0.269)	(0.038)

Balance Diagnostics

	Pseudo R^2	p> χ^2	Mean Bias	Med. Bias	Rubin's R	Rubin's B
Unmatched	0.520	0.000	37.2	10.5	141.25	0.14
Matched	0.066	0.000	17.1	15.3	61.22	2.10

Means, standard deviations, and difference in means and the t-statistic from the regression of the variable on a dummy variable taking a value of 1 if MMI>7. Columns (1)-(3) use unweighted data; columns (4)-(6) balancing weights as described in section 5.4.

Table 5.4: Cash/no-cash effects on coping strategies and outcomes

A. Kernel Matching

	i. Cash vs. Untreated		ii. No-cash vs. Untreated		iii. Cash vs. No-cash	
	Comp. mean (SD)	Cash==1 Untreated==0	Comp. mean (SD)	No-cash==1 Untreated==0	Comp. mean (SD)	Cash==1 No cash==0
Sold LST	0.278	-0.122	0.103	-0.067	0.101	0.059
	(0.449)	(0.069)	(0.305)	(0.047)	(0.302)	(0.056)
Goat Herd	4.256	1.338**	4.903	3.095**	4.645	1.466*
	(3.285)	(0.461)	(4.390)	(1.021)	(3.215)	(0.653)
TLUs	2.544	0.337	2.762	0.318	2.898	0.264
	(1.646)	(0.224)	(1.848)	(0.168)	(1.584)	(0.160)
LST Inc.	178.919	-17.061	79.648	59.625	77.241	72.395***
	(359.522)	(19.216)	(223.241)	(36.581)	(205.673)	(20.274)
Crop Inc.	51.936	-0.039	67.308	17.999	74.604	60.249
	(162.547)	(39.288)	(185.056)	(35.541)	(270.664)	(38.252)
CSI-FS	6.045	-4.030*	4.008	-3.338*	4.336	0.585
	(7.263)	(2.023)	(5.455)	(1.471)	(5.980)	(1.230)
Took Loan	0.449	-0.214**	0.339	-0.078	0.340	-0.033
	(0.498)	(0.065)	(0.474)	(0.121)	(0.475)	(0.093)
N (matched)	954		634		517	

*** p < 0.01, ** p < 0.05, * p < 0.1. Star levels based on wild-cluster bootstrap t-distribution. KM matching with Epanechnikov kernel, bandwidth = 0.06. "Sold LST" is a binary outcome that indicates whether the household sold livestock as a coping strategy; "goat herd" indicates the number of animals; "TLUs" is tropical livestock units as described in the text; "LST income" is revenue (USD) from the sale of livestock and livestock products; "Crop income" is revenue (USD) from the sale of crops; "Took Loan" is the probablity the household took out a loan to cope.

B. Nearest Neighbor

	i. Cash vs. Untreated		ii. No-cash vs. Untreated		iii. Cash vs. No-cash	
	Comp. mean (SD)	Cash==1 Untreated==0	Comp. mean (SD)	No-cash==1 Untreated==0	Comp. mean (SD)	Cash==1 No cash==0
Sold LST	0.331	-0.176*	0.117	-0.071	0.089	0.088
	(0.471)	(0.073)	(0.322)	(0.052)	(0.286)	(0.084)
Goat Herd	4.160	1.388**	5.191	2.756*	4.622	2.629*
	(3.083)	(0.421)	(4.494)	(1.013)	(3.252)	(1.114)
TLUs	2.560	0.243	2.823	0.122	2.857	0.589*
	(1.579)	(0.231)	(1.884)	(0.149)	(1.480)	(0.119)
LST Inc.	166.911	-11.479	81.239	59.094	69.538	19.111
	(327.787)	(20.906)	(238.596)	(38.772)	(185.821)	(56.868)
Crop Inc.	41.664	9.772	61.645	42.479	83.384	-95.312
	(144.141)	(36.149)	(156.327)	(36.832)	(290.411)	(74.990)
CSI-FS	6.641	-4.490*	3.892	-3.307*	4.152	2.114
	(7.235)	(1.995)	(5.235)	(1.474)	(6.141)	(1.552)
Took Loan	0.512	-0.283**	0.372	-0.126	0.317	0.104
	(0.501)	(0.074)	(0.485)	(0.134)	(0.467)	(0.109)
N (matched)	820		390		467	

*** p < 0.01, ** p < 0.05, * p < 0.1. Star levels based on wild-cluster bootstrap t-distribution. NN5 matching algorithm. "Sold LST" is a binary outcome that indicates whether the household sold livestock as a coping strategy; "goat herd" indicates the number of animals; "TLUs" is tropical livestock units as described in the text; "LST income" is revenue (USD) from the sale of livestock and livestock products; "Crop income" is revenue (USD) from the sale of crops; "Took Loan" is the probablity the household took out a loan to cope.

Table 5.5: Cash/no-cash effects on food security

A. Kernel Matching

	i. Cash vs. Untreated		ii. No-cash vs. Untreated		iii. Cash vs. No-cash	
	Comp. mean (SD)	Cash==1 Untreated==0	Comp. mean (SD)	No-cash==1 Untreated==0	Comp. mean (SD)	Cash==1 No cash==0
CSI-FS	6.045	-4.030*	4.008	-3.338*	4.336	0.585
	(7.263)	(2.023)	(5.455)	(1.471)	(5.980)	(1.230)
Cut Meals	0.185	-0.202*	0.096	-0.173*	0.064	0.039*
	(0.389)	(0.072)	(0.295)	(0.083)	(0.246)	(0.019)
Smaller Meals	0.190	-0.161*	0.038	-0.073*	0.081	0.094
	(0.393)	(0.070)	(0.191)	(0.030)	(0.273)	(0.071)
Less Exp/Pref	0.262	-0.205*	0.170	-0.104*	0.185	0.052
	(0.440)	(0.066)	(0.376)	(0.048)	(0.389)	(0.059)
On Account	0.371	-0.166***	0.349	-0.156	0.326	-0.055
	(0.483)	(0.099)	(0.477)	(0.111)	(0.470)	(0.085)
Share Food	0.323	-0.064	0.162	-0.166**	0.044	0.356***
	(0.468)	(0.088)	(0.369)	(0.065)	(0.205)	(0.087)
N (matched)	954		634		517	

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Star levels based on wild-cluster bootstrap t-distribution. KM matching with Epanechnikov kernel, bandwidth = 0.06. CSI-FS is an index as described in the text; “Cut meals”, “Smaller Meals”, “Less exp/pref”, “On Account”, and “Share food” are all binary coping outcomes.

