RURAL HOUSEHOLDS AND SHOCKS: ASSET TRANSFER, MIGRATION AND CIVIL CONFLICT

BY

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DISSERTATION

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Abstract

In this dissertation, I present research on three topics in development economics, with overarching theme being the long-term implications of positive and negative shocks on rural poor's economic wellbeing. In the first paper, based on the joint work with Hope Michelson, Alex Winter-Nelson and Peter Goldsmith, I estimate the impact of an asset transfer program on household resilience, where resilience is defined as the probability that a household will sustain at least the threshold asset level required to support consumption above the poverty line. Using six rounds of data collected over 42 months in rural Zambia, I construct a measure of resilience based on households' conditional welfare distributions to estimate program impacts. The study finds that the program increased household resilience; beneficiaries' likelihood of being non-poor in future periods increased by 44%. The program both increased mean assets and decreased variance, signaling an upward shift in households' conditional asset distributions. The method used in the study demonstrates the added value of the resilience estimation compared with a conventional impact assessment; numerous households classified as non-poor are unlikely to remain nonpoor. In the second paper, I analyze the differential impact of migration on labor supply of the left-behind household members in Nepal, where international migration for employment, predominantly a male phenomenon, increased substantially between 2001 and 2011. Using the NLSS III data, this study extends the analysis further by incorpo-

rating the impacts on both extensive and intensive margins and answering the question of if they are not wage-employed, what the remaining members in the household engaging in instead. The paper finds that, in response to out-migration of some family members, women realign their priorities and reallocate their time from market employment to self-employment and home production, possibly filling in the roles vacated by the migrants. In contrast, the income effect dominates the impact of migration on the left-behind men; that is, men value their leisure more because of the remittances from abroad and decrease their overall supply of labor. In the final paper, I analyze the long-term health impacts of the 1996-2006 Nepalese civil conflict using information on conflict incidents at the village level, which allows me to identify the effects of exposure to conflict more accurately than prior studies. Moreover, I am able to track the impact of conflict on health outcomes across generations. Growth stunting is a known outcome of health shocks in childhood, and height has long been recognized as an important factor influencing individuals' professional and personal success. I exploit the heterogeneity in conflict intensity across villages and birth cohorts to document long-term health and intergenerational impacts. I find that childhood exposure to conflict and, in particular, exposure starting in infancy, has highly significant and negative impacts on final adult height – each additional month of exposure decreases womens height by 1.36 millimeters. Additionally, this is among the first papers to document the intergenerational impacts of early childhood conflict exposure. I find that mother's exposure to conflict in her childhood is detrimental to her children's health. Exposed mothers have more children and live in less wealthy households, likely reducing their ability to invest during critical periods of their children's development.

This dissertation is dedicated to my mother, father, sister, and to the memory of Khadka.

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CHAPTER 1

INTRODUCTION

Empirical evidences suggest that negative shocks have more persistent effects on poorer households than wealthier ones. How persistent are the impacts of shocks on rural poor? Do they transmit into the future generations? This dissertation intends to explore these questions by combining research on three topics in development economics. The dissertation focuses on understanding the capital and skill constraint and incomplete labor market rural poor face, and in particular, long-term implication of positive and negative shocks that alter these constraints on women's economic wellbeing, health and poverty dynamics.

The theory of the bifurcated growth dynamics suggests existence of multiple technologies associated with distinct growth paths. Poor households may be on a lower growth trajectory that leads to a steady state equilibrium below poverty line and may lack capacity to switch to a technology that would allow them to reach a higher steady state equilibrium and escape poverty trap. Structural barriers such as credit, skills, and capital constraints, geographical isolation, social and other economic exclusions prevent poor households from accessing the more remunerative path. However, big enough shocks can alter one's future prospect – while a negative shock can knock a nonpoor household from its higher growth trajectory to a lower growth path setting it on a course towards a steady state equilibrium below poverty threshold, a sufficiently large enough positive shock can alter a poor household's prospect by setting it on a course of higher growth. In this dissertation, I explore welfare impact of such shocks and assess their persistency. While first paper investigates the impact of a large asset transfer program in rural Zambia that was designed to ease capital and skill constraint on resilience to poverty, second paper explores heterogeneous labor response by gender to a large-scale international migration for employment in Nepal. In the third paper, I measure the causal impact of Nepal's 1996-2006 civil conflict on long-term health, including intergenerational health.

Because they lack access to capital and face incomplete labor markets, livelihood strategies for most of the world's poor center on casual wage work and subsistence agriculture. Many anti-poverty programs seek to move the poor to more secure, reliable, and remunerative streams of income. Such programs are often motivated by an expectation that sufficiently large transfers can enable households trapped in poverty to move onto a different growth trajectory towards a non-poor steady state. While the theory of bifurcated growth dynamics justifies "big-push" interventions, impact evaluation that focuses only on the first moment of outcomes ignores the potential for shocks or stressors to move households who have received transfers back to a low-level equilibrium. To date, the economic impact evaluation literature has mostly estimated programmatic effects under an assumption of full certainty. Retrospective evaluations have focused on the first moment of the household welfare distribution, rather than on changes in household ability to withstand shocks and maintain consumption above a poverty threshold. Such an approach can say little about resilience to shocks in a dynamic context of uncertainty. Forward-looking poverty evaluations are obviously critical for assessing the lasting effects of interventions, as well as for distinguishing between households that have received a transient welfare boost and those that have experienced a structural change likely to alter their future economic circumstances. The first chapter of this dissertation addresses this shortcoming by operationalizing the concept of resilience in an impact evaluation application. I measure resilience as the probability that a household will sustain at least the level of assets required to support consumption above the poverty line. Using six rounds of primary data collected from an asset transfer and training program in rural Zambia, I estimate households' conditional welfare distributions and construct measures of resilience to poverty.

The results suggest that the program significantly increased resilience among participants. Households receiving both training and livestock at the baseline are 44% more likely to be non-poor than Control households 42 months after the intervention. Moreover, I find that the program increased headcount resilience among participant households. While more than 80% of the treatment households are resilient at the endline, the comparable endline headcount resilience rate for Controls is only 28.6%. Decomposing these effects into first (central tendency) and second (spread) moments reveals that the livestock transfer, and training program has both increased mean household asset holdings and decreased the variance in asset holdings. The program has shifted the conditional asset distribution upward and truncated uncertainty in asset holdings. While comparing resilience results with standard estimates of program impact on asset poverty, I demonstrate the value of measuring resilience in the context of impact assessment. The results reveal that many households classified as non-poor under conventional methods based on mean asset poverty levels are unlikely to remain non-poor over time. The resilience measure thus provides new and important insight into households' longer-term capacity to escape or remain out of poverty. Additionally, the results suggest that households that became resilient through the transfer of livestock gained access to capital and entered into higher return capital-intensive self-employment activities.

The second chapter investigates the effects of large-scale international labor migration on the economic activities of left-behind family members. Positive impacts of migration on income and consumption are well established in the literature, but evidence regarding the effects on non-income-based household outcomes is mixed. In particular, non-migrants tend to assume a larger burden of work to compensate for the loss of the migrant's local income and labor. I study this issue in the context of Nepal, where more than two million prime-age (mostly male) Nepalese are working outside the country and the inflow of remittances accounts for 30% of the country's GDP. Decrease in labor stock and substantial income from abroad is likely to have profound effect on labor market and, yet, the impacts of the migration on the non-income dimensions in Nepal remain relatively unexplored in the literature. This chapter addresses this issue by documenting the differential impact of international migration on labor supply of the left-behind family members. Using nationally representative Nepal Living Standard Survey, I find that the left-behind women increase their total time spent working; specifically, women reallocate their time from casual labor to household entrepreneurial and non-entrepreneurial activities. In contrast, left-behind men reduce overall work time; they decrease their time working both in wage and household activities; that is, men value their leisure more because of the remittances from abroad and decrease their overall labor supply.

While the first two chapters of the dissertation focus on positive shocks, the third chapter investigates the longer-term consequences of a catastrophic, violent event. While most of the economic literature on the legacies of war is focused on human capital effects observed during or shortly after the conflict, the long-term evidences have rarely been established. This paper extends the literature by producing evidences of the long-term impacts and, in particular, this is among the first paper to document the intergenerational impacts of war. Using the most reliable database on the Nepalese civil conflict, I create individual level data set of the victims of the war with exact geographical location (village) and date of the incident. This allows me to explore the variation in conflict intensity at a granular geographical unit (village) and identify the effects of exposure to conflict more accurately than prior studies. I exploit the heterogeneity in conflict intensity across villages and birth cohorts of women interviewed in the 2016 Nepal Demographic Survey. I find that childhood exposure to conflict and, in particular, exposure starting in infancy, has highly significant and negative impact on final adult height additional month of exposure decreases womens height by 1.36 millimeters. Additionally, this is among the first papers to document the intergenerational impacts of early childhood conflict exposure. I find that mother's exposure to conflict in her childhood is detrimental to her children's health. Exposed mothers have more children and live in less wealthy households, likely reducing their ability to invest during critical periods of their childrens development. These results show that negative impacts of violent conflict experienced during childhood are not limited to one's own life and transmit into second generation as well.

CHAPTER 2

DO ASSET TRANSFERS BUILD HOUSEHOLD RESILIENCE?

2.1 Introduction

In response to perceived increases in the severity of climate and economic shocks in developing countries, anti-poverty programs have begun to prioritize household resilience (World Bank, 2016; Hallegatte et al., 2017; Fernández-Gimenez et al., 2011, 2012; Venton et al., 2012). Despite considerable discussion of building resilience through development initiatives, the question of whether an initiative can alter the likelihood that a household will fall into poverty in the foreseeable future has rarely been examined empirically.

To date, the economic impact evaluation literature has mostly estimated programmatic effects under an assumption of full certainty. Retrospective evaluations have focused on the first moment of the household welfare distribution, rather than on changes in household ability to withstand shocks and maintain consumption above a poverty threshold. Forward-looking poverty evaluations are obviously critical for assessing the lasting effects of interventions, as well as for distinguishing between households that have received a transient welfare boost and those that have experienced a structural change likely to alter their future economic circumstances.

This paper applies Barrett and Constas's (2014) moment-based definition of development resilience: "the capacity over time of a person, household or other aggregate unit to avoid poverty in the face of various stressors and in the wake of myriad shocks. If and only if that capacity is and remains high over time, then the unit is resilient." Drawing together the methods and theories related to poverty traps, vulnerability, and ecological resilience, development resilience is a probabilistic and forward-looking concept that takes into account both the first and second moments of the household welfare distribution and quantifies the capacity of households to escape poverty or remain non-poor over time. We measure household resilience as a probability of accumulating and retaining a minimum level of assets required to remain non-poor in the face of diverse shocks and stressors. We employ the econometric technique proposed by Cissé and Barrett (2016) to construct household-specific resilience scores, and we use these estimated resilience scores as an outcome variable in our analysis.

The integrated asset transfer program studied in this paper makes a one-time livestock transfer to participant households, provides training on livestock management and other livelihood skills, and provides veterinary and agricultural extension services. We estimate the causal impacts of the program on the mean and variance of outcomes of interest and on development resilience itself by exploiting the program rollout to overcome problems related to endogenous household investment and production decisions. Contemporaneous with Cissé and Ikegami (2016), this research is among the first to estimate the impact of a development intervention on household resilience.

Reinforcing the results of other recent analyses of livestock transfer programs (Bandiera et al., 2017; Ahmed et al., 2009; Das and Misha, 2010; Emran et al., 2014; Banerjee et al., 2015; Rawlins et al., 2014; Jodlowski et al., 2016; Kafle et al., 2016), as well as Dercon (1998) who models livestock acquisition as a stochastic path out of poverty for households, our results show that this multifaceted "big-push" intervention decreased poverty rates, increased consumption expenditures, increased livestock production, and increased asset holdings and earnings from self-employment. These effects are found to continue three and half years after the initial round of the intervention, and to have increased over time. Assuming that the program's benefits at year 3 are repeated through the 20$^{\text{th}}$ year of the intervention, the ratio of program benefits to costs is approximately 4.5.[1]

Extending previous work, our results show that the integrated livestock transfer program significantly increased household development resilience. The program increases beneficiaries' likelihood of being non-poor in future periods; households receiving both training and livestock at the baseline are 44% more likely to be non-poor than Control households 42 months after

[1] Most early livestock transfer programs, however, were plagued by implementation and targeting problems and hence have been deemed largely to have failed (Ashley et al., 1999). India's Integrated Rural Development Program (IRDP), for example, is thought to have been highly ineffective because of flaws in targeting and design (Drèze, 1990; Pulley, 1989).

the intervention. Moreover, we find that the program increased headcount resilience among participant households. While more than 80% of the treatment households are resilient at the endline, the comparable endline headcount resilience rate for Controls is only 28.6%. Decomposing these effects into first (central tendency) and second (spread) moments reveals that the livestock transfer and training program has both increased mean household asset holdings and decreased the variance in asset holdings. The program has shifted the conditional asset distribution upward and truncated uncertainty in asset holdings.

Measurement of program impact on resilience is especially relevant to understanding the impact of asset transfers. Such programs are often motivated by an expectation that sufficiently large transfers can enable households trapped in poverty to move onto a different growth trajectory towards a non-poor steady state. Transitioning from a growth dynamic associated with a low-level equilibrium to one that leads to a non-poor equilibrium state may be impossible without asset transfers or other programs to enable sufficient fixed investment. While the theory of bifurcated growth dynamics justifies "big-push" interventions, impact evaluation that focuses only on the first moment of outcomes ignores the potential for shocks or stressors to move households who have received transfers back to a low-level equilibrium. Development resilience, in contrast, quantifies the probability that a beneficiary household might move back into poverty and permits assessment of an intervention's effect on that probability.

By comparing resilience results with standard estimates of program impact on asset poverty, we demonstrate the value of measuring resilience in the context of impact assessment. Though both resilience and the conventional impact measures show that the program improved the welfare of recipients, we find notable differences in magnitudes across the methods. Differences are most striking for households observed around the asset poverty threshold. We find that while a substantial number of households who received partial treatment from the program gained sufficient assets to be classified as non-poor at the midline, they demonstrated too low a probability of remaining non-poor over time to be classified as resilient. This discrepancy points to the practical significance of failing to account for nonlinearities in welfare dynamics and limiting analysis to the first moment in the distributions of welfare outcomes. In this case, resilience measurement provides more insight about household status than conventional measures.

The next section of this paper presents the theory of development resilience and discusses a primary mechanism through which a transfer program is likely to affect poor households' livelihoods. Section 3 explains the empirical implementation of the development resilience concept. Section 4 describes the program setting, the intervention and the research design. Program treatment effects are presented in section 5; development resilience results and their comparison with impact evaluation results are presented in section 6. Section 7 explores the mechanism of program impacts by presenting evidence on reallocation of household labor. Section 8 compares program benefits relative to costs. Section 9 concludes by discussing the merits of estimating development resilience in impact evaluation and possible limitations and drawbacks to development resilience.

2.2 Development Resilience

Resilience as a development concept draws on ideas from ecology, engineering and economics. Resilience has roots in ecology focusing on the capacity of a system to maintain functionality when shocked (Holling, 1973) as well as on the systems ability to persist, renew, and redevelop (Holling, 1996) in the face of uncertainty and perturbations.[2] The concept of vulnerability in economics is closely related to ecological resilience, and refers to a probabilistic ex-ante measure of the likelihood that future consumption will fall below a defined (normative) poverty threshold (Chaudhuri et al., 2002; Calvo and Dercon, 2007; Ligon and Schechter, 2003; Christiaensen and Subbarao, 2005).

Development resilience builds on the concept of vulnerability in two important ways. First, the vulnerability measurement literature is predominantly concerned with the immediate impacts of shocks and does not account for exposure to stressors. Resilience, on the other hand, focuses on the longer-term impacts of both shocks and stressors. The emphasis on stressors is important in light of studies such as Rockmore's (2017) study of conflict in Northern Uganda, which finds that aggregate welfare losses from insecurity are larger than the realized violence. Second, the emphasis of the vulnerability literature on the immediate impact of shocks largely ignores welfare

[2]See Folke (2006) for a review of resilience in the ecology literature.

path dynamics. In contrast, development resilience is the study of well-being dynamics incorporating the possibility of nonlinear welfare growth paths. Operationally, these differences mean that while analysis of vulnerability can be implemented using cross sectional or short term panel data exploiting heterogeneity among the households or individuals within a sample, resilience measurement requires data collected over a longer time frame to exploit the inter-temporal variation of a household or individual.

This paper follows Barrett and Constas's (2014) conceptualization of resilience: the capacity over time of a person, household or other aggregate unit to avoid poverty in the face of various stressors and in the wake of myriad shocks. If and only if that capacity is and remains high over time, then the unit is resilient.

Barrett and Constas (2014) use a conditional moment function for well-being in a multiple equilibiria poverty trap to represent resilience, $m^k(W_{t+s} \mid W_t, \varepsilon_t)$, where m^k is a k^{th} moment of well-being at time $t+s$ and $s > 0$; with resilience a function of well-being W_t and random shock ε_t at time t. The deterministic relationship between W_t and W_{t+s} typically employed in the poverty trap literature is replaced with a conditional moment growth function and associated conditional dynamic transitional distribution functions. Although demonstrated using a multiple equilibria poverty trap, Barrett and Constas's (2014) resilience concept does not require a nonlinear path dynamic with multiple steady-state equilibria and is equally relevant in the case of the existence of a single steady-state equilibrium below the poverty line. A household's development resilience can be measured as the cumulative probability above the dynamic poverty threshold in the case of multiple equilibria and as the cumulative probability above the static poverty line $\bar{W}$ in the case of a single equilibrium. Unless the entire probability distribution sits above $\bar{W}$, there exists some probability that the household will fall into poverty. As less of the probability distribution falls below the poverty threshold, a household becomes more resilient. The likelihood of falling into poverty therefore depends on the household's level of well-being at time t and the dispersion in the distribution of outcomes.

As a simple descriptor, this resilience measure provides a consistent estimate of the true population conditional poverty level. However, a simple conditional mean of poverty status should provide a similar result. For example, if there were a single asset type or uniform asset types and *iid* shocks and stressors, a non-parametric regression of poverty status in time t on treatment should provide the same answer as the resilience calculation based the conditional moment functions. Nonetheless, estimating the conditional moment functions offers additional value in two ways. First, estimation of the value of the polynomial on lagged wealth allows for nonlinear persistence, which can enhance both forecasting and identification of heterogeneous response to common wealth shocks. The practical significance of this is suggested by studies such as Jalan and Ravallion (2001) and Lokshin and Ravallion (2004), which demonstrate that the same negative shocks have more persistent effects on poorer households than wealthier ones. The second advantage of the method is that it allows distinguishing whether the estimated relationship between wealth and resilience is driven by effects on conditional mean or on conditional variance. Simple theory and the prior literature (Rosenzweig and Binswanger, 1993) would suggest the effect of an asset transfer program is likely to be mainly in the conditional mean as decreasing absolute risk aversion should lead wealthier households to pursue higher return, higher variance strategies. Estimation of the conditional moment functions permits one to test that theory directly; a simple descriptive of the conditional poverty rate cannot provide these insights. For example, we find that numerous households are non-poor based on their mean asset holdings but are not resilient to remain non-poor over time once we account for the estimated asset holding variance (Section 2.6.1).

Resilience theory implies that development policies and interventions should focus on increasing household capital, decreasing downside risk and changing underlying development-impeding structural characteristics at time t (Barrett and Constas, 2014). The intervention analyzed in this paper is focused on enacting precisely these sorts of changes: transferring improved breeds of livestock, providing livelihood skills through training, and providing agricultural and veterinary extension services. To reflect the program, we define resilience exclusively in asset space and understand it as the capacity of a household to hold productive asset stock above a minimum critical asset poverty threshold (either dynamic or static) over time. Increasing resilience therefore means increasing the probability of holding assets above the defined threshold. Such an improvement could be the result of increases in the conditional mean asset stock, a decrease in the conditional variance or both. Given potential stochastic welfare outcomes related to uncertainty in herd dynamics and to the variety of risks households are exposed to, assessing the programmatic effects beyond the first moment of the outcomes of interest may provide greater insight about the status of the program recipients. This is especially true in the presence of bifurcating dynamics (Carter et al., 2007; Barrett et al., 2016). For example, a negative shock could imply a draw below the dynamic asset poverty threshold, setting the household on a trajectory towards a lower level equilibrium. As with negative shocks, large enough positive nudges have the potential to move the poor onto a path towards a non-poor, higher resilience state. Limiting the analysis to the first moment in the distributions of welfare outcomes, however, will not provide such insights.

2.3 Development Resilience Measurement

We construct resilience scores using the econometric technique proposed by Cissé and Barrett (2016) and applied in different contexts by Upton et al. (2016) and Cissé and Ikegami (2016). We then use the estimated resilience scores as outcome variables in an impact evaluation of the livestock transfer program. First, assuming a first-order Markov processes, the mean (indicated by the M subscript) stochastic asset level of household i at time t, (W_{it}), is modeled as a polynomial function of its lagged asset ($W_{i,t-1}$), a vector of household characteristics, X_{it}, and its exposure to random shocks ε_{it}:

$$W_{it} = \sum_{j=1}^{k} \beta_{Mj} W_{i,t-1}^{j} + \gamma_M X_{it} + \varepsilon_{Mit} \tag{2.1}$$

Included in the household characteristics are indicators for survey wave dummies and the interaction between each treatment assignment and survey wave dummy. The polynomial lagged asset measures are included to allow for S-shaped dynamics that are typical of multiple equilibria poverty traps, where $k = 3$ is its most parsimonious parametric specification (Barrett et al., 2006). Assuming $\mathbb{E}[\varepsilon_{Mit}] = 0$, the first conditional moment (μ_{1it}) is predicted as:

$$\hat{\mu}_{1it} = \mathbb{E}[W_{it}] = \sum_{j=1}^{k} \hat{\beta}_{Mj} W_{i,t-1}^{j} + \hat{\gamma}_M X_{it} \tag{2.2}$$

Following Just and Pope (1979) and Antle (1983), residuals from the first moment equation can be used to model the second moment (subscript V) as below:

$$\hat{\varepsilon}^2_{Mit} = \sum_{j=1}^{k} \beta_{Vj} W^j_{i,t-1} + \gamma_V X_{it} + \varepsilon_{Vit} \tag{2.3}$$

Again, assuming $\mathbb{E}[\varepsilon_{Vit}] = 0$, the predicted variance of a household i at time t (μ_{2it}) then is:

$$\hat{\mu}_{2it} = \sum_{j=1}^{k} \hat{\beta}_{Vj} W^j_{i,t-1} + \hat{\gamma}_V X_{it} \tag{2.4}$$

The first two moments are sufficient to describe household i's conditional transition distribution function of asset holding at time t if $W_{i,t-1}$ is distributed normally, lognormally or gamma. Once the function is identified, the development resilience of a household i at time t ($\hat{\rho}_{it}$) is the probability that the household will hold assets above a critical asset poverty threshold ($\bar{W}$) at period t:

$$\hat{\rho}_{it} \equiv P\left(W_{it} \geq \bar{W}\right) = \bar{F}_{W_{it}}\left(\bar{W}; \hat{\mu}_{1it}(W_{it}, X_{it}), \hat{\mu}_{2it}(W_{it}, X_{it})\right) \tag{2.5}$$

where $\bar{F}(.)$ is the assumed cumulative distribution function. Since the resilience measure increases with the upward shift of the conditional transitional distribution, greater resilience will be achieved by increasing the conditional mean, decreasing the conditional variance when mean is above the minimum threshold, $\bar{W}$, or both. The next section describes the intervention studied in the paper.

2.4 Program Intervention and Research Design

The Copperbelt Rural Livelihoods Enhancement Support Project (CRLESP) was implemented by Heifer International with funding from Elanco Animal Health (USA). The project operated in twelve rural communities in Zambia's Copperbelt province. The region, which relied heavily on copper, has gone through a difficult economic transition over the last three decades resulting in the loss of employment and loss of remittances in rural areas (World Bank, 2007). Many dislocated mine workers have turned to agriculture. Despite the availability of good quality farm land, limited asset holdings, limited farm and livestock management skills, and credit and market constraints have contributed to low agricultural and economic productivity, food insecurity, and poor child nutrition (Heifer International, 2010).

2.4.1 The Intervention

The CRLESP encouraged poor households to engage in commercial livestock activities through livestock transfers, training on livestock management and basic household livelihood skills, and provision of agricultural extension and veterinary services. Further, the program attempted to mitigate poor health and raise awareness regarding HIV/AIDS, and the importance of improved hygiene and sanitation through various community health trainings. Communities and households had to pass a screening process and follow a set of guidelines to qualify for program participation. Community members first organized themselves into groups and submitted an application to one of Heifers Zambia offices. Households in approved groups had to demonstrate their eligibility, which was contingent on commitment to participate in training activities, commitment to construct an animal shed, and payment into a community insurance fund. The screening excluded the poorest members of the community but the program participants were poor; about 60% of the households in our survey lived on less than USD 1.90 purchasing power parity (PPP) per person per day at baseline. Similarly, households with professional employment or sufficient assets to generate reliable income were screened out of the recipient pool.[3]

Due to the implementer's capacity constraints, the program was implemented in phases based on a queue that was established using date of application. Communities earlier in the queue received support in the initial round, while other qualified communities, referred to as "Prospectives", were wait-listed until a future date when resources would become available. However, every community in the target district had equal opportunity to apply at the same time. Heifer Zambia advertised the program intensively through the local media and through the government agricultural extension agents

[3]The screening process implies that the group may not represent the population of Zambia or the Copperbelt. In addition, individuals self-selected into groups (and hence into the program) to have access to livestock. Participant households, therefore, may differ from a typical Zambian household in preferences and other unobservable factors.

working in the region. The information dissemination across the communities regarding the program and application process was consistent in timing and content. Geographically, there is no significant disparity in distance to Heifers regional office in Ndola, Zambia from these communities. Applications were primarily submitted by women-led self-help groups. Groups based in twelve different communities qualified for the program. The sample for this study consisted of groups from the three communities scheduled to receive services around the time of the planned baseline survey plus groups from communities that were slated to receive services in the next opportunity. The communities that had already begun to receive services and those that were further down in the queue were excluded from the study. While all households in groups identified to receive treatment at the baseline received livelihood skill trainings and associated benefits of enhanced social capital, resource constraints meant only a randomly selected subset of these households could received livestock at the start of the project; we refer to these early recipients as "Originals". Depending on the ecological and market conditions of their location, Originals were given either a pregnant dairy cow, two pregnant draft cattle or one male and seven female meat goats. A bull was also given to each group that received draft or dairy cattle to service members' donated animals. Irrespective of animal type, the monetary value of the livestock transfer was similar across recipients, USD 1629 PPP on average. Originals were required to pass on a female offspring for each female animal they received through the program to the members of their groups that did not receive a transfer in the initial round. These second-phase recipients are referred to as a "Pass on the Gift" (POG) households. While Originals received full treatment (training and productive assets) and POGs received partial treatment (training at the baseline and a lower value asset transfer after a delay), Prospective households, which are spatially separate from other groups, received neither.

2.4.2 Data and Research Design

The project collected six rounds of detailed demographic and socioeconomic information from sampled households. The baseline included 106 Original, 111 POG and 67 Control households and was conducted in January and

February of 2012, overlapping with the timing of the initial livestock transfer. Follow-up surveys began six months later and were conducted July/August 2012, January/February 2013, July/August 2013, January/February 2015 and July/August 2015.[4]

We exploit the rollout of the program to identify the program impacts. Since both the early recipients (Originals) and future recipients (Prospectives) passed identical screening, self-selected for participation, and have equivalent eligibility, we assume the two groups to be comparable on unobservables and treat the Prospectives as a pseudo Control group. These two groups differ on timing of application to the program only. Correlation between unobservable group characteristics and application timing could threaten identification, but observable data provide no evidence that such correlation exists. Furthermore, the Original and Control households reside in different villages and spillover across communities is unlikely. Nonetheless, a challenge to our identification is that Control households might alter their behavior in the anticipation of receiving the livestock transfer.[5] Jodlowski et al. (2016) find no such anticipatory behavior in the first four rounds of the panel. We acknowledge that the experimental design based on the heterogeneity in the application timing is not a pure RCT, however, the window between the call for application and choosing the program recipients was very narrow.[6] Given the rural setting with limited transportation and communication infrastructure, we believe the heterogeneity in application timing between the first three and the next two communities is random rather than systematic. Based on equal eligibility, the fact that Controls went through

[4]The household surveys collected household consumption and asset holdings. We utilize community-level food prices collected during the baseline survey to calculate households' food expenditures. Regarding asset values, for each household we calculated a per unit value for each asset owned. We used the median of the asset unit values in the community as the community level price/value for each asset. All monetary amounts in the paper are PPP-adjusted USD terms and are deflated using CPI to 2012 prices using PPP and CPI published by the World Bank. In 2012, 1 USD was equivalent to 2.5 PPP adjusted Zambian Kwacha.

[5]For example, the Control households might begin focusing on livestock and give up other activities in expectation of the arrival of the livestock. This kind of anticipatory behavior would bias the treatment effect downward if returns from livestock are at least as high as the other activities. An upward bias could emerge if households divest from some income generating activities or decrease total labor supply in advance of the transfer and and hence appear worse off than they otherwise would (Ashenfelter, 1978; Ashenfelter and Card, 1985).

[6]Unfortunately, we do not have exact dates but Heifer Zambia staff report that applications were submitted within a short period of time - on the order of 1-2 weeks.

the same selection process as treated households, observation from our field visits, focus group meetings, and multiple discussions with the implementing staff and extension agents in the field, we believe that the Prospective household are appropriate counterfactuals.

Although the POGs were left out of the initial livestock transfer at random and come from the same groups as the Originals, we do not use POGs as a comparison group in the analysis for three reasons. First, the POGs received all the trainings regarding animal management, livelihood skills and health at the same time as the Originals, which could affect management of farm animals and other productive assets they already owned. Second, POG households started receiving immature animals from the Originals as early as six months after the baseline, therefore, anticipatory behavior among the POGs could be a factor. Third, POG households reside in the same communities as the Originals and are more likely to experience project spillovers. An additional complication is the significant heterogeneity in the timing of asset transfer to POG households; while some households received livestock as early as six months after the baseline, others waited up to 36 months. We normalize the timing of transfer and perform an event-study analysis on the outcomes of interest to check the appropriateness of POG households as a comparison group for Originals (Appendix A.3). The results suggest POG households are not a suitable comparison group for the Originals. Thus, we use the POG households in our analysis as a second treatment group.

Table 2.1 provides baseline balance tests for the Treatment and Control groups. The tests suggest no significant differences in means between the Control and Original households asset and revenue and income variables (Panels B and D). We do see differences in household size (Panel A), poverty status (per capita), and per capita household expenditures (Panel C). All household characteristics in Panel A are balanced except the household size. Compared to the Controls, Original household have more nonelderly female adult members and children. In the presence of economies of scale, failure to adjust the consumption for household size may lead to overestimation of poverty for large households and underestimation for small households, driving the differences in per capita expenditures and poverty status. The household-level (as opposed to per capita) expenditures between the groups is balanced (Panel C: line 3). We assume that the variation in poverty and expenditure variables at the baseline (Panel C) does not reflect a systematic difference in groups ability to organize, willingness to participate in the program, or capability to rear animals; rather, differences are likely due to relatively small sample sizes and differences in household size. As a robustness check, we adjust the household size using the OECD adult equivalency (ae) method and report adult equivalence adjusted poverty and expenditure variables in Panel E. The poverty rates between the groups using the adult equivalence correction are statistically equivalent. The differences in per capita expenditures between the groups are still significant but small in magnitude. Moreover, one may expect richer farmers to be better organized and apply earlier; however, this is not the case as Control households are less likely to be poor than the treated households. Similarly, if greater poverty reflects lesser livestock entrepreneurial ability, our strategy, should underestimate the program effects. To control for any unobserved individual heterogeneity, we use household fixed effects in our estimation.

The attrition rate of 13% (Table 2.2) is comparable to other asset transfer program evaluations with similar durations and survey lags (Banerjee et al., 2015; Bandiera et al., 2017). POG households are less likely than the Control households to be interviewed in all six rounds. Original households, on the other hand are as likely to be followed throughout the panel as the Control households and we find no difference in attrition by baseline outcomes and characteristics. For our analysis we restrict the sample to the 247 households interviewed in all six survey rounds.

2.5 Program Treatment Effects

We begin the program evaluation with the standard first-moment impact assessment both to motivate our resilience estimations and to demonstrate that measuring a positive asset change is a necessary but not sufficient component of determining changes in household development resilience. Exploiting the experimental variation caused by the rollout of the program into two treatment arms and a control group, we estimate the following difference-in-differences/fixed-effect specification:

$$
\begin{aligned}
y_{it} = & \alpha + \sum_{t=1}^{2} \beta_t (T_t \times Original_i) + \sum_{t=1}^{2} \delta_t (T_t \times POG_i) + \sum_{t=1}^{2} T_t \\
& + Original_i + POG_i + \eta_i + \varepsilon_{it}
\end{aligned} \tag{2.6}
$$

where y_{it} is an outcome of interest for household i at time t and t takes the values of 0, 1 and 2 for 2012 baseline, 2013 midline and 2015 endline respectively. Although the project collected five rounds of follow-up surveys, the information collected was not identical across rounds. Depending on the availability of data on the outcome variable, we define 2013 midline (time 1) either as 12 months or 18 months, and 2015 endline (time 2) either as 36 months or 42 months post baseline. T_t are indicator variables that refer to survey waves. $Original_i$ and POG_i are indicators for two treatment arms. As the household's timing of application to the program determined the treatment status, we include household fixed effects η_i to control for unobserved heterogeneity and cluster the error term ε_{it} at the household level. As a result, the coefficients on $Original_i$ and POG_i in Equation (2.6) are not identified. The equation, nonetheless, can be treated as the garden variety difference-in-difference specification.