B. Nearest Neighbor

	i. Cash vs. Untreated		ii. No-cash vs. Untreated		iii. Cash vs. No-cash	
	Comp. mean (SD)	Cash==1 Untreated==0	Comp. mean (SD)	No-cash==1 Untreated==0	Comp. mean (SD)	Cash==1 No cash==0
CSI-FS	6.641	-4.490**	3.892	-3.307**	4.152	2.114
	(7.235)	(1.995)	(5.235)	(1.474)	(6.141)	(1.552)
Cut meals	0.194	-0.210**	0.087	-0.148*	0.089	0.070
	(0.396)	(0.074)	(0.283)	(0.083)	(0.286)	(0.040)
Smaller meals	0.193	-0.170*	0.028	-0.055	0.075	0.143*
	(0.396)	(0.088)	(0.165)	(0.034)	(0.264)	(0.079)
Less Exp/Pref	0.312	-0.249***	0.165	-0.081	0.148	-0.089
	(0.464)	(0.079)	(0.372)	(0.056)	(0.357)	(0.066)
On Account	0.449	-0.222**	0.361	-0.201*	0.287	0.102
	(0.498)	(0.095)	(0.481)	(0.108)	(0.454)	(0.103)
Share food	0.317	-0.061	0.183	-0.167**	0.038	0.247***
	(0.466)	(0.095)	(0.388)	(0.066)	(0.192)	(0.065)
N (matched)	820		390		467	

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Star levels based on wild-cluster bootstrap t-distribution. NN5 matching algorithm. CSI-FS is an index as described in the text; “Cut meals”, “Smaller Meals”, “Less exp/pref”, “Account”, and “Share food” are all binary coping outcomes.

Table 5.6: Cash/no-cash effects on financial behavior

A. Kernel Matching

	i. Cash vs. Untreated		ii. No-cash vs. Untreated		iii. Cash vs. No-cash	
	Comp. mean (SD)	Cash==1 Untreated==0	Comp. mean (SD)	No-cash==1 Untreated==0	Comp. mean (SD)	Cash==1 No cash==0
Saved	0.461	0.246***	0.561	0.251***	0.691	0.005
	(0.499)	(0.095)	(0.497)	(0.066)	(0.463)	(0.062)
Amount Saved	6.558	2.163	13.792	-12.257	1.436	6.268***
	(29.048)	(3.854)	(48.661)	(6.071)	(2.481)	(1.750)
Total Debt	940.184	-600.269**	1,455.577	200.896	1,395.734	-403.497**
	(1,443.732)	(191.274)	(1,811.324)	(182.223)	(1,494.356)	(112.307)
Formal Debt	129.625	-71.032	467.539	-295.755*	305.824	102.935*
	(342.955)	(48.244)	(1,005.244)	(120.230)	(717.950)	(39.819)
Informal Debt	438.574	-285.967**	886.853	361.246**	908.185	-473.671***
	(799.648)	(97.358)	(1,170.423)	(121.008)	(1,111.260)	(87.385)
N (matched)	954		634		517	

*** p < 0.01, ** p < 0.05, * p < 0.1. Star levels based on wild-cluster bootstrap t-distribution. KM matching with Epanechnikov kernel, bandwidth = 0.06.

B. Nearest Neighbor

	i. Cash vs. Untreated		ii. No-cash vs. Untreated		iii. Cash vs. No-cash	
	Comp. mean (SD)	Cash==1 Untreated==0	Comp. mean (SD)	No-cash==1 Untreated==0	Comp. mean (SD)	Cash==1 No cash==0
Saved	0.466	0.238**	0.557	0.241**	0.687	0.025
	(0.500)	(0.091)	(0.498)	(0.067)	(0.466)	(0.067)
Amount Saved	6.098	5.016*	14.879	-13.097*	1.379	6.088
	(27.494)	(2.832)	(50.661)	(6.363)	(1.973)	(4.075)
Total Debt	827.156	-495.107**	1,412.448	119.977	1,416.441	-482.164*
	(1,290.264)	(143.565)	(1,779.294)	(227.312)	(1,481.749)	(171.784)
Formal Debt	108.914	-47.366	477.697	-360.962*	290.594	119.896
	(311.069)	(47.535)	(1,022.446)	(124.078)	(682.535)	(87.177)
Informal Debt	403.325	-284.531*	821.540	371.853*	944.284	-549.815**
	(731.237)	(117.470)	(1,125.633)	(144.199)	(1,125.301)	(174.652)
N (matched)	820		390		467	

*** p < 0.01, ** p < 0.05, * p < 0.1. Star levels based on wild-cluster bootstrap t-distribution. NN5 matching algorithm.

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Appendix A

Essay One Appendix

Deviations from the pre-analysis plan

In this section we note material deviations from our original pre-analysis plan. First, the pre-analysis plan describes an analysis of spillover effects beyond the pay-it-forward targeted areas. In addition to sampling from central targeted wards (within the targeted VDC), our original sampling design selected households at random from a neighboring ward and a second additional ward within the targeted VDC. To reduce costs, we only collected data on households in these "spillover wards" in VDCs assigned to a treatment, but not in control VDCs. At the time, our assumption was that central wards would be statistically indistinguishable from non-central wards, so that a random sample from central wards in control VDCs would be an appropriate counterfactual to a random sample from non-central wards in treatment VDCs. Unfortunately, balance checks revealed some important differences between these two samples at baseline. Given these issues, we did not proceed with the initially proposed analysis of broader spillover effects.

Second, the pre-analysis plan also hypothesizes heterogeneity of impacts across several dimensions, including assets (wealth), income, empowerment, literacy levels of respondent and the head of household, and gender (the latter being applicable only for a small number of individual-level outcomes). In addition, for analysis of PIF impacts, we originally planned to consider heterogeneity by ward-level population since it's likely PIF impacts will be stronger in smaller wards than larger ones because a greater proportion of sampled individuals will have been treated. We do not conduct the originally proposed heterogeneity analysis due to a lack of power. We admit the heterogeneity analysis was overly ambitious, and even more so after removing 10/60 clusters from the sample following the aforementioned earthquake.

Third, we consider only nine of the originally proposed ten outcome dimensions, omitting time use. There are two reasons we do not include the pre-specified analysis for time use in the main paper. First, time use was included in the original list of outcomes as a potential unintended consequence of the intervention - that is, we do not expect the program to increase time spent in leisure activities, rather, we are worried the intervention will increase time spent working to the detriment of individual welfare. Given that, we should not include it in our estimations that control for the FDR; doing so penalizes us for a null result that should be interpreted positively. Second, time use is a subindicator of our empowerment summary index. Including it as a main summary index and a subindicator seems duplicative. For completeness, we do report the pre-specified results on time use in A.1 with some discussion of the findings included in the sub-section on empowerment.