β_t and δ_t are the coefficients of interest, which under the assumptions of "parallel trends" and stable unit treatment value assumption (SUTVA) identify intent-to-treat (ITT) effects of the program on Original and POG groups respectively. As discussed in the research design, we expect both assumptions to hold. First, pre-treatment, the Control (Prospective) group is identified through a process identical to that of the Original and POG groups. Second, Equation (2.6) controls for all household-specific time-invariant factors and time-varying factors that are equal across all groups. Third, we expect zero spillovers across treatment and comparison communities because of their relative geographical separation and hence SUTVA holds. SUTVA between the two treatment groups, however, may not hold as both Original and POG groups reside in the same communities. Hence, we cannot explicitly distinguish between the pure program effects and the general equilibrium responses induced by the program in the community and this is an important distinction. Nonetheless, the spillovers within the communities are due to the program itself; the coefficients, therefore, can be viewed as the overall program treatment effects. Similarly, complete compliance implies that the coefficients also identify treatment-on the treated (TOT) impact of the program.

2.5.1 Productive Assets and Household Durables

Table 2.3 presents the program impacts on accumulation of productive and durable assets using Equation (2.6). Information on the full asset portfolio was collected in the baseline and in follow up survey waves of July/August 2013 and July/August 2015 (18 and 42 months after baseline); we refer to these follow up rounds as time 1 and 2 in the table.

First we analyze whether beneficiary households undertake the livestock activities prescribed by the program and measure the direct impact on livestock holdings and earnings. Table 2.3 reports impacts on herd size and quarterly income from livestock related activities. Originals received an average of 0.88 tropical livestock units (TLU), which is not included in the baseline herd size. A one-unit TLU gain, 0.99 to be precise, relative to the Controls one year post-intervention represents an increase of 0.11 TLU above the transfer amount, meaning the recipients had begun to increase their holdings beyond the initial transfer. The Originals' gains are particularly notable since they are required to pass on female offspring to POGs. Within one year, the value of the Originals' livestock holdings increased by USD 460.6 per capita relative to the Control households. Half of the increase was due to the initial livestock gift.[7] Moreover, an increase of USD 64.6 in quarterly income from selling livestock and livestock products during that time period implies that the transfers were productive within the first year of the intervention. Among POGs we find a small increase in herd size and herd value but no significant effect on livestock revenue in the first year, con-

[7] In the first wave of the transfers, the Original households received livestock worth about USD 1629, about 229 per capita, which is not included in the baseline asset value. Therefore, 49.8% (= 229/460.6) of the first year rise in the value of livestock can be attributed to the transfer itself. The per capita change in the herd size may not directly reflect the change in the value of herd size because; first the value of the same type of livestock may change over time in the community - after seeing the benefits, livestock may become more valuable or the presence of too many livestock may decrease the price etc. Second, we calculate the value of herd size using the method described above and use country-level CPI to deflate the value to the baseline. However, if the increase in the price of animal is more than the CPI adjustment, we may face this discrepancy, which is exactly the case.

sistent with POGs receiving immature animals after the Originals' donated livestock produce offspring.

Three years after the baseline, intervention effects are large among both the Originals and POGs. Relative to the Control group, the herd size of the Original households increases by 1.11 TLU or 92% of the baseline mean, and POGs' herd size increase by about one TLU unit. The gains in herd sizes are associated with increases in livestock-based revenue for both groups. The Originals experience an increase in livestock-based revenues of 821.6% (USD 110.7) relative to the baseline. POGs, meanwhile, see an increase of USD 72.1 (imprecisely estimated) in income from livestock. Comparing the Originals' 18 and 42 month impacts indicates that the program effects are sustained with continued growth in herd size and related earnings. After 42 months, the value of animals owned by Originals has increased by 261% (USD 497.1) relative to the baseline, which is 141% net of the transfer value. The 18 month and 42 month impacts on POG households' livestock values are USD 173.8 and 305.5 respectively. Because the livestock transfers to POGs were spread over the period analyzed, we are unable to separate out the direct transfer value from the added value generated after the transfer. Finding that the treatment effects grow after the initial transfer suggests the transfers helped households sustain economic growth and perhaps provided a path out of poverty. The resilience estimations will test this hypothesis.

Aggregating across asset types, Table 2.3 shows that by three years post-intervention total household asset value increased by 124.6% (USD 495.7) among the Originals. The increment is robust relative to the first-year increment of USD 477.1 (with the p-value of 0.825 on the equality between the two periods' impacts). The impacts are significant among POG households as well: USD 279.3 and 294.5 after one and three years post-intervention, respectively. The growth in livestock assets is the major component driving the aggregate change. Overall, these results suggest that the poor households in rural Copperbelt province are able to take on and sustain livestock rearing activities that are likely to be more rewarding than the available alternatives.

2.5.2 Consumption Expenditure, Food Security, and Asset Poverty

We analyze program impacts on poverty status, consumption expenditures, a subjective food security measure and asset poverty status at 12 and 36 months after the intervention using Equation (2.6) and present the results in Table 2.4. These two survey rounds occurred in the same season as the baseline and are therefore more appropriate for analysis of consumption impacts than the later rounds used in analysis of assets in Table 2.3. Relative to the Control group, the share of Original households with expenditure below the USD 1.90 poverty line drops by 22.0 percentage points (pp) after one year. The impact is even greater after three years: 31.4pp drop or 50.3% decrease from the baseline mean. The impact on the partially treated POG group is more modest and is statistically insignificant.

Relative to the Controls, the weekly per capita total expenditure of the Originals increases by USD 7.47 or 58.8% of the baseline mean after three years. This is higher relative to the one-year effect of USD 3.34 indicating increase in gains over time. Although positive, gains among POGs are not precisely estimated. Columns 2 and 3 decompose the total expenditures into food and nonfood expenditures. Three-year gains of 3.72 and 3.75 USD among the Originals in food and nonfood expenditures, respectively, relative to the Controls are significantly greater than the one-year impacts. Consumption changes for POGs are statistically indistinguishable from zero. Because of the program design, all POG households received training but not every POG received animals early enough to be productive or affect consumption over the observed time-period. These effects are comparable to Kafle et al. (2016) which analyzed data from the first 18 months of the same program. Although consumption expenditures show no evidence of impact for POGs, significantly higher shares of both Original and POG households consider themselves to be food secure compared to the Controls (Column 5).

Based on the relationship between consumption and assets, explored in Appendix A.2.2, we estimate an asset poverty line at USD 308 (PPP) per capita. This asset poverty line represents the per capita asset wealth that is associated with consumption at the expenditure poverty line. As the table shows, we find a significant reduction in the number of Original and POG households below this threshold, compared to the Control group. While

POGs show little change with respect to the expenditure poverty line, we find that the program has successfully moved some of them above the asset poverty line. The apparent decrease in the magnitude of the treatment effects on asset poverty over-time among the Original group raises concern about sustainability of impacts, however, the test of equality of the treatment effects between the two periods is negative. Indeed, three-year impacts for both the treatment groups (Original and POG households) are statistically equal if not higher in magnitude than the one-year impacts for almost all the outcomes considered in this section. These findings suggests that program impacts do not dissipate and likely increase over time.[8]

Given the sequence of program implementation, the possibility that the early entrants (Originals) may crowd out others in the community from livestock rearing activities is of concern. Our results show no evidence of such crowding out. Although we cannot entirely rule out the general equilibrium responses to greater demand for livestock labor or increased local supply of milk, meat, or animal traction, the differences in treatment effects between the Originals and the POGs are mostly attributable to delayed impacts rather than to accrual of unique benefits to early adopters. The differences diminish over time in almost all the outcomes considered in this section. In particular for herd size, the outcome that is directly affected by the program and is most likely to be affected by the Originals' head start, we observe that POGs experience the same impact as the Originals (1.03 vs 1.11) three years after the baseline. Rather than early adopters crowding out others, we see evidence that the differences in treatment effects between the two groups are likely to disappear over time.

2.6 Effects on Development Resilience

We model resilience explicitly in asset space because assets serve as an input for future household asset accumulation and hence welfare gains. Information on assets in the panel was collected at baseline and 18 months, 36 months and 42 months after the baseline. Given the structure of the data and Markov

[8]Analyses of heterogeneity in impacts using quantile regression methods in Appendix A.5 shows that these effects are consistent across quantiles, though weaker at the extremes.

first-order path dynamics, we can recover parameters only on the last three rounds in the regression setting. Equation (2.1) reduces to:

$$W_{it} = \alpha + \sum_{j=1}^{k} \beta_j W_{i,t-1}^{j} + \sum_{l} \sum_{t=1}^{3} \gamma_{lt}(T_t \times D_l) + \sum_{t=2}^{3} \delta_t T_t + \theta Z_{it} + \varepsilon_{it} \quad (2.7)$$

where W_{it} is asset value of household i at time t in natural log. Time period t takes the values of 0, 1, 2, and 3 for baseline, 18, 36 and 42 months after the baseline respectively. T_t are indicators for survey waves 18 months, 36 months and 42 months. D_l, where $l \in (Original_i, POG_i)$, are dummy variables for the two treatment arms. Z_{it} refer to family composition and other characteristics that influence asset accumulation, and ε_{it} are random shocks that household i faces. The originals received pregnant livestock during or soon after the baseline survey. The initial recipients reap benefits (milk, meat, ploughing, increase in herd size etc.) from the transfers well within 18 months. Therefore, we add transfer values to the Originals' baseline asset values, which serve as the lagged term for the survey round 4 (18 months) or $t = 1$ in the specification. Figure A.2 in Appendix A.2, which provides discussion on model selection, shows that the cubic fit and locally weighted regression (Lowess smoothing) of asset values on lagged values follow each other closely. We choose cubic ($k = 3$) as our preferred functional form.

Asset values are non-negative for all the households in the sample. Consequently, we assume the dependent variable to be distributed Poisson and fit a GLM log link using maximum likelihood on the mean. Using the parameter estimates from Equation (2.7), we predict the first moment of the asset distribution of household i at time t as in Equation (2.2). Squared residuals from Equation (2.7) are used to estimate Equation (2.3),[9] which recovers parameters to predict the second moment (Equation 2.4). We calculate each household's probability density function (pdf) of asset holdings for each period assuming the conditional transition distribution function to be gamma distribution.[10] We convert the poverty line of USD 1.90 PPP into an asset poverty line ($\bar{W}$) of USD 308 PPP as shown in Figure A.3 (Appendix A.2.2).

[9]Because variance must be nonnegative, we, again, assume the dependent variable to be distributed Poisson and fit GLM log link using maximum likelihood.

[10]The parameters (shape and scale) for Gamma distribution are: $W_t \mid W_{t-1} \sim \Gamma(\frac{\mu_{1t}^2}{\mu_{2t}}, \frac{\mu_{2t}}{\mu_{1t}})$.

Using the calculated minimum asset threshold, we estimate each household's development resilience in each period (ρ_{it}).

2.6.1 Resilience Treatment Effects and Headcount Resilient Rate

In order to assess the program's impact on development resilience, we follow Cissé and Barrett (2016) that $\partial\hat{\rho}_{it}/\partial X_{it}$ is a characteristic X_i's impact on development resilience and estimate the following specification:

$$\hat{\rho}_{it} = \alpha + \sum_{j=1}^{k} \beta_j W_{i,t-1}^{j} + \sum_{l}\sum_{t=1}^{3} \gamma_{lt}(T_t \times D_l) + \sum_{t=2}^{3} \delta_t T_t + \theta Z_{it} + \varepsilon_{it} \tag{2.8}$$

$$\begin{aligned}\text{Note that:} \quad \frac{\partial\hat{\rho}_{it}}{\partial(T_t \times D_l)} &= \hat{\gamma}_{lt} \\ &= \mathbb{E}[\hat{\rho}_{it} \mid W_{i,t-1}^{j}, Z_{it}, T_t, D_l = 1] \\ &\quad - \mathbb{E}[\hat{\rho}_{it} \mid W_{i,t-1}^{j}, Z_{it}, T_t, D_l = 0] \\ &\quad \text{where } t \in [1, 2, 3]\end{aligned} \tag{2.9}$$

which are the differences of the conditional means between the treatment and Control groups at time t. The causal inference of the program's impacts, γ_{it}, is based on the conditional independence assumption:

$$\mathbb{E}[\hat{\rho}_{0it} \mid W_{i,t-1}^{j}, Z_{it}, T_t, D_l] = \mathbb{E}[\hat{\rho}_{0it} \mid W_{i,t-1}^{j}, Z_{it}, T_t] \tag{2.10}$$

As discussed in Section 2.4.2, the treatment assignment was quasi-randomized with each group having equal eligibility into the program. Pre-intervention, the Treatment and Control groups are balanced on observables, including mean assets (Table 2.1). We expect both the first and second moments of the asset holding to be equivalent between the Treatments and Control households prior to the intervention.

Panel A in Table 2.5 presents the estimated average marginal treatment effects on development resilience, measured as the share of the probability distribution of asset holding of a household that is above the asset poverty line.[11] Relative to the Controls, both the Originals and POGs have signifi-

[11] Since the resilience outcome is measured in fractions i.e. $\hat{\rho}_{it} \in [0, 1]$, we assume the dependent variable is distributed binomially and fit the GLM logit link regression using

cantly higher resilience to poverty in all the three rounds. The development resilience score is 0.228 points or 87.7% higher for the Originals after 18 months of the treatment than the Controls. Similarly, the Originals are 41.3% (0.145 points) and 44.1% (0.167 points) more resilient than the Controls at 36 and 42 months post-intervention respectively. Among POGs, the program has increased household development resilience by 73.8% (0.192 points), 31.6% (0.111 points) and 29.0% (0.110 points) after 18, 36 and 42 months respectively. Although significantly higher in all rounds relative to the Controls, the impact appears to decrease in magnitude over time for both treatment groups. To provide evidence on this we test whether the 36 and 42 months impacts are equivalent to impacts 18 months post-intervention. The tests of equality of impacts between rounds, however, show no such evidence (with all the p-values from the tests above 0.35). These results are consistent with the treatment effects in Table 2.3, where the program impacts on asset values are also robust over time. Both the resilience and the difference-in-difference results suggest that the program has improved households welfare. Resilience results, in addition, show that the program has improved households ability to remain non-poor into the future.

Household resilience increases if the conditional mean of asset values increases, if the conditional variance decreases when the conditional mean is above the minimum threshold $\bar{W}$, or both. Estimating Equation (2.8) using predicted conditional household-time specific mean and variance as the dependent variables reveals that the mean asset holding among the treated groups increases compared to the Controls in all rounds (Panel B).[12] Moreover, the impacts on mean outcomes are similar for Originals and POGs. The impacts on the variance are significant for the Originals but are statistically insignificant for POGs (Panel C). While the conditional asset spread among the Originals drops significantly relative to the Controls (except in round 5), the asset spread for the POGs is equivalent to that of the Controls.

The absence of an effect on asset spread for the POGs likely reflects the heterogeneity in the timing of livestock transfer to the POGs, and the Just

maximum likelihood. We calculate the standard errors of the parameter estimates by bootstrapping the whole process (from mean specification to the resilience specification) and clustering at household level using 400 replications.

[12]Because both the first and second moments are nonnegative, we assume the dependent variables are distributed Poisson and fit the GLM log link regression using maximum likelihood.

and Pope (1979) and Antle (1983) method we use to calculate variance, which depends on the predictive power of the explanatory variables (lagged assets) and does not distinguish between positive and negative shocks. While the transfer value is included in Originals' baseline assets, the delayed transfer to the POGs is not. Therefore, though positive, the transfer acts as a shock and is likely to increase residuals in Equation (2.2) among POGs that receive the transfer. In the earlier rounds, because of the lower value asset transfer and the fact that only a few POGs had received transfers, there is no change in the estimated residuals compared to the Controls. The $\hat{\beta}_V$'s for POGs in Equation (2.3) are statistically equivalent to zero (not shown). Similarly, in the later rounds, although more POGs received their gifts, the immature animal early POG recipients received is likely to mature and stabilize in value. The difference in the transfer timing, therefore, is likely to lead to heterogeneous estimated residuals in Equation (2.2). Figure A.1 in Appendix A.1 reports the relationship between assets and estimated variance in our sample. The U-shaped curves suggest households at the extremes face higher asset volatility. Similarly, POGs, in general, face the highest level of variance in their asset holding 18 month post-intervention but as more and more POGs receive their transfer and for longer periods, the variance decreases and the distribution moves closer to that of Originals. In addition, the limited impact of the treatment in terms of reducing the dispersion of outcomes for POGs explains the smaller estimated program impact on POGs resilience compared to the Originals (Panel A).

Relating these results to the theoretical mechanism discussed in Section 2.2 suggests that the program shifted the first-moment dynamic growth path upward for both the treated groups. While the conditional transition distribution associated with the first-moment shrinks for the Originals, it remains unchanged for the POGs. Both cases, however, imply increasing resilience when the expected asset value is above the poverty line. In short, these results together with the difference-in-difference specification imply that the program has increased households' asset holdings and decreased their probability of falling into poverty.

Figure 2.1 presents the headcount resilience rate by treatment groups for each survey wave. We define household i to be resilient at time t if its probability of falling below the asset poverty line (i.e. its estimated resilience, $\hat{\rho}_{it}$) is greater than a minimum normative threshold ($\bar{R}$) at time t i.e. $R_{it} = 1$

if $\hat{\rho}_{it} > \bar{R}$; 0 otherwise.[13] Eighteen months after the initial treatment, most of the originals (77.1%) are resilient compared to only 18.2% and 17.5% of the POGs and Controls respectively. The number increases slightly for the Originals after 36 and 42 months of the intervention more than 80% become asset resilient. Similarly, the headcount resilience rate among the POG and Control households increases in later periods but more so for POGs. The gap between the number of resilient POG and Control households widens noticeably over time. However, among POGs the resilience treatment effects (the difference of average resilience scores between POGs and Controls) presented in Table 2.5 Panel A decreases over time. The distribution of resilience scores among the Control group, thus, is likely to be positively skewed in the later periods, whereas the distribution among POGs is likely to be more symmetric.

2.6.2 Resilience vs Impact Evaluation Measures

To provide the direct comparison between resilience and standard impact evaluation methods, we compare asset poverty rates and resilience rates. While households with an asset value above the calculated asset poverty threshold of USD 308 are defined as asset non-poor, households with estimated resilience score of 0.5 and above are classified as resilient. The treatment effects from the difference-in-difference specification for the two outcomes are reported in Table A.1 in Appendix A.1. While Originals are 47.0% less likely to be asset poor, they are 59.6% more resilient compared to the Controls 18 months post-intervention. Similarly, 42 months post-treatment Originals are more likely to be resilient than asset non-poor (52.7% vs 39.0%).

The difference in the effect size between the two outcomes is more pronounced among the POGs. Although the POG households are significantly more likely (24.3%) to be asset non-poor compared to the Controls at 18

[13]The resilience threshold ($\bar{R}$) is comparable to poverty line used in headcount poverty calculation; a unit is classified as resilient if it is above the threshold and non-resilient if below. Unlike the poverty line, which is generally rooted to some necessary expenditure requirement for household's functioning, the resilience threshold is arbitrary. We set the initial resilience threshold at 0.5 ($\bar{R} = 0.5$) and present the headcount resilience rate by treatment groups for each survey wave. The threshold of 0.5 is greater than the 0.25 used in Upton et al. (2016) but lower than 0.8 used in Cissé and Barrett (2016). However, we also calculate headcount resilience rate using 0.8 for sensitivity.

months after intervention, there is no difference in resilience rates between the two groups. We observe a similar pattern even after increasing the resilience threshold to 0.8 and changing distribution and functional form of the asset holding (Figure 2.2). This result emerges because a relatively high number of POG households are observed just above the asset poverty threshold with sufficient assets to be classified as asset non-poor but with inadequate probability of holding onto assets above the threshold in the future to be classified as resilient. In order to investigate this possibility, we report Kernel density household asset distribution over time by treatment status in Figure 2.3 (Panel A). While more Control households are likely to be observed above the threshold at the baseline compared to the POGs, the pattern reverses after 18 months, which is likely to generate significant positive treatment effects on POGs in the difference-in-difference estimations. However, we see no such clear pattern in resilience score distribution among Control and POG households that are above the resilience threshold (Figure 2.3: Panel B). Moreover, among the asset non-poor households at 18 months post-baseline, significantly fewer POG households are development resilient compared to their Control counterparts (37.0% vs 42.9% results not shown). In such scenarios, the standard static measurements such as asset poverty headcount might be misleading. The resilience measure, on the other hand, provides the likelihood of one's future outcome relative to the threshold given its present status. Hence, the resilience measurement yields more insight about households' capacity to escape or remain out of poverty.

2.6.3 Robustness Check

We re-estimate the effects on resilience using an alternative functional form, an alternative distributional assumption and an alternative estimation technique. Column 1 of Table 2.6 presents the program impacts assuming the polynomial lagged asset to be quadratic i.e. $k = 2$ to incorporate the single-steady- state equilibrium poverty trap discussed in Section 2.2. Column 2 presents the estimates assuming W_{t-1} to be normally distributed. Both sets of the estimates are comparable (in significance and magnitude) to the initial results presented in Table 2.5. Additionally, we estimate effects on resilience using OLS and again find results to be consistent with the earlier estimates. Figure 2.2 presents headcount asset resilience rates using alternative resilience thresholds. Figure 2.2a and Figure 2.2d are resilience rate using $\bar{R} = 0.8$ as the resilience cutoff i.e. a household is development resilient only if its resilience measure is above 0.8 ($\rho_{it} > 0.8$) assuming W_{t-1} to be distributed gamma and normal respectively. As expected, the count of resilient households decreases across the treatment groups and over-time (about 20% less) in both methods. Originals, nonetheless, are the most resilient across all survey waves. Functional form and distribution assumptions appear to be of no significance in the resilience rate calculation for our estimations. While Figure 2.2b shows the headcount resilience rates using the quadratic functional form, Figure 2.2c shows the headcount rates assuming a normal distribution. Estimates of the number of resilient households across the treatment groups in both specifications are consistent with the initial estimates. The estimated program impacts on asset resilience are robust across the choices of threshold, functional form, distributional assumption, and estimation technique.

2.7 Mechanism

We find that a time-limited integrated asset transfer program led to sustained gains in household consumption, income, asset holdings, and resilience. While we find no evidence of a bifurcated growth path inducing a poverty trap, the conditions in the research site suggest a single low-level equilibrium in absence of the intervention. In this setting, a large one-time asset and skill transfer is likely to ease households' capital and skill constraints and shift their growth curve in a northeast direction, which represents improvements both in well-being and resilience. Similarly, the program is likely to help households transition to more remunerative technologies, which, again, improves well-being and resilience. Lack of access to capital alone, however, is not a sufficient condition to keep poor in persistent poverty if they can sell their labor optimally. While the poor are generally endowed with labor but few productive assets, imperfections in rural labor markets can prevent them from fully utilizing their labor resources and prompt them to accept low-paying casual jobs (Bardhan, 1984; Drèze, 1988; Rose, 2001; Banerjee and Duflo, 2007; Kaur, 2014; Bandiera et al., 2017). A one-time productive asset transfer and training program, however, is likely to break the barriers the rural poor face in accessing capital, facilitating entry into higher return activities and moving them from a low-level growth path to a higher level one (Bandiera et al., 2017).

The program analyzed here intended to use livestock transfer and training to enable households to engage in more capital intensive self-employment. Analysis of adults' occupation choices and households' income from different streams can reveal whether change in labor allocation was actually part of the mechanism by which this program achieved impact. Table 2.7 shows that the program prompted households to take on self-employment activities and leave casual labor. Adult women in the Original households are 20.4% and 16.2% (23.6% increase relative to the baseline) more likely to be engaged in self-employment 36 and 42 months after the intervention. Additionally, three and one half years after the baseline, Original households have decreased participation in casual labor employment by 7.5% a decrease of 157.7% since the baseline relative to Controls.

Three years after the baseline, quarterly income from selling livestock products and cattle increases by 821% among Originals - an increase of USD 111 relative to Controls, which is significantly greater than the one-year increase of USD 64.6. Although statistically insignificant, POG households also experience increases in their quarterly income from livestock rearing (USD 72) in the three years since baseline relative to Control households. In addition to increased income from livestock, the results show treated households shift out of paid employment – relative to the Controls both the Originals' and POGs' paid income decreases in both periods. The results for total revenue show that shifts out of casual employment into livestock activities led to substantial increases in household revenues. Overall, these results suggest that the transfer of livestock and skills helped remove the barriers to entry into higher return labor activities which is consistent with a more stable asset base and greater resilience.

2.8 Cost-Benefit Analysis

A number of observers have called for increased attention to the costs of achieving impacts associated with asset transfers. Table 2.8 presents conventional benefit-cost measures for project assessment and extends them to indicate the cost of achieving increases in resilience. Details of the cost-benefit calculation are presented in Appendix A.4. In total, the direct costs of the program amount to USD 1853 per household 1629 for livestock and 224 for equipment and supplies. Most of the program costs, however, are indirect and related to supervision and program implementation (USD 2474 per household), which are spread over the duration of the program. Costs include staff wages and salary support for veterinarians and agricultural experts for the duration of the program. In addition, indirect costs include training, evaluation, travel and vehicle operation and other office expenses. The total program cost is USD 5009 per household for the full duration of the program. Compared to similar programs, this cost is higher than the BRAC program, USD 1363 (Bandiera et al., 2017), but comparable to the six Graduation programs, ranging from USD 1455 to 5962 (Banerjee et al., 2015).

Following Banerjee et al. (2015) and Bandiera et al. (2017) gains in household nondurable consumption are the core benefit measure. Estimated changes in household consumption expenditures are calculated by multiplying the weekly treatment impacts with average household size (7.1 in year one and 6.3 in year three) times 52. Year two impacts are assumed to be equal to the gains in year one. Similarly, we assume year three consumption gains to persist after the third year through year 20 and we report net present value of future gains in year four and beyond. We add year 3 asset gains and the total benefits amount to USD 22299 over the 20 year time horizon. Additional indirect benefits such as gains in human capital through better nutrition, increase school expenditure on children etc., however, are not accounted for in the analysis. Similarly, the program promotes social cohesion and learning; the potential gains through these avenues are difficult to capture. Our benefit analysis, therefore, underestimates true program benefits.

Row 7 shows the benefit ratio of the program, which is obtained dividing the total benefit by the total program cost. On average the benefit from the program is 4.45 times higher than its cost. The ratio is comparable to the findings from other livestock transfer programs. It is slightly higher than the ratios reported in Banerjee et al. (2015) (ranges from 2.6 to -1.9) and in Bandiera et al. (2017), which is 3.21. The ratio of benefit to cost is robust to different values of the discount rate and different time horizons.

Row 8 presents the calculated internal rates of return (IRR), which are based on the estimated changes in household nondurable consumption expenditures and calculated as the discount rate at which the net present value of the benefits are equal to the program cost. We follow Bandiera et al. (2017) and assume these gains last for a period of 20 years. The IRR is 24% at the mean – clearly exceeding the formal lending interest rate of 12.1% at the beginning of the project(World Development Indicators, 2017).[14] This implies that households in rural Zambia can finance these high-return activities if provided the access to formal credit. The IRR is robust to different values of the social discount rate and different program benefit time-horizons.

Panel C in Table 2.8 focuses on the cost of improving the resilience headcount by one percent at different resilience cutoffs. Costs are calculated as total transfer value divided by the gains in resilience headcount rate (see Appendix A.4.1). The original transfer value of USD 2145 (USD 1853 inflated to year 3) helped increase headcount resilience by 20.8% among the treated group (Original + POG) compared to the Control group at the 0.8 resilience cutoff 18 months post-intervention. If households are distributed uniformly over asset-holdings, an investment of USD 103 into the program moves 1% of the non-resilient households into resilience after 18 months of the investment, such that they have less than a 20% probability of falling into poverty in the future.[15] Consistent with the treatment effects (Table 2.3), the cost of increasing headcount resilience by 1% decreases after three and half years (USD 84); a greater number of treated households become resilient as transfers become more productive and/or higher numbers of the POGs receive transfers over time. As expected, the cost is lower at the 0.5 resilience cutoff - USD 100 and USD 58 at 18 and 42 months post-transfer, respectively.

[14] Internal rates of return are heterogeneous across livestock transfer programs. While the rate varies from 6.9% to 23.4% in the six Graduation pilots (Banerjee et al., 2015), Bandiera et al. (2017) report the rate of 22% for BRAC program in Bangladesh. The IRR for cash transfer programs are similar to the livestock transfer programs. Blattman et al. (2016) report IRR of 24% for a cash transfer of USD 150 towards non-farm self-employment activities along with training and follow-up supervision to ultra-poor in post-war Uganda.

[15] For this to hold households either have equal livestock rearing abilities or the abilities are orthogonal to the baseline assets. Since all households (treated and control) self-select themselves into the program and quasi-random treatment assignment means, on average, livestock rearing abilities between the groups are equal.

2.9 Discussion and Conclusions

This paper implements a quantifiable measure of household resilience and demonstrates its application and relevance in the context of an impact evaluation. Results from the impact evaluation find that a one-off transfer of assets and training increased household development resilience; the intervention shifted the conditional transition distribution of households' asset holdings upward, increasing expected asset holdings and decreasing conditional variance. Findings demonstrate that attention to conditional variance in impact on assets provides important insights into program effectiveness and persistence of estimated effects.

Resilience as a household outcome offers three important advantages for impact analysis. First, because it is based on the full distribution of household welfare, the development resilience measure provides a more complete picture of intervention impacts, yielding insights into household capacity to avoid falling into poverty in the foreseeable future. In particular, estimation of the conditional moment functions allows for nonlinear persistence, which can improve forecasting of households' future states. In addition, the conditional moment functions make it possible to distinguish whether estimated effects are primarily attributable to changes in the conditional mean or the conditional variance. These inferences are especially significant for households at or near the poverty threshold. Our finding that a substantial share of households in the analysis are asset non-poor and yet not resilient illustrates this point. Resilience measurement yields policy-relevant insights into household well-being that conventional measures like poverty headcount miss.

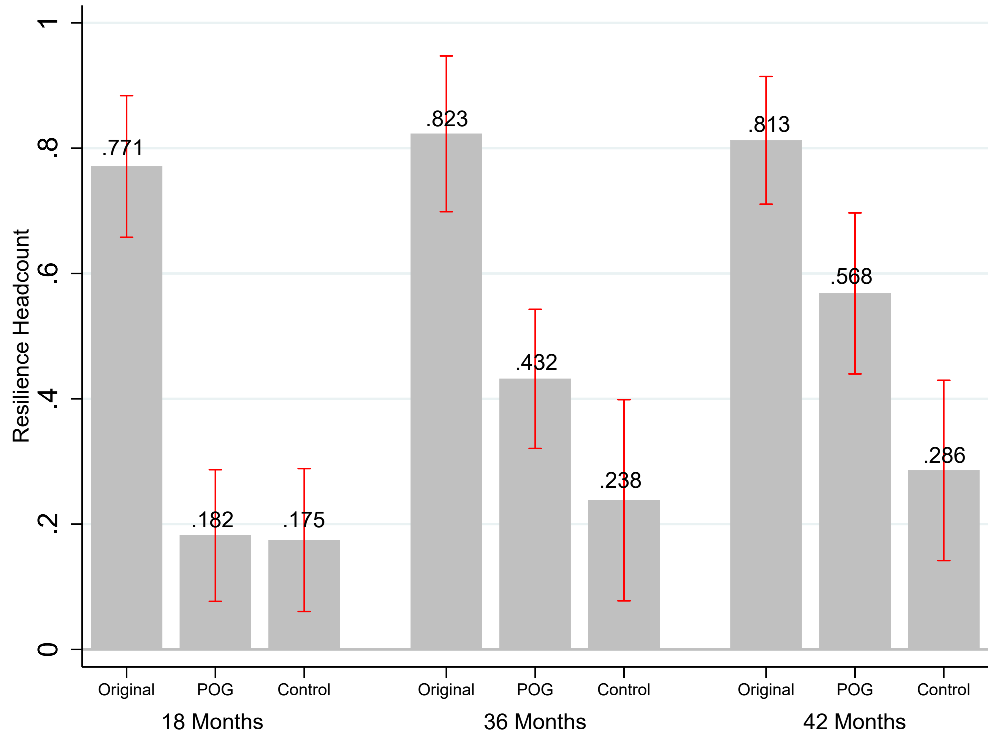
Second, because conventional methods use cross-sectional variation as a proxy for inter-temporal variation, they offer only limited insight into longer-term household welfare status. In contrast, the resilience estimation implemented in this paper exploits inter-temporal variance in prior periods to predict future outcomes based on estimated poverty dynamics. Third, central to poverty traps theory is the possible existence of nonlinear welfare path dynamics. With regard to policy, such dynamics have important implications, most notably that one-time "big-push" interventions can indeed foster a sustainable trajectory out of poverty. While the impact evaluation literature largely ignores the possibility of such nonlinear dynamics, the concept of resilience, rooted in poverty trap theory, takes into account the potential importance of such nonlinearities.

Measurement of development resilience as proposed by Barrett and Constas (2014) and implemented here does have important limitations. First, the measure is sensitive to assumptions governing its estimation. Central to quantifying development resilience is the estimation of higher order moments of the welfare distribution using techniques from Just and Pope (1979) and Antle (1983) and the method relies on the goodness-of-fit of the first moment regression equation. The resilience estimate is therefore sensitive to the choice of explanatory variables and weighs negative shocks as equally as positive shocks. Moreover, the measure could have a perverse implication: for a household with a mean asset level just below the poverty threshold, increasing variability would raise measured resilience. Finally, the method applied here is data intensive, as multiple rounds of follow-up data are required to estimate the probability distributions on wealth. Nonetheless, at different levels of population aggregation, the concept of development resilience and its measurement complement and in many cases serve as an improvement over conventional impact evaluations focused only on the first moments of outcomes.