Finally, three of the summary indices (aspirations, food security and physical health) were not calculated in strict accordance with the pre-analysis plan because of poor data quality. First, we use Anderson (2008) to calculate the aspirations index, rather than the pre-specified Bernard and Taffesse (2014) aspirations index. Although the survey questions used to create the index are the same, in the Bernard and Taffesse (2014) categorical weights across four aspirational dimensions are assigned by respondents through the distribution of 20 tokens across four bins in proportion to how heavily they value a particular category. Our data reveals limited variation in the assignment of weights across categories. For this reason, we prefer a data-driven approach to assigning weights. We note that the reported results are not sensitive to this change.

In the case of food security, the PAP specified that the summary index would consist of six subindicators related to meals eaten/skipped (number of meals/day eaten in the household hold, number of snacks/day eaten in the household, skipped at least one meal in the past week (binary, respondent/child), went a full day without eating in the past week (binary, respondent/child)), as well as separate household dietary diversity scores (HDDS) for the household as whole and for the children in the household. First, analysis showed that there was virtually no difference between the respondent and children for the meal skipping variables. Therefore, because this information is redundant and because calculating the index with child-specific sub-indicators included would have required us to impute a large number of values to childless households, we omit the child-specific subindicators. Second, the data suggest that meal skipping is extremely uncommon and that meals per day is very stable. This is consistent with the guidance of our Nepali partners, who point out that food insecure households in rural Nepal eat smaller quantities of a less diverse or nurtritious diet, but virtually never skip a meal entirely. Therefore, in addition to the pre-specified HDDS, we add two more measurements of dietary diversity: modified versions of the dietary diversity index (DDI) and the Food Consumption Score (FCS). To the extent that our data permit it we calculate DDI as suggested by (citation) and FCS as suggested by (citation), but in both cases our the structure of the data required us to deviate somewhat from established norms.

The physical health dimension as pre-specified included several measurements of child anthropometrics (HAZ, WAZ, MUAC) as sub-indicator components of the physical health index. An initial review of the descriptive statistics of the raw measurements and the calculated anthropometrics revealed implausible means and extremely high variances, suggesting very high levels of measurement error in the collected data. After follow-up with the field team and consultation with anthropometrics experts, we concluded that the equipment used was inadequate and protocols employed insufficient to accurately conduct anthropometric measurements under difficult field conditions. We therefore omit these subindicators from physical health.

Table A.1: ITT effects on time use

	Control mean	FT	NG	NVT	FT=NG	FT=NVT	NG=NVT	N
Panel A: Direct impacts								
Time working	8.673	-0.213	0.461	0.270	-0.674	-0.482	0.192	974
	(3.258)	(0.441)	(0.377)	(0.390)	(0.170)	(0.342)	(0.662)	
Ag Work	2.037	-0.489	0.203	-0.257	-0.693*	-0.232	0.461	974
	(2.929)	(0.339)	(0.382)	(0.334)	(0.051)	(0.480)	(0.200)	
LST Work	2.166	0.292	-0.182	0.462*	0.474	-0.170	-0.644**	974
	(2.394)	(0.281)	(0.250)	(0.251)	(0.111)	(0.593)	(0.028)	
Other work	4.471	-0.010	0.457	0.136	-0.468	-0.147	0.321	974
	(2.808)	(0.303)	(0.364)	(0.305)	(0.144)	(0.635)	(0.354)	
Leisure	14.173	0.306	-0.363	-0.094	0.669	0.400	-0.269	974
	(2.902)	(0.330)	(0.306)	(0.285)	(0.122)	(0.329)	(0.477)	
Panel B: Pay-it-forward impacts								
Time working	9.057	-0.333	0.253	-0.249	-0.586	-0.084	0.502	757
	(3.174)	(0.324)	(0.407)	(0.435)	(0.120)	(0.837)	(0.288)	
Ag Work	2.266	-0.235	0.607	-0.035	-0.842	-0.199	0.643	757
	(3.025)	(0.448)	(0.524)	(0.477)	(0.122)	(0.661)	(0.250)	
LST Work	2.008	0.191	-0.052	-0.337	0.243	0.528*	0.285	757
	(2.334)	(0.272)	(0.252)	(0.238)	(0.390)	(0.067)	(0.288)	
Other work	4.822	-0.517	-0.333	0.126	-0.184	-0.643**	-0.459	757
	(3.224)	(0.333)	(0.343)	(0.361)	(0.566)	(0.042)	(0.228)	
Leisure	14.112	0.297	-0.202	-0.000	0.498*	0.297	-0.201	757
	(2.790)	(0.257)	(0.325)	(0.324)	(0.072)	(0.311)	(0.552)	

OLS regressions, clustered (VDC) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. N.B. that FT=NG, FT=NVT, NG=NVT represent Wald tests of a null of equal treatment effects, p-values are reported as the sub-statistic. Control variables include baseline dependent variable, stratification bin dummies, and imbalanced variables at baseline. FT: full treatment, NG: no-goats treatment, NVT: no-values-based-training treatment.

Long-run outcomes

Table A.2: ITT effects on household asset holdings

	Control mean	FT	NG	NVT	FT=NG	FT=NVT	NG=NVT	N
Panel A: Direct impacts								
Asset index	0.080	-0.001	0.012	0.019	-0.013	-0.020	-0.007	1,032
	(1.031)	(0.124)	(0.152)	(0.115)	(0.924)	(0.847)	(0.959)	
Productive assets	0.134	0.001	0.008	-0.010	-0.007	0.011	0.018*	1,029
	(0.068)	(0.008)	(0.009)	(0.010)	(0.369)	(0.210)	(0.073)	
Non-productive assets	0.475	-0.018	-0.004	-0.007	-0.015	-0.012	0.003	1,030
	(0.083)	(0.013)	(0.015)	(0.013)	(0.208)	(0.212)	(0.783)	
Livestock (TLU)	2.576	0.260	0.051	0.300	0.209	-0.039	-0.249	1,032
	(2.353)	(0.169)	(0.230)	(0.218)	(0.340)	(0.855)	(0.327)	
Land (hectares)	0.470	0.055*	0.010	0.101***	0.045	-0.046	-0.091**	1,029
	(0.538)	(0.030)	(0.038)	(0.037)	(0.239)	(0.223)	(0.040)	
Housing index	2.534	0.065	-0.109	0.016	0.174	0.049	-0.125	1,032
	(0.824)	(0.132)	(0.110)	(0.096)	(0.176)	(0.663)	(0.236)	
Panel B: Pay-it-forward impacts								
Asset index	-0.037	0.094	-0.099	-0.073	0.193*	0.167	-0.026	795
	(1.064)	(0.127)	(0.099)	(0.109)	(0.070)	(0.155)	(0.765)	
Productive assets	0.135	0.002	0.012	-0.003	-0.009	0.005	0.015	794
	(0.069)	(0.007)	(0.010)	(0.008)	(0.264)	(0.431)	(0.141)	
Non-productive assets	0.472	-0.012	-0.021*	-0.015	0.008	0.002	-0.006	794
	(0.083)	(0.011)	(0.012)	(0.011)	(0.375)	(0.790)	(0.535)	
Livestock (TLU)	2.382	0.435**	-0.017	0.202	0.452**	0.234	-0.218	795
	(2.050)	(0.187)	(0.165)	(0.162)	(0.021)	(0.232)	(0.240)	
Land (hectares)	0.392	0.030	0.022	0.022	0.008	0.008	-0.000	794
	(0.475)	(0.038)	(0.040)	(0.039)	(0.817)	(0.835)	(0.989)	
Housing index	2.435	0.160	-0.089	0.043	0.249**	0.116	-0.132*	795
	(0.927)	(0.149)	(0.102)	(0.114)	(0.033)	(0.342)	(0.094)	

OLS regressions, clustered (VDC) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. N.B. that FT=NG, FT=NVT, NG=NVT represent Wald tests of a null of equal treatment effects, p-values are reported as the sub-statistic. Control variables include baseline dependent variable, stratification bin dummies, and imbalanced variables at baseline. FT: full treatment, NG: no-goats treatment, NVT: no-values-based-training treatment. Indices for aspirations, mental health, assets, financial inclusion, food security, and physical health are calculated as a standardized weighted average of subindicators following Anderson (2008). Income is the logged sum of total household income. Non-food expenditures is the logged sum of total expenditures. The empowerment index is a modified 5DE index of the A-WEAI (Alkire et al., 2012; Malapit et al., 2015).

Table A.3: ITT effects on household income

	Control mean	FT	NG	NVT	FT=NG	FT=NVT	NG=NVT	N
Panel A: Direct impacts								
Total income	11.503	-0.123	0.095	-0.060	-0.218*	-0.063	0.155	1,031
	(1.228)	(0.172)	(0.144)	(0.162)	(0.090)	(0.681)	(0.217)	
Livestock income	4.908	-0.151	0.555	0.596*	-0.706	-0.747*	-0.040	1,031
	(5.100)	(0.364)	(0.399)	(0.329)	(0.105)	(0.072)	(0.925)	
Crop income	3.274	-1.025**	-0.471	-0.246	-0.554	-0.779	-0.225	1,031
	(4.902)	(0.462)	(0.462)	(0.492)	(0.229)	(0.109)	(0.594)	
Permanent income	2.317	-0.456	0.615	-0.035	-1.071	-0.421	0.650	1,031
	(4.699)	(0.498)	(0.656)	(0.549)	(0.120)	(0.467)	(0.368)	
Business income	2.750	0.241	1.139*	0.243	-0.898	-0.002	0.896*	1,031
	(4.633)	(0.676)	(0.583)	(0.516)	(0.120)	(0.997)	(0.062)	
Cash income	2.827	-0.353	0.119	-0.162	-0.472	-0.191	0.281	1,031
	(5.012)	(0.710)	(0.801)	(0.783)	(0.403)	(0.749)	(0.677)	
Other income	2.241	0.422	0.922	0.363	-0.500	0.059	0.559	1,031
	(4.505)	(0.614)	(0.611)	(0.518)	(0.365)	(0.910)	(0.279)	
Panel B: Pay-it-forward impacts								
Total income	11.506	-0.168	-0.230	-0.061	0.062	-0.106	-0.168	795
	(1.134)	(0.186)	(0.153)	(0.148)	(0.701)	(0.552)	(0.231)	
Livestock income	4.427	2.011***	1.227*	1.269**	0.784	0.742	-0.042	795
	(4.983)	(0.626)	(0.622)	(0.492)	(0.275)	(0.185)	(0.941)	
Crop income	2.834	0.486	0.252	-0.139	0.234	0.625	0.391	795
	(4.628)	(0.549)	(0.389)	(0.668)	(0.628)	(0.381)	(0.532)	
Permanent income	2.584	-0.387	0.159	-0.479	-0.546	0.092	0.638	795
	(4.897)	(0.522)	(0.659)	(0.604)	(0.348)	(0.859)	(0.330)	
Business income	2.750	0.128	1.284**	0.557	-1.157	-0.430	0.727*	795
	(4.663)	(0.808)	(0.570)	(0.555)	(0.106)	(0.536)	(0.096)	
Day labor income	2.452	-0.631	-0.303	0.457	-0.328	-1.088*	-0.759	795
	(4.785)	(0.621)	(0.697)	(0.631)	(0.572)	(0.051)	(0.212)	
Other income	2.748	0.249	0.697	0.528	-0.448	-0.278	0.169	795
	(4.855)	(0.675)	(0.603)	(0.573)	(0.450)	(0.620)	(0.718)	

OLS regressions, clustered (VDC) standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$. N.B. that FT=NG, FT=NVT, NG=NVT represent Wald tests of a null of equal treatment effects, p-values are reported as the sub-statistic. Control variables include baseline dependent variable, stratification bin dummies, and imbalanced variables at baseline. FT: full treatment, NG: no-goats treatment, NVT: no-values-based-training treatment. Total income is the logged sum of total household income. Each subindicator is also a logged sum. Total household income includes livestock income, crop income, permanent salaried income, small business (entrepreneurial) income, income earned as a day laborer and other miscellaneous income including remittances.

Table A.4: ITT effects on non-food expenditures

	Control mean	FT	NG	NVT	FT=NG	FT=NVT	NG=NVT	N
Panel A: Direct impacts								
Total expenditure	9.641	-0.157	-0.069	-0.084	-0.088	-0.073	0.015	1,033
	(1.397)	(0.142)	(0.144)	(0.140)	(0.543)	(0.575)	(0.909)	
Medical expenditures	6.713	-0.185	0.129	-0.597*	-0.314	0.411	0.725*	1,033
	(3.578)	(0.379)	(0.388)	(0.334)	(0.454)	(0.272)	(0.058)	
Clothing expenditures	7.358	-0.207	0.175	0.138	-0.382*	-0.345*	0.037	1,033
	(1.805)	(0.240)	(0.195)	(0.194)	(0.068)	(0.071)	(0.788)	
Misc. expenditures	6.084	-0.672	-0.849*	-0.517	0.177	-0.156	-0.333	1,033
	(3.468)	(0.471)	(0.451)	(0.452)	(0.712)	(0.727)	(0.436)	
Panel B: Pay-it-forward impacts								
Total expenditure	9.537	0.133	-0.033	-0.030	0.166	0.163	-0.003	796
	(1.449)	(0.173)	(0.183)	(0.147)	(0.307)	(0.230)	(0.983)	
Medical expenditures	6.650	0.192	-0.375	-0.433	0.567**	0.625*	0.058	796
	(3.766)	(0.323)	(0.352)	(0.398)	(0.039)	(0.057)	(0.860)	
Clothing expenditures	7.478	-0.072	-0.069	-0.091	-0.004	0.019	0.023	796
	(1.666)	(0.174)	(0.188)	(0.150)	(0.984)	(0.889)	(0.881)	
Misc. expenditures	6.178	-0.253	-1.213**	-0.959*	0.960*	0.706	-0.254	796
	(3.110)	(0.576)	(0.475)	(0.517)	(0.075)	(0.210)	(0.585)	