Given the science-based predictions of increasingly frequent natural disasters, unstable weather patterns, macroeconomic shocks, and other humanitarian emergencies, anti-poverty interventions will continue to focus on bolstering the capacity of poor households to mitigate risks. Our resilience estimation results suggest that the multifaceted approach focused on improving well-being through transfers, decreasing downside risk, and changing underlying structural barriers to economic progress, can have lasting impact on households' ability to accumulate and retain productive assets and to withstand covariate and idiosyncratic shocks. We argue, moreover, that resilience theory can guide development practitioners in the design and evaluation of future anti-poverty programs. Our findings suggest that standard impact evaluation measurements are insufficient to establish households' resilience against future poverty spells and should be complemented, where possible, by estimation and evaluation of higher moments of the household welfare distribution. Researchers and practitioners interested in understanding and evaluating household well-being using resilience will need to rethink their impact evaluation plans by, for example, shifting to the collection of highfrequency data over longer time periods. The contributions in terms of policy design and assessment could be considerable and are important areas for future work.

2.10 Figures and Tables

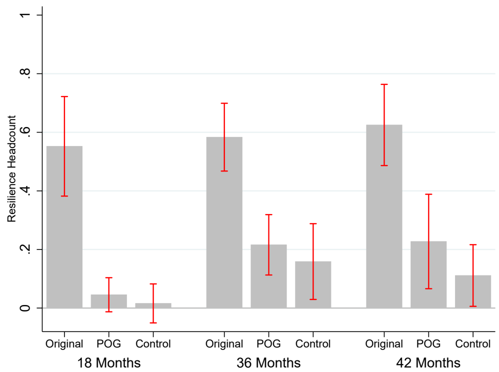
Figure 2.1: Headcount Resilience Rate (Gamma, $\bar{W} = 308$, $\bar{R} = 0.5$ and $k = 3$)



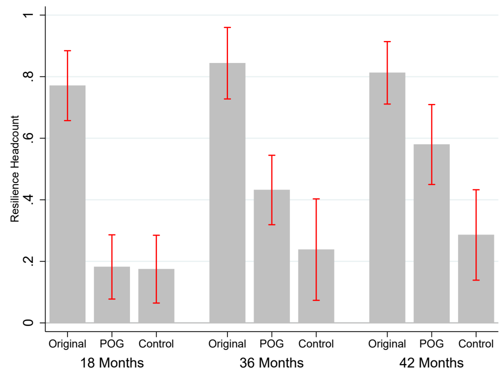
Notes: Bootstrapped 95% confidence intervals are calculated using 400 replications. Standard errors are clustered at household level. Household i at time t is classified as resilient, R_{it}, if its resilience score is greater than 0.5 i.e. $R_{it} = 1$ if $\hat{\rho}_{it} > \bar{R}$; 0 otherwise; where ,$\bar{R} = 0.5$. Expected assets of each household in each round is assumed to follow gamma distribution with first and second moments estimated from path dynamic equations using GLM with Poisson family and log link function.

Figure 2.2: Headcount Resilience Rate - Robustness Checks

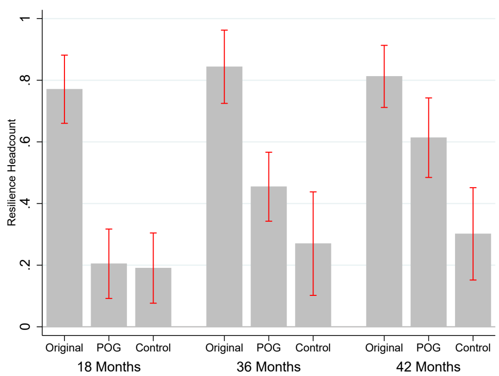
(a) Gamma, $\bar{W} = 308$, $\bar{R} = 0.8$ and $k = 3$



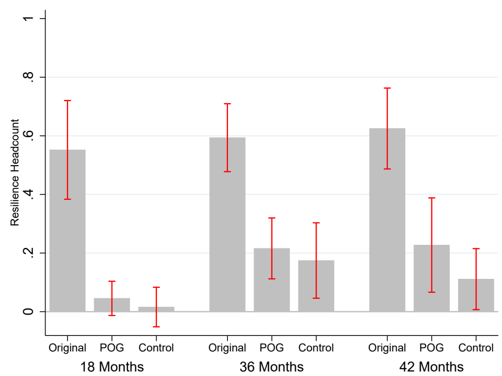
(b) Gamma, $\bar{W} = 308$, $\bar{R} = 0.5$ and $k = 2$



(c) Normal, $\bar{W} = 308$, $\bar{R} = 0.5$ and $k = 3$

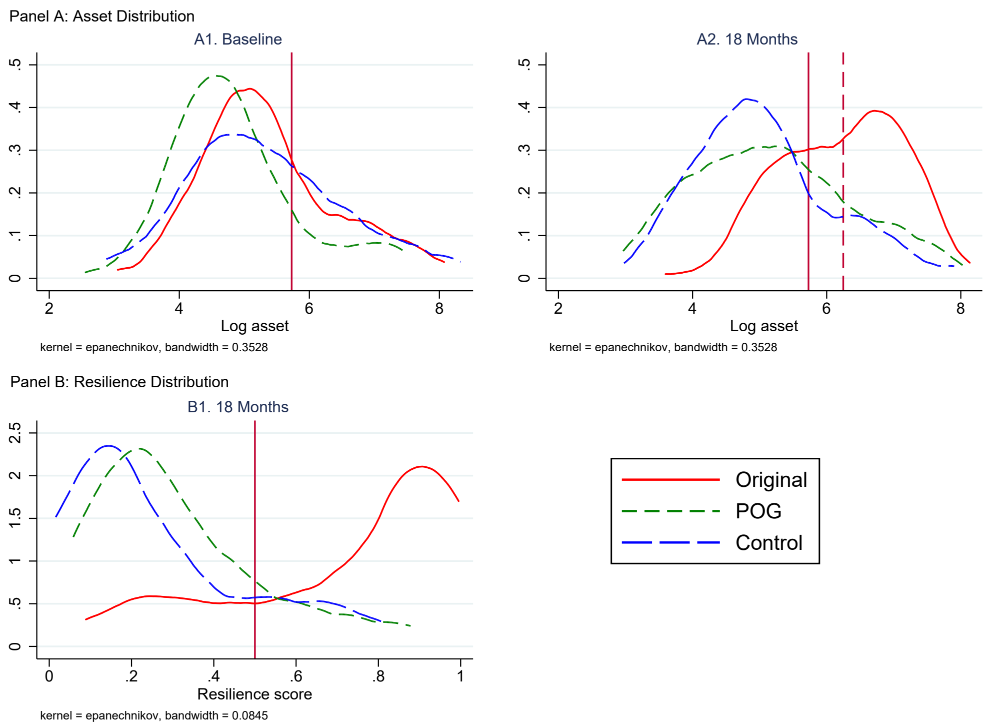


(d) Normal, $\bar{W} = 308$, $\bar{R} = 0.8$ and $k = 3$



Notes: Expected assets of each household in each round is assumed to follow a gamma distribution in Figure 2.2a and 2.2b, and a normal distribution in Figure 2.2c and 2.2d. First and second moments estimated from path dynamic equations using GLM with Poisson family and log link function with polynomial lagged asset to be cubic i.e. $k = 3$ is a preferred functional form except Figure 2.2b where $k = 2$.

Figure 2.3: Kernel density estimate of asset and resilience



Notes: Panel A shows Kernel density estimate of household asset by treatment groups at the baseline and 18 months post-intervention. While the vertical solid lines represent asset poverty threshold of 5.73 ($log(\bar{W}) = 5.73 \implies \bar{W} = 308$ USD PPP per person), the dash vertical line in A2 is the asset poverty threshold plus the half of standard deviation of asset distribution among the Controls 18 months post-intervention. Panel B shows Kernel density estimate of resilience at 18 months after the baseline. The vertical solid line represents resilience threshold of 0.5.

Table 2.1: Baseline characteristics and balance

	Means (SD)			Test of equality of means [P-val]	
	(1) Original	(2) POG	(3) Control	(4) Original= Control	(5) POG= Control
Panel A: Demography					
Head is female	0.283	0.252	0.209	0.267	0.506
	(0.453)	(0.436)	(0.410)		
Head is illiterate	0.057	0.090	0.030	0.385	0.081
	(0.232)	(0.288)	(0.171)		
Head is married	0.821	0.874	0.791	0.635	0.163
	(0.385)	(0.333)	(0.410)		
Household size	7.377	6.928	5.627	0.000	0.000
	(2.799)	(2.762)	(2.059)		
Household size (Adult equivalence)	4.862	4.491	3.807	0.000	0.002
	(1.717)	(1.666)	(1.282)		
Panel B: Assets					
Herd size (TLU)	1.162	0.741	1.233	0.849	0.173
	(1.930)	(1.797)	(2.595)		
Total asset value (Per capita)	382.054	222.757	460.133	0.453	0.013
	(551.714)	(346.283)	(728.025)		
Asset non-poor	0.302	0.144	0.328	0.718	0.006
	(0.461)	(0.353)	(0.473)		
Panel C: Poverty & Expenditure					
Poverty status (Below USD 1.90)	0.623	0.622	0.418	0.008	0.008
	(0.487)	(0.487)	(0.497)		
Total weekly expenditure (Per capita)	12.872	13.340	18.298	0.003	0.007
	(9.574)	(10.028)	(12.693)		
Total weekly expenditure (HH level)	86.384	80.834	88.731	0.787	0.335
	(55.898)	(48.458)	(55.276)		
Panel D: Revenue & Income					
Total revenue last year (Per capita)	527.539	543.884	1083.991	0.103	0.114
	(724.437)	(768.995)	(2727.464)		
Livestock revenue last 3 months (Per capita)	13.603	19.640	78.138	0.120	0.160
	(40.139)	(54.555)	(336.992)		
Crops revenue last year (Per capita)	301.593	332.440	240.678	0.295	0.173
	(475.180)	(602.685)	(287.585)		
Other labor & non-labor income	32.015	26.666	106.658	0.250	0.214
last 3 months (Per capita)	(98.283)	(55.531)	(523.726)		
Panel D: Poverty & Expenditure Per adult equivalence)					
Poverty status (Below USD 1.90)	0.349	0.333	0.239	0.117	0.172
	(0.479)	(0.474)	(0.430)		
Total weekly expenditure	19.054	19.907	26.020	0.007	0.015
(Per adult equivalence)	(13.893)	(13.466)	(17.622)		

Notes: All monetary amounts are measured in USD PPP-adjusted. Household assets refer to value of livestock, durables, agricultural tools, and livestock equipment. The expenditure items covered are: food, clothing, household durables, schooling, medical, alcohol-tobacco, fuel and other home expenditures. Other labor and non-labor income refers to paid income and micro-enterprise profits. Total revenue last year is calculated by adding yearly revenues from crops and livestock, paid income, micro-enterprise profits, remittance and other transfers (total revenue = 4× livestock revenue last 3 months + 4× other labor and non-labor income last 3 months + crops revenue last year + remittances and other transfers last year). Poverty status is a binary variable equal to 1 if per day per person (or per adult equivalence) expenditure is below the 1.90 USD poverty line, and 0 otherwise.

Table 2.2: Attrition

	(1)	(2)	(3)	(4)
Original	-0.035	-0.046	-0.021	-0.149
	(0.052)	(0.053)	(0.093)	(0.300)
POG	-0.148***	-0.151***	-0.094	-0.876***
	(0.051)	(0.053)	(0.091)	(0.279)
Total per capita expenditure		-0.003	0.001	
		(0.002)	(0.003)	
Herd size (TLU)		-0.000	-0.025	
		(0.014)	(0.031)	
Total per capita assets		0.000	0.000	
		(0.000)	(0.000)	
Total per capita expenditure × Original			-0.003	
			(0.005)	
Total per capita expenditure × POG			-0.008*	
			(0.005)	
Herd size (TLU) × Original			0.024	
			(0.039)	
Herd size (TLU) × POG			0.039	
			(0.038)	
Total per capita assets × Original			0.000	
			(0.000)	
Total per capita assets × POG			0.000	
			(0.000)	
Baseline characteristics				Yes
Baseline characteristics interacted with Treatment				Yes
Attrition Rate: Baseline to Endline	0.130			
Test: OG and all OG interacted jointly 0 [p-val]			0.738	0.0900
Test: POG and all POG interacted jointly 0 [p-val]			0.00502	9.76e-05
Adjusted R-squared	0.028	0.027	0.031	0.109
Observations	284	284	284	284

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. OLS estimates are reported based on the sample of households observed at baseline. The dependent variable is a binary variable equal to one if the household is observed in all 6 survey waves (baseline, 6 months, 12 months, 18 months, 36 months, and 42 months post-intervention), and zero otherwise.

Table 2.3: Treatment effects on productive asset

	(1) Household herd size (TLU)	(2) Livestock value, per capita	(3) Total asset value, per capita	(4) Revenue from livestock, per capita per quarter
Time 1 Original (18 months post treatment)	0.99***	460.60***	477.12***	64.56*
	(0.24)	(73.04)	(99.95)	(34.53)
Time 1 POG (18 months post treatment)	0.46**	173.77***	279.29***	36.97
	(0.20)	(55.15)	(87.61)	(34.61)
Time 2 Original (42 months post treatment)	1.11***	497.10***	495.75***	110.74**
	(0.35)	(89.22)	(114.51)	(46.56)
Time 2 POG (42 months post treatment)	1.03***	305.45***	294.46***	72.09
	(0.35)	(59.62)	(89.79)	(46.42)
Baseline mean (Original)	1.201	190.4	397.9	13.48
Time 2 impact: % change (Original)	92	261.1	124.6	821.6
Time 1 impact = Time 2 [p-value] (Original)	0.738	0.601	0.825	0.0365
Adjusted R-squared	0.218	0.233	0.145	0.045
Observations	741	741	741	741

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference, Equation (2.6), specification. All outcomes are measured at the household level. Time 1 and time 2 refer to 18 and 42 months post-intervention except in column 4, where they refer to 12 and 36 months post-intervention. In Column 1, herd size is measured in tropical livestock units (TLU) which assign a value of 0.7 for adult cattle, 0.5 for immature cattle, and 0.1 for a sheep or a goat. Livestock value (Column 2) is the value of the household's herd size. Values in column 3 include herd, household durables, agricultural and livestock tools.

Table 2.4: Treatment effects on consumption expenditure, food Security, and asset poverty

		Per Capita Consumption				
	(1) Below Poverty Line	(2) Food (last week)	(3) Nonfood (avg weekly)	(4) Total (avg weekly)	(5) Enough Food	(6) Asset Non-poor
Time 1 Original (12 months post treatment)	-0.220**	1.59	1.75	3.34	0.182***	0.470***
	(0.088)	(1.41)	(1.28)	(2.11)	(0.060)	(0.088)
Time 1 POG (12 months post treatment)	-0.029	-0.91	1.36	0.45	0.111*	0.243***
	(0.087)	(1.42)	(1.10)	(2.03)	(0.060)	(0.082)
Time 2 Original (36 months post treatment)	-0.314***	3.72***	3.75***	7.47***	0.213***	0.390***
	(0.086)	(1.40)	(1.27)	(2.11)	(0.075)	(0.091)
Time 2 POG (36 months post treatment)	-0.059	0.16	1.48	1.64	0.155**	0.384***
	(0.085)	(1.32)	(1.04)	(1.96)	(0.070)	(0.087)
Baseline mean (Original)	0.625	6.480	6.238	12.72	0.750	0.302
Time 2 impact: % change (Original)	-50.32	57.48	60.12	58.77	28.44	129.1
Time 1 impact = Time 2 [p-value] (Original)	0.299	0.282	0.0653	0.121	0.523	0.277
Adjusted R-squared	0.025	0.070	0.032	0.040	0.125	0.216
Observations	741	741	741	741	741	741

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference, Equation (2.6), specification. All outcomes are measured at the household level. Time 1 and time 2 refer to 12 and 36 months post-intervention except in column 6, where they refer to 18 and 42 months post-intervention. In Column 1, the poverty line threshold used is USD 1.90 PPP per person per day, as measured in 2012 prices. Column 2 is per person food expenditure in the last seven days from own production, purchased and gift. In Column 3, nonfood expenditure includes average weekly per person expenditures on clothing, household durables, schooling, medical, alcohol-tobacco and other home expenditures. Column 4 is total of food and nonfood weekly expenditures. Column 5 is an indicator variable for subjective food security, which takes the value of 1 if the survey respondent report the household usually or always has enough food to feed all the members. Asset non-poor in column 6 is an indicator variable that takes the value of 1 if total household asset value is above 308 USD PPP per person, 0 otherwise. The asset poverty threshold calculation is discussed in Appendix A.2.2. Columns 1, 5, and 6 are linear probability models; nonlinear estimates using logistic regressions are reported in Table A.2.

Table 2.5: Treatment effects on household resilience

	Originals (OG)			Pass on the Gift (POG)			
	18 Months	36 Months	42 Months	18 Months	36 Months	42 Months	Observations
Panel A:							
Development resilience	0.228***	0.145***	0.167***	0.192***	0.111**	0.110*	741
	(0.0563)	(0.0512)	(0.0627)	(0.0536)	(0.0470)	(0.0564)	
Resilience mean (Control group)	0.26	0.351	0.379				
Impact: % change	87.7	41.3	44.1	73.8	31.6	29.0	
Round impact = round 4 impact [p-value]	–	0.365	0.517	–	0.381	0.384	
Panel B:							
First Moment (Mean)	0.591***	0.350***	0.341***	0.490***	0.330**	0.289**	741
	(0.142)	(0.131)	(0.128)	(0.166)	(0.141)	(0.140)	
Panel C:							
Second Moment (Variance)	-0.365**	-0.0929	-0.404**	0.258	0.228	-0.175	741
	(0.161)	(0.159)	(0.179)	(0.198)	(0.160)	(0.192)	

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Each panel in the table represents a separate regression. Panel A reports average marginal treatment effects estimated using generalized linear model (GLM) with binomial family and logit link function. Panels B and C show average marginal treatment effects for mean and variance respectively, which are estimated using GLM with Poisson family and log link function. Each estimation regresses the outcome of interest for household i in survey round t on a constant, dummies for each survey round, the interaction between each treatment assignment dummy and each survey wave dummy, cubic polynomial of a first-lagged outcome and time t household characteristics (head is female, household size, head is married head age, head education level and number of children under 5). The coefficients shown are those on the treatment-survey wave interaction terms, which is the difference between the Treatment and Control means for that survey wave. Bootstrapped household level cluster standard errors using 400 replications are in parenthesis. Expected asset in each period is assumed to follow gamma distribution with first and second moments estimated from path dynamic equations using GLM with Poisson family and log link function. In Panel A, we report the mean resilience of the Control group for each survey wave and p-value on the null hypothesis that the later periods (36 and 42 months) are equal to the earlier period (18 months) impact.

Table 2.6: Treatment effects on household resilience - robustness checks

			OLS	
	Gamma ($k = 2$)	Normal ($k = 3$)	Gamma ($k = 3$)	Normal ($k = 3$)
Time 1 Original (18 months from baseline)	0.237***	0.226***	0.228***	0.225***
	(0.0567)	(0.0563)	(0.0573)	(0.0575)
Time 1 POG (18 months from baseline)	0.186***	0.198***	0.179***	0.185***
	(0.0534)	(0.0556)	(0.0518)	(0.0540)
Time 2 Original (36 monhts from baseline)	0.157***	0.143***	0.137***	0.136***
	(0.0514)	(0.0510)	(0.0516)	(0.0514)
Time 2 POG (36 monhts from baseline)	0.118**	0.116**	0.110**	0.114**
	(0.0469)	(0.0480)	(0.0476)	(0.0486)
Time 3 Original (42 months from baseline)	0.167***	0.162**	0.156***	0.151**
	(0.0629)	(0.0629)	(0.0600)	(0.0603)
Time 3 POG (42 months from baseline)	0.113**	0.108*	0.111**	0.110*
	(0.0561)	(0.0576)	(0.0563)	(0.0574)
Test of Equality of Impacts [p-value]				
Original: Time 1 = Time 2	0.381	0.368	0.000	0.000
Original: Time 1 = Time 3	0.456	0.496	0.000	0.000
POG: Time 1 = Time 2	0.460	0.373	0.000	0.000
POG: Time 1 = Time 3	0.437	0.342	0.000	0.000
Observations	741	741	741	741

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Each column in the table represents a separate regression. Column 1 reports average marginal treatment effects estimated using generalized linear model (GLM) with binomial family and logit link function with polynomial lagged asset to be quadratic ($k = 2$) in the path dynamics equation. Column 2 shows average marginal treatment effects estimated using GLM with binomial family and logit link function assuming conditional transition distribution function to be normal. Columns 3 and 4 show treatment effects from OLS assuming conditional transitional distribution function to be gamma and normal respectively.

Table 2.7: Treatment effects on employment and income

			Per Capita Revenue and Income		
	(1) Self Employment	(2) Casual Labor	(3) Total Revenue	(4) Live-stock	(5) Paid Income
Time 1 Original (12 months post treatment)	0.204***	-0.043	541.23	64.56*	-2.52
	(0.070)	(0.041)	(332.63)	(34.53)	(7.97)
Time 1 POG (12 months post treatment)	0.107	0.003	429.10	36.97	1.79
	(0.076)	(0.039)	(333.68)	(34.61)	(8.84)
Time 2 Original (36 months post treatment)	0.162**	-0.075*	723.99*	110.74**	-16.85
	(0.068)	(0.039)	(368.88)	(46.56)	(14.04)
Time 1 POG (36 months post treatment)	0.122	-0.018	400.46	72.09	-24.56*
	(0.078)	(0.039)	(368.21)	(46.42)	(13.88)
Baseline mean (Original)	0.696	0.0476	523.1	13.48	0.536
Time 2 impact: % change (Original)	23.27	-157.7	138.4	821.6	-3147
Time 1 impact = Time 2 [p-value] (Original)	0.560	0.470	0.199	0.0365	0.164
Adjusted R-squared	0.029	0.001	0.039	0.045	0.026
Observations	988	988	741	741	741

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference, Equation (2.6), specification. All outcomes are measured at the household level except (1) self employment and (2) casual labor, which are at individual level - women 18 to 65 years of age. Standard errors are clustered at the household level in column 1. Due to data limitations time 1 and 2 refer to 36 and 42 months post-intervention respectively in column 1 and 2. Self employment is defined as people reporting working on their own farms or non-farm enterprises as their main occupation. Casual laborers are those who reported selling their labor for farm or non-farm activities. Livestock revenue is the value of livestock and livestock products (milk, meat, eggs, hire out of draft animal, manure and other products) households sold in last 3 months. Paid income is wage income from labor (salaried or casual) in the last 3 months. Total revenue, column 3, is yearly income calculated by adding yearly revenues from agriculture and livestock, paid income, micro-enterprise profits, remittance and other transfers.

Table 2.8: Cost-benefit analysis

Panel A. External parameters		
a Direct asset transfer costs at year 0		1853
b Training, salaries, supervision etc. at year 0		2474
c Total costs at year 0 (a+b)		4327
d Total costs discounted at year 3		5009
Social Discount = 5%		
Year 3 PPP Exchange = 2.94		
Panel B. Estimated Benefits		
1 Year 1 change in annual nondurable consumption expenditure		1293.9
2 Year 2 change in annual nondurable consumption expenditure, assuming treatment effect equal to year 1		1293.9
3 Year 3 change in annual nondurable expenditure		1418.3
4 From year 4 till year 20 NPV change in nondurable expenditure, assuming year 3 gains persist		15990.2
5 Year 3 change in asset value		2302.7
6 Total Benefits (1+2+3+4+5)		22299.0
7 **Benefits/Cost ratio** (assuming benefits last 20 years from transfer date)		**4.45**
Sensitivity to different time horizons/discount rates		
i *Benefits last 5 years post-intervention*		1.79
ii *Benefits last 10 years post-intervention*		2.90
iii *Social discount = 7%*		3.80
iv *Social discount = 10%*		3.07
8 **IRR** (assuming benefits last 20 years from transfer date)		**0.24**
i *Benefits last 5 years post-intervention*		0.10
ii *Benefits last 10 years post-intervention*		0.22
iii *Social discount = 7%*		0.23
iv *Social discount = 10%*		0.22
Panel C. Cost of increasing headcount resilient rate by 1%		
Resilience threshold cutoff	$\bar{R} = 0.5$	$\bar{R} = 0.8$
i Year 1 post-intervention (USD)	99.83	102.95
ii Year 3 post-intervention (USD)	58.22	83.64

Notes: Panel A reports per household costs. Direct asset transfer cost equal to the value of livestock (1629 USD), horticulture (20 USD) and agricultural equipment and supplies (204 USD) transfers. Household nondurable consumption includes both food (own production and purchased) and nonfood expenditures (clothing, schooling, medical, alcohol-tobacco , transportation, cosmetics, fuel and other home expenditures). Annual changes in household consumption are calculated multiplying treatment effects with average household size in the year (7.1 in year one and 6.3 in year three) times 52. Assets equal the value of herd size, agricultural tools, durables and livestock equipment minus the value of transfer. Internal rate of return (IRR) is based on estimated nondurable consumption gains, assuming that these last for 20 years. Year 1 and year 3 in panel C refer to 18 and 42 months after the intervention respectively. The average cost of increasing head count resilience by one percent is the value of the transfer divided by the gains in the headcount resilient rate (see Appendix A.4.1).

CHAPTER 3

IMPACT OF INTERNATIONAL MIGRATION ON LABOR SUPPLY IN NEPAL

3.1 Introduction

One-fifth of the 30% poverty reduction in Nepal occurring between 1995 and 2004 is attributed to work-related international migration and remittances sent home (Lokshin et al., 2010). More than two million prime-age (mostly male) Nepalese are working outside the country and the inflow of remittances accounts for 30% of the country's GDP (Ministry of Finance, 2014). Decrease in labor stock and substantial income from abroad is likely to have profound effect on labor market and, yet, the impacts of the migration on the non-income dimensions in Nepal remain relatively unexplored. The paper addresses this issue by documenting the differential impact of international migration on labor supply of the left-behind family members.

Traditionally, the literature on the household-level impacts of migration has focused on income and consumption of the left-behind families. There is a general consensus among this literature that temporary out-migration for employment helps increase income and reduce poverty (Adams and Page, 2005, summarizes the results of microlevel analysis in several countries).[1] However, a relatively new strand of the literature focusing on the non-consumption dimension of left-behind family members' wellbeing provides more mixed evidence. These new studies suggest that male migration decreases non-migrating women's labor market participation and increases their labor supply in farming and unpaid family work (Lokshin and Glinskaya,

[1] Remittances help smooth consumption (Yang and Choi, 2007), provide mutual insurance (Stark and Lucas, 1988), relax credit constraints (Yang, 2008) and alleviate liquidity constraints (Taylor et al., 2003) allowing non-migrating members to engage in higher remunerative activities. At the same time, other community members may benefit from positive spillovers: Amuedo-Dorantes et al. (2010) find children in communities with higher migrants have greater school attendance and McKenzie and Rapoport (2007) find migration reduces inequality in sending communities in the long run.

2009, in Nepal; Binzel and Assaad, 2011, in Egypt; Mu and van de Walle, 2011, in China; Mendola and Carletto, 2012, in Albania). Similarly, migration has negative effect on the left-behind elderly parents' health (Antman, 2010) and on children's educational attainment (Antman, 2011; McKenzie and Rapoport, 2011). So, how beneficial is international migration for the left-behind members? Murard (2016) provides a more complete theoretical framework incorporating both household consumption and labor supply. Using data from Mexico, the paper finds that temporary migration for employment leads to both increase in consumption expenditure and farm labor of household members staying behind. The consumption gains, however, are too large to be purely explained by the endogenous increase in non-migrant's labor supply.

This paper contributes to the growing literature of migration's impacts on non-income based outcomes and, in particular, this is among the first papers to investigate the impact of international migration on time allocation and leisure consumption by gender in Nepal. To my knowledge, Lokshin and Glinskaya (2009) is the only other study that explores this topic in the Nepalese setting. However, they limit their analysis to women's labor supply for wage-employment and exclusively on extensive margin. Using the 2003/04 round of Nepal Living Standard Survey (NLSS II) data, Lokshin and Glinskaya (2009) find that men's migration from Nepal has discouraging effect on women labor force participation. In contrast, this paper uses the 2010/11 round of NLSS, when work-related international migration was at record high, and incorporates both extensive and intensive margins by gender. This is especially important given the traditional roles of rearing children and household chores women assume within the household and society, the left-behind women are expected to be affected differently than men. One of the most salient features of the employment related emigration in Nepal is that it is predominantly a male phenomenon. With males migrating abroad, the authority over household decision making may shift to women. However, the shift may be accompanied by extra responsibilities requiring extra hours of work or it might compel women to give up their jobs to assume the new roles. Additional important distinction between the papers is the timing of the surveys: while the NLSS II was implemented during a critical phase of the Maoist insurgency in the country, the NLSS III data was collected well after the end of the civil conflict.

The primary identification strategy of the paper relies on an instrumental variable (IV) approach using a popular instrument in migration literature i.e. historical migration networks. Specifically, using the 2001 Nepal Census, I compute the percentage of international migrants from a village as an instrument for migration in 2010/11. A decade lagged migration shares are unlikely to affect local economic conditions and hence, labor supply decisions. Furthermore, I use GDP growth between 2001 and 2011 of the most popular destination countries interacted with the local level decade lagged migration shares as an additional instrument and results are robust.

The analysis has four major findings. First, solely on extensive margin, having international migrants in the family discourage the left-behind members from participating in wage-employment. This is true for both male and female members. However, female members significantly increase participation in self-employment, almost entirely through subsistence farming. Whereas, having migrants in the household does not affect male members' decisions to participate in self-employment. Second, both self-employed and wage-employed adults decrease their weekly hours of labor supply. Third, while women staying behind significantly increase labor time in household activities, I observe no such impact among men. Fourth, when analyzing the aggregate time allocation (wage work + self-employment + household activities), I find 0 effect of migration on women's overall labor supply and significantly negative effect on men's. Therefore, women staying behind realign their priorities and reallocate their time from wage-employment to farm and household activities, while men value their leisure more because of the remittances from abroad and decrease their overall supply of labor.

These are reasonable findings in a country with the traditional household norms and social culture that is likely to see women as subordinate to men. In order to understand the intra-household bargaining channel for labor allocation decisions further, I limit the sample to staying behind women members only and observe that unlike other women in the family, household heads are less likely to participate in wage-employment and do not increase participation in self-employment either. On top of already having greater say in family decision making, household heads are the likeliest recipients of transfer from abroad, which in turn, may further increase bargaining power.

This paper contributes to the existing literature in the following ways. First, it complements Lokshin and Glinskaya (2009) by extending the analysis of women's wage-employment on both extensive and intensive margins. Second, the paper also includes males' labor supply to investigate the presence of differential impact between men and women in Nepal. The most important contribution of the paper is that it analyses the time allocation of left-behind members beyond their time in self-employment and market work. That is, it answers the question of if they are not employed, what do members of migrants sending household do instead. This is an important question that the literature, including the studies in other country settings, has mostly ignored. Furthermore, by adding the total time spent in the market, self-employment and household activities, the paper investigates whether migration and remittances increase the consumption of leisure as the microeconomic theory predicts. The answer may have important implications towards the effect on an individual's welfare gains, which have been rarely explored in previous studies. By dissecting the analysis in these multiple ways, this study provides one of the most complete pictures of migration's impact on labor supply of the remaining family members.

The results presented in the paper have important policy implications. They highlight the need for tailored policy initiatives targeting specific subpopulations. Male-dominated migration pushes females to give up wage-employment and increase labor supply in their own farms. Labor markets in rural Nepal tend to be incomplete and not fully integrated due to information asymmetry, lack of mobility, and lack of strong institutional implementations. Policy initiatives should be focused on these aspects to improve the rural wage labor markets thus allowing households to hire workers to replace those who migrate.

The rest of the paper is organized as follows. Section 2 provides background of out-bound migration from Nepal, brief scenario of current labor market in Nepal, and the motivation for the paper. Section 3 describes the data set used for the analysis while empirical strategy and identification is discussed in section 4. The findings of the analysis are presented in section 5 and section 6 concludes the paper.

3.2 Background and Motivation

3.2.1 Emigration from and Remittances to Nepal

With 25.2% of its population earning less than US$ 1.25 per day (World Bank, 2014b), Nepal is one of the least-economically-developed nations in the world. However, with recent international labor treaties, Nepal has been experiencing large outflows of migrants and hence, remittance inflows from abroad. Figure (3.1) presents the historical international migration trend from Nepal. Close to 2 million Nepalese, 7.3% of the population, were living abroad during the census in 2011. This is a substantial increase compared to earlier decades. Only 3.2% (0.76 million), and 3.4% (0.66 million) of the population was living abroad in 2001 and 1991 respectively. The rise in numbers of Nepalese living abroad in the last decade is mainly due to low skilled employment related migration. The Foreign Employment Act of 2007 - which was designed to provide security, protect the welfare of migrants, provide migrants with education and training before leaving the country, and monitor the businesses that facilitate migration processes - along with the bilateral labor treaties that Nepal signed[2] have facilitated the migration process. The end of the Maoist insurgency, during which mobility within the country was severely restrained and government offices were destroyed, making it difficult to obtain travel documents, also helped improve conditions to migrate internationally.