OLS regressions, clustered (VDC) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. N.B. that FT=NG, FT=NVT, NG=NVT represent Wald tests of a null of equal treatment effects, p-values are reported as the sub-statistic. Control variables include baseline dependent variable, stratification bin dummies, and imbalanced variables at baseline. FT: full treatment, NG: no-goats treatment, NVT: no-values-based-training treatment. Expenditures is the logged sum of total non-food expenditures. Each subindicator is also a logged sum. For expenditure categories that were not reported annually, we multiply the monthly or quarterly figures by the appropriate factor to achieve an annualized amount. Total expenditures include medical expenditures, clothing expenditures and miscellaneous expenditures.

Table A.5: ITT effects on physical health

	Control mean	FT	NG	NVT	FT=NG	FT=NVT	NG=NVT	N
Panel A: Direct impacts								
Physical health index	0.041	-0.002	0.058	0.083	-0.059	-0.085	-0	1,031
	(0.813)	(0.075)	(0.103)	(0.088)	(0.571)	(0.353)	(1)	
Days work missed	1.554	0.098	0.384	0.099	-0.286	-0.000	0	1,030
	(2.985)	(0.371)	(0.391)	(0.326)	(0.555)	(0.999)	(1)	
Own health	6.468	-0.090	0.223	0.055	-0.313	-0.144	0	1,030
	(1.886)	(0.167)	(0.193)	(0.150)	(0.158)	(0.425)	(0)	
Child health	7.054	0.188	0.461**	0.399**	-0.273	-0.210	0	680
	(1.596)	(0.184)	(0.217)	(0.192)	(0.244)	(0.292)	(1)	
Panel B: Pay-it-forward impacts								
Physical health index	0.064	-0.123	0.158	-0.069	-0.281**	-0.054	0.226**	794
	(0.936)	(0.106)	(0.100)	(0.091)	(0.017)	(0.628)	(0.026)	
Days work missed	1.590	0.536	0.264	0.363	0.272	0.172	-0.099	794
	(3.540)	(0.411)	(0.462)	(0.329)	(0.592)	(0.641)	(0.811)	
Own health	6.518	-0.109	0.501**	-0.250	-0.610**	0.141	0.751***	794
	(1.845)	(0.239)	(0.222)	(0.188)	(0.018)	(0.555)	(0.001)	
Child health	7.134	-0.008	0.440	0.212	-0.449	-0.220	0.228	517
	(1.797)	(0.342)	(0.312)	(0.291)	(0.178)	(0.489)	(0.341)	

OLS regressions, clustered (VDC) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. N.B. that FT=NG, FT=NVT, NG=NVT represent Wald tests of a null of equal treatment effects, p-values are reported as the sub-statistic. Control variables include baseline dependent variable, stratification bin dummies, and imbalanced variables at baseline. FT: full treatment, NG: no-goats treatment, NVT: no-values-based-training treatment. The physical health index is a weighted standardized average of each subindicator presented in this table. Subindicators include the number of days of work missed due to illness in the past month by the respondent, and the response to the following two questions: and

Table A.6: ITT effects on food security

	Control mean	FT	NG	NVT	FT=NG	FT=NVT	NG=NVT	N
Panel A: Direct impacts								
Food Security Index	0.004	0.027	0.150	-0.104	-0.123	0.130	0.253**	1,032
	(0.953)	(0.092)	(0.104)	(0.099)	(0.297)	(0.218)	(0.044)	
Meals	2.019	0.041**	0.009	0.004	0.032	0.037*	0.004	1,031
	(0.235)	(0.020)	(0.019)	(0.010)	(0.204)	(0.060)	(0.804)	
Snacks	1.296	-0.009	0.054	-0.067	-0.063	0.058	0.121	1,031
	(0.511)	(0.080)	(0.088)	(0.069)	(0.498)	(0.420)	(0.144)	
Skipped Meals (Resp)	0.019	0.001	0.006	0.018	-0.004	-0.017	-0.012	1,031
	(0.135)	(0.010)	(0.010)	(0.012)	(0.728)	(0.225)	(0.362)	
Skipped Day (Resp)	0.022	0.002	-0.004	0.016*	0.006	-0.014	-0.020**	1,031
	(0.148)	(0.012)	(0.009)	(0.009)	(0.618)	(0.246)	(0.019)	
Skipped Meal (Child)	0.016	0.023	-0.006	0.009	0.029*	0.014	-0.015	483
	(0.128)	(0.014)	(0.013)	(0.012)	(0.090)	(0.346)	(0.328)	
Skipped Day (Child)	0.000	0.004	0.011	0.024**	-0.007	-0.021**	-0.014	483
	(0.000)	(0.006)	(0.008)	(0.012)	(0.415)	(0.046)	(0.287)	
Enough to eat	0.759	0.071	0.068	0.066	0.003	0.005	0.001	1,031
	(0.428)	(0.062)	(0.058)	(0.059)	(0.955)	(0.933)	(0.980)	
DDI	9.248	-0.626**	0.367	-0.358	-0.993***	-0.268	0.725**	1,032
	(2.876)	(0.278)	(0.311)	(0.310)	(0.006)	(0.364)	(0.049)	
HDDS	4.789	-0.141	0.072	-0.026	-0.213	-0.115	0.098	1,032
	(1.155)	(0.130)	(0.146)	(0.162)	(0.121)	(0.413)	(0.551)	
FCS	45.933	-3.154*	-0.173	-2.223	-2.981	-0.931	2.050	1,032
	(15.254)	(1.662)	(1.879)	(1.832)	(0.127)	(0.603)	(0.325)	
Panel B: Pay-it-forward impacts								
Food Security Index	-0.059	0.125	0.013	-0.009	0.112	0.133	0.022	795
	(0.874)	(0.089)	(0.097)	(0.098)	(0.296)	(0.206)	(0.845)	
Meals	2.026	0.035	-0.006	-0.011	0.040	0.045	0.005	794
	(0.188)	(0.021)	(0.019)	(0.016)	(0.078)	(0.031)	(0.790)	
Snacks	1.231	0.114	0.096	0.021	0.018	0.093	0.075	794
	(0.480)	(0.059)	(0.065)	(0.064)	(0.766)	(0.132)	(0.272)	
Skipped Meals (Resp)	0.021	0.023	-0.007	0.022	0.030	0.002	-0.029	794
	(0.142)	(0.018)	(0.018)	(0.016)	(0.164)	(0.931)	(0.123)	
Skipped Day (Resp)	0.010	0.021	0.005	0.003	0.016	0.018	0.002	794
	(0.101)	(0.018)	(0.014)	(0.011)	(0.464)	(0.354)	(0.883)	
Skipped Meal (Child)	0.011	0.040	-0.014	-0.011	0.054	0.051	-0.003	381
	(0.103)	(0.022)	(0.010)	(0.009)	(0.037)	(0.035)	(0.794)	
Skipped Day (Child)	0.011	-0.011	-0.009	-0.010	-0.002	-0.002	0.000	381
	(0.103)	(0.008)	(0.006)	(0.007)	(0.590)	(0.717)	(0.875)	
Enough to eat	0.749	0.066	-0.011	0.059	0.077	0.007	-0.069	794
	(0.435)	(0.061)	(0.058)	(0.055)	(0.198)	(0.897)	(0.211)	
DDI	9.205	-0.197	-0.120	-0.431	-0.078	0.233	0.311	795
	(3.000)	(0.491)	(0.446)	(0.398)	(0.878)	(0.605)	(0.425)	
HDDS	4.759	-0.031	-0.080	-0.057	0.049	0.026	-0.023	795
	(1.121)	(0.157)	(0.176)	(0.155)	(0.770)	(0.849)	(0.884)	
FCS	45.633	-2.015	-3.331	-3.323	1.316	1.308	-0.008	795
	(16.201)	(2.002)	(2.100)	(1.714)	(0.525)	(0.444)	(0.996)	