Figure (B.1) shows migration trends to the top five destination countries for labor employment.[3] Malaysia, Qatar, and UAE, countries Nepal signed treaties with, are among the most favored destinations for work. India, not shown in the figure, is the largest recipient country of Nepalese workers. Due to the open border the two countries share, it is difficult to track migrants

[2]Nepal signed major international labor treaties in last decade, with Qatar (2005), UAE and Korea (2007), Bahrain (2008), Japan (2009), and Malaysia (started process in 2007), with the aim to protect migrants and facilitate the migration process.

[3] Workers going abroad are required to obtain labor-permits from the Department of Foreign Employment. Migrants can apply on their own or can apply through foreign-employment recruiting agencies. The numbers reported in Figure (B.1) are those who opted to apply through the recruiting agencies and were issued the permit. Many of the females migrating abroad from Nepal tend to migrate along with their male household members and not necessarily for employment purposes. Therefore, NLSS, which asks if the household has members abroad, is likely to have a higher percentage of female migrants in the sample than reported here.

and most workers migrating to India do not report to the Department of Foreign Employment, which keeps the records. Additionally, many workers migrating to countries other than India travel through India, so labor-related migration might be significantly higher than is officially reported. With these outflows of workers, it is not surprising that remittances have become major financial flows to Nepal.

Remittance income has become a major factor in the economic development of Nepal. According to the Ministry of Finance (2014), Nepalese households received 430 and 560 billion rupees accounting for 25.7% and 29.1% of the GDP in fiscal years 2012-13 and 2013-14 respectively. About 32% of Nepalese households received remittances in 2004 (World Bank, 2006). At a country level, remittances have helped sustain balance of payments covering 169.5% of imports and are equivalent to 82.9% of the foreign reserves in 2013 (World Bank, 2014a). This trend is likely to continue in the near future as the growth in outflows of migrant workers is on the rise. Migrant outflow grew by 16% between 2012 and 2013 (World Bank, 2014a). Remittances sent through unofficial channels could be as large. Thus, resource inflows from abroad are becoming an increasingly larger share of household budgets to growing number of families in Nepal.

3.2.2 Employment

International migration and paid-employment are male-dominated phenomena in Nepal. The labor migration trend by gender is presented in Figure (B.2).[3] Among labor-related migrants, only 6.0% (about 23 thousand), 6.2% (about 28 thousand) and 5.6% (about 30 thousand) of migrants were female in 2011, 2012 and 2013 respectively. In my sample, 15.6% of migrants are female.[3] Similarly, there is a variation in labor market participation across gender. Among working age (18 - 60 years) males, 49.8% of them reported to be paid employees and 70.9% of them reported participating in self-employment, mostly subsistence farming. Among females, only 23.1% reported to be paid employees while 66.3% of them reported participating in self-employment. This is not surprising given the strong social and traditional family norms in Nepal, which discourage women from participating in paid-work, and where women mostly engage in taking care of children and household chores (CEDAW, 2003). However, with males migrating abroad, the authority over household consumption and investment might shift to female members. Women might become more involved in making decisions on labor market participation as well. Besides continuing to care for children and engaging in household chores, women in Nepal often take up men's roles in family farming and enterprises when male members are abroad (Nandini, 1999). Similarly, women play key roles in deciding the use of remittances and running bazaar economics when husbands are away (Brown and Conneil, 1993). The male-dominated international migration in Nepal may affect non-migrant male and female members in the household differently.

3.3 Data and Descriptive Analysis

For this study I use the 2010-2011 round of Nepal Living Standard Survey (NLSS III) as the primary data source. It is a nationally representative survey of households and communities that is conducted by the Central Bureau of Statistics (CBS) Nepal, with assistance from the World Bank. It was administered between February 2010 and February 2011. It has a panel component of 1,128 households. Half of the households were followed from the first round and the other half from the second round. The cross-sectional sample has 5,988 households, which was selected in three stages.[4] The survey collected detailed information on multiple topics related to household welfare. The survey provides rich information on household consumption, sociodemographic composition of households, health and education attainment of the members, labor market outcomes of the household members, and the source of a wide range of household incomes. It contains detailed information on time-allocation for wage-employment and household production of every household members. Households were also asked to provide information on remittances received by the households in the previous 12 months and identified the age, gender, educational attainment, and the destination country of the remittance sender.

This study uses 7,108 NLSS III households from both the cross-section and panel components that have complete information on the variables used

[4] For detailed description of the sample design and the methodology, see http://cbs.gov.np/

in the analysis. 33.1% (2,212) of the households in the sample reported having at least a member abroad in 2010-11. Characteristics of migrants are reported in table (B.1). Migrants tend to be young (mean age of 28), dominated by males (84.4%) and mostly composed of the daughters/sons of the household head. They tend to have achieved some grade level of education, 53.7% have completed 1 to 10 grade while 20.3% have completed grade 10 (School Leaving Certificate) and intermediate level (high school). India and the Middle East seem to be the favored destinations for most of the migrants, 44.4% and 24.7% respectively. Although, remittances coming from abroad tend to be relatively large, there are very little difference in total and non-wage household incomes (Figure 3.2); households with migrant have a slightly higher total and non-wage incomes.

A total of 16,879 working age adults (ages 18-60) are used for the analysis. 4,985 boys and 5,195 girls (both ages 6-17) are added in the sample for the analysis of time allocation in household activities. Adults from households with and without migrants differ on demographics, household composition, their labor market outcomes, and communities they reside in. Table 3.1 reports the descriptive statistics of the 16,879 adults used in the analysis. Adults from migrating families, both male and female, are less likely to be employed in wage-employment but are more likely to be involved in self-employment activities compared to the adults from non-migrating families. Consistent with the theory, (for bottom 70% of the houseeholds) adults decrease wage-employment with household non-wage income (Figure 3.3). Migration seems to have a disincentive effect on wage-employment; adults from migrating families are less likely to have wage-work over all of non-wage household income distribution.[5]

Adults from migrating families are older by a year and have achieved a year less education than those in households with no migrants. When comparing only female members, females from families with a migrant are more likely to be married and head the household when compared to females from families with no migrants. This is the opposite when comparing male members. Adults from families with at least one member abroad have larger household sizes and are more likely to come from a family with a female head. Most

[5]These results are suggestive, they do not not account for the fact that the adult most likely to be the wage earner in the family might be more likely to migrate, leaving adults that are intrinsically less likely to have wage employment behind.

of the agriculture in Nepal is subsistence farming, so households own small amounts of land. Migrating families tend to own more land compared to non-migrating families. At the community level, migrant-sending families live areas with sightly lower unemployment rates, literacy rates, and higher poverty rates.

3.4 Empirical Specification and Identification Strategy

The goal of the analysis is to evaluate the impact of migration on the labor market outcomes of the left-behind household members. The simplest strategy is to estimate the following equation.

$$y_{ij} = \beta_0 + \beta_1 M_j + \boldsymbol{X'_{ij}\beta_2} + \epsilon_{ij} \tag{3.1}$$

where $y_{i,j}$ is a outcome variable of individual i in household j. It is either employment status, or total hours spent in paid work, household production or other household activities. $\boldsymbol{X_{ij}}$ is a vector of controls - individual and household characteristics that influence individual i's productivity and local labor market conditions. M_j is an endogenous binary variable that takes the value of 1 if household j has at least one migrant - 0 otherwise, and ϵ_{ij} is the unobserved error term.

3.4.1 Identification

The decisions to migrate and work are selective processes which depend on observed and unobserved household and individual characteristics such as asset level, taste for work, human capital level, opportunities at home and abroad etc. Cross-sectional analysis of migration's effect on labor supply cannot identify a causal relationship because of the endogeneity of migration and household labor supply. Unobserved household and individual characteristics that influence labor supply are likely to influence the decision to migrate as well. Similarly, households may endogenize labor supply decisions and hence, earned income while making migration decisions. Thus, estimated effects of migration on labor supply will be biased using the OLS strategy. To address the potential endogeneity bias, this paper exploits a very popular instrument in the literature: the local historical migrant network serves as an instrumental variable (IV) for the current migration decision. Specifically, using the 2001 Nepal Census I compute the percentage of international migrants from a Village Development Committee (VDC)[6] as an instrument for migration in 2010-2011 as in (3.3). Following is the equation for migration decisions.

$$M_{ijc} = \alpha_0 + \boldsymbol{X'_{ijc}\alpha_1} + \alpha_2 Z_{ijc} + \nu_{ijc} \tag{3.2}$$

where $M_{i,j}$ is an identifier that individual i lives in household j with or without a migrant, $\boldsymbol{X_{ijc}}$ is a vector of controls as defined in equation (3.1). Z_{ijc}, defined in (3.3), is an exogenous instrumental variable that must satisfy as good as randomly assigned, the first stage, and the exclusion criteria conditions. The 2001 migration decisions are likely to be random to the 2011 labor supply and migration decisions as migrants from a community in 2001 would not have anticipated the community's labor market conditions in 2011, which satisfies as good as randomly assigned condition. However, migration networks provide information about the economic opportunities at the destination, potential costs, and might reveal migrations' impact on their family's wellbeings to the community, which might influence other community members' migration decisions (first stage). Similarly, as long as a decade lagged unobserved community characteristics do not influence individual's labor supply in 2011, the 2001 community level migration shares are unlikely to feature in the 2011 labor supply equations. Hence, the instrument is likely to affect the outcome only through the endogenous variable, satisfying the exclusion criteria condition. Following is the instrument.

$$IV_1 = \frac{MIG_{2001,c}}{POP_{2001,c}} \tag{3.3}$$

where $MIG_{2001,c}$ is the total number of people living abroad in 2001 from a VDC c and $POP_{2001,c}$ is the population of the VDC c in 2001. It is important to note that the IV estimates are likely to be greater than the OLS estimates as the instrumental variable approach identifies the causal impact

[6]Village Development Committee (VDC) is the lowest level of administrative unit. Similar to a municipality, it is responsible for the proper use and distribution of state funds and local level service delivery. Depending on the size, it may represent a single community or multiple communities. It is divided into 9 subdivisions called wards and currently there are 3,276 VDCs in Nepal.

of treatment on outcome only on the compliers; that is, IV can recover only the local treatment effect (LATE) (Angrist, 1991).[7]

The historical migrant networks are extensively used in the literature to estimate current levels of migration. Migrant networks, which are ties between migrants, former migrants, and non-migrants at the origin through bonds of kinship, friendship, and shared community origins, might be the most important mechanism for international migration (Massey, 1988). Sociologists and anthropologists have been studying the role of networks on migration for a long-time (Tilly and Brown, 1967; Mitchell, 1969; Choldin, 1973; Hugo, 1981) and economists have also found that networks play an important role in migration decisions (Hägerstrand, 1957; Greenwood, 1969; McKenzie and Rapoport, 2007; Woodruff and Zenteno, 2007; Foged and Peri, 2013). This is because migrant networks reduce the potential hazards at both the destination and the origin and decrease the cost of relocation (Massey, 1988; McKenzie and Rapoport, 2007).

Historically inernational migration networks are region specific in Nepal. For example, most of the people joining Indian and British armies in the 1930's (and up to the present day) were Gorkhas from the Pokhara region when the recruitment started. Similarly, people from the southern plains migrated after the beginning of the extensive cultivation of tea in north-

[7] OLS assumes treatment to have homogeneous effect. It estimates an average slope, which, in reality, might not be true as there are likely to be heterogeneous responses to the treatment. Let y be a potential outcome, d be a potential treatment and z be an instrument, then under the assumptions of independence, exclusion and monotonicity IV is a LATE. Independence assumption states that difference in outcome (y) and difference in treatment (d) between $z_i = 0$ and $z_i = 1$ should capture the causal effect of the instrument on outcome and treatment. This is satisfied when as good as randomly assigned condition in IV is fulfilled. The exclusion assumption, $y(d_i, z_i = 0) = y(d_i, z_i = 1)$ for $d_i = 0, 1$, is same as the exclusion criteria condition in IV. Whereas, the monotonicity assumption states that the instrument, if it has any effect, should affect everyone in the same direction i.e. $d_{1i} \geq d_{0i}$ or $d_{1i} \leq d_{0i}$. Given these assumptions, $IV = LATE = \dfrac{E(y_i \mid z_i = 1) - E(y_i \mid z_i = 0)}{E(d_i \mid z_i = 1) - E(d_i \mid z_i = 0)} = E(y_{1i} - y_{0i} \mid d_{1i} > d_{0i})$. Notice that the denominator is just the shares of compliers i.e. the percentage of the sample that participate in the treatment only because of the instrument. So, when there is treatment effect heterogeneity, IV estimates the causal effect of treatment on outcome only among compliers. In our case, compliers are those who decide to migrate due to the higher share of migrants in the community in the past. Since all three assumptions are likely to be satisfied, the estimated IVs in the paper are LATEs. Contrarily , under the homogeneity assumption or the perfect compliance, the denominator would be 1 and hence, LATE = IV = ITT (intention to treatment effect), which is what OLS assumes. Since share of compliance is always less than or equal to 1, the LATE is always greater than the ITT.

ern India (Seddon et al., 2001). People from Far-Western villages in Nepal tend to migrate to a same destination in India as their co-villagers (Thieme, 2006). Figure (B.3) shows the top ten origin districts for labor-employment migration at present. These top districts, which are located mostly in South-Eastern Terai, account for 36.5% of the total labor-related migration from Nepal between 2008 and 2014.

In order to investigate the role of past community level migration on current levels of migration in the community, I calculate the shares of migrants in 2001 and 2011 to a particular destination country from a VDC using the 2001 and 2011 Nepal Censuses. Then, for each destination country, I regress the share of migrants to the destination from a VDC in 2011 on the share of migrants from the same VDC to the same destination in 2001.[8] Estimated correlations for the top four destination countries are presented in Figure (3.4). There is a strong correlation between historical and current level migration shares. Coefficients are either closer to 1 or greater than 1 and are highly significant.[9] Additionally, I regress the number of migrants from a VDC to a particular destination country in 2011 on shares of migrants from the VDC that went to the same destination in 2001. Estimations are presented in Table (3.2). Again, the 2011 migration levels are highly correlated with the 2001 destination specific propensity scores to migrate.

A potential complication with the instrument is that, although lagged by a decade, lagged unobserved VDC characteristics can influence labor supply decisions. Historically, migrant sending communities might be systematically different to those less migrant sending communities on economic and labor market characteristics. High historical migration might be linked to bad economic conditions at the origin or remittances might have improved the local economic conditions over time. Depending on these conditions, IV estimates might be biased downward or upward. To address this problem, I control for a host of community level economic characteristics such as poverty rate, illiteracy rate, unemployment, and inequality within a VDC.[10] Even so, I cannot claim with certainty that the instrument captures no unobserved VDC characteristics that triggered the past migration and influenced the present labor

[8] For detailed calculations of shares of migrants and regression equations see the notes of the figures and tables.

[9] These results hold for other destination countries as well.

[10] It is more appropriate to control the community level characteristics at the baseline, but due to the data limitation I use the 2011 community conditions as the controls.

supply. However, the identification is at least as valid as those used in previous studies. Furthermore, I create an additional instrument, average GDP growth between 2001 and 2011 of the top 8 destination countries in 2011 and interact it with the share of migrants to those destinations from a VDC in 2001 as in (3.4).

$$IV_2 = \frac{\sum_{i=1}^{8} MIG_{2001,i,c}}{POP_{2001,c}} X \text{ GDP Growth}_{01-11} \tag{3.4}$$

where $MIG_{2001,i,c}$ is international migrants in 2001 from a VDC c to destination i and $POP_{2001,c}$ is the population of a VDC c in 2001. The top eight destination countries in 2011 are India, Malaysia, Saudi Arabia, Qatar, Kuwait, the UAE, the UK, and the USA. GDP Growth_{01-11} is the average GDP growth of the top 8 countries between 2001 and 2011. Since the GDP growth of the destination countries is exogenous to the local labor market conditions the interacted term is likely to be exogenous as well. I report results from both IV_1 and IV_2 in the main specification. Despite the binary endogenous variable, I use the linear 2SLS estimation strategy as suggested by Angrist and Pischke (2009). Because of the binary endogenous variable, the conditional expectation function (CEF) associated with the first-stage might be nonlinear. One can use a nonlinear first-stage and use the predicted probabilities as an instrument in a garden-variety 2SLS as suggested by Angrist and Pischke (2009) and Wooldridge (2010) to avoid ''forbidden regression" in the second step. However, this requires making distributional assumption of the first-stage CEF. In contrast, with the linear 2SLS, one need not worry whether the first-stage is linear (Angrist and Pischke, 2009). [11]

3.5 Results

A linear estimation of equation (3.2) using IV_1 (equation 3.3), and IV_2 (equation 3.4), are presented in Table (B.2) columns (1) and (2) respectively.[11] Coefficients on both the instruments are highly correlated with the household's

[11]With a linear estimation, one would be worried about predicted probabilities from the first-stage not being within 0 and 1. I performed the check and all the predicted values in the estimated models are within the range.

migration decisions. Adults in VDCs with higher proportions of migrants in 2001 were more likely to live in a household with at least one international migrant in 2011. Interestingly, there is a positive relationship between local unemployment rate and illiteracy rate. The negative correlation between the migrant outflows and the local market conditions results in downward biased OLS estimates of the impact on the labor market outcomes (Foged and Peri, 2013).

The OLS estimates of specification (3.1) are presented in columns (3) and (4), the 2SLS using IV_1 are presented in columns (5) and (6) and IV_2 are presented in columns (7) and (8) of table (B.2). The dependent variables are weekly hours supplied for wage-employment and self-employment. I use all the adults, both employed and non-employed in wage and self-employment. Technically, not working is equivalent to observing 0 hour of labor supply, therefore, including people who do not work in the sample and assigning 0 hour to their labor supply does not create the problem of sample selection. The estimated effects are a combination of intensive and extensive margins. Columns (3), (5) and (7) are estimated wage labor supply equations while (4), (6) and (8) are self-employment labor supply equations.

Overall, the estimates of β_2s, coefficients on X's in equation (3.1), are comparable across the estimation strategies. $\hat{\beta}_2$'s direction corresponds well with the economic intuitions. Individual characteristics, age, and household head status strongly determine the level of labor supply for both the wage and self-employment. It is not surprising that women, in Nepal, are less likely to work outside their homes, and the supply of wage-hours increases with years of schooling. Similarly, household characteristics, and land ownership decrease wage-hours and increases time spent in self-employment. Owning a home and being from a higher social caste, Brahman/Chhetri, discourage adults from working.

The coefficient of interest, $\hat{\beta}_1$, which is the coefficient on migration decision in equation (3.1) is statistically significant across all the econometric techniques used for time spent in wage employment. While comparable with each other, IV estimates are significantly greater in magnitude than the OLS estimate. As discussed earlier, OLS suffers from selection bias and the negative correlation between the local market conditions and the migration flows is likely to bias the effects downward. Furthermore, the 2SLS can recover the impact only on the compliers i.e. local average treatment effect (LATE), which is always greater than the intention to treat effect (ITT).[7] Both the instruments are reasonably strong, with a high correlation between the endogenous variable and the instruments. F-statistics of the first stage are always above 60, which are greater than the threshold value of 10 researchers usually consider below which one might run into the problem of weak instrument (Stock and Yogo, 2005).

Results from the IV regressions suggest that having a migrant in the household discourage the left-behind members from working in wage-employment. Adults from migrant-families decrease their weekly hours of labor supply for wage-employment by about 8 hours when compare to the adults from the non-migrant households. This is a decrease of almost one official work day. The direction of the effect is consistent with the prediction of the standard labor supply model. The income transfers through remittances increase the reservation wages of non-migrating members, which is likely to discourage people working in market employment. Migration has a negative effect on self-employment; however, both the IV results are small in magnitude and statistically not different from 0. As discussed earlier, these IV results are combinations of intensive and extensive margins. Separating these effects provides better insight into the economic behavior of the left-behind members and may assist better in policy design.

I apply the following strategy to separate out effects on intensive and extensive margins. I use a binary employment status, 1 for employed - 0 otherwise, as the dependent variable to estimate the impact on extensive margins. To examine the impact on intensive margins, I analyze the hours supplied for a particular employment by limiting the sample to those who are engaged in that employment. That is, depending on the left-hand side outcome variable, the analyzed sample is conditioned on being employed in that particular sector.

Tables (3.3) and (3.4) reports 2SLS estimates of the impact of migration on wage employment and self employment outcomes, respectively, by sex using IV_1 as the instrument. Panel A of the tables shows the impacts on extensive margins while panel B shows the effects on intensive margins. Again, the instrument is reasonably strong with all F-statistics from the first stage above the threshold value of 10 except for the hours supplied for non-agricultural wage employment, which is at 7.1. Both male and female adults from the migrant sending families have smaller involvement in wage-employment, both on extensive and intensive margins when compared to adults from families without migrants. Meanwhile, migration has greater negative effect on left-behind men's propensity to participate in market employment than women's, 31.1% vs 20.5%, (Table 3.3, Panel A). However, among working adults, left-behind women supply fewer hours in wage-employment than the left-behind men (Table 3.3, Panel B), which could be a result of Nepali women, if employed outside their homes, having mostly part-time jobs. At the same time, having migrants in the family affects left-behind women's self-employment decisions on both extensive and intensive margins, but not men's. Compared to women from households without migrants, women in households with migrants are 28.0% more likely to be engaged in self-employment, mostly working in their own farms. However, among those employed, women decrease weekly hours worked by 13 hours (Table 3.4). Overall, migration has negative effect on all the left-behind household adults' involvement in wage-employment but it is only women who increase their involvement in self-employment. In a country with traditional household norms and social culture, where women tend to be subordinate to men, it is not surprising that only women redistribute their time allocation in response to sending some family members abroad.

Table (3.5) presents the 2SLS estimates of the impacts of migration on the time spent in household production and other activities by the left-behind adults and children using IV_1 as the instrument.[12] Sending some members abroad has 0 effect on time allocation in household activities of left-behind male adults and boys. Women and girls, on the contrary, significantly decreased their weekly hours, 7.8% and 4.1% respectively, in less productive activities and increase their weekly hours in more productive activities more than proportionally, 8.2% and 5.5% respectively.

When a household sends some of its members aboard, there are several pathways through which non-migrating member's labor supply is affected. First, the income transfers through remittances increase the valuation of

[12]Fetching water, collecting firewood and dung, taking care of animals, making mats, knitting, weaving, tailoring and processing preserved food are classified as household production activities. Whereas, minor house repair, cooking food, cleaning, laundry, dishes, babysitting, and taking care of the elderly are categorized as other household activities. They are classified as such because the CBS takes into account only the time spent on former activities while calculating unemployment. Male and females are ages between 18 and 60 while boys and girls are ages between 6 to 17.

leisure of left-behind members making leisure more appealing. Second, because of the decrease in the household's stock of labor hours, left-behind members might overburden themselves by adding the role vacated by the migrants onto their own workload. Third, households might realign their priorities and redistribute the remaining stock of labor time once they send members abroad. In order to distinguish the later two pathways, I add weekly hours supplied by the left-behind household members across all activities. The results are presented in table (B.3). The second scenario is true neither for the left-behind women nor men. As a matter of fact, non-migrating male members increase their weekly leisure consumption by 18 hours by cutting their involvement in all forms of employment (wage + self-employment). This fits well with the first scenario rather than the later two. To isolate the first path, of impacts through income transfers, one has to model migrants' decision to send back remittances, which requires a new identification strategy and potentially a new instrument. Due to limitations in the data, I believe the analysis is beyond this papers' scope. Meanwhile, the aggregate effect is statistically 0 for the left-behind women members, which corresponds well with the third scenario. In Nepal, while left-behind males increase their leisure consumption in response to sending members abroad, women realign their priorities and assume the roles in home production and self-employment - roles likely to have been vacated by the migrants.

The differential impacts between the left-behind male and female members are most likely to be a result of differential bargaining power male and female members have within a household. Bargaining heterogeneity, however, is not limited to gender differences. Even within a gender group, members might have different levels of bargaining, creating heterogeneous responses to migration within the group. Among female members, older females and household heads are likely to have a higher level of bargaining than other female members in the family. Table (B.4) presents 2SLS results of the impact of migration on total labor supply by women's age (Panel A) and women's household head status (Panel B). Consistent with the hypothesis, older women from migrant-household supply substantially less overall labor hours (16) mostly by reducing their hours in wage and self employment (13). In contrary, migration has no effect on younger women. At the same time, when a household sends its members abroad, it negatively affects overall labor supply of women who are head of the household but has no effect on other female members's labor supply (Table B.4, Panel B). It is plausible that depending on each member's bargaining power within a household, the decision to send members aboard have differential impacts on each left-behind member' labor supply decision.

3.5.1 Heterogeneous Effects

As with the sex of the left-behind members, household's migration decisions are likely to affect different groups within the household differently. I divide the sample by skill level, age, and the household head status to analyze the potential treatment effect heterogeneity. Table (3.6) presents the 2SLS estimates by skill level. Adults with school leaving certificate (SLC) or more are defined as high skilled.[13] It is only the low skilled left-behind members that are affected by the migration. There is statistically zero effect on labor supply of high skilled adults. While less likely to participate in market jobs, low skilled adults are more likely to be involved in self employment activities. At the same time, low skilled adults, if employed, supply less weekly hours for both wage-employment and self-employment. The differential impact by skill level is reasonable as high skilled workers are likely to be already involved in more formal and permanent jobs that have higher opportunity costs of switching.

Tables (3.7), and (3.8) present the effect of migration on labor supply from the 2SLS estimation using IV_1 as an instrument by age and household head status respectively. Adults with ages between 18 to 40 are defined as young adults. Irrespective of age, migration affect the left-behind members similarly, both younger and older adults decrease their participation in wage employment while increase participation in self-employment. Likewise, both type of adults have similar responses in their intensive margins - lower weekly labor supply (Table 3.7).

Although outmigration of some family members has similar effect on the intensive margins for wage-work and own work, there is a significantly different effect on the participation rate by household head status. The left-behind

[13] All the students whether in private or public schools that follow Nepali education system, have to take the school leaving certificate (SLC) exam at the end of tenth grade. All schools in the country follow the system with very few exceptions, which follow the Indian or American system. It is mandatory that students pass the exam to continue their studies further within the country and qualify for most of the government and private jobs.

household heads are 41.4% less likely to participate in paid work compared to 23.1% of the other household members. Concurrently, outmigration of family members does not affect the household head's decision to participate in self-employment activities but significantly increases other members' involvement, 29.8% (Table 3.8). This corresponds well with the intuition that the household heads are likely to have higher bargaining power within the household and are likely to receive remittances sent by the migrants, which in turn, may further increase their bargaining power.

3.6 Discussion and Conclusions

In this paper I use a unique source of nationally representative data during the period that Nepal experienced a boom in outmigration. The data set contains detailed information on time allocation of every individual in the household, which allows me to extend the analysis further and answer the question that previous studies on the topic could not answer in Nepal. Using the NLSS III data set, this paper explores the impact of migration on labor supply of the left-behind household members both on extensive and intensive margins for wage-employment, self-employment, and household activities. The paper also provides an answer to the question, what are the remaining members in the households engage in instead of work employment?

I find that having international migrants in the family discourage members staying behind from participation in wage-employment. This is true for both male and female members. However, female members increase participation in self-employment, almost entirely through subsistence farming. The paper also finds that both self-employed and wage-employed adults decrease weekly hours of labor supply and only women in the migrant-sending household increase time in household activities. Findings presented in the paper suggest that male-dominated migration forces women to realign priorities and reallocate time from market-work to farming and household activities. In contrast, because of the income transfers, men now value their leisure more and decrease their overall labor supply. These are reasonable findings in a country where the traditional household norms and social culture see women as subordinate to men.

The question of the impact of outmigration on the well-being of the left-behind members is of importance for Nepal, which already has high levels of outmigration and the trend for outmigration is on the rise. The neoclassical micro theory identifies wage and employment opportunity differentials between the place of origin and the place of destination as the main cause of migration. Therefore, women switching from the formal labor market to self-employment should speed-up the process of equalization of wages and opportunities between the two places (Lokshin and Glinskaya, 2009). However, with the opening of new destinations, economic incentives abroad, and increasingly simpler migration processes, outmigration from Nepal will not decline anytime soon. International migration for labor-employment from Nepal, however, is risky.[14] Many Nepali emigrant laborers find themselves working in hazardous conditions, work long hours, face delays in getting paid, and some even lose their lives (Kaphle, 2014). It is imperative that Nepal put in place a broad set of policies that protect the welfare of migrants, many of which are breadwinners in their families, and safeguard the wellbeing of the left-behind members.[15]

The results presented in the paper may play an important role in designing some of these policies, especially in protecting the wellbeing of the remaining household members. They highlight the need for tailored policy initiatives that target specific subpopulations. Male-dominated outmigration pushes left-behind women to withdraw from wage-employment and increase the labor supply in their own farms. Policy initiatives should be focused on improving the rural wage labor markets, which would allow households to hire workers to replace those who migrate. Additionally, these policies may help in insuring households against the negative migrant-related shocks if they can encourage the left-behind members to remain in the formal wage-employment, which may have higher returns than self-employment.

[14] International migration from Nepal, especially for labor, is costly and required an extensive planning ahead. It requires obtaining passport and visa, purchasing ticket and saving up or borrowing for the associated costs. Similarly, most of the international work-related migration involves migration brokers who charge high fees for their services and there are contractual agreements in place between migrants and hiring agency ahead of migration and reversing the decision once made can be very costly (Bhattarai, 2005).

[15] See McKenzie and Yang (2015) for reviews of different policies about migrations.