OLS regressions, clustered (VDC) standard errors in parentheses; *** $p<0.01$, ** $p<0.05$, * $p<0.1$. N.B. that FT=NG, FT=NVT, NG=NVT represent Wald tests of a null of equal treatment effects, p-values are reported as the sub-statistic. Control variables include baseline dependent variable, stratification bin dummies, and imbalanced variables at baseline. FT: full treatment, NG: no-goats treatment, NVT: no-values-based-training treatment. The food security index is a weighted standardized average of each subindicator presented in this table, excluding child-specific indicators.

Sensitivity Analysis

Table A.7: Robustness checks for empowerment and financial inclusion indices

	Control mean	FT	NG	NVT	FT=NG	FT=NVT	NG=NVT	N
Panel A: Direct impacts								
Empowerment (1)	0.777	0.043**	0.044*	0.048**	-0.000	-0.005	-0.004	1,020
	(0.189)	(0.020)	(0.023)	(0.022)	(0.989)	(0.813)	(0.836)	
Empowerment (2)	0.054	0.243**	0.255*	0.280**	-0.012	-0.038	-0.026	1,031
	(1.171)	(0.105)	(0.132)	(0.127)	(0.918)	(0.730)	(0.836)	
Empowerment (3)	0.031	0.162*	0.147	0.197	0.015	-0.035	-0.050	1,031
	(1.193)	(0.093)	(0.128)	(0.119)	(0.893)	(0.731)	(0.688)	
Finance (1)	0.077	0.291**	0.341**	0.329***	-0.050	-0.038	0.012	1,033
	(0.981)	(0.130)	(0.140)	(0.122)	(0.743)	(0.739)	(0.931)	
Finance (2)	0.078	0.227*	0.321**	0.288**	-0.094	-0.061	0.033	1,033
	(0.975)	(0.131)	(0.137)	(0.122)	(0.539)	(0.615)	(0.800)	
Panel B: Pay-it-forward impacts								
Empowerment (1)	0.749	0.067***	0.078***	0.031	-0.011	0.037	0.048**	786
	(0.195)	(0.023)	(0.021)	(0.021)	(0.649)	(0.140)	(0.032)	
Empowerment (2)	-0.110	0.406***	0.480***	0.256**	-0.074	0.150	0.223**	795
	(1.232)	(0.131)	(0.115)	(0.116)	(0.562)	(0.250)	(0.047)	
Empowerment (3)	-0.111	0.353***	0.392***	0.289**	-0.039	0.064	0.103	795
	(1.257)	(0.124)	(0.120)	(0.119)	(0.749)	(0.598)	(0.372)	
Finance (1)	-0.041	0.171*	0.377***	0.145	-0.205*	0.026	0.232**	796
	(1.032)	(0.098)	(0.121)	(0.111)	(0.098)	(0.805)	(0.049)	
Finance (2)	-0.033	0.104	0.336***	0.129	-0.232*	-0.025	0.207*	796
	(1.039)	(0.095)	(0.115)	(0.105)	(0.056)	(0.810)	(0.062)	

OLS regressions, clustered (VDC) standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. N.B. that FT=NG, FT=NVT, NG=NVT represent Wald tests of a null of equal treatment effects, p-values are reported as the sub-statistic. Control variables include baseline dependent variable, stratification bin dummies, and imbalanced variables at baseline. FT: full treatment, NG: no-goats treatment, NVT: no-values-based-training treatment. Three empowerment indices are employed. The pre-specified empowerment index (1) is a modified 5DE index of the A-WEAI (Alkire et al., 2012; Malapit et al., 2015). Subindicators and weights are based on the A-WEAI and defined in the main text and table 3.6. We also calculate two standardized weighted averages following Anderson (2008): index (2) uses subindicators identical to (1), while (3) uses all subindicators excluding group membership. For financial inclusion we calculate two standardized weighted averages following Anderson (2008): index (1) uses the set of pre-specified subindicators defined in the main text and table 3.7, and index (2) uses all pre-specified subindicators excluding group membership.

Results of Attrition Analysis

In this section, we assess whether the observed attrition is systematic in a way that might bias our results. For simplicity, the approach presented below considers the full sample, and does not distinguish between direct and PIF beneficiaries.

First, equation A.1 estimates whether attrition rates differ across treatment types and control households, where $attrit_{hv}$ is a binary variable indicating that a household was surveyed at baseline but is missing from the endline data set.

$$attrit_{hv} = \beta_0 + \beta_1 T1_{hv} + \beta_1 T2_{hv} + \beta_1 T3_{hv} + \varepsilon_{hv} \tag{A.1}$$

The results of estimating equation A.1 are presented in table A.8. We do not observe any statistically significant treatment effects on attrition status.

Table A.8: Differential attrition by treatment

	Control mean (SD)	Any	FT	NG	NVT	N
Attrition	0.000	-0.006	0.031	-0.031	-0.017	1,982
	(0.000)	(0.029)	(0.034)	(0.033)	(0.030)	

Next, we assess whether attrition rates differ across households with respect to a set of baseline characteristics. To do this we regress a variety of baseline indicators on attrition status as estimated in equation A.2:

$$y_{hv} = \beta_0 + \beta_1 attrit_{hv} + \varepsilon_{hv} \tag{A.2}$$

We estimate equation A.2 for a set of demographic variables (table A.9) and each of the outcome indices (table A.10). We do find scattered individual cases where attrition status correlates with a baseline characteristic or outcome index. Specifically, households with large herds at baseline are less likely to have left the sample, and households with higher financial inclusion and/or lower aspirations are more likely to have left the sample. These instances do not appear to be systematic or to threaten the integrity of our results. We also note that we should expect to randomly observe some statistically significant relationships.

Table A.9: Differential attrition by baseline demographic characteristics

	Attrited	N
HH Size	-0.141	1,981
	(0.290)	
N Male	0.005	1,942
	(0.159)	
Average Age	0.535	1,981
	(1.214)	
Resp. Age	1.490	1,980
	(1.052)	
Resp. Edu	0.114	1,980
	(0.370)	
Resp. Lit.	-0.041	1,979
	(0.054)	
Income	0.046	1,982
	(0.117)	
Land	0.075	1,977
	(0.073)	
TLU	-0.392	1,982
	(0.165)**	
Has Migrant	0.004	1,981
	(0.037)	

Table A.10: Differential attrition by baseline outcome indices

	Attrited	N
Empowerment	0.012	1,968
	(0.016)	
Finance	0.194	1,982
	(0.087)**	
Aspirations	-1.297	1,978
	(0.516)**	
Mental health	-0.075	1,982
	(0.098)	
Asset index	-0.050	1,982
	(0.111)	
Income (Rs.)	0.046	1,982
	(0.117)	
Non-food consumption	0.310	1,978
	(0.166)*	
Physical health	-0.130	1,982
	(0.082)	
Food Security	-0.103	1,982
	(0.116)	

Appendix B

Essay Two Appendix

Full Balance Checks

The following table tests equality of means for the 29 variables used to generate balancing weights as described in the data section of the text. It contains balance checks on the unweighted ("unmatched") and weighted ("matched") samples. The variables fall into four main categories: time invariant demographic characteristics, assets (mainly livestock and housing), income (total and disaggregated by sector), and savings/credit. All comparisons are on baseline data.

Table B.1: Descriptives and balance

	Unmatched			Matched		
	MMI>7	MMI<7	Diff	MMI>7	MMI<7	Diff
Age	40.22	42.34	-2.12*	40.61	41.706	-1.094
	(13.11)	(14.37)	(1.11)	(13.16)	(13.794)	(1.298)
HH Size	5.84	5.89	-0.05	5.88	5.891	-0.007
	(2.66)	(2.73)	(0.21)	(2.66)	(2.966)	(0.263)
No. Children	1.52	1.35	0.16	1.50	1.504	0.001
	(1.34)	(1.33)	(0.10)	(1.34)	(1.376)	(0.129)
No. Elderly	0.35	0.36	-0.01	0.35	0.398	-0.048
	(0.64)	(0.62)	(0.05)	(0.64)	(0.683)	(0.065)
Max Edu	8.63	9.61	-0.99***	8.72	8.642	0.076
	(3.80)	(3.26)	(0.27)	(3.81)	(3.877)	(0.373)
Resp. Lit.	0.49	0.61	-0.13***	0.49	0.516	-0.026
	(0.50)	(0.49)	(0.04)	(0.50)	(0.500)	(0.048)
High Caste	0.40	0.25	0.15***	0.40	0.366	0.032
	(0.49)	(0.43)	(0.04)	(0.49)	(0.482)	(0.047)
Janajati	0.44	0.52	-0.08**	0.43	0.442	-0.010
	(0.50)	(0.50)	(0.04)	(0.50)	(0.497)	(0.047)
Has Migrant	0.44	0.60	-0.16***	0.46	0.482	-0.025
	(0.50)	(0.49)	(0.04)	(0.50)	(0.500)	(0.048)
Farm HH	0.86	0.81	0.04	0.86	0.835	0.025
	(0.35)	(0.39)	(0.03)	(0.35)	(0.372)	(0.034)
FCS	35.76	39.28	-3.52***	35.92	36.122	-0.202
	(15.19)	(14.89)	(1.18)	(15.25)	(14.559)	(1.422)
Non-dirt	0.13	0.22	-0.09***	0.13	0.124	0.002
	(0.33)	(0.41)	(0.03)	(0.33)	(0.330)	(0.030)
Non-straw	0.98	0.87	0.11***	0.98	0.978	0.003
	(0.14)	(0.34)	(0.02)	(0.14)	(0.148)	(0.013)
Non-wood	0.18	0.18	-0.00	0.18	0.175	0.004
	(0.38)	(0.38)	(0.03)	(0.38)	(0.381)	(0.036)
Electric	0.79	0.79	0.00	0.79	0.809	-0.018
	(0.41)	(0.41)	(0.03)	(0.41)	(0.393)	(0.038)
Flush	0.61	0.86	-0.25***	0.62	0.674	-0.053
	(0.49)	(0.35)	(0.03)	(0.49)	(0.469)	(0.047)
Land (hectares)	0.51	0.50	0.01	0.51	0.487	0.021
	(0.49)	(0.70)	(0.05)	(0.49)	(0.649)	(0.052)
Goats	5.41	4.48	0.93**	5.46	5.552	-0.091
	(4.20)	(5.29)	(0.40)	(4.25)	(6.916)	(0.580)
Cattle	0.83	0.62	0.21*	0.84	0.798	0.046
	(1.31)	(1.54)	(0.12)	(1.33)	(1.849)	(0.162)

Continued

	MMI>7	MMI<7	Diff	MMI>7	MMI<7	Diff
Livestock Inc.	21,649.77	14,277.62	7,372.15***	21,517.97	19,143.793	2,374.173
	(34,443.77)	(31,042.34)	(2,506.28)	(34,679.04)	(40,685.034)	(3,727.268)
Crop Inc.	15,504.19	2,915.49	12,588.70***	14,094.17	8,655.123	5,439.051**
	(25,488.14)	(11,454.94)	(1,262.63)	(23,304.42)	(21,118.393)	(2,322.067)
Labor Inc.	10,488.37	14,534.90	-4,046.52	9,053.40	9,275.077	-221.679
	(43,501.99)	(54,609.63)	(4,092.23)	(37,039.54)	(38,931.861)	(3,262.326)
Cash Inc.	10,320.93	34,655.99	-24,335.06***	10,189.32	11,916.252	-1,726.932
	(48,581.12)	(96,057.64)	(6,820.75)	(48,998.84)	(50,420.976)	(4,404.889)
Total Inc.	137,395.82	170,439.23	-33,043.41	133,874.28	127,203.341	6,670.936
	(260,836.15)	(303,395.14)	(23,053.41)	(259,090.53)	(215,560.950)	(21,246.377)
Amount Saved	211.42	235.74	-24.32	217.28	242.918	-25.637
	(925.63)	(1,001.97)	(77.27)	(944.74)	(1,048.445)	(94.109)
Loan Balances	95,878.00	77,111.53	18,766.47	98,270.73	104,240.172	-5,969.443
	(231,607.12)	(188,308.17)	(15,692.79)	(235,542.90)	(250,684.239)	(23,564.815)
Formal	4,186.05	5,641.88	-1,455.84	4,368.93	4,532.926	-163.994
	(32,514.02)	(37,212.47)	(2,836.57)	(33,207.99)	(29,928.877)	(2,868.710)
Informal	79,643.12	61,726.10	17,917.02	81,326.55	89,557.462	-8,230.909
	(219,739.68)	(158,491.33)	(13,784.87)	(223,513.71)	(236,714.402)	(22,699.919)
Lending	7,595.35	18,681.34	-11,085.99**	7,927.18	6,466.476	1,460.708
	(37,711.26)	(61,728.41)	(4,459.75)	(38,495.86)	(27,574.057)	(3,022.478)