3.7 Figures and Tables

Figure 3.1: International Migration Trend from Nepal

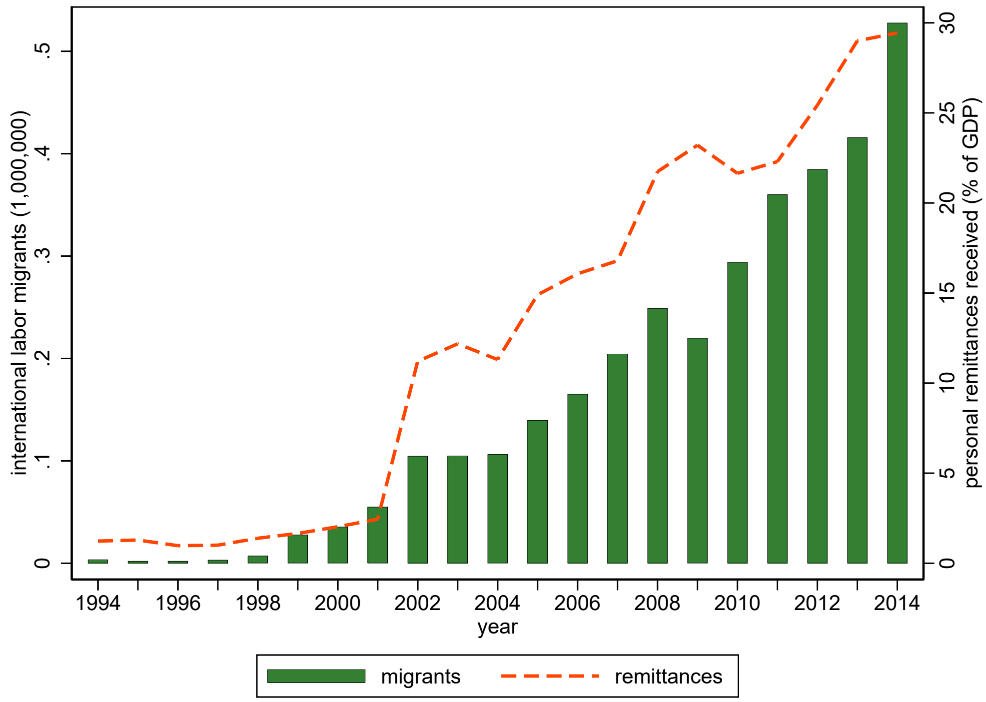


Figure 3.2: Household Income- Kernel Density

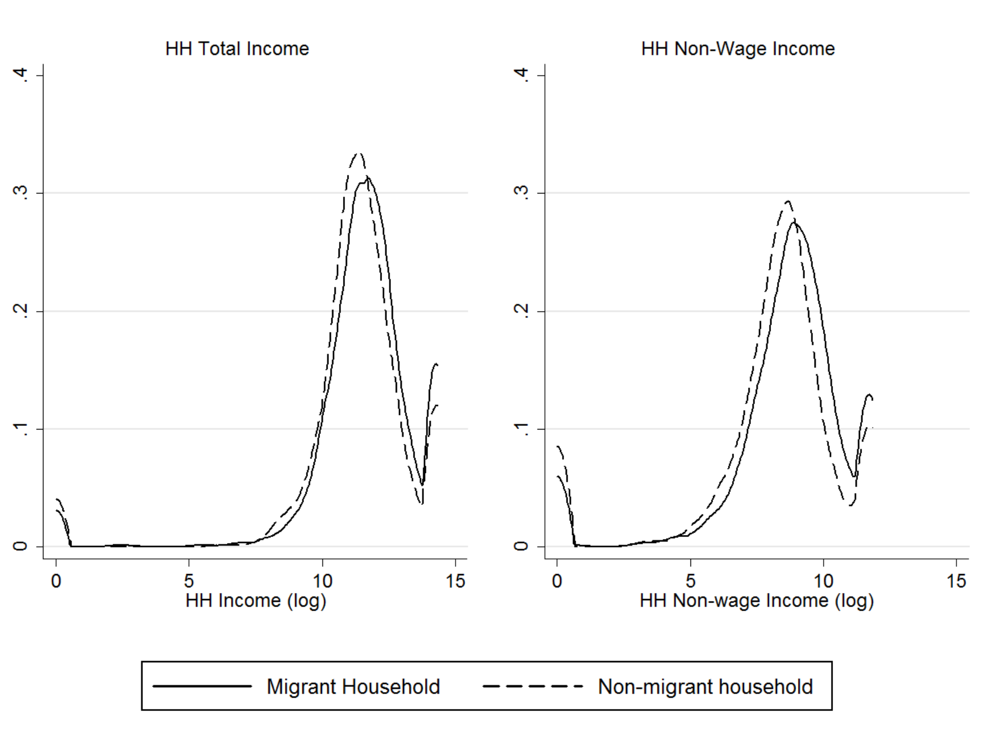
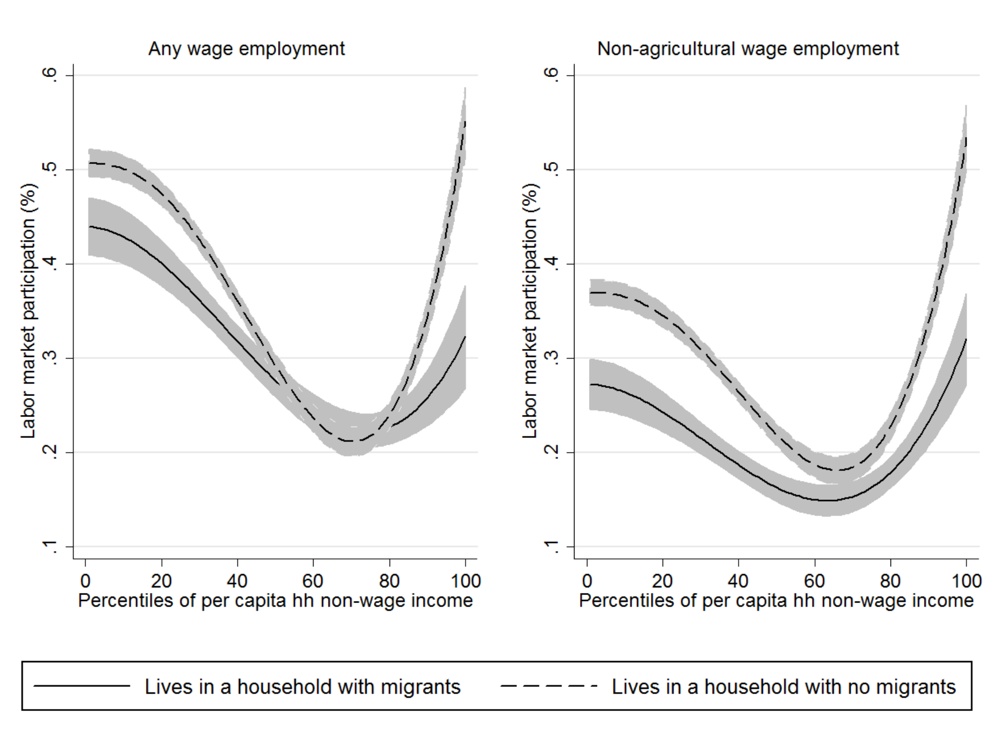
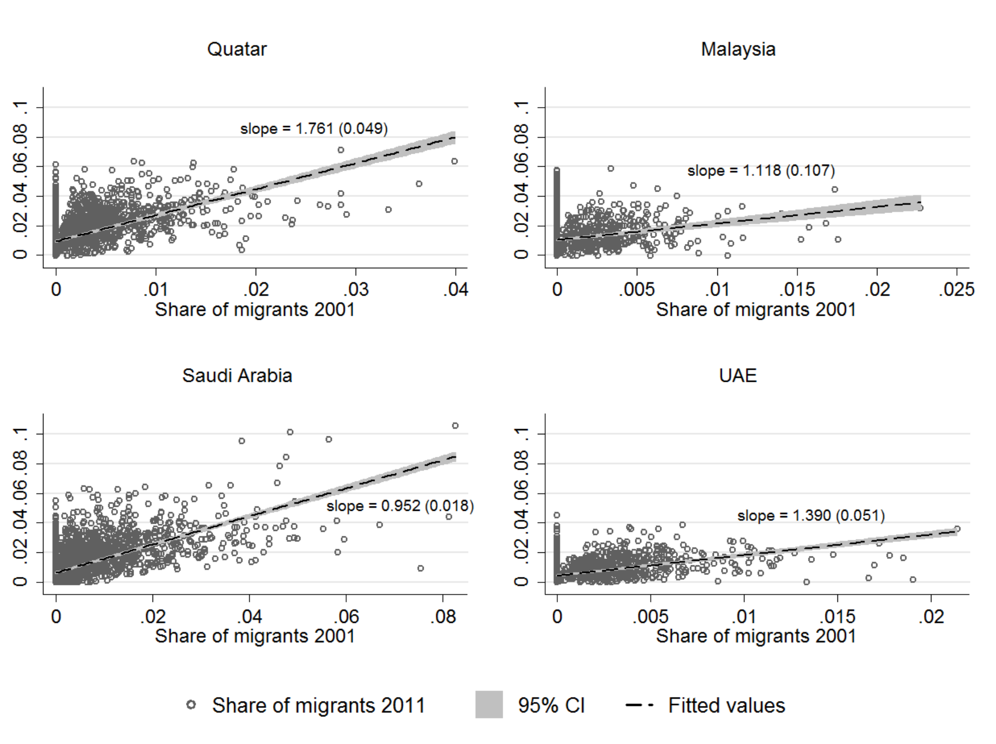


Figure 3.3: Rate of Labor Market Participation - All Adults



Note: Sample is limited to working-age (18 to 60) population. Figures are created using fractional polynomial regression. Non-wage income is monthly per-capita.

Figure 3.4: Correlation Between 2001 and 2011 Proportions of Migrants by Major Destination Countries.



Data Source: 2001 and 2011 Nepal Census. Linear fit is $Y_{j,d,11} = \alpha_0 + \alpha_1 Y_{j,d,01}$, where $Y_{j,d,11}$, and $Y_{j,d,01}$ are share of migrants to destination d from VDC j in 2011 and 2001 respectively. Share of migrants is calculated as $Y_{j,d,t} = \dfrac{M_{j,d,t}}{POP_{j,t}}$, where $M_{j,d,t}$ is number of migrants from VDC j to destination d in year t. $POP_{j,t}$ is the population of VDC j in year t.

Table 3.1: Descriptive Statistics

	All Adults		Female		Male	
	Nonmigrant (Mean)	Migrant-Nonmigrant (Difference)	Nonmigrant (Mean)	Migrant-Nonmigrant (Difference)	Nonmigrant (Mean)	Migrant-Nonmigrant (Difference)
Wage employment	0.370	-0.079 ***	0.239	-0.021 **	0.517	-0.084 ***
Self employment	0.670	0.044 ***	0.644	0.055 ***	0.699	0.044 ***
Individual Characteristics						
Age	35.699	0.915 ***	35.046	0.454 *	36.436	2.337 ***
Years of education	5.303	-0.912 ***	4.025	-0.495 ***	6.744	-0.686 ***
Married	0.793	0.005	0.798	0.040 ***	0.786	-0.067 ***
Household head	0.340	0.010	0.113	0.154 ***	0.596	-0.085 ***
Household Characteristics						
Female household head	0.131	0.163 ***	0.170	0.201 ***	0.087	0.059 ***
Household size (AE)	5.217	0.251 ***	5.175	0.139 **	5.265	0.502 ***
Share of children 0-7	0.122	0.020 ***	0.125	0.028 ***	0.118	0.002
Share of children 8-15	0.186	-0.005	0.189	0.006	0.184	-0.028 ***
Share of female adult	0.310	0.048 ***	0.341	0.038 ***	0.274	0.041 ***
Share of male adult	0.286	-0.073 ***	0.243	-0.080 ***	0.334	-0.024 ***
Share of elderly	0.055	0.010 ***	0.060	0.008 ***	0.049	0.009 ***
Landless household	0.298	-0.092 ***	0.285	-0.083 ***	0.313	-0.099 ***
Own less than 1 acres	0.316	0.011	0.319	0.019 *	0.313	-0.008
Own 1-2 acres	0.184	0.024 ***	0.192	0.021 **	0.174	0.024 **
Own 2-5 acres	0.152	0.057 ***	0.154	0.047 ***	0.150	0.075 ***
Own 5 or more acres	0.049	0.000	0.049	-0.004	0.050	0.009
Own a house	0.854	0.072 ***	0.868	0.057 ***	0.839	0.091 ***
VDC and Region Characteristics						
Unemployment rate	0.083	-0.005 ***	0.082	-0.005 ***	0.084	-0.005 **
Illiteracy rate	0.395	0.039 ***	0.400	0.039 ***	0.389	0.036 ***
VDC inequality (Gini)	0.469	0.001	0.471	0.002	0.466	-0.002
District poverty rate	0.212	0.011 ***	0.215	0.011 ***	0.209	0.008 **
Rural	0.615	0.105 ***	0.629	0.101 ***	0.599	0.100 ***
Observations	16879		9597		7282	

Note: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Sample is working age (18 to 60) adults.

Table 3.2: Migration Network: Dependent Variable - Number of Migrants in 2011 (1,000)

	All VDCs in Nepal		NLSS VDCs	
	(1)	(2)	(3)	(4)
Share of migrants 2001	3.758***	3.779***	6.606***	6.714***
	(0.101)	(0.101)	(0.676)	(0.678)
Constant	0.013***	0.001	0.037***	-0.005***
	(0.001)	(0.001)	(0.006)	(0.002)
District Fixed Effect		Yes		Yes
Observations	100880	100880	11648	11648
R^2	0.145	0.161	0.060	0.097

Data Source: 2001 and 2011 Nepal Census. Standard errors in parentheses.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors are clustered at VDC level. Estimated model is $M_{j,d,11} = \beta_0 + \beta_1 Y_{j,d,01}$, where $M_{j,d,11}$ is number of migrants, in 1,000, to destination country d from a VDC j in 2011. $Y_{j,d,01}$ is share of migrants to destination d from a VDC j in 2001. Share of migrants is calculated as $Y_{i,d,t} = \frac{M_{j,d,t}}{POP_{j,t}}$, where $M_{j,d,t}$ is number of migrants from VDC j to destination d in year t. $POP_{j,t}$ is the population of VDC j in year t.

Table 3.3: 2SLS Estimation of Labor Supply by Gender- *Wage Employment*

	Female			Male		
	(1) Any Wage Emp.	(2) Agri.	(3) Non-Agri.	(4) Any Wage Emp.	(5) Agri.	(6) Non-Agri.
Panel A: Labor market participation						
Household with migrant	-0.205*	-0.049	-0.221**	-0.311*	0.027	-0.371**
	(0.104)	(0.083)	(0.088)	(0.175)	(0.114)	(0.185)
Observations	9597	9597	9597	7282	7282	7282
Wald χ^2	1236.455	576.071	1744.580	941.219	476.251	858.504
F-test 1stage	70.938	70.938	70.938	41.195	41.195	41.195
Panel B: Hours supplied						
Household with migrant	-15.219**	7.292	-26.255*	-13.564*	2.955	-13.505*
	(6.826)	(6.134)	(13.613)	(7.066)	(4.996)	(7.730)
Observations	2222	1069	1264	3623	773	3189
Wald χ^2	2004.359	205.487	920.637	1538.916	314.929	1475.447
F-test 1stage	18.791	16.894	7.140	37.343	20.071	31.814
Controls						
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
VDC characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes

Note: 2SLS estimates are reported in the table. Share of international migrants in a VDC in 2001 is used as an instrument for the estimations. Sample is working age (18 to 60) adults and standard errors are clustered at VDC level. Standard errors in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Individual controls are age age^2, years of education, marital status, and household head identifier. Household controls are female HH head, share of male and female adults, share of elderly, share of children, amount of land-owned, house ownership and social caste. Similarly VDC level controls are, unemployment rate, poverty rate, illiteracy rate, inequality (Gini), and urban/rural location. Regions are Kathmandu, other urban areas, Western hills, Eastern hills, Western Terai, and Eastern Terai. Sample in Panel B is conditioned on being employed in that particular sector. Log of hourly wages is added as an extra individual control.

Table 3.4: 2SLS Estimation of Labor Supply by Sex- *Self Employment*

	Female			Male		
	(1) Any Self Emp.	(2) Agri.	(3) Non-Agri.	(4) Any Self Emp.	(5) Agri.	(6) Non-Agri.
Panel A: Labor market participation						
Household with migrant	0.280***	0.308***	-0.098	0.141	0.205	-0.179
	(0.089)	(0.113)	(0.087)	(0.120)	(0.148)	(0.166)
Observations	9597	9597	9597	7282	7282	7282
Wald χ^2	8883.913	15715.418	703.258	7294.166	18469.293	841.418
F-test 1stage	70.938	70.938	70.938	41.195	41.195	41.195
Panel B: Hours supplied						
Household with migrant	-13.030***	-6.460*	-24.345	-10.252	-5.819	4.337
	(4.559)	(3.787)	(16.457)	(6.781)	(4.098)	(15.669)
Observations	6358	5550	1557	5160	4006	2109
Wald χ^2	525.033	434.614	504.186	2193.401	662.555	863.127
F-test 1stage	61.724	58.076	10.609	42.797	35.274	11.702
Controls						
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
VDC characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes

Note: 2SLS estimates are reported in the table. Share of international migrants in a VDC in 2001 is used as an instrument for the estimations. Sample is working age (18 to 60) adults and standard errors are clustered at VDC level. Standard errors in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Individual controls are age age^2, years of education, marital status, and household head identifier. Household controls are female HH head, share of male and female adults, share of elderly, share of children, amount of land-owned, house ownership and social caste. Similarly VDC level controls are, unemployment rate, poverty rate, illiteracy rate, inequality (Gini), and urban/rural location. Regions are Kathmandu, other urban areas, Western hills, Eastern hills, Western Terai, and Eastern Terai. Sample in Panel B is conditioned on being employed in that particular sector.

Table 3.5: 2SLS Estimation of Labor Supply for Household Activities

	Household production				Other household activities			
	(1) Adult-Male	(2) Adult-Female	(3) Boys	(4) Girls	(5) Adult-Male	(6) Adult-Female	(7) Boys	(8) Girls
Household with migrant	1.034	8.184**	2.006	5.541**	-2.409	-7.776**	0.366	-4.107*
	(4.456)	(3.810)	(2.326)	(2.354)	(2.361)	(3.218)	(0.970)	(2.151)
Observations	7282	9597	4985	5195	7282	9597	4985	5195
Wald χ^2	1805.517	3126.243	755.167	1678.898	1055.533	1854.033	583.091	1950.024
F-test 1stage	41.195	70.938	55.647	47.598	41.195	70.938	55.647	47.598
Coefficient 1stage	1.611***	1.913***	1.976***	1.815***	1.611***	1.913***	1.976***	1.815***
Controls								
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
VDC characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Fetching water, collecting firewood and dung, taking care of animals, making mats, knitting, weaving, tailoring and processing preserved food are classified as household production activities. Whereas, minor house repair, cooking food, cleaning, laundry dishes, babysitting and taking care of elderly are categorized as other household activities. 2SLS estimates are reported in the table. Share of international migrants in a VDC in 2001 is used as an instrument for the estimations. Male and female samples are working age,18 to 60, adults while boys and girls samples are restricted to ages between 6 and 17. Standard errors are clustered at VDC level. Standard errors in parentheses.* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Individual controls are age age^2, years of education, marital status, and household head identifier. Household controls are female HH head, share of male and female adults, share of elderly, share of children, amount of land-owned, house ownership and social caste. Similarly VDC level controls are, unemployment rate, poverty rate, illiteracy rate, inequality (Gini), and urban/rural location. Regions are Kathmandu, other urban areas, Western hills, Eastern hills, Western Terai, and Eastern Terai. Coefficient 1stage is the estimated coefficient on the exogenous instrument in the first-stage.

Table 3.6: 2SLS Estimation of Labor Supply by Skill Level

	Wage Employment		Self Employment	
	(1) Low Skilled	(2) High Skilled	(3) Low Skilled	(4) High Skilled
Panel A: Labor market participation				
Household with migrant	-0.205**	-0.486	0.199***	0.324
	(0.098)	(0.305)	(0.069)	(0.346)
Observations	14054	2825	14054	2825
Wald χ^2	3577.578	997.890	3864.717	2175.469
F-test 1stage	68.765	8.179	68.765	8.179
Coefficient 1stage	1.925***	1.460***	1.925***	1.460***
Panel B: Hours supply				
Household with migrant	-15.028***	1.032	-13.064***	5.048
	(5.754)	(11.830)	(4.454)	(14.563)
Observations	4714	1131	10319	1199
Wald χ^2	2170.635	505.787	1130.111	1815.099
F-test 1stage	33.235	7.061	63.906	6.082
Coefficient 1stage	1.820***	1.665***	1.921***	1.503**
Controls				
Individual characteristics	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
VDC characteristics	Yes	Yes	Yes	Yes
Region Fixed Effect	Yes	Yes	Yes	Yes

Note: High skilled adults are defined as adults with 11 years or more of education. Sample in Panel B is conditioned on being employed in that particular sector. Log of hourly wage is added as an extra individual control for column (1) and (2). 2SLS estimates are reported in the table. Instrument used for 2SLS is share of international migrants in a VDC in 2001 (IV1). Sample is working age (18 to 60) adults and standard errors are clustered at VDC level. Standard errors in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Individual controls are age age^2, years of education, gender, household head identifier, and marital status. Household controls are female HH head, share of male and female adults, share of elderly, share of children, amount of land-owned, house ownership and social caste. Similarly VDC level controls are, unemployment rate, poverty rate, illiteracy rate, inequality (Gini), and urban/rural location. Regions are Kathmandu, other urban areas, Western hills, Eastern hills, Western Terai, and Eastern Terai. Coefficient 1stage is the estimated coefficient on the exogenous instrument in the first-stage.

Table 3.7: 2SLS Estimation of Labor Supply by Age

	Wage Employment		Self Employment	
	(1) Young Adults	(2) Old Adult	(3) Young Adults	(4) Old Adults
Panel A: Labor market participation				
Household with migrant	-0.292***	-0.260**	0.252**	0.236***
	(0.113)	(0.122)	(0.103)	(0.085)
Observations	10861	6018	10861	6018
Wald χ^2	2028.588	1552.717	15778.586	3651.974
F-test 1stage	58.207	50.061	58.207	50.061
Coefficient 1stage	1.836***	1.937***	1.836***	1.937***
Panel B: Hours supply				
Household with migrant	-13.546*	-13.516**	-8.632*	-14.457**
	(7.380)	(5.618)	(5.037)	(5.925)
Observations	3913	1932	6901	4617
Wald χ^2	2379.303	1482.415	2401.545	545.837
F-test 1stage	31.766	24.765	52.713	43.511
Coefficient 1stage	1.645***	2.078***	1.804***	1.957***
Controls				
Individual characteristics	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
VDC characteristics	Yes	Yes	Yes	Yes
Region Fixed Effect	Yes	Yes	Yes	Yes

Note: Young adults are ages between 18 and 40 while old adults are ages between 41 and 60. Sample in Panel B is conditioned on being employed in that particular sector. Log of hourly wage is added as an extra individual control for column (1) and (2). 2SLS estimates are reported in the table. Instrument used for 2SLS is share of international migrants in a VDC in 2001 (IV1). Sample is working age (18 to 60) adults and standard errors are clustered at VDC level. Standard errors in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Individual controls are age age^2, years of education, gender, household head identifier, and marital status. Household controls are female HH head, share of male and female adults, share of elderly, share of children, amount of land-owned, house ownership and social caste. Similarly VDC level controls are, unemployment rate, poverty rate, illiteracy rate, inequality (Gini), and urban/rural location. Regions are Kathmandu, other urban areas, Western hills, Eastern hills, Western Terai, and Eastern Terai. Coefficient 1stage is the estimated coefficient on the exogenous instrument in the first-stage.

Table 3.8: 2SLS Estimates of Labor Supply by Household Head Status

	Wage Employment		Self Employment	
	(1) Household Head	(2) Other Members	(3) Household Head	(4) Other Members
Panel A: Labor market participation				
Household with migrant	-0.414***	-0.231**	0.125	0.298***
	(0.147)	(0.100)	(0.098)	(0.091)
Observations	5791	11088	5791	11088
Wald χ^2	906.861	1882.340	6353.722	8786.879
F-test 1stage	53.309	62.508	53.309	62.508
Coefficient 1stage	1.633***	2.030***	1.633***	2.030***
Panel B: Hours supply				
Household with migrant	-17.642**	-11.559*	-11.369*	-10.493**
	(7.434)	(6.610)	(6.662)	(4.285)
Observations	2772	3073	4450	7068
Wald χ^2	2009.570	2551.071	943.402	1107.469
F-test 1stage	24.419	30.862	50.477	57.718
Coefficient 1stage	1.515***	2.032***	1.647***	2.036***
Controls				
Individual characteristics	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
VDC characteristics	Yes	Yes	Yes	Yes
Region Fixed Effect	Yes	Yes	Yes	Yes

Note: 2SLS estimates are reported in the table. Instrument used for 2SLS is share of international migrants in a VDC in 2001 (IV1). Sample is working age (18 to 60) adults and standard errors are clustered at VDC level. Standard errors in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Individual controls are age age^2, years of education, gender, household head identifier, and marital status. Household controls are female HH head, share of male and female adults, share of elderly, share of children, amount of land-owned, house ownership and social caste. Similarly VDC level controls are, unemployment rate, poverty rate, illiteracy rate, inequality (Gini), and urban/rural location. Regions are Kathmandu, other urban areas, Western hills, Eastern hills, Western Terai, and Eastern Terai. Coefficient 1stage is the estimated coefficient on the exogenous instrument in the first-stage. Sample in Panel B is conditioned on being employed in that particular sector. Log of hourly wage is added as an extra individual control for column (1) and (2).

CHAPTER 4

UNFORTUNATE MOMS AND UNFORTUNATE CHILDREN: IMPACT OF NEPALESE CIVIL WAR ON WOMEN'S STATURE AND INTERGENERATIONAL HEALTH

4.1 Introduction

The environmental conditions experienced *in utero* and in early life have profound influence on human biology and long-term health (Golden, 1994; Martorell et al., 1994; Forsdahl, 1977; Barker, 1992; Bateson et al., 2004; Gluckman et al., 2007, 2008). Similarly, early life conditions have lasting and significant impacts on adulthood economic outcomes (see reviews by Strauss and Thomas, 2008; Currie and Vogl, 2013). These results are highly relevant in the context of civil conflicts, which represent sources of considerable human suffering, death and property destruction. Despite the potential of conflict to contribute to lasting impacts on health, the empirical evidence on long-term and intergenerational effects of conflict on health is limited.[1] Lack of conflict data at detailed geographic units provide a significant challenge in measuring the consequences of conflicts precisely.

In this paper, I investigate the impacts of early childhood exposure to Nepal's 1996-2006 civil conflict on women's final adult height and on second-generation health using the 2016 Nepal Demographic Health Survey (NDHS)

[1]Previous studies have documented long-run effects at the cross-country level, suggesting conflicts have large and negative immediate effects on overall economic growth. However, the recovery to equilibrium is rapid (see review by Blattman and Miguel, 2010). Correlated with war exposure, the literature has extensively documented long-run effects of exposures to stress on mental health (Persson and Rossin-Slater, 2018), height and diseases (Bozzoli et al., 2009), birth weight (Camacho, 2008; Quintana-Domeque and Rdenas-Serrano, 2017), and education and socioeconomic status (Almond, 2006). Previous research on conflict has mostly focused on human capital accumulation during or shortly after conflict (Akresh and De Walque, 2008; Bundervoet et al., 2009; Valente, 2014; Akresh et al., 2014; Pivovarova and Swee, 2015). A few recent studies have focused on the long-term impact of conflict on human capital accumulation, with some finding no effect (Miguel and Roland, 2011) and others (Akresh et al., 2012; Len, 2012; Justino et al., 2014; Palmer et al., 2016; Akbulut-Yuksel, 2017) finding significant negative impacts.

and village level variation in conflict intensity. By exploiting the detailed geographic information on conflict incidents (village-level conflict intensity), I am able to identify the effects of exposure to conflict more accurately than previous research.[2] The literature on the consequences of war, including in Nepal[3] has thus far mostly focused on conflict variation at a broader regional level (see Valente, 2014, 2015; Pivovarova and Swee, 2015; Akresh et al., 2012). Moreover, this study extends the literature on legacies of war by documenting the long-term effect of exposure to conflict on women's final stature development. Along with Akresh et al. (2018), which examines the impacts of Biafran war using the variation in exposure to the war by ethnicity, this is among the first papers to document the intergenerational transmission of the impact of early childhood conflict exposure on second generation health.

Nepal experienced a decade-long violent civil conflict between 1996 and 2006, which resulted in more than 13,000 fatalities, significant destruction of infrastructure, severe hindrance in delivery of basic services and generated pervasive and strong feelings of fear, insecurity, and stress among its citizens. I use Informal Sector Service Center's (INSEC) records of conflict victims to create casualty-level data set with exact geographical locations (villages) and dates of incidents. I merge the village-level conflict intensity with the 2016 Nepal Demographic Health Survey (NDHS), which is a nationally represen-

[2]A typical approach in the literature is to exploit conflict variation at a broader regional level, which has the potential to misclassify one's exposure to conflict and induce measurement errors. For instance, using detailed GPS data on distance between survey villages and conflict sites, Akresh et al. (2014) show that substantial number of households in Eritrea were misclassified as being in non-conflict region in the Akresh et al. (2012) paper that used less precise regional conflict data and there are significant differences in the estimated effects of the Eritrea-Ethiopia conflict between the two measures of the conflict: effects are 87-188% larger using the GPS based measure than the regional based measure of the conflict.

[3]Previous studies of Nepal's civil war are mostly focused on understanding the causes of the war: geographical terrain (Murshed and Gates, 2005; Bohara et al., 2006; Do and Iyer, 2010; Menon and van der Meulen Rodgers, 2015), economic exclusion and poverty (Murshed and Gates, 2005; Onesto, 2005; Do and Iyer, 2010), inequality (Murshed and Gates, 2005; Macours, 2011; Nepal et al., 2011), and lack of political representation (Murshed and Gates, 2005; Bohara et al., 2006; Macours, 2011). The little evidence documenting the consequences of the war, thus far, is focused on district level disaggregation and results are mixed - little to zero impact on human capital accumulation (Valente, 2014; Pivovarova and Swee, 2015), increased miscarriages (Valente, 2015) and positive impact on women employment (Menon and van der Meulen Rodgers, 2015). Libois (2016), on the other hand, using conflict measurement at more detailed geographical area (distance from the conflict sites) find significant negative immediate impact on consumption and income. Failure to capture the substantial conflict heterogeneity across villages within district, thus, may explain the little or no impact of conflict at district level.

tative survey of the female population aged 15 to 49. I limit the analytical sample to women who were either born before or who were *in utero* at the start of the war in February 1996 to assess the lasting impact of conflict on height. In 2016, these women were old enough (20 to 49) to be sampled in the ever-married NDHS 2016 women sample and had documented survey responses about their children that can be used for second-generation analysis. Limiting the sample to women born before the conflict's start date also reduces potential confoundedness through selective fertility and migration (further discussed in empirical strategy section 4). I rely on the biomedical literature that height development in human beings is characterized by rapid growth during the first three years of life, followed by lower level of constant growth and then a secondary growth spurt during adolescence (Figure C.5), and classify women into three treatment cohorts, namely: ages 0 to 3, 4 to 8, and 9 to 15 at the start of the war in February 1996. While women in age cohort 0 to 3 would have been exposed to conflict their entire three stages of growth, the age cohorts 4 to 8 and 9 to 15 would have been exposed only in their latter stages of growth. I define women ages 16 to 21 in 1996 as a comparison group because they would have passed their pubertal ages and gained full adult height by the time of the conflict's start in 1996. I also include women ages 22 to 29 as a second control group to validate parallel-trend dynamics in difference-in-difference specification.

This research makes two primary contributions to the literature on the legacies of war. First, using the variation in exposure to conflict, as measured by months of war, by birth cohort and village of residence, I find that conflict and, in particular, exposure starting in child's infancy, has a highly significant and negative impact on womens final adult height. Findings are robust across model specifications and measures of conflict. In validating the difference-in-differences estimation strategy used, I find no evidence of presence of non-parallel dynamics nor of selective migration and fertility. These results are significant in the face of growing evidence of the lasting impacts of stunting and slow growth in height early in life on overall physical, biological and cognitive development, school achievement, economic productivity and maternal reproductive outcomes (see review by Dewey and Begum, 2011). Additionally, given the established literature on the existence of a height premium (Persico et al., 2004; Case and Paxson, 2008; Vogl, 2014; Bargain and Zeidan, 2017) these results have important economic importance.

A second contribution: I find that the mothers' exposure to conflict is detrimental for their children's health, especially child weight as measured by weight-for-height, weight-for-age and BMI z-scores. Results are robust to alternative measures of conflict intensity including the one defined at mother's district of birth. I find strong evidence that the women exposed to conflict during childhood have more children and live in poorer households as adults. The combination of these two factors may result in meaningful decreases in parental ability to invest in children. However, other unobserved factors such as stress and genomic changes may also influence the intergenerational transmission.

This paper links to two important strands of economics literature. First, the established literature of *in utero* and early life shocks on adult outcomes (see review by Almond and Currie, 2011) and that insufficient or lack of parental investment during critical periods of child development can lead to irreversible damage (Cunha and Heckman, 2007). Second, the paper is related to the literature providing strong positive intergenerational human capital transmission (Currie and Moretti, 2003, 2007; Almond et al., 2012; Justino et al., 2014; Bhalotra and Rawlings, 2013).

The reminder of the paper is organized as follows. Section 2 provides the background on Nepal's civil conflict, major events that helped shape the war, and the physical and economic costs of the war. Conflict intensity from the INSEC's database and individual data from the 2016 NDHS are discussed in section 3. Section 4 presents empirical strategies for evaluating both the first and second-generation impacts. Empirical results along with identification validation and potential mechanisms for intergenerational transmission are presented in section 5 and concluding remarks are presented in section 6.

4.2 Background

Nepal is a landlocked country between India and China. Because of its highly mountainous and rugged terrain and lack of adequate infrastructure and economic development, most parts of the country remain remote, and access to basic services remains unattainable to many. With two-thirds of its 30 million inhabitants (estimated as of July 2017) relying on agriculture and a quarter living under the poverty line, Nepal is one of the least developed nations in the world (CIA World Factbook, 2019) and is among the lowest in health, sanitation, primary education and electricity in South Asia.

Figure C.1 shows the administrative divisions of Nepal before the implementation of a new constitution in 2015. The country was divided into five geographically homogeneous development regions, which were further divided to form 75 districts. Districts were further divided into rural (village development committee, VDC) and urban (municipalities) areas, which were the lowest level of administrative units. At the time of the 2011 population census, Nepal consisted of 3,914 VDCs and 58 municipalities (Central Bureau of Statistics, 2012). I calculate conflict intensity at the level of these 3,972 local administrative areas.[4]

4.2.1 Conflict in Nepal

For most of modern history, Nepal was governed by absolute monarchs. In the early 1990s several political parties launched pro-democracy street protests, known as the "Jana Andholan" (People's movement), leading to the emergence of multi-party democracy and the introduction of a new constitution. Despite participating in the 1991 legislative democratic elections and winning 9 of the 205 parliamentary seats, the Communist Party of Nepal Maoist (CPN-M) launched an armed struggle, the so-called "People's War", against the state on February 13, 1996 (or in Nepali calendar 2052 Falgun 1 Bikram Sambat). A week before the conflicts start, the CPN Maoist submitted a 40-point memorandum to the government and warned of armed militant struggle if demands were not met. Demands included drafting a new constitution through an election of a constituent assembly; land redistribution; and political equality for all castes, language groups, and women. The government refused to meet the demands of the Maoists. In response, the Maoists attacked an agricultural bank and three police posts in rural western Nepal and formally launched the "People's War".

Over the following decade, the insurgency developed into an entrenched and brutal country-wide civil war. By the end of the insurgency, conflict related killings were recorded in 73 of the 75 Nepalese districts. Figure 4.1 presents the timeline of the war including major events that shaped the con-

[4] For convenience, throughout the paper I refer to these local administrative units as villages although some of them are urban municipalities.

flict and monthly casualty numbers. As part of the Maoists' strategy, in the early years of the insurgency they launched a guerrilla warfare mostly harassing police forces and garnering support in a few rural areas with communist strongholds (Thapa and Sijapati, 2004). Nepal's remote terrain, under-development, extreme rural poverty, deeply rooted caste and ethnic discrimination, sentiments of political and economic exclusion among rural communities, and lack of government presence in rural areas propelled the Maoists' cause further (Onesto, 2005). The initial inability of the government to recognize the underlying problems that fueled the conflict and to acknowledge the connection between armed conflict and political, economic, and social grievances of the period enabled small communist political elites to mobilize a large base and eventually challenge the government militarily and politically (Kreuttner, 2008).

The year 2001 was a crucial moment for the insurgency. In June 2001, the killing of King Birendra along with most of his immediate family members in a royal massacre shocked the nation. The King's brother, Gyanendra, was then crowned King. A conspiracy theory emerged centering on Gyanendra's possible involvement in the massacre and questioning the findings of the official investigation further destabilized the country, increasing distrust in the government and the King (Thapa and Sijapati, 2004). A state of emergency was declared in November 2001 and the Royal Nepal Army (RNA) officially got involved in the war after the Maoists walked away from a two-month long ceasefire and attacked an RNA barrack. Thereafter, the conflict intensified and extended geographically. As illustrated in Figure 4.1, most killings occurred after 2001. However, the insurgency drastically changed its course after King Gyanendra, citing prolonged conflict and growing attacks by the Maoists, dismissed the elected government, placed major political figures under arrest, and assumed direct control over the country in February 2005. Joining the widespread disapproval of the King's actions, the Maoists formed a pact with seven major political parties to present a common front against the monarchy. This eventually led to a signing of a peace accord in November 2006 (or 2063 Mangshir Bikram Sambat), the formation of an interim seven party plus the Maoists coalition government, and an official end to the war. At the time of the signing of the peace agreement, the death toll of the war had reached more than 13,000 (Table 4.1).