Means, standard deviations, and difference in means and the t-statistic from the regression of the variable on a dummy variable taking a value of 1 if MMI>7. Columns (1)-(3) use unweighted data; columns (4)-(6) balancing weights as described in chapter 4.

Power utility

The risk-sharing implication that individual consumption varies positively with aggregegate consumption also holds for power utility with multiplicative preference shocks. Rather than assuming exponential utility as in 4.3, assume that

$$U[C_{ht}; b_{ht}] = \exp(\sigma b_{ht})\frac{1}{\sigma}(C_{ht})^{\sigma}, \qquad \sigma < 1 \tag{B.1}$$

ignoring the state notation. The natural logarithm of consumption for individual h is

$$\ln C_{ht} = \ln C_{at} + \frac{1}{1-\sigma}(\ln \omega_h - \omega_a) + \frac{\sigma}{1-\sigma}(b_{ht} - b_{at}), \tag{B.2}$$

where ω_a, and b_{at} are as above, and $C_{at} = \exp(\frac{1}{H}\sum_{h=1}^{H} \ln C_{ht})$. Again, the implication of risk sharing is reflected in equation (B.2). Taking the first differences yields

$$\Delta \ln C_{ht} = \Delta \ln C_{at} + \frac{1}{1-\sigma}(\Delta b_{ht} - \Delta b_{at}) \tag{B.3}$$

and the analog in natural logs to equation 4.6

$$\Delta \ln C_{ht} = \alpha + \theta \Delta \ln C_{at} + \gamma \Delta \ln y_{ht} + \epsilon_{ht} \tag{B.4}$$

as before, the risk-sharing model predicts that $\theta = 1$, $\gamma = 0$, and all other explanatory variables enter insignificantly.

Sensitivity analysis

Consumption Smoothing

Table B.2: Impact of income shock on expenditures, MMI>6.5

	FCS	FCS	NFC	NFC
$\Delta \ln y_{hv} < 0$	-0.015	-0.018	-0.100	-0.093
	(0.019)	(0.017)	(0.064)	(0.062)
$\Delta \ln y_{hv} > 0$	-0.004	-0.003	0.023	0.081
	(0.016)	(0.016)	(0.055)	(0.052)
MMI		-0.056***		0.140**
		(0.017)		(0.065)
N	828	828	827	827
R^2	0.54	0.44	0.25	0.09
MMI/FE	FE	MMI	FE	MMI

Table B.3: Impact of income shock on expenditures, no weights

	FCS	FCS	NFC	NFC
$\Delta \ln y_{hv} < 0$	0.000	-0.001	-0.061	-0.042
	(0.017)	(0.015)	(0.051)	(0.047)
$\Delta \ln y_{hv} > 0$	-0.000	0.001	0.047	0.091**
	(0.013)	(0.013)	(0.047)	(0.043)
MMI		-0.063***		0.143***
		(0.015)		(0.049)
N	851	851	850	850
R^2	0.51	0.43	0.18	0.08
MMI/FE	FE	MMI	FE	MMI

$*p < 0.1, **p < 0.05, ***p < 0.01$. Cluster robust (ward) standard errors in parentheses.

Note: The tables present parameter estimates based on specifications (4.8) and (4.9) from the text. Columns (1)-(2) present results for FCS; (3)-(4) present non-food consumption expenditures. Income shocks $\Delta \ln y_{hv}$ are continuous variables; ($\Delta \ln y_{hv} < 0$) is coded as an absolute value. Demographic controls and baseline levels of time-variant household characteristics are included in the regressions but not in these results.

Local Borrowing

Table B.4: Impact of income shock on borrowing, MMI>6.5

	Total	Total	Outside	Outside	Local	Local
$\Delta \ln y_{hv} < 0$	0.480*	0.594**	-0.367**	-0.262*	0.719***	0.741***
	(0.274)	(0.266)	(0.158)	(0.145)	(0.251)	(0.237)
$\Delta \ln y_{hv} > 0$	-0.052	-0.014	0.016	0.027	0.140	0.179
	(0.279)	(0.259)	(0.142)	(0.131)	(0.224)	(0.224)
MMI		1.082***		0.156		0.865***
		(0.286)		(0.129)		(0.255)
N	828	828	828	828	828	828
R^2	0.18	0.06	0.15	0.03	0.17	0.07
MMI/FE	FE	MMI	FE	MMI	FE	MMI

Table B.5: Impact of income shock on borrowing, no weights

	Total	Total	Outside	Outside	Local	Local
$\Delta \ln y_{hv} < 0$	0.412	0.457*	-0.370***	-0.352***	0.851***	0.837***
	(0.272)	(0.259)	(0.128)	(0.116)	(0.238)	(0.227)
$\Delta \ln y_{hv} > 0$	-0.150	-0.149	-0.094	-0.091	0.178	0.167
	(0.215)	(0.212)	(0.099)	(0.093)	(0.182)	(0.189)
MMI		0.994***		0.168		0.800***
		(0.287)		(0.120)		(0.254)
N	851	851	851	851	851	851
R^2	0.14	0.05	0.12	0.03	0.12	0.05
MMI/FE	FE	MMI	FE	MMI	FE	MMI

$*p < 0.1$, $**p < 0.05$, $***p < 0.01$. Cluster robust (ward) standard errors in parentheses.

Note: The tables present parameter estimates based on specification (4.14) from the text. Columns (1), (3), and (5) model the earthquake as part of a ward fixed effect. Columns (2), (4), and (6) control for the earthquake directly. Income shocks ($\Delta \ln y_{hv}$) are continuous variables; $\Delta \ln y_{hv} < 0$ is coded as an absolute value. Demographic controls and baseline levels of time-variant household characteristics are included in the regressions but not in these results.