The CPN Maoist's presence across the country over the course of the conflict varied greatly. While the Maoists had a weak presence in urban areas – failing to control even a single city or a district headquarters – they dominated rural Nepal. In October 2003, they declared control over 80% of rural areas (Onesto, 2005) and in many places established fully functional local governments and law courts of their own. They also, however, selectively targeted government forces, attacking army barracks and police posts in urban areas and destroying local government buildings (Do and Iyer, 2010). There were widespread human rights violations and abuses throughout the insurgency by both the government forces and the CPN Maoists (OHCHR, 2012). Physical assault, abduction, and torture of civilians, and looting of individual properties by the Maoists were reported extensively throughout the conflict (Bohara et al., 2006). The security forces, on the other hand, were the major perpetrators of sexual violence, arbitrary arrests and disappearances of civilians and were accused of murder, torture, mutilation, and other cruel and inhumane treatment of civilians to extract information from anyone they deemed appropriate (OHCHR, 2012).

4.2.2 Consequences of the Civil Conflict and Mechanism

The conflict had widespread impact on economic development and severely hampered delivery of government services. The Maoists' unofficial motto of "Destruction before construction" was very popular among its cadres and was heavily advertised (Nepal, 2004). Maoists destroyed key infrastructures linking urban areas to their rural strongholds and sabotaged public delivery systems. Maoists, often, targeted rural bridges that linked rural to urban areas and district headquarters, and in many parts of the country, destroyed health posts, drinking water systems, public communication systems, and schools (Jha, 2008). Between 1996 and 2003, physical infrastructure worth at least $250 million was destroyed (Mahat, 2006) and the cost of the conflict was estimated at $66.2 billion (Ra and Singh, 2005).

The conflict in Nepal is likely to have affected adult health and economic outcomes in multiple ways including direct physiological and mental stress, nutritional shocks and reduced access to healthcare.[5] First, the conflict severely affected the delivery of government services in rural areas; in partic-

[5]See Akresh et al. (2018) for the discussion of mechanisms through which stress and inadequate resources during civil conflict is likely affect health.

ular, decreasing health care delivery. Hundreds of community health posts were destroyed; several health care workers were killed; many fled their posts; and cold-chain delivery of vaccines became unsustainable (Singh, 2004). Second, the conflict likely led to direct physiological and mental stress on residents, especially rural residents. As reported in Table 4.1, by the end of the conflict, more than 13,000 people lost their lives, and more than 1,500 people were either disappeared, injured, or disabled. Among the family members of these casualties (estimated between 400,000 and 500,000), many suffered from mental and psychological trauma (Media Foundation, 2011). Third, many lost their sources of income, were displaced, widowed, and many children were orphaned. Fourth, the complete disruption of public delivery systems came as a major shock to nutrition and food security, especially in the northern region that had relied heavily on the government-subsidized rations.

4.3 Data

One of the major impediments to analysis of conflict is the lack of conflict data at sufficiently granular geographic scale. Most of the literature on the legacies of war therefore is focused on assessing the effects of conflict intensity at relatively high geographical levels. For example, previous studies exploring the determinants of Nepal's civil conflict (Murshed and Gates, 2005; Bohara et al., 2006; Do and Iyer, 2010; Macours, 2011) and the consequences of the conflict (Valente, 2014; Pivovarova and Swee, 2015; Menon and van der Meulen Rodgers, 2015) exploit variation in intensity of the insurgency at district level or even higher geographical units. Defining conflict variables at broader geographical level measures individuals exposure to conflict less precisely and can create measurement error. Moreover, significant heterogeneity in geography, socioeconomic status and development among areas within districts can creates difficulty in addressing the association between the determinants of war at district level and explained variables of interests, likely leading to omitted variable bias.

In contrast, this analysis uses detailed and geographically granular conflict intensity data; i.e. village-level insurgency. As households within a village tend to be highly homogeneous in socioeconomic status, ethnicity, and live in the same geographical terrain, this paper is able to avoid many of the concerns induced by the imprecise measure of individual's exposure to conflict. Additionally, combining these high-resolution conflict data with the 2016 Nepal Demographic Health Survey (NDHS 2016) allows me to explore the impacts of the conflict on the children of individuals who were exposed to the war in their own childhoods.

4.3.1 Conflict Data

I use the Informal Sector Service Center's (INSEC) records of the conflict victims to create conflict intensity variables. INSEC is an active Nepalese non-governmental human rights organization. Throughout the war, INSEC documented human rights violations and abuses extensively and its archive of the casualties provides detailed information on each victim's demographic, social and economic characteristics. The database is considered the most reliable data source on casualties of the conflict. Numerous studies including Do and Iyer (2010); Nepal et al. (2011); Valente (2014, 2015) and Libois (2016) have used the database. I extract demographic, educational achievement, social and economic characteristics, and political affiliation of each victim. Most importantly, I extract exact geographical location (village) and the date of the incident.

Table 4.1 reports descriptive statistics of the war fatalities. In total the INSEC dataset contains information on 14,982 victims; most (13,210) are fatal casualties. More than 60% of the casualties are perpetrated by the state. The CPN Maoists deny exploiting the grievances among ethnic groups regarding political, social and economic exclusion to advance their agenda. However, the majority of their cadres belonged to ethnic groups such as Magars, Gurungs, and Dalits of the hills and mountains. It is, therefore, not surprising that the majority of casualties among the Maoists are ethnic minorities (60% not reported) and more than half of the total victims are also from the minority groups. Apart from attacking security forces, the Maoists also frequently targeted upper caste civilians, especially Bramins and Chhetris; these upper castes the Maoists labeled as counterrevolutionary elements. The average age of the victims is 28.3 years and almost 90% are male. Many were actively involved in politics- 54% are affiliated to either the rebel party or other political parties.

The conflict intensity varied greatly over time. I summarize the number of monthly casualties and major events that helped shape the insurgency in Figure 4.1. As illustrated, the period after 2001 when the country was under the state of emergency and the RNA was actively involved was the bloodiest. The conflict lasted for total of 131 months from February 1996 to November 2006. Using information on each victims village and date of incident, I define months of war$_v$ in a village, v, as the baseline conflict intensity variable, constructed as follow:

$$\begin{gathered}\text{conflict}_v = \text{months of war}_v = \sum_{m=1}^{131} \mathbf{1}(\text{casualty}_{vm}) \\ \text{where, } \mathbf{1}(\text{casualty}_{vm}) = 1 \text{ if casualty}_{vm} > 0\end{gathered} \tag{4.1}$$

and subscripts v and m index a village and a month since the beginning of the war i.e. m takes the value of 1 for February 1996 and 131 for November 2006. Variable casualty$_{vm}$ is number of casualties in a village v in a month m.

Villages in Nepal experienced different levels of conflict as illustrated in Figure 4.2, which depicts the number of months each village experienced conflict out of the total 131 months of the war. The intensity of the conflict varied substantially across villages within districts. Defining the conflict intensity as in Equation (4.1), however, may create a possibility of under measuring the conflict intensity for example, a village could be under the siege of the Maoists, reducing access to public services but without any casualties; similarly, one-off destruction of infrastructure could have longer-term ramifications. Unfortunately, we do not have records on infrastructure damages during the conflict. Nonetheless, total months of exposure to conflict and casualty count is used extensively in the literature to measure conflict intensity and assuming classical measurement error, because of the tendency to attenuate towards zero, the estimated coefficients will provide lower bound of the conflict impacts. Additionally, I use several other measures of conflict intensity as below:

$$\text{conflict}_v = \text{number of casualties}_v = \sum_{m=1}^{131} \text{casualty}_{vm} \tag{4.2}$$

$$\text{conflict}_{vN1} = \text{months of war}_{vN} = \sum_{m=1}^{131} \mathbf{1}(\text{casualty}_{vNm}) \tag{4.3}$$

$$\text{conflict}_{vN2} = \text{number of casualties}_{vN} = \sum_{m=1}^{131} \text{casualty}_{vNm} \tag{4.4}$$

$$\text{conflict}_{v50a} = \text{months of war}_{v50} = \sum_{m=1}^{131} \mathbf{1}(\text{casualty}_{v50m}) \tag{4.5}$$

$$\text{conflict}_{v50b} = \text{number of casualties}_{v50} = \sum_{m=1}^{131} \text{casualty}_{v50m} \tag{4.6}$$

Conflict intensity based on Equation (4.2), measure of total casualties in a village over the duration of the war, is illustrated in Figure C.2. Again, the measure exhibits significant variation across villages and the intensity pattern is highly similar to Figure 4.2. The next four Equations (4.3) to (4.6) are defined at higher geographic level with the consideration for potential spatial spillovers – conflict in nearby villages may induce stress, limit one's access to health care or other services. While Equations (4.3) to (4.4) are months of war and casualty counts, respectively, in a village including in its contiguous neighboring villages, Equations (4.5) to (4.6) report months of war and casualty count in a village including the villages around 50-kilometer radius from the center of the village.

4.3.2 Individual Data

I also use the 2016 Nepal Demographic Health Survey (NDHS 2016) in the analysis. The survey was implemented by New ERA under the aegis of the Ministry of Health of Nepal and was funded by the United States Agency for International Development (USAID). The data collection took place between June 19, 2016 and January 31, 2017.

The NDHS 2016 is a nationally representative survey of the female population ages 15 to 49. The sampling frame for the survey was based on the updated version of the 2011 Nepal Population Census. After the implementation of the 2015 constitution, based on the population several VDCs and Municipalities within districts were merged to form rural development areas and urban areas. The old Village Development Committees (VDC) in the rural and enumeration areas (EAs) in urban places essentially form primary sampling units (PSU) for the 2016 NDHS. In the final sample, 383 clusters or PSUs were selected with probability proportional to their population size (see Ministry of Health, 2017). Figure C.3 illustrates the coverage of the survey. All 75 districts except Manang and Mustang were sampled; however, these two districts also had zero casualties during the conflict and are excluded from the baseline analysis. As illustrated in Figure 4.3 (months of war) and Figure C.4 (casualty count), conflict intensity varied significantly across the 383 NDHS villages (clusters).

Within the selected DHS clusters, 30 randomly selected households were interviewed, and all women aged 15 to 49 who were permanent residents or visitors who stayed in the household the night before were eligible for the interview. A sub-sample of about half of the households were selected for biomarker information. All children aged 0 to 59 months and women 15 to 49 years in these households were administered the anthropometry, hemoglobin, and blood pressure measurements. I limit the analytical sample to those that are born before or in utero during the start of the conflict in February 1996 so that they are old enough to be sampled in the ever-married NDHS 2016 women sample and have children that can be incorporated in the second-generation analysis. The final analytical sample size is 4,421 women ages 20 to 49 at the time of the survey (0 to 29 at the start of the conflict) and their 2,168 under age 5 children.

Table 4.2 summarizes women's health outcomes and their exposure to conflict and demographic characteristics. I divide women into two cohorts ages 0 to 15 (treatment) and 16 to 29 (control) at the time of the start of the conflict. While the women in the former cohort would have still been in a period of physical growth during the conflict years, the latter cohort would have already gained full adult height (detail discussion is presented in empirical strategy section 4). On average, women in the sample are 151.6 centimeters tall, with the younger treated cohort 0.46 cm taller on average. Similarly, compared to the control cohort, treated cohorts are less likely to have had any incidence of pregnancy loss, report having had fewer live births, were slightly younger at first birth, attained more years of education, and were less likely to be employed at the time of the survey. However, there is no difference in economic status (wealth index) between the two groups.

By construction, the older cohort faced zero level of conflict during the first fifteen years of life (Panel B.1). Balance in lifetime exposure to conflict between the two sets of women (Panel B.2) is reassuring for the empirical strategy used in section 4, which implies that the two groups do not come from different types of sampled clusters. On average, women are 33.4 years old with treatment and control cohorts being 27.9 and 41.8 years old respectively (Panel C). Women in treatment groups are more likely to live in a household with a female head, are more likely to be of lower caste, and are less likely to be from Eastern Development region. All other controls are balanced between the groups.

Summary statistics of the children sample is presented in Table 4.3. Children are divided into treated and control group by their mother's age at the start of the conflict. 47% of the children are girls and on average tend to be the second child (Panel C). There is no difference in control variables (Panel C) and health outcome variables between the two groups except treated children are slightly taller. Panel B reports mothers exposure to conflict. Again, it is reassuring that there is no disparity in their mothers lifetime conflict experience.

4.4 Empirical Strategy

Women surveyed in the NDHS 2016 experienced different levels of exposure to conflict intensity according to their village of residence and year of birth. The identification strategy exploits this variation, specifically, the variation in exposure to the conflict during the individual's critical period of physical growth.

Height development in humans is characterized by three distinct stages. There is a rapid growth during the first three years of life, followed by a lower level of constant gain in height until the start of adolescence and then a second growth spurt during adolescence ending in gaining full adult height (Tanner et al., 1966a,b; Beard and Blaser, 2002; Bozzola and Meazza, 2012). Figure C.5 demonstrates height velocity curves for a typical boy and a typical girl. Under adequate nutritional and environmental conditions, the height growth rate is highest during infancy, 26 centimeters per year, and progressively declines until around age three, then stabilizes around 6 centimeters per year until the start of puberty (Beard and Blaser, 2002; Bozzola and Meazza, 2012). Pubertal height spurt among girls starts after age nine, peaks at about twelve years, and stops around the age of 15 (Figure C.5).

I borrow these stylized facts from the clinical and bio-medical literature to establish causality. NDHS collects individuals' month and year of birth, which I use to create the age of women at the start of the conflict in February 1996. Figure 4.4 presents cohorts by age at the start of the conflict and potential exposure to conflict at different stages of their physical growing periods. As demonstrated in Figure C.5, girls past their pubertal age i.e. cohorts aged 16 to 21 and 21 to 29 in 1996 (control 1 and 2 respectively) would have gained full adult stature by the time of the start of the conflict, and the effect of the conflict should be minimal or zero. I use cohort 16 to 21 as the main comparison group and use cohort 22 to 29 as a control placebo experiment in a difference-in-difference specification. Based on their growing phases at the beginning of the conflict, I create three conflict-exposed cohorts. Although in the important phase of adolescence height spurt, girls aged 9 to 15 (treatment 3) would have faced conflict only in their third phase of height growth. While girls aged 4 to 8 (treatment 2) would have been exposed to conflict in the second and third stages of growth, cohort 0 to 3 (treatment 1) would have been exposed to conflict through the entire growing period (all three stages).[6] In the baseline specification, I define conflict intensity at the village level, hence, all five cohorts from any given village would have been exposed to the same total amount of conflict during their lifetimes. However, the exposure would have started at different times of their lives.

4.4.1 First Generation Impact

To explore the impact of early childhood exposure to conflict on adult outcome, I employ the following estimation strategy:

$$\begin{aligned} Y_{imntcvdr} = &\beta_c(\text{conflict}_v \times \lambda_c) + \text{conflict}_v + \lambda_c + \alpha_t + \eta_m \\ &\delta_v + \gamma_r^T + X_i + \omega_n + \varepsilon_{imntcvdr} \end{aligned} \tag{4.7}$$

where Y is an outcome of a woman i born in month m and year t, interviewed in month n, and residing in village v, district d and development

[6] These five cohorts aged 0 to 3, 4 to 8, 9 to 15, 16 to 21, and 22 to 29 at the start of the conflict in 1996 would become 20 to 23, 24 to 28, 29 to 35, 36 to 41, and 42 to 49 at the time of the NDHS 2016 survey.

region r. While women's adult stature is the main outcome of interest, I also explore conflicts impact on women's reproductive health, sexual behavior, educational attainment, employment, and wealth. The independent variable of interest, $\text{conflict}_v \times \lambda_c$, is constructed as a vector of age-cohort specific coefficients. The baseline conflict intensity variable is months of war in a village; however, I also estimate the same equation using all the other conflict variables defined in the data section. Equation (4.7) also includes the main conflict variable, conflict_v, and cohort fixed effects, λ_c, as part of the independent variables. While α_t are year of birth fixed-effects, η_m are month of birth fixed effects added to control for any seasonality - whether women were born in peak or lean season. While δ_v are village (NDHS cluster) fixed effects, ε is a random, idiosyncratic error term.[7] Equation (4.7) also includes five development-region-specific trends, γ_r^T, to isolate variance in a cohort's outcome in deviation from the long-run trend in her development region of residence. The five development regions were relatively homogeneous in terms of development, geographical terrain and ethnic composition before the war.

The socioeconomic status of the household is likely to play a significant role in child development and would be desirable to control for in the regression. Albeit observed ten years after the end of the civil war during the time of the survey, household characteristics may still be influenced by the conflict. X_i, therefore, includes only variables that are time-invariant. Contrary to the Maoists' denial, ethnicity played an important role in the insurgency. X_i, therefore, includes indicators for belonging to a high caste. In addition, month of the survey interview fixed effects, ω_n, are included in the regressions to control for the variation in seasonality due to the timing of the survey.

β_cs in Equation (4.7) are the main coefficients of interest. Under a standard difference-in differences model assumption and, in particular, under the assumption that there is no correlation between village level conflict and unobserved factors varying with village and birth year cohort within the development region, β_c coefficients indicate the causal impact of early childhood exposure to civil conflict on adult stature. While interpreting the results, given the set of fixed effects in Equation (4.7), β_c do not identify the effects

[7] I also estimate equation 6 using district fixed effects instead and the results are robust. All the standard errors are clustered at the village level to allow for the correlation among error terms within village.

at a national level. Rather effects are identified due to womens exposure to conflict by village of residence and birth year cohort net of birth year trends common to all the villages within the development region. The goal of the paper is to measure the total effect of the conflict on one's life and specification 7 does exactly that. Rather than measuring the impact of exposure to conflict at a specific period of one's life, it measures the cumulative impact of exposure during one's entire growth period.[8]

Studies exploring the determinants of the Nepal's civil conflict have advanced several arguments regarding the insurgency heterogeneity across Nepal including geographical terrain (Murshed and Gates, 2005; Bohara et al., 2006; Do and Iyer, 2010; Menon and van der Meulen Rodgers, 2015) economic exclusion and poverty (Murshed and Gates, 2005; Do and Iyer, 2010), inequality (Macours, 2011; Nepal et al., 2011), and lack of political representation (Murshed and Gates, 2005; Bohara et al., 2006; Macours, 2011). These determinants of variation in insurgency intensity are, therefore, likely to be correlated with the outcomes of interest, threatening the validity of the identification. However, all these studies have focused on the determinants of the conflict at the district level. Therefore, the application to village level conflict are at most minimal because unlike districts, villages in Nepal are highly homogeneous in terms of ethnic composition, socioeconomic status, and geography. A major advantage over previous studies of the conflict is that this paper uses a detailed geographical level conflict intensity allowing for the inclusion of village-level fixed-effects, which eliminates any village-level time-invariant factors. The timing of the beginning of the conflict in women's lives within a village therefore forms the comparison divide in the estimation strategy.

Selective fertility and endogenous migration are other major concerns re-

[8] A typical approach in the literature is to measure conflict based on one's exposure at specific age and use fixed effects models to identify ages or age-periods during which the exposure was most critical as below:

$$\begin{aligned} Y_{imntvdr} = &\beta_0 + \beta_1 \times \text{Exposure during 0 to 3 years} + \beta_2 \times \text{Exposure during 4 to 8 years} + \\ &\beta_3 \times \text{Exposure during 9 to 15 years} + \alpha_t + \eta_m + \delta_v + \gamma_r^T + X_i + \omega_n + \varepsilon_{imntvdr} \end{aligned}$$

Conflict variables are defined as woman's exposure to conflict during her age of 0 to 3, 4 to 8 and 9 to 15 years and all other variables have the same meanings as in Equation (4.7). The results are presented in Tables C.1 to C.2. While important in identifying what part of one's life was important, the specification does not identify the overall impact of the conflict.

garding the identification strategy. As discussed in the data section, I limit the analytical sample to those who were already born or *in utero* during the start of the conflict in 1996. Purely on identification prospective, it helps limit the potential confoundedness between the explained variables of interest and selective fertility and migration. For instance, the strategy helps mitigate the scenario in which after grasping the seriousness of the war, couples that are highly concerned about their children's health in high conflict areas may choose to delay having children or migrate to low conflict areas to start a family. However, it could be true for the periods prior to the start of the conflict that in anticipation of the war concerned couples may have delayed having children or may have migrated. Detailed robustness checks are presented in the empirical results section.

4.4.2 Intergenerational Health Impacts

The gap between the start of the Nepals civil conflict and the time of the NDHS 2016 survey is sufficient enough that I can explore the impacts the conflict had on the children of women who were exposed to the war in their childhood. Anthropometric measures were collected for children under the age of 5 at the time of the survey and hence I limit the second-generation sample to children under 5 in 2016. I employ the same strategy as in equation 6 and add child specific controls to estimate. Following the estimation equation:

$$\begin{aligned} Y_{jklnimtcvdr} =& \beta_c(\text{mother's conflict exposure}_v \times \text{mother's cohort}(\lambda_c)) \\ &+ \text{mother's conflict exposure}_v + \lambda_c + \alpha_t + \eta_m + \delta_v \\ &+ \gamma_r^T + +X_i + \mu_k + \theta_l + \pi_n + X_j + \varepsilon_{jklnimtcvdr} \end{aligned} \tag{4.8}$$

where Y is a health outcome of a child j whose anthropometrics were measured in month n, was born in month k and year l to a woman i who was born in month m and year t, and resides in village v, district d and development region r. Child health endowment is defined as function of all mothers controls and exposure to war as defined in section 4.1 and child specific characteristics. θ_l are childs birth-year fixed effects. As with mothers, child month of birth fixed effects, μ_k, are included to account for season of birth. Similarly, child anthropometric measures are sensitive to the timing of the measurement; in particular child weight, hence, Equation (4.8) also includes month of measurement fixed effects, π_n. ε is a random, idiosyncratic error term and all the standard errors are clustered at the village level to allow for correlation among error terms within villages. X_j is a vector of time-invariant child controls dummy variables equal to one if the child is a girl and if the child is a twin and child birth order fixed effects. As in Equation (4.7), β_cs are the coefficients of interest and have the same meaning as in Equation (4.7) but identify to the impact of mothers childhood exposure to conflict on her child's outcomes. The equation under the standard assumptions of difference-in-differences models provides estimates of the causal impact of conflict on second-generation health.

4.5 Results

In this section, first I present the impact of childhood exposure on adult stature and establish validity for the identification strategy. Second, I report impact on health and economic outcomes that are very important to women's well-being, but also provide additional explanation for the impact on second-generation health. Finally, I present the health impacts on the children of women who were exposed to the war in their childhoods.

4.5.1 Impact on Women's Stature

Table 4.4 presents the impact of early childhood exposure to conflict on adult stature using difference-in-differences Equation (4.7). The conflict intensity variable used is months of war in the village of residence. While the outcome variable, height, in columns 1 to 3 is measured in centimeters, columns 4 to 6 present height-for-age standard deviation (HAZ). The possibility of "non-parallel dynamics" in the difference-in-differences estimation could be problematic. Because of difference in overall trends (health, education, poverty, environmental etc.), changes in adult stature could vary systematically across villages and, in particular, there could be mean reversion. Given the data structure, I can, however, test for the identification assumption. Besides the control group (aged 16 to 21 in 1996), women in age cohort 22 to 29 in 1996 would have gained full adult height by the start of the conflict, hence, the changes in adult height between these two cohorts should not differ systematically across villages. Age cohort 22 to 29 is therefore included in all the regressions as a control to validate the identification assumption.

Village fixed effects are included in all specifications. I start estimation with no additional controls (column 1) and progressively include extra controls. Column 3 is the full baseline specification. The estimated differences-in-differences for cohort 22 to 29 are close to 0 in size and statistically not different from 0 across all specifications and measures of height. This provides strong evidence that the difference-in-differences coefficients of interests are not driven by inappropriate identification assumptions.

Exposure to civil conflict only during the pubertal spurt appears to have no significant effect on adult height. Across all specifications, conflict had statistically zero impact on height of the women aged 9 to 15 in 1996 compared to women in the control group. On the surface, this finding is slightly at odds with Akresh et al.'s (2018) analysis of Biafran war, where they find conflict intensity during womens adolescent years to have significantly negative impact on their adult stature. However, as illustrated in Figure 4.1, Nepal's conflict started as a small-scale rebellion and only after 2001 developed into countrywide brutal civil war. Unlike the other two treatment cohorts in the paper, most women in this age group would have escaped the most brutal phase of the civil conflict during their growing period. In addition, my identification strategy differs from Akresh et al.'s (2018) in that women in this age group start facing conflict only during their adolescent years.

Women aged 4 to 8 in 1996, on the other hand, would have just entered or already be in their adolescent spurt years when the conflict started to intensify in 2001.Therefore, it is not surprising to see the cumulative violence, which was at a lower level in the second growth stage and intense in the third stage, has significant and negative impact on their adult stature. Effects vary between 0.67 to 0.71 millimeters (0.011 to 0.012 sd) across the specifications. The coefficient in the full model can be interpreted as: an additional month of exposure to civil war during the latter two stages of the growing period decreases final adult height by 0.071 millimeters or 0.012 sd.

Besides being a period of a rapid growth, the first three years are also the most sensitive period to environmental influences on height in human beings (Schmidt et al., 1995). Early childhood height at ages 2 (Luo and Karlberg, 2000) and 5 (Satyanarayana et al., 1986), is a strong predictor of final adult height. Similarly, the pubertal growth spurt plays an important role in determining the final adult stature (Case and Paxson, 2008). Women in the age group 0 to 3 in February 1996, who would have been exposed to conflict during all three phases of growth, therefore would suffer the most from the conflict compared to the other age groups. On average, girls aged 0 to 3 in 1996 suffered a reduction in adult height of 1.36 millimeters or 0.023 standard deviations due to an additional month of exposure to conflict during their entire period of physical development (Columns 3 and 6). The effect size is twice as big as the effect on the cohort aged 4 to 8 (0.071mm and 0.012 sd) that experienced same level of violence but only after the age of 3, hence signifying the importance of environmental influences on height during the first three years. The result is highly significant and robust across all specifications and for both measures of height ranging from 1.22 to 1.36 mm and 0.021 to 0.023 sd reduction in adult height.

Figure 4.5 presents the impact of exposure to conflict starting at different ages using the baseline specification. Again, conflict exposure in first 8 years of life reduce adult height, however, ages 0 to 3 are the only ages that are statistically significant. As discussed in the data section, I define the conflict intensity variable multiple ways and results using the baseline specification are presented in Table 4.5. Conflict intensity used in column 1 is the number of casualties in one's own village of residence. Consistent with the earlier findings, among women who were aged 0 to 3 and 4 to 8 in 1996, increased casualties in the village of residence significantly decreased their final adult stature. In columns 3, 4 and 5, I define war intensity as months of war including the contiguous neighboring villages and within a 50-kilometer radius from the village center. Again, the results are robust, especially among age cohort 0 to 3.

Height has long been recognized as an important factor influencing individuals professional and personal success. Results presented in this section, thus, have important economic significance. Taller workers receive a wage premium. An additional inch of height is associated with 1 to 3 percentage increase in earning among the British and American adults (Persico et al., 2004; Case and Paxson, 2008). There is even greater height premium in lower income settings: an additional centimeter of height is associated with a 2 percent increase in hourly earnings both in Mexico (Vogl, 2014) and Indonesia (Bargain and Zeidan, 2017). Using the results from Indonesia and Mexico, a quick back-of-the-envelope calculation implies that an additional month of conflict exposure starting at infancy is associated with a decrease in hourly earnings of 0.27 percent.

4.5.2 Identification Validation

In this section, I present additional evidence in support of the estimation strategy. In addition to the possible presence of non-parallel dynamics discussed in the earlier subsection, selective migration and fertility are other major threats to the identification strategy. The conflict intensity variable is defined at the level of an individual's village of residence at the time of the survey. Systematic sorting by economic and physical status between the stayers at high conflict villages and movers from the high conflict to low conflict villages is of concern, which will lead to overestimation of the impact of conflict. Unfortunately, I do not observe womens village of birth. However, the 2016 NDHS asked each individual how long she has been residing at the place where she was surveyed and if not her entire life, the name of the district from which she migrated. Twenty-six percent of the women in the sample are living in a different district. Although the survey did not collect the information, marriage is the most likely reason for womens migration, as most women in Nepal move to their husband's home permanently from their maternal home, which is also likely to be their place of birth.

I define conflict at the level of women's districts of birth (district they moved from) and estimate the same specification as Equation (4.7) except with district of birth fixed effects and region of birth trends and present the results in Table 4.6. Columns 1 to 3 examine the presences of differences in migration patterns between the control and treated cohorts. The difference-in-differences across the specifications are zero, suggesting no selective migration. Columns 4 to 9 re-estimate the main results from Table 4.4. The differences-in-differences estimates, although reduced in magnitude, are statistically significant and in the same direction as in the main specification for age cohort 0 to 3. Villages within district are highly diverse and defining conflict at the district level takes away that variation. Additionally, I limit the sample to women living in the village where they were surveyed their entire life and estimate Equation (4.7). The results are presented in Table C.3.

The effect sizes are comparable to the baseline results in Table 4.4. These results, overall, suggest there is minimal or no selective migration among the women in the sample.

Limiting the sample to women already born at the start of the conflict limits the scope for confoundedness through selective fertility. However, prior to the start of the conflict couples that were highly concerned about their childrens health may have delayed having children in anticipation of the war. If true, we expect to see significantly different level of births between high and low conflict areas periods just before the start of the war. However, the conflict lasted for a decade and concerned couples may have had to wait for the full decade to have a child a highly unlikely scenario. To formally examine this issue, I use the information on district of birth and year of birth from every individual observed during the 2001 Nepal Population Census and calculate yearly district birth rates. As reported in Figure 4.6, there is no difference in birth rates between the districts experiencing above and below the median conflict intensity (months of war). Table C.4 reports regression results for the same test and we see no difference in periods long before and just before the start of the conflict.

4.5.3 Impact on Fertility, Education, Employment and Wealth

Estimates in Table 4.7 show the impact of conflict on reproductive health and fertility using the baseline specification. Early childhood exposure to conflict has no significant impact on probability of miscarriage or of stillbirth among women in age cohorts 9 to 15 and 4 to 8 in 1996. Women experiencing conflict during their entire growth period, however, are significantly more like to have had stillbirth or a miscarriage: each additional month of conflict exposure increases the risk of stillbirth by 0.03 and of miscarriage by 0.06 percentage respectively. Valente (2015) finds similar results that pregnancies that were exposed to Nepal's conflict were more likely to result in a miscarriage. There is no significant impact on the probability of abortion. Women exposed to conflict during their early growing periods have significantly more live births, 0.024 and 0.013 more births per month of exposure among cohorts aged 0 to 3 and 4 to 8 in 1996 respectively. The impact of conflict on the number of infant deaths is zero (column 5).

Impacts of early childhood exposure to conflict on other fertility outcomes are presented in Table 4.8. The conflict has statistically no impact on the likeliness of contraceptive use (column 1), number of women's sex partners, age at first sexual intercourse and age at first birth. There are also no significant differences between women in control and treatment groups on their age at first cohabitation with their domestic partner and number of marriages and unions. Additionally, there is no difference in the smoking or chewing tobacco habits of women in treatment groups and control group.

Table 4.9 shows impact on women's human capital accumulation, employment and wealth. While there is no significant impact on years of education, women in age cohort 0 to 3 are significantly less likely to have completed a school leaving certificate (SLC). The lack of results in years of schooling are consistent with Valente (2014) and Pivovarova and Swee (2015). Both examine the conflict's impact on human capital accumulation. Similarly, there is no impact on the probability of being employed. However, women exposed to conflict are significantly more likely be employed in agricultural sector. As a measure of a households cumulative living standard, the DHS reports a wealth index using ownership of easy to collect assets, materials used in housing construction, and access to type of water and sanitation facilities (see Rutstein and Johnson, 2004). Women exposed to conflict early in their childhood are likely to live in households with poorer living conditions (column 7). The wealth factor score is lower by 977 and 675 per additional month of exposure to conflict among women aged 0 to 3 and ages 4 to 8 in 1996 respectively. In addition to providing information on women's adult living conditions, outcomes discussed in this subsection provide a window to what types of households the children of individuals exposed to conflict in early childhood are living in. Therefore, these effects are likely to provide explanations for the second-generation health impacts presented in the next subsection.

4.5.4 Impact on Intergenerational Health

The intergenerational impacts of conflict on health are presented in Table 4.10. As discussed in section 4.2, the estimation strategy used is the same as for the first generation, except I add child-specific controls. The sample consists of children under the age 5 whose anthropometrics were measured. I also include children born to women aged 22 to 29 in 1996 to provide support for the identification validation required by difference-in-differences assumptions. The falsification test supports that identification strategy, as estimates for all outcomes for children born to the experimental control cohort of mothers are statistically zero.

Although statistically imprecise, mothers' exposures to conflict negatively affects child's height (column 1). Children born to women in all the three treatment groups are shorter by 0.005 to 0.011 standard deviations for their age. Child development in terms of weight gain is significantly hampered by mother's exposure to conflict in her childhood. Compared to children born to the control cohort of mothers, an additional month of mother's exposure to conflict during her entire growth period decreases her child's weight for height z-scores by 0.030 standard deviations (5.2 percent less than control mean). Exposure to conflict starting at older age is even more severe for second-generation weight for height. Women in cohorts age 4 to 8 and 9 to 15 in 1996 have children 0.039 and 0.041 standard deviations lighter for their height (6.7 and 7.1 percent less than the control mean). All the coefficients are estimated precisely. Column 3 reports the conflicts impact on second-generation weight-for-age. Mother's exposure during her childhood, again, has significant negative impact on child weight-for-age, especially among children born to women in the cohort aged 9 to 15 in 1996. Additionally, maternal war exposure is strongly associated with significant lower body mass of children (column 4). As with the weight-for-height, the impact size is increasing with the age at which the mother started experiencing war. Compared to the control, mothers exposed in all three stages, the latter two stages, and the final adolescence stage (aged 0 to 3, 4 to 8, and 9 to 15 at the start of conflict), have children with lower BMI by 0.031, 0.040, and 0.044 standard deviations respectively. Alternatively, an extra month of exposure led to a decrease in children's BMI by 0.030 to 0.044 standard deviations. These results are consistent with Akresh et al. (2018), in that the adolescent exposure to conflict has strongest impacts on second-generation health. Table C.5 presents the same results using other conflict measures discussed in section 3 and the results are robust across all conflict definitions.

As a robustness check, Table 4.11 presents the intergenerational impact using conflict intensity based on mother's district of birth. The estimate shows stronger results than when defining maternal exposure to conflict at the level of village of residence. Compared to the children of women in the control group, weight-for-height z-scores are significantly less among children of women in the treatment group. The impact size between the treatment groups are highly comparable. Similarly, impacts on weight for age and BMI have similar strong negative impacts. These coefficients are smaller than those reported in table 11 but are more precisely estimated. In both the specifications, we observe negative but statistically zero impact on child height. However, at this stage of physical growth, children may be too young to develop stunting and negative impacts on weight measurements provide strong indication for future stunting.

Channels for intergenerational transmission of maternal exposure to conflict to child may vary greatly. In addition to unobserved factors such as the physiological stress and genomic changes, the intergenerational transmission may be working through the maternal health, education or wealth endowment or through early childhood investment (Cunha and Heckman, 2007). Results presented in section 5.3 are likely to explain some of these channels. Nepalese women exposed to conflict during their childhood development are more likely to have had pregnancy losses and at the same time have more live births (Table 4.7). Although I find no evidence for sexual behavioral changes (Table 4.8), women are less likely to have completed SLC, and most work on their own farms (Table 4.9). Additionally, highly significant in terms of parental ability to invest in children, exposed women have significantly less wealth. Combined with having more children, this drastically decreases parent's ability to invest in children during their critical period of development.

4.6 Concluding Remarks

This paper exploits variation in conflict at a detailed geographical level to establish causality between early childhood exposure to conflict and women's final stature. Additionally, along with Akresh et al. (2018), this paper is among the first to document the intergenerational transmission of the impact of early childhood conflict exposure on second generation health. Nepal experienced a decade-long violent civil conflict between 1996 and 2006, which resulted in more than thirteen thousand fatal casualties, significant infrastructure damages, severe hindrance in delivery of basic services and generated extreme fear, sense of insecurity and stress among its citizens. Considered the most reliable, I use INSEC's database on the conflict casualties and create an individual level victims' data set with exact geographical location (village) and dates of the incidents. This allows me to exploit variation in conflict intensity at the village level for identification.

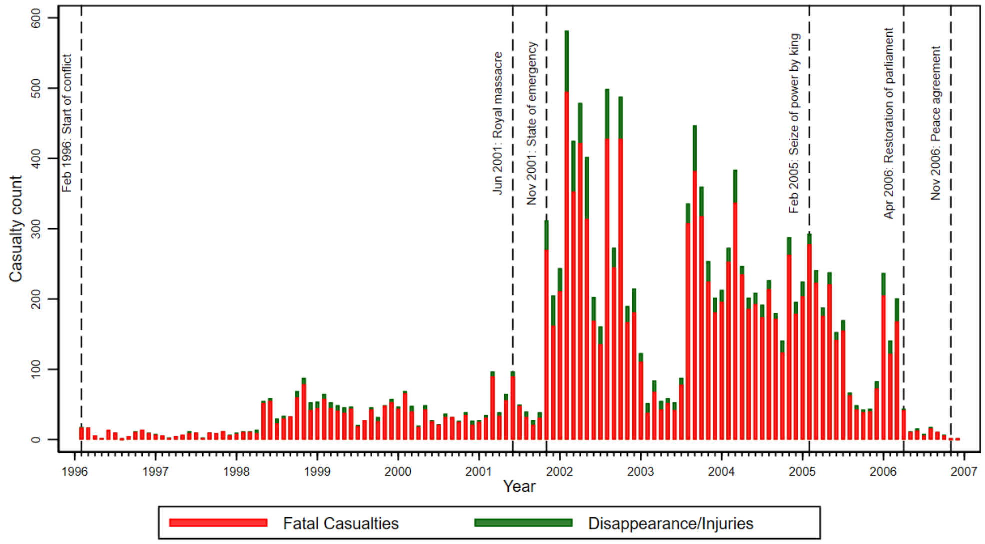
Fueled by international remittances, Nepal enjoyed consistent economic growth and poverty reduction (Uematsu et al., 2016) during the period of conflict. The country also made significant improvement in other dimensions of development including health (Headey and Hoddinott, 2015) and non-income based multidimensional poverty (OPHI, 2013). These aggregate development trends, however, may mask disparities at a more disaggregated level due to significant variation in conflict intensity. The little research documenting the consequences of the war thus far is focused on district-level disaggregation, and results are mixed - little to zero impact on human capital accumulation (Valente, 2014; Pivovarova and Swee, 2015) and positive impact on women employment (Menon and van der Meulen Rodgers, 2015). Libois (2016), on the other hand, using conflict measurement at a more detailed geographical level (distance from the conflict sites) finds significant negative immediate impact on consumption and income. Failure to capture the substantial conflict heterogeneity across villages within district may explain the lack of evidence of conflict effects at the district level.

In contrast, I exploit variation in early childhood exposure to conflict by birth cohort and village of residence to estimate the impact of conflict intensity, as measured by months of war, on adult height. Using the 2016 NDHS women sample, I find that conflict and, in particular, exposure starting very early in one's growing period, has highly significant and negative impact on final women's adult height. Findings are robust across (i) model specifications and (ii) measures of conflict. In validating the difference-in-differences estimation strategy used, I find no evidence of presence of non-parallel dynamics nor of selective migration and fertility. These results aligned with the biomedical literature that early childhood conditions are highly significant in determining final height early life stunting increases the risk of being short as an adult (Golden, 1994; Martorell et al., 1994). Conditions in the fetal period and early years after birth are profound in influencing human biology and long-term health. Responses to lack of adequate nutrition of developing fetus may be coded permanently, which is likely to increase later life health hazards (Barker, 1992). Similarly, early years of life are highly susceptible to environmental influence on height (Schmidt et al., 1995). Nepalese children growing up during the conflict experienced substantial levels of physiological and mental stress, and for many, mostly in rural areas under the control of the Maoists, it was a major nutritional shock and reduced access to health-care. These are likely mechanisms at work and describe the results presented in the paper.

As sufficient time gap between the start of the conflict and the time of the NDHS 2016 allows me to explore the impacts of the conflict on children of the women who were exposed to conflict in their childhood. I find that the mothers' exposure to conflict is detrimental for their children's health. Although imprecise, impacts on children's height-for-age z-scores are negative. Results for children's weight-for-height, weight-for-age and BMI z-scores, on the other hand, are precisely estimated and again negative. Results are robust to alternative measure of conflict intensity (mother's district of birth). To explore possible intergenerational transmission mechanisms, I investigate the impacts on mother's economic and fertility outcomes. I find that women exposed to conflict during childhood have more children and live in a household with significantly less wealth. The combined effect of the two likely result in drastic decreases in the parental ability to invest in children.

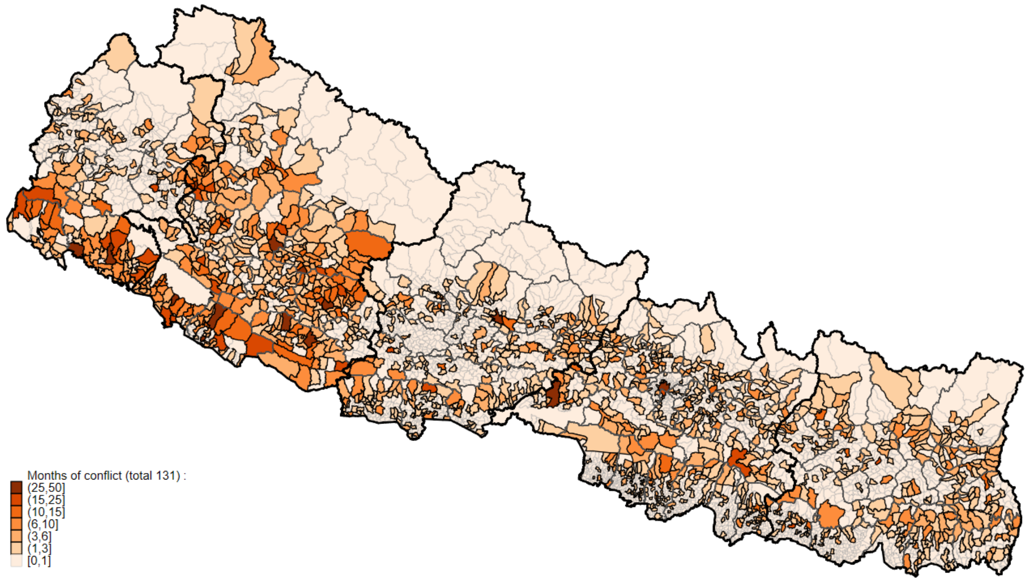
4.7 Figures and Tables

Figure 4.1: Conflict timeline



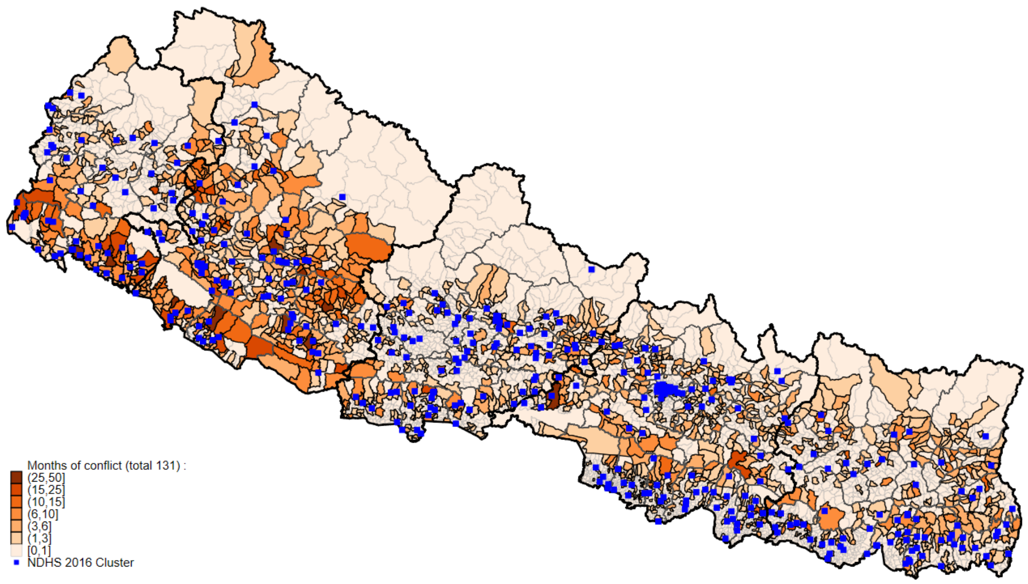
Source: Author's calculation based on the INSEC's archive on the conflict victims.

Figure 4.2: Conflict intensity heterogeneity: Months of war



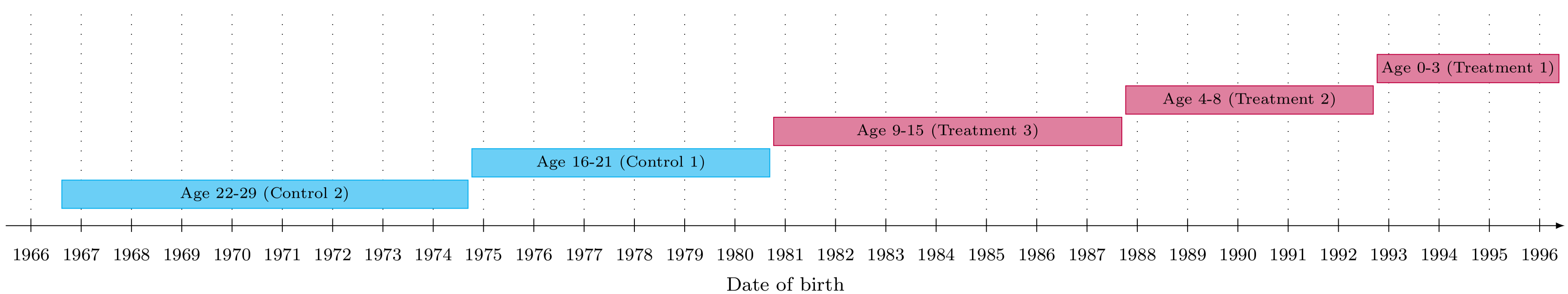
Source: Author's calculation based on the INSEC's archive on the conflict victims.

Figure 4.3: Conflict intensity heterogeneity: Months of war and NDHS 2016 clusters



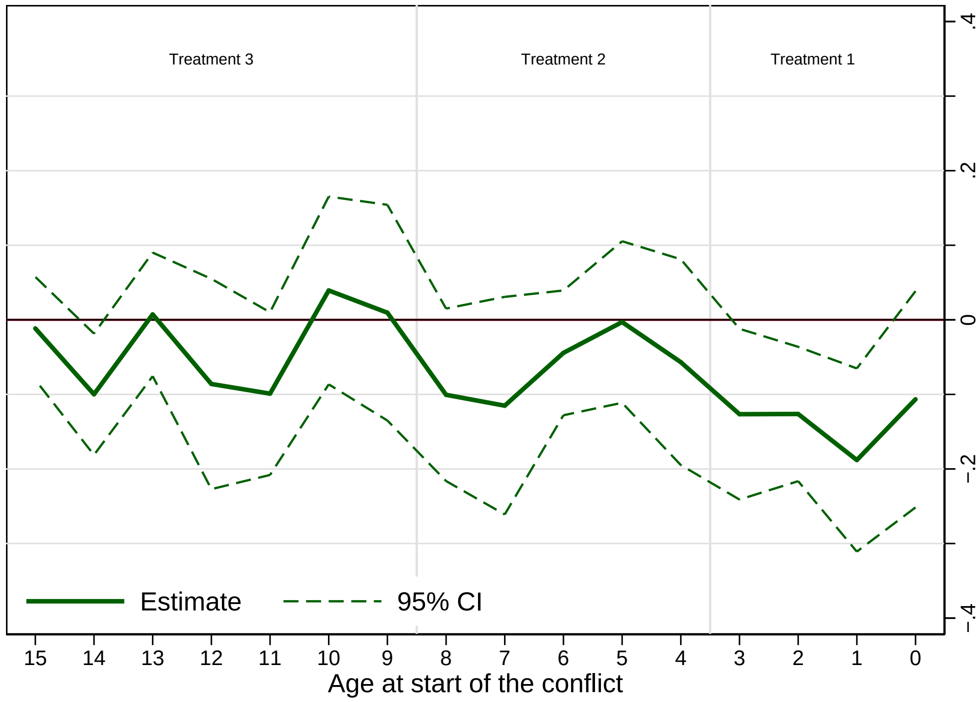
Source: Author's calculation based on the INSEC's archive on the conflict victims.

Figure 4.4: Cohorts by age at the start of the war in 1996



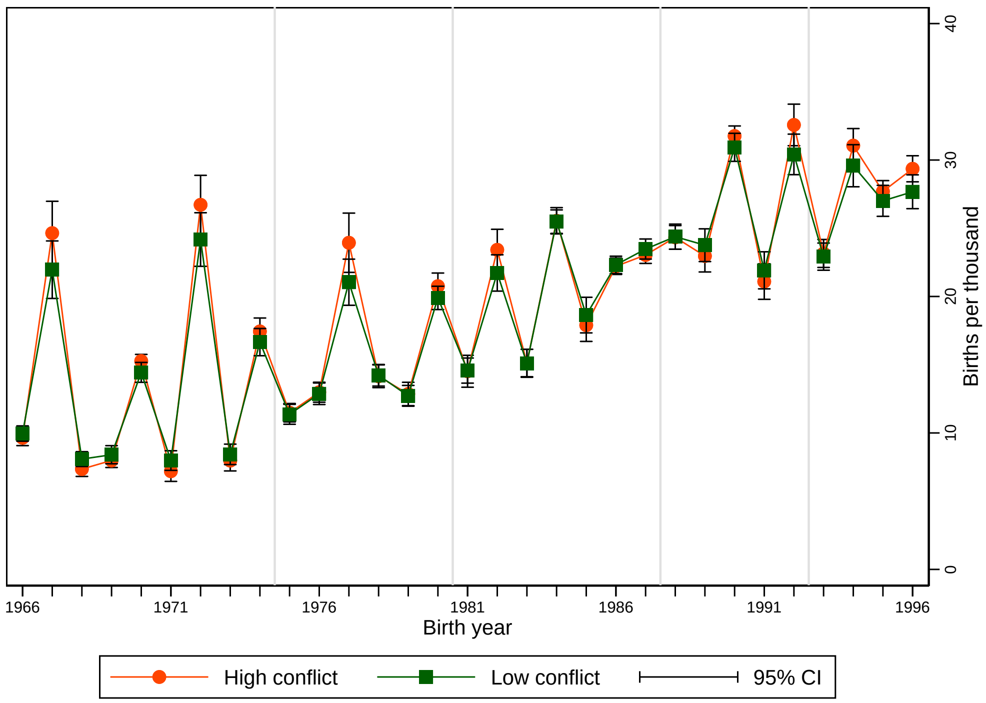
Note: Conflict started on February 13, 1996 or Falgun 1, 2052 Bikram Sambat (11^{th} month of year 2052 according to Nepali calendar). Age in the figure refers to the age at the beginning of the Nepal's civil war.

Figure 4.5: Impact on women's adult height (cm) by age at start of the war



Note: Figure presents coefficients on interaction between exposure to months of war and age at start of the civil war in main specification.

Figure 4.6: Births per 1000 district population using the 2001 Nepal population census



Note: High conflict areas are defined as districts that experienced above median level of conflict intensity (months of war). Birth rates are calculated using individuals observed in the 2001 Nepal population census and their year of birth.

Table 4.1: Characteristics of the victims of civil war in Nepal

Total casualties	14982	Political affiliation (%)	
Killed	13210	Nepali Congress	3.19
Disappeared	998	CPN-UML or ML	1.50
Injuried	774	CPN Maoist (rebel)	48.32
		Other parties	0.91
Perpetrator		No affiliation	46.07
State	9208		
Maoist	5302	Occupation (%)	
Other	472	Agriculture	21.01
		Wage laborer	2.27
Age (mean)	28.34	Employed	1.47
Female (%)	11.10	Teacher	1.68
		Police	11.92
Social caste (%)		Army	6.53
Bramin or Chettrey	44.76	Lawyer	0.05
Janajati, Aadibashi or Dalit	46.82	Doctor	0.04
Madeshi or Muslim	6.25	Politician	43.89
Other	2.17	Social worker	0.16
		Rights activists	0.03
Education (%)		Sports personality	0.05
Bachelors degree or more	2.61	Driver	0.23
Intermediate	7.30	Student	5.50
Secondary school	26.26	Journalist	0.03
Lower secondary school	21.99	Businessman	1.57
Primary school	14.32	Ex-security personnel	0.01
Literate	15.05	Other, not clear	3.56
Illiterate	12.47		

Source: Author's calculation based on the INSEC's archive on the conflict victims (http://www.insec.org.np/victim/).

Note: While classes 8 to 10 are defined as secondary school, 6 and 7 are lower secondary school. Nepali Congress (Democratic) and Nepali Congress are combined as one as the former was formed due to a vertical split of Nepali Congress into two in 2002. However, the parties merged into one in 2007. Similarly, Communist Party of Nepal - Marxist Leninists (CPN-ML) was reunited with the Communist Party of Nepal - Unified Marxist Leninists (CPN-UML) in 2002 but few members refused to go along the merger forming a new party with the same name. Party's sister organizations and student wings are also accounted for while assigning party affiliation.

Table 4.2: Summary statistics of women aged 20 to 49 at time of the survey

Variables	Age at start of the war in 1996 0 to 29 (All women)	 0 to 15 (Treatment)	 16 to 29 (Control)	Difference
Panel A: Outcomes				
Height (cm)	151.56	151.75	151.28	0.46***
	[5.50]	[5.45]	[5.56]	(0.17)
Weight (kg)	52.04	51.36	53.05	-1.69***
	[10.05]	[9.52]	[10.73]	(0.31)
Body mass index	22.63	22.29	23.14	-0.85***
	[4.06]	[3.86]	[4.30]	(0.12)
Pregnancy loss (yes=1)	0.28	0.27	0.30	-0.04***
	[0.45]	[0.44]	[0.46]	(0.01)
Total births	2.88	2.25	3.81	-1.56***
	[1.65]	[1.20]	[1.77]	(0.04)
Age at first birth	19.74	19.54	20.04	-0.51***
	[3.19]	[2.99]	[3.45]	(0.10)
Years of education	3.83	5.05	2.04	3.01***
	[4.25]	[4.30]	[3.48]	(0.12)
Employed last 12 months (yes=1)	0.72	0.69	0.76	-0.07***
	[0.45]	[0.46]	[0.43]	(0.01)
Wealth index factor score	3879.78	3274.44	4776.24	-1501.79
	[96436.27]	[93601.64]	[100507.35]	(2957.08)
Panel B: Exposure to conflict				
B1: During 0-15 years				
Months of war	1.72	2.87	0.00	2.87***
	[4.63]	[5.71]	[0.00]	(0.14)
Number of casualties	3.12	5.22	0.00	5.22***
	[10.76]	[13.53]	[0.00]	(0.32)
Months inc. neighboring villages	6.22	10.43	0.00	10.43***
	[11.08]	[12.73]	[0.00]	(0.30)
B2: Whole life				
Months of war	5.45	5.49	5.39	0.10
	[8.27]	[8.28]	[8.26]	(0.25)
Number of casualties	10.41	10.55	10.20	0.35
	[20.85]	[21.01]	[20.60]	(0.64)
Months inc. neighboring villages	18.67	18.71	18.61	0.10
	[14.42]	[14.40]	[14.45]	(0.44)
Panel C: Controls				
Current age	33.48	27.85	41.83	-13.99***
	[8.07]	[4.45]	[3.93]	(0.13)
Female headed HH (yes=1)	0.34	0.37	0.30	0.07***
	[0.47]	[0.48]	[0.46]	(0.01)
Hight caste (yes=1)	0.39	0.37	0.41	-0.04***
	[0.49]	[0.48]	[0.49]	(0.01)
Rural (yes =1)	0.86	0.86	0.86	0.00
	[0.35]	[0.34]	[0.35]	(0.01)
Eastern region	0.19	0.18	0.21	-0.02**
	[0.39]	[0.39]	[0.40]	(0.01)
Central region	0.24	0.24	0.25	-0.00
	[0.43]	[0.43]	[0.43]	(0.01)
Western region	0.21	0.21	0.21	-0.00
	[0.41]	[0.41]	[0.41]	(0.01)
Mid-western region	0.21	0.22	0.20	0.02
	[0.41]	[0.42]	[0.40]	(0.01)
Far-western region	0.14	0.14	0.13	0.01
	[0.34]	[0.35]	[0.34]	(0.01)
Number of women	4,421	2,639	1,782	4,421

Note: Standard deviations are in brackets and standard errors are in parentheses and significance levels are denoted as follows: * p<0.10, ** p<0.05, *** p<0.01.

Table 4.3: Summary statistics of children under 5 at time of the survey in 2016

Variables	0 to 29 (All women)	0 to 15 (Treatment)	16 to 29 (Control)	Difference
	Mother's age at start of the war in 1996			
Panel A: Outcomes				
Height/age sd	-1.55	-1.52	-1.85	0.33***
	[1.34]	[1.33]	[1.39]	(0.11)
Weight/age sd	-1.35	-1.33	-1.50	0.17*
	[1.07]	[1.06]	[1.16]	(0.09)
Weight/height sd	-0.65	-0.65	-0.62	-0.03
	[1.10]	[1.09]	[1.15]	(0.09)
Body mass index sd	-0.51	-0.51	-0.45	-0.06
	[1.11]	[1.11]	[1.14]	(0.09)
Panel B: Mother's exposure to conflict				
B1: During 0-15 years				
Months of war	3.17	3.43	0.00	3.43***
	[6.01]	[6.17]	[0.00]	(0.49)
Number of casualties	5.83	6.29	0.00	6.29***
	[14.28]	[14.74]	[0.00]	(1.16)
Months inc. neighboring villages	11.79	12.73	0.00	12.73***
	[13.10]	[13.16]	[0.00]	(1.04)
B2: Whole life				
Months of war	4.69	4.73	4.10	0.63
	[7.42]	[7.41]	[7.64]	(0.61)
Number of casualties	8.85	8.92	8.09	0.82
	[18.74]	[18.55]	[20.92]	(1.53)
Months inc. neighboring villages	17.15	17.20	16.54	0.66
	[13.73]	[13.73]	[13.83]	(1.12)
Panel C: Controls				
Child sex (girl=1)	0.47	0.48	0.42	0.06
	[0.50]	[0.50]	[0.49]	(0.04)
Child birth order number	2.36	2.16	4.97	-2.81***
	[1.57]	[1.25]	[2.53]	(0.11)
Child is a twin (yes =1)	0.01	0.01	0.02	-0.01
	[0.11]	[0.11]	[0.14]	(0.01)
Female headed HH (yes=1)	0.33	0.33	0.30	0.03
	[0.47]	[0.47]	[0.46]	(0.04)
Hight caste (yes=1)	0.36	0.37	0.32	0.04
	[0.48]	[0.48]	[0.47]	(0.04)
Rural (yes =1)	0.89	0.89	0.93	-0.03
	[0.31]	[0.31]	[0.26]	(0.03)
Eastern region	0.18	0.18	0.24	-0.06*
	[0.39]	[0.38]	[0.43]	(0.03)
Central region	0.27	0.27	0.28	-0.01
	[0.44]	[0.44]	[0.45]	(0.04)
Western region	0.19	0.20	0.16	0.04
	[0.40]	[0.40]	[0.37]	(0.03)
Mid-western region	0.22	0.22	0.22	0.00
	[0.42]	[0.42]	[0.41]	(0.03)
Far-western region	0.13	0.14	0.11	0.03
	[0.34]	[0.34]	[0.31]	(0.03)
Number of children	2,168	2,007	161	2,168

Note: Standard deviations are in brackets and standard errors are in parentheses and significance levels are denoted as follows: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 4.4: Impact on first generation adult stature by age at start of the civil war

	Height in cm			Height for age sd (HAZ)		
	(1)	(2)	(3)	(4)	(5)	(6)
Age 22 to 29 × Months of war	0.011	0.011	0.014	0.002	0.002	0.002
	(0.033)	(0.034)	(0.035)	(0.006)	(0.006)	(0.006)
Age 9 to 15 × Months of war	-0.026	-0.026	-0.027	-0.004	-0.004	-0.005
	(0.026)	(0.026)	(0.025)	(0.004)	(0.004)	(0.004)
Age 4 to 8 × Months of war	-0.068**	-0.067**	-0.071**	-0.012**	-0.011**	-0.012**
	(0.031)	(0.031)	(0.032)	(0.005)	(0.005)	(0.005)
Age0 to 3 × Months of war	-0.122***	-0.127***	-0.136***	-0.021***	-0.021***	-0.023***
	(0.033)	(0.032)	(0.032)	(0.006)	(0.005)	(0.005)
Observations	4,421	4,421	4,421	4,418	4,418	4,418
Adjusted R-squared	0.031	0.031	0.043	0.031	0.031	0.043
Birth year and month fixed effects		Yes	Yes		Yes	Yes
Regional trends and other controls			Yes			Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	383	383	383	383	383	383
Control mean (Age 16 to 21)	151.4	151.4	151.4	151.4	151.4	151.4

Note: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference specification. Standard errors are clustered at village level. All ages in the table refer to age at the start of the war. Comparison cohort is age 16 to 21 and cohort 22 to 29 is a second comparison group that serves as a placebo test. Other controls include an indicator for high caste and month of interview fixed-effects.

Table 4.5: Impact on first generation adult height (cm) by alternative measure of conflict

	Casualty count	Including contiguous villages		Including villages within 50 km	
Conflict variable =	(1) Own village	(2) Months of war	(3) Casualty count	(4) Months of war	(5) Casualty count
Age 22 to 29 × Conflict	0.005	0.022	0.004	0.011	0.001
	(0.014)	(0.022)	(0.006)	(0.016)	(0.001)
Age 9 to 15 × Conflict	-0.013	-0.006	-0.002	-0.011	0.000
	(0.009)	(0.017)	(0.006)	(0.013)	(0.001)
Age 4 to 8 × Conflict	-0.030***	-0.013	-0.006	-0.006	0.000
	(0.011)	(0.020)	(0.006)	(0.016)	(0.001)
Age 0 to 3 × Conflict	-0.050***	-0.068***	-0.019***	-0.036*	-0.001
	(0.012)	(0.023)	(0.007)	(0.020)	(0.001)
Observations	4,421	4,421	4,421	4,421	4,421
Adjusted R-squared	0.042	0.042	0.042	0.040	0.040
Birth year and month fixed effects	Yes	Yes	Yes	Yes	Yes
Regional trends and other controls	Yes	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes
Number of clusters	383	383	383	383	383
Control mean (Age 16 to 21)	151.4	151.4	151.4	151.4	151.4

Note: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference specification. Standard errors are clustered at village level. All ages in the table refer to age at the start of the war. Comparison cohort is age 16 to 21 and cohort 22 to 29 is a second comparison group that serves as a placebo test. Other controls include an indicator for high caste and month of interview fixed-effects.

Table 4.6: Impact of conflict at district of birth

	Migration			Height in cm			Height for age sd (HAZ)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Age 22 to 29 × Months of war	0.001	0.001	0.001	-0.008	-0.007	-0.006	-0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.019)	(0.019)	(0.018)	(0.003)	(0.003)	(0.003)
Age 9 to 15 × Months of war	-0.001	-0.001	-0.001	-0.010	-0.008	-0.007	-0.002	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.012)	(0.011)	(0.011)	(0.002)	(0.002)	(0.002)
Age 4 to 8 × Months of war	-0.002	-0.001	-0.001	0.001	0.004	0.001	0.000	0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.013)	(0.013)	(0.012)	(0.002)	(0.002)	(0.002)
Age 0 to 3 × Months of war	-0.001	-0.001	-0.001	-0.030**	-0.030**	-0.030**	-0.005**	-0.005**	-0.005**
	(0.001)	(0.001)	(0.001)	(0.012)	(0.013)	(0.013)	(0.002)	(0.002)	(0.002)
Observations	4,421	4,421	4,421	4,421	4,421	4,421	4,418	4,418	4,418
Adjusted R-squared	0.064	0.066	0.123	0.008	0.008	0.019	0.008	0.007	0.019
Birth year and month fixed effects		Yes	Yes		Yes	Yes		Yes	Yes
Regional trends and other controls			Yes			Yes			Yes
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	74	74	74	74	74	74	74	74	74
Control mean (Age 16 to 21	0.253	0.253	0.253	151.4	151.4	151.4	-2.056	-2.056	-2.056

Note: Conflict is defined at district level i.e. district of birth. *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference specification. Standard errors are clustered at the district of birth. All ages in the table refer to age at the start of the war. Comparison cohort is age 16 to 21 and cohort 22 to 29 is a second comparison group that serves as a placebo test. Other controls include an indicator for high caste and month of interview fixed-effects.

Table 4.7: Impact on first generation reproductive health by age at start of the civil war

	(1) Ever had a stillbirth	(2) Ever had a miscarriage	(3) Ever had an abortion	(4) Total live births	(5) Number of infant deaths
Age 22 to 29 × Months of war	0.002	0.001	-0.007**	-0.004	-0.000
	(0.001)	(0.002)	(0.003)	(0.008)	(0.001)
Age 9 to 15 × Months of war	0.001	0.000	-0.004	0.002	0.000
	(0.001)	(0.002)	(0.002)	(0.006)	(0.001)
Age 4 to 8 × Months of war	0.002	0.001	-0.005*	0.013*	0.000
	(0.001)	(0.002)	(0.003)	(0.007)	(0.001)
Age0 to 3 × Months of war	0.003***	0.006*	-0.002	0.024***	-0.001
	(0.001)	(0.003)	(0.004)	(0.007)	(0.002)
Observations	4,421	4,421	4,421	4,421	4,421
Adjusted R-squared	0.022	0.018	0.061	0.466	0.003
Birth year and month fixed effects	Yes	Yes	Yes	Yes	Yes
Regional trends and other controls	Yes	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes
Number of clusters	383	383	383	383	383
Control mean (Age 16 to 21)	0.0764	0.164	0.142	3.520	0.003

Note: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference specification. Standard errors are clustered at village level. All ages in the table refer to age at the start of the war. Comparison cohort is age 16 to 21 and cohort 22 to 29 is a second comparison group that serves as a placebo test. Other controls include an indicator for high caste and month of interview fixed-effects.

Table 4.8: Impact on sexual behavior and marriage by age at start of the civil war

	(1) Ever use contraceptive	(2) Number of sex partners	(3) Age at first sexual intercourse	(4) Age at first birth	(5) Age at first cohabitation	(6) Number of marriages or unions	(7) Smoke or chew tobacco
Age 22 to 29 × Months of war	-0.001	-0.003	-0.026	-0.030*	0.001	-0.020	-0.005**
	(0.002)	(0.003)	(0.019)	(0.017)	(0.002)	(0.020)	(0.002)
Age 9 to 15 × Months of war	-0.001	0.001	0.018	0.016	0.000	0.032*	-0.002
	(0.001)	(0.001)	(0.015)	(0.016)	(0.001)	(0.018)	(0.002)
Age 4 to 8 × Months of war	-0.002	-0.003	0.012	0.009	0.000	0.024	-0.001
	(0.002)	(0.003)	(0.019)	(0.020)	(0.001)	(0.020)	(0.002)
Age0 to 3 × Months of war	0.001	0.000	-0.028	-0.009	0.000	0.002	0.000
	(0.003)	(0.001)	(0.025)	(0.023)	(0.001)	(0.021)	(0.002)
Observations	4,421	4,421	4,421	4,420	4,420	4,421	4,421
Adjusted R-squared	0.110	-0.013	0.144	0.146	0.039	0.108	0.155
Birth year and month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional trends and other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	383	383	383	383	383	383	383
Control mean (Age 16 to 21)	0.872	1.065	17.56	17.50	1.059	19.89	0.195

Note: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference specification. Standard errors are clustered at village level. All ages in the table refer to age at the start of the war. Comparison cohort is age 16 to 21 and cohort 22 to 29 is a second comparison group that serves as a placebo test. Other controls include an indicator for high caste and month of interview fixed-effects.

Table 4.9: Impact on education, employment and wealth by age at start of the civil war

	Education		Employment				
	(1) Years of schooling	(2) Completed SLC	(3) Employed in last 12 months	(4) Professional sales clerical etc	(5) Agri - own farm	(6) manual work	(7) wealth index factor score
Age 22 to 29 × Months of war	-0.015	-0.003	-0.005	-0.007**	0.006**	0.001	-182.432
	(0.025)	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(440.164)
Age 9 to 15 × Months of war	0.003	-0.001	-0.003	-0.001	0.004**	-0.003	-504.890
	(0.018)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(323.558)
Age 4 to 8 × Months of war	0.007	-0.003	0.001	-0.005*	0.004*	0.002	-675.200*
	(0.025)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(367.459)
Age0 to 3 × Months of war	-0.020	-0.006**	-0.002	-0.004	0.005*	-0.001	-977.280**
	(0.031)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(393.707)
Observations	4,421	4,421	4,421	3,188	3,188	3,188	4,421
Adjusted R-squared	0.440	0.215	0.259	0.300	0.382	0.165	0.712
Birth year and month fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional trends and other controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	383	383	383	381	381	381	383
Control mean (Age 16 to 21)	2.472	0.0831	0.765	0.200	0.722	0.0781	3484

Note: School leaving certificate (SLC) is a national exam that everyone takes at the end of grade ten. DHS uses asset ownership such as televisions and bicycles, materials used for housing construction, and types of water access and sanitation facilities to calculate wealth index using factor analysis (see Rutstein, and Johnson, 2004 for detail). *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference specification. Standard errors are clustered at village level. All ages in the table refer to age at the start of the war. Comparison cohort is age 16 to 21 and cohort 22 to 29 is a second comparison group that serves as a placebo test. Other controls include an indicator for high caste and month of interview fixed-effects.

Table 4.10: Second generation health impact

	(1) Height for age sd (HAZ)	(2) Weight for height sd (WHZ)	(3) Weight for age sd (WAZ)	(4) Body mass index sd (BMIZ)
Mother's age 22 to 29 ×	0.048	-0.032	0.005	-0.045
Mother's exposure to conflict	(0.046)	(0.032)	(0.040)	(0.028)
Mother's age 9 to 15 ×	-0.010	-0.041**	-0.034*	-0.044***
Mother's exposure to conflict	(0.033)	(0.017)	(0.020)	(0.015)
Mother's age 4 to 8 ×	-0.005	-0.039**	-0.025	-0.040**
Mother's exposure to conflict	(0.030)	(0.017)	(0.022)	(0.017)
Mother's age 0 to 3 ×	-0.011	-0.030**	-0.025	-0.031**
Mother's exposure to conflict	(0.027)	(0.015)	(0.020)	(0.014)
Observations	2,165	2,163	2,169	2,164
Adjusted R-squared	0.131	0.114	0.132	0.110
Mother and other controls	Yes	Yes	Yes	Yes
Children contrls	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes
Number of clusters	373	373	373	373
Control mean (Mother's age 16 to 21)	-1.836	-0.580	-1.472	-0.423

Note: Sample is children aged 0 to 59 months at the time of the survey. *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference specification. Standard errors are clustered at village level. Mother's age in the table refers to mother's age at the start of the war. Comparison cohort is children born to mothers whose age was 16 to 21 at the start of the war. Children born to mother's cohort 22 to 29 is a second comparison group that serves as a placebo test. Mother's controls are mother's years of birth fixed effects, mother's month of birth fixed effects, region specific trends. Household controls are indicator for high caste, female headed households, and whether residing in a rural area. Child controls are indicator if child is a girl, a twin, birth order fixed effect, and fixed effects for child years of birth, month of birth, and month of anthropometric measurements. Reported outcomes are z-scores based on the WHO anthropometric measurement standards.

Table 4.11: Second generation health impact by mother's district of birth

	(1) Height for age sd	(2) Weight for height sd	(3) Weight for age sd	(4) Body mass index sd
Mother's age 22 to 29 ×	0.016	-0.007	-0.001	-0.011
Mother's exposure to conflict	(0.013)	(0.008)	(0.009)	(0.008)
Mother's age 9 to 15 ×	-0.006	-0.014**	-0.016**	-0.015**
Mother's exposure to conflict	(0.007)	(0.006)	(0.006)	(0.006)
Mother's age 4 to 8 ×	-0.003	-0.013**	-0.012**	-0.015**
Mother's exposure to conflict	(0.006)	(0.006)	(0.006)	(0.006)
Mother's age 0 to 3 ×	-0.003	-0.013**	-0.012**	-0.016***
Mother's exposure to conflict	(0.006)	(0.006)	(0.005)	(0.005)
Observations	2,165	2,163	2,169	2,164
Adjusted R-squared	0.181	0.094	0.130	0.095
Mother and household controls	Yes	Yes	Yes	Yes
Children controls	Yes	Yes	Yes	Yes
Mother's district of birth fixed effects	Yes	Yes	Yes	Yes
Number of clusters	74	74	74	74
Control mean (Age 16 to 21)	-1.836	-0.580	-1.472	-0.423

Note: Conflict is defined at district level i.e. mother's district of birth. Sample is children aged 0 to 59 months at the time of the survey. *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference specification. Standard errors are clustered at village level. Mother's age in the table refers to mother's age at the start of the war. Comparison cohort is children born to mothers whose age was 16 to 21 at the start of the war. Children born to mother's cohort 22 to 29 is a second comparison group that serves as a placebo test. Mother's controls are mother's years of birth fixed effects, mother's month of birth fixed effects, region specific trends. Household controls are indicator for high caste, female headed households, and whether residing in a rural area. Child controls are indicator if child is a girl, a twin, birth order fixed effect, and fixed effects for child years of birth, month of birth, and month of anthropometric measurements. Reported outcomes are z-scores based on the WHO anthropometric measurement standards.

CHAPTER 5

CONCLUSION

In this dissertation, I present research on three topics in development economics, with overarching theme being the long-term implications of positive and negative shocks on rural poor's economic wellbeing. First paper of this dissertation implements a quantifiable measure of household resilience and demonstrates its application and relevance in the context of an impact evaluation. Results from the impact evaluation find that a one-off transfer of assets and training increased household development resilience; the intervention shifted the conditional transition distribution of households' asset holdings upward, increasing expected asset holdings and decreasing conditional variance. Findings demonstrate that attention to conditional variance in impact on assets provides important insights into program effectiveness and persistence of estimated effects.

Resilience as a household outcome offers important advantages for impact analysis. Because it is based on the full distribution of household welfare, the development resilience measure provides a more complete picture of intervention impacts, yielding insights into household capacity to avoid falling into poverty in the foreseeable future. In particular, estimation of the conditional moment functions allows for nonlinear persistence, which can improve forecasting of households' future states. In addition, the conditional moment functions make it possible to distinguish whether estimated effects are primarily attributable to changes in the conditional mean or the conditional variance. These inferences are especially significant for households at or near the poverty threshold. Our finding that a substantial share of households in the analysis are asset non-poor and yet not resilient illustrates this point. Resilience measurement yields policy relevant insights into household well-being that conventional measures like poverty headcount miss.

Resilience estimation results suggest that the multifaceted approach focused on improving well-being through transfers, decreasing downside risk, and changing underlying structural barriers to economic progress, can have lasting impact on households' ability to accumulate and retain productive assets and to withstand covariate and idiosyncratic shocks. We argue, moreover, that resilience theory can guide development practitioners in the design and evaluation of future anti-poverty programs. Our findings suggest that standard impact evaluation measurements are insufficient to establish households resilience against future poverty spells and should be complemented, where possible, by estimation and evaluation of higher moments of the household welfare distribution.

The second paper uses a unique source of nationally representative data during the period that Nepal experienced a boom in outmigration that allows one to explore the impact of migration on labor supply of the left-behind household members both on extensive and intensive margins for wage-employment, self-employment, and household activities. The paper also provides an answer to the question, what are the remaining members in the households engage in instead of work employment?

The paper finds that having international migrants in the family discourage members staying behind from participation in wage-employment. This is true for both male and female members. However, female members increase participation in self-employment, almost entirely through subsistence farming. The paper also finds that both self-employed and wage-employed adults decrease weekly hours of labor supply and only women in the migrant-sending household increase time in household activities. Findings presented in this chapter suggest that male-dominated migration forces women to realign priorities and reallocate time from market-work to farming and household activities. In contrast, because of the income transfers, men now value their leisure more and decrease their overall labor supply. These are reasonable findings in a country where the traditional household norms and social culture see women as subordinate to men.

In the third paper, in addition to presenting long-term health effects on first generation, I show that the impact of exposure to violent conflict during childhood persist into second generation. I exploit variation in conflict at a detailed geographical level to establish causality between early childhood exposure to conflict and women's final stature. Additionally, this study is among the first to document the intergenerational transmission of the impact of early childhood conflict exposure on second generation health. Nepal experienced a decade-long violent civil conflict between 1996 and 2006, which resulted in more than thirteen thousand fatal casualties, significant infrastructure damages, severe hindrance in delivery of basic services and generated extreme fear, sense of insecurity and stress among its citizens. Using the variation in early childhood exposure to conflict by birth cohort and village of residence and the 2016 NDHS women sample, I find that conflict and, in particular, exposure starting very early in ones growing period, has highly significant and negative impact on final womens adult height. Findings are robust across (i) model specifications and (ii) measures of conflict. In validating the difference-in-differences estimation strategy used, I find no evidence of presence of non-parallel dynamics nor of selective migration and fertility. These results aligned well with the biomedical literature that early childhood conditions are highly significant in determining final height early life stunting increases the risk of being short as an adult.

As sufficient time gap between the start of the conflict and the time of the NDHS 2016 allows me to explore the impacts of the conflict on children of the women who were exposed to conflict in their childhood. I find that the mothers' exposure to conflict is detrimental for their children's health, in particular, children's weight-for-height, weight-for-age and BMI z-scores. Results are robust to alternative measure of conflict intensity (mother's district of birth). To explore possible intergenerational transmission mechanisms, I investigate the impacts on mother's economic and fertility outcomes. I find that women exposed to conflict during childhood have more children and live in a household with significantly less wealth. The combined effect of the two likely result in drastic decreases in the parental ability to invest in children.

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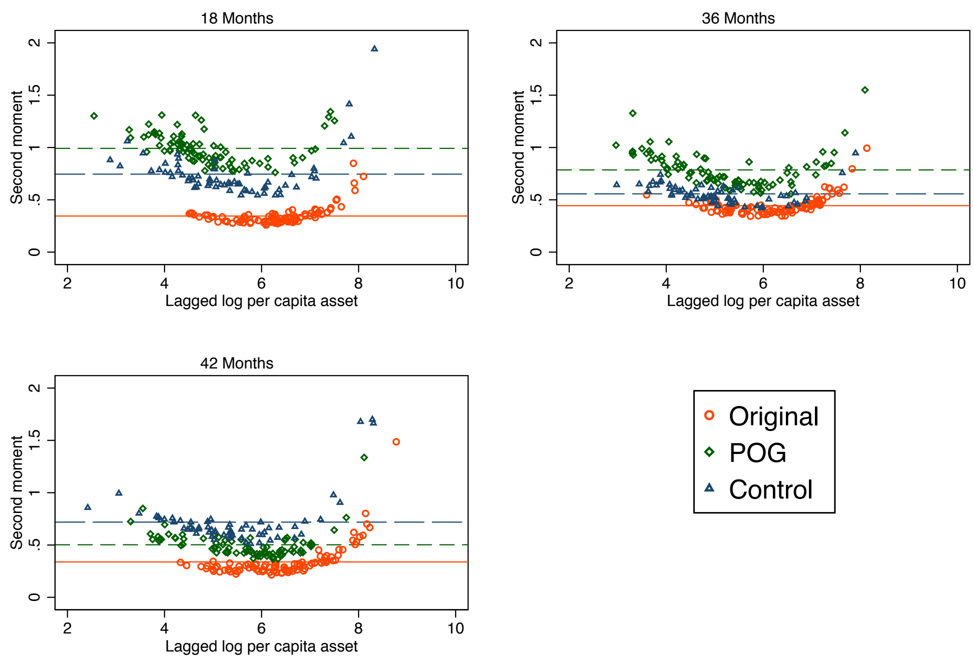
APPENDIX A

SUPPLEMENTAL MATERIALS FOR CHAPTER 2

A.1 Additional Tables and Figure

Figure A.1: Distribution of second moment by treatment and time



Note: Horizontal lines represent group means.

Table A.1: Resilience vs asset poverty - difference-in-difference results

	(1) Resilient ($\hat{\rho}_{it} > 0.5$)	(2) Asset Non-poor
Time 1 Original (18 months post treatment)	0.596***	0.470***
	(0.082)	(0.088)
Time 1 POG (18 months post treatment)	0.007	0.243***
	(0.074)	(0.082)
Time 2 Original (42 months post treatment)	0.527***	0.390***
	(0.087)	(0.091)
Time 2 POG (42 months post treatment)	0.282***	0.384***
	(0.098)	(0.087)
Baseline mean	–	0.302
Time 2 impact: % change Original	–	129.1
Time 1 impact = Time 2 impact [p-value]	0.463	0.277
Adjusted R-squared	–	0.216
Observations	741	741

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level.

Table A.2: Treatment effects on poverty, food Security, and asset poverty - Robustness check using nonlinear estimation

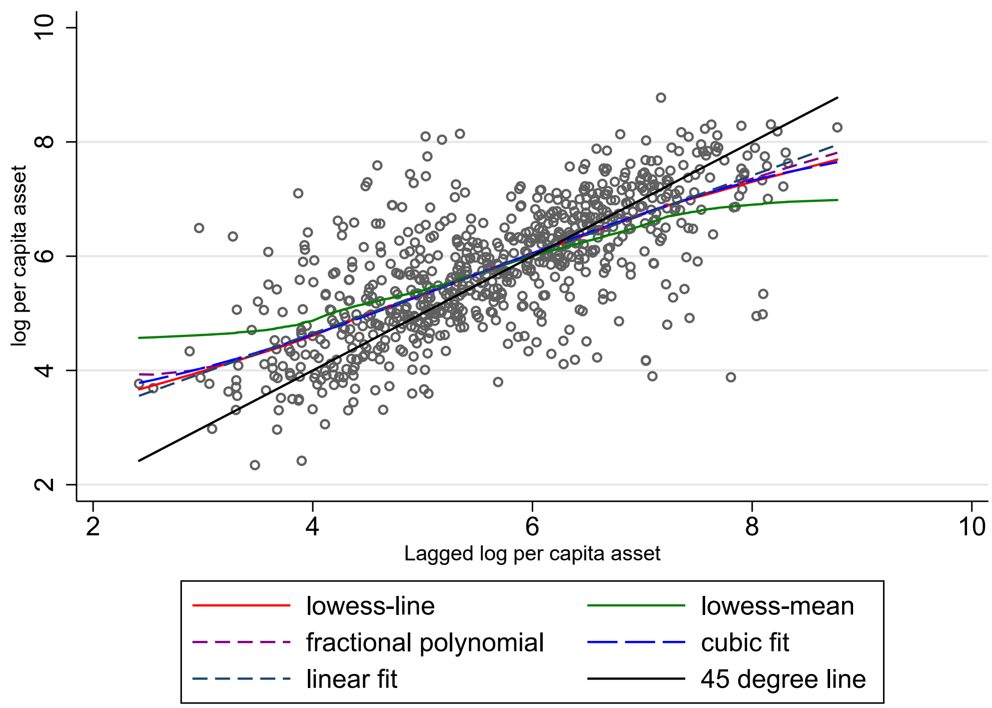
	(1) Below Poverty Line	(2) Enough Food	(3) Asset Non-poor
Time 1 Original (12 months post treatment)	-0.218**	0.181***	0.468***
	(0.094)	(0.066)	(0.086)
Time 1 POG (12 months post treatment)	-0.033	0.111*	0.246***
	(0.095)	(0.064)	(0.084)
Time 2 Original (36 months post treatment)	-0.316***	0.213***	0.384***
	(0.094)	(0.068)	(0.088)
Time 2 POG (36 months post treatment)	-0.066	0.154**	0.383***
	(0.095)	(0.066)	(0.089)
Observations	741	741	741

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Reported are average marginal effects using logistic regressions. Time 1 and time 2 refer to 12 and 36 months post-intervention except in column 3, where they refer to 18 and 42 months post-intervention. In Column 1, the poverty line threshold used is USD 1.90 PPP per person per day, as measured in 2012 prices. Column 2 is an indicator variable for subjective food security, which takes the value of 1 if the survey respondent report the household usually or always has enough food to feed all the members. Asset non-poor in column 3 is an indicator variable that takes the value of 1 if total household asset value is above 308 USD PPP per person, 0 otherwise. The asset poverty threshold calculation is discussed in Appendix A.2.2.

A.2 Path Dynamics and Asset Threshold

A.2.1 Well-being path dynamics and treatment (First Stage)

Figure A.2: Asset dynamics



Notes: Asset includes livestock, bicycle, radio, television, solar panel, motorbike, bed, hoes, sickle, shovel, slasher, pangas, mortar, sieve, wheel barrow, sprayer, maize sheller, grain mill, oil press, axe, ox yoke, ox plough, ox cart, livestock shed, feeder, chaff cutter, fencing, milking buckets and chairs, salt/mineral feeder and ripper/cultivator.

In order to choose the optimal functional form for the polynomial of lagged well-being we use AIC, BIC, Log likelihood criteria and the LR test. Figure A.2 presents different fits of the asset holding at time t on its lagged. The cubic fit and locally weighted regression (Lowess smoothing) of asset values on lagged values are very similar. From above tests (not shown) and the graph, we choose cubic ($k = 3$) as our preferred functional form.

$$y_{it} = \alpha + \sum_{j=1}^{3} \theta_j y_{i,t-1}^j + \lambda_t + \delta D_{it} + \beta X_{it} + \epsilon_{it} \tag{A.1}$$

$$y_{it} = \alpha + \sum_{j=1}^{3} \theta_j y_{it}^j + \sum_{j=1}^{3} \phi_j D_{it} \times y_{i,t-1} + \lambda_t + \delta D_{it} + \beta X_{it} + \epsilon_{it} \tag{A.2}$$

We estimate Equation (A.1) and test if the cubic lagged term (θ_3) is significant, which provides the evidence of a dynamic asset growth to be S-shaped. In order to examine whether the treatment has altered the path dynamics we estimate Equation (A.2) and perform following tests:

$$H_0 : \phi_1 = \phi_2 = \phi_3 = 0 \tag{A.3}$$

$$H_0 : \phi_2 = \phi_3 = \delta = 0 \tag{A.4}$$

$$H_0 : \delta = 0 \tag{A.5}$$

Hypothesis (A.3) tests whether the treatment has altered the rate of change of the curvature. Hypotheses (A.4) and (A.5) test if the treatment shifted the growth curve horizontally or vertically respectively.

A.2.2 Transforming consumption threshold to asset threshold

We map the income/consumption poverty line, above which one is considered non-poor, to asset levels and create an asset base threshold as below:

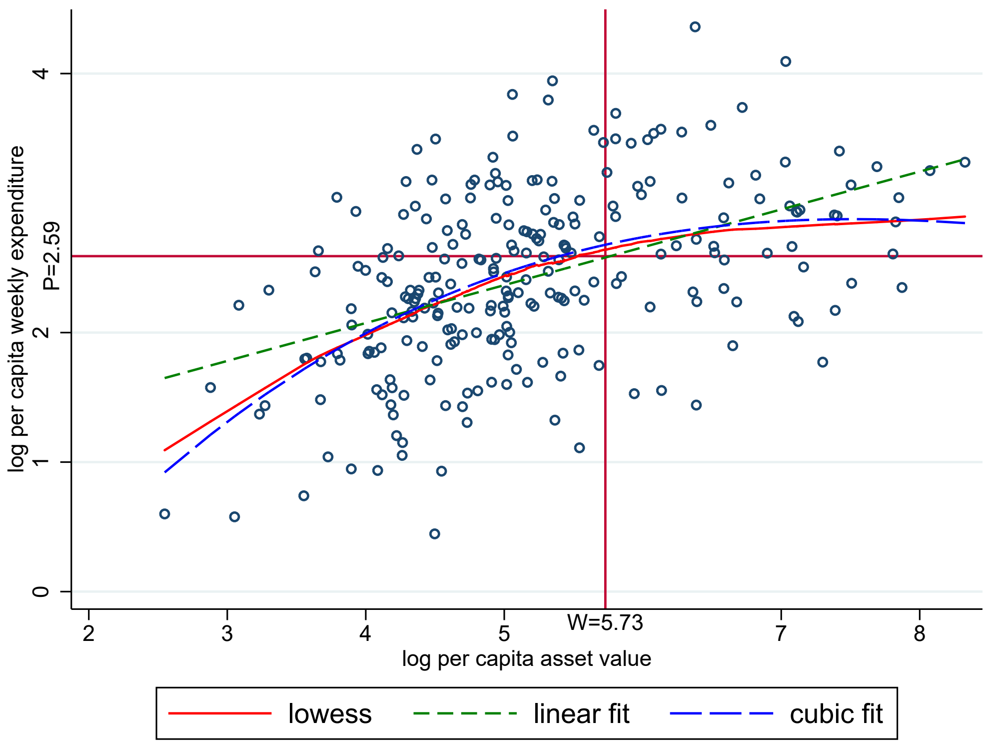
$$Log(C_{it}) = \alpha + \gamma Log(W_{it}) + \beta X_{it} + \epsilon_{it} \tag{A.6}$$

where, C_{it} is per capita per day consumption of household i at time t, W_{it} is per capita value of total asset at time t of household i and X_{it} is vector of controls affecting household's consumption. We limit analysis to baseline data only. We subtract the value of asset transfer made to households at the baseline and estimate (A.6) using OLS. Using the estimated coefficients and median characteristics of the sample, we map 1.90 USD PPP ($\bar{P}$) consumption to household per capita asset level as:

$$Log(\bar{W}) = \frac{Log(\bar{P}) - \hat{\alpha} - \hat{\beta} X_m}{\hat{\gamma}} \tag{A.7}$$

where $\bar{P}$ is a consumption poverty line. The hat, $\hat{()}$, caret refers to estimated coefficients, m subscripts represents the median value of the sample and $\bar{W}$ is the asset threshold, below which households will be considered vulnerable to poverty. Figure A.3 presents the consumption poverty line mapping to asset threshold using baseline data. As shown in Figure A.3 the asset poverty threshold in natural log is 5.73 ($= Log(\bar{W}) \implies \bar{W} = \exp 5.73 \approx$ 308 USD PPP).

Figure A.3: Asset poverty line ($\bar{W}$)



A.3 Test for POGs as a comparison group

We normalize the timing of transfer and perform an event-study type of analysis on the outcome of interests to check whether it could be appropriate to use POG households as a comparison group for Originals. Unfortunately, we cannot normalize the transfer amount; first, POGs received an immature animal whereas the Originals received mature pregnant animals and second, while the Originals gift was preconditioned on passing on the first female offspring from the gift they received, the POGs do not have such requirements.

In the sample, the last of the livestock transfers to POGs were made about a year before the sixth round of data collection. Therefore, after normalizing the transfer dates, we can investigate the program effects for one-year post transfer. We limit the sample to Originals and POGs and estimate the following equation (same as the main specification in the paper, Equation (2.6)).

$$y_{it} = \alpha + T_t + Original_i + \beta(T_t \times Original_i) + \eta_i + \epsilon_{it} \tag{A.8}$$

where, y_{it}, is an outcome of interest for household i in period t. The period, t, takes the value of 0 to indicate the time of the transfer (baseline for all the Original households) and 1 to refer to one year after the transfer was made. T_t is a binary variable for period 1. Estimated β's are reported in Table A.3 (below). As expected, the treatment effects for Originals are different from POGs after one year for nearly all key outcome variables. Statistically, we do not see the differences in herd size after one year; the reason for this is likely explained by the fact that while the POGs gain immature animals (which likely mature after one year and thereafter begin increasing herd size in TLU), Originals lose one immature animal. We see the benefits of receiving the matured animals on consumption; Originals have higher consumption, less poverty and perceive themselves to be more food secure than the POGs.

Based on these results along with the experimental design discussed in the text, we do not think using POGs as the comparison group for Originals, in this particular case, will improve the identification strategy to answer the questions we explore: what are the effects of a large one-off asset transfer program on household welfare and resilience to poverty.

Table A.3: Treatment effects using POGs as a comparison group

	(1) Household herd size (TLU)	(2) Livestock value, per capita	(3) Total asset, value, per capita	(4) Below poverty line	(5) Total expenditure, per capita	(6) Enough food
One year post transfer × Original	0.22	190.81***	115.33	-0.16*	3.01*	0.14**
	(0.26)	(67.99)	(97.11)	(0.09)	(1.75)	(0.06)
Observations	368	368	368	368	368	368
Adjusted R-squared	0.222	0.306	0.141	0.035	0.034	0.147

Notes: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference specification estimated using OLS.

A.4 Cost-benefit calculation

We exploit estimated ITT treatment effects of the program to perform the cost-benefit analysis. As discussed in the research design section, while the Original households received both the livestock and training at the baseline, pass on the gift (POG) households received only training. The Originals, however, are required to pass on the first female offspring from every female animal they received through the program. Therefore, the POGs also benefit from the initial asset transfer; the Originals, however, do not fully reap the benefits from the initial livestock transfer. Thus, to evaluate the full program benefits we need to incorporate the benefits POGs enjoy as well. We use following strategy to calculate the specific program benefit $\hat{B}$:

$$\hat{B} = B^O \times S^O + B^P \times S^P \tag{A.9}$$

where, B^O is the ITT treatment effect for Original households and B^P is the ITT treatment effect on POGs. S^O and S^P are the shares of Original and POG program participants respectively. Overall, 35% of the beneficiaries are Original households while the remaining 65% are POGs. We include changes in household nondurable consumption, household asset accumulation and estimated future consumption gains as the program benefits. Following, Banerjee et al. (2015), we do not include household expenditures on durable goods as these will be captured in the asset accumulation. We include only third year changes in asset accumulation in the total benefits. To calculate future gains in household consumption, we assume the consumption gains observed in year three last till additional 17 years i.e. we assume program benefits to last for 20 years. Household second year gains are assumed to be same as the first-year gains.

Following the joint guideline set by the World Bank Group (2013), we set the initial social discount rate of 5% but also calculate benefits/cost ratios using 7% and 10% for sensitivity.

The total project cost was USD 1 million. The program implementing partner provided us with the detailed budget and the number of beneficiaries. Although, the costs are spread-out over the duration of the program, we assume all the costs exist at year 0 and inflate to year three net present value given by:

$$C_3 = C_0 \times (1.05)^3 \tag{A.10}$$

where C_0 is the per household total program cost, which includes the value of direct transfers, trainings costs, staff salaries, and all other program implementing, monitoring and supervision costs at year 0. All the costs are converted to purchasing power parity (PPP) for cross-country comparison purposes.

A.4.1 Calculating cost of increasing resilience headcount by 1%

Given the first-order Markov process used to estimate households' development resilience, we cannot estimate resilience at the baseline. However, given the quasi-randomized program design, Control and Treatments groups are likely be balance, on average, at the baseline. Assuming balance at baseline we calculate gain in headcount development resilient rate, $\hat{R}_t$, at time t is follow:

$$\hat{R}_t = R_t^O \times S^O + R_t^P \times S^P - R_t^C \tag{A.11}$$

where, R_t^O, R_t^P and R_t^C, are the headcount resilient rate among Original, POG and Control households at time t, where $t \in [18, 33, 42 \text{ months}]$. Again S^O and S^P are the shares of Original and POG program participants respectively. The cost of increasing resilient rate by 1% at time t, $\hat{C}$ is calculated as follow:

$$\hat{C}_t = \frac{\text{Total value of transfers at year 3}}{\hat{R}_t} \tag{A.12}$$

Transfer values are inflated from year 0 to year 3 using Equation (A.10).

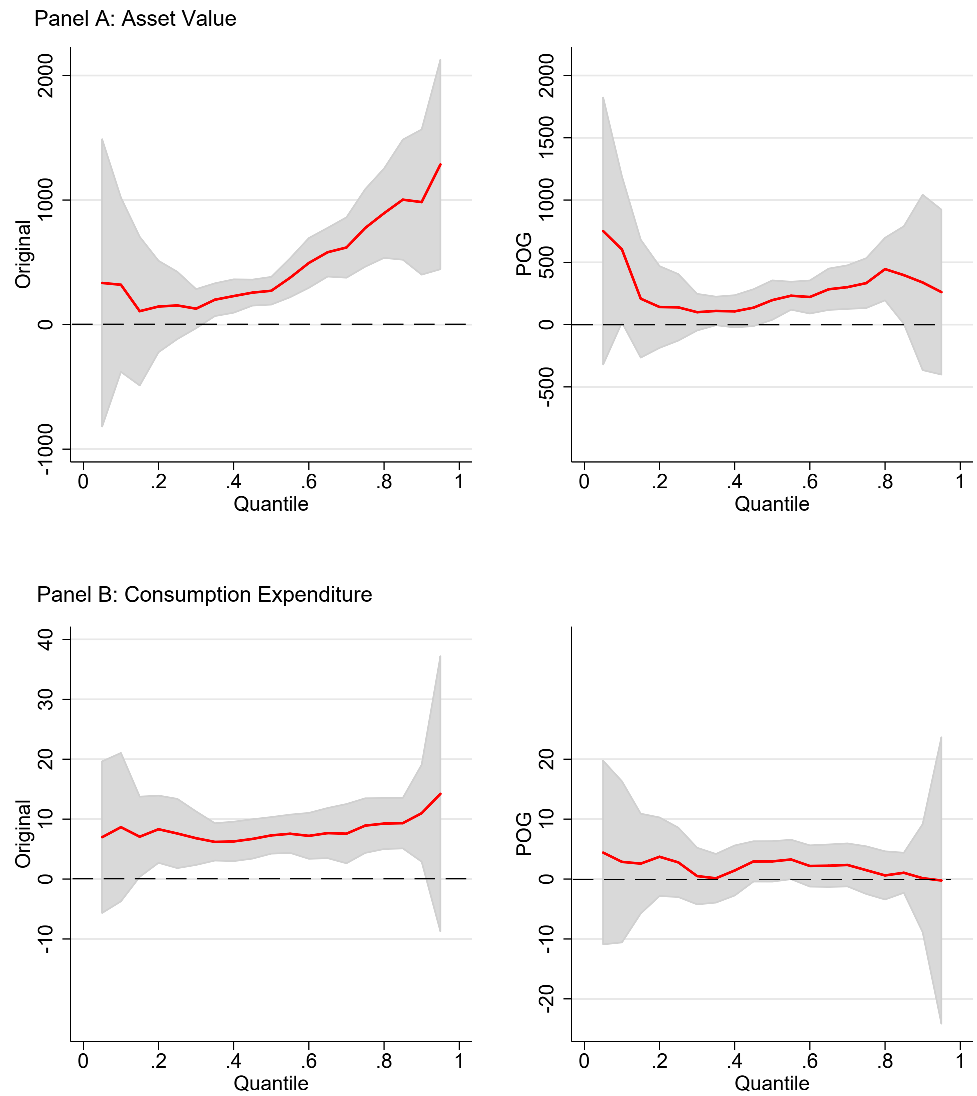
A.5 Outcome heterogeneity

Besides physical asset constraints, households may face ability constraints associated with managing animals. Although households select themselves into the program and receive basic training and veterinary extension support, the program effects are likely to be heterogeneous on innate ability for animal husbandry. Given substantially large livestock gifts, over five times the initial average asset level (Jodlowski et al., 2016), some families may be persuaded to engage in animal husbandry even if it makes them worse off than they otherwise would be from their usual alternatives. Hence, despite the positive average program benefits, this may be of concern. We use the following quantile treatment effects (QTE) specification to explore such heterogeneity in impacts.

$$Q_{\Delta y_i}(\tau) = \alpha(\tau) + \beta_1(\tau)OG_i + \beta_2(\tau)POG_i \tag{A.13}$$

where Δy_i is a the difference between the three year and baseline values of outcomes y for household i. The program impacts on distribution of outcomes are reported in Figure A.4. Panel A shows the quantile treatment effects on distributions of total asset value. For both the treatment groups (Originals and POGs) the effects are more pronounced at higher centiles. While the impact on asset value is increasing on centiles for Originals, the treatment effects among POGs at the top centiles are statistically equivalent to zero. Panel B shows the treatment effect on consumption among the Originals at consistently higher level at each centile except at the extreme top and bottom centiles where the effects are imprecisely estimated. The distributional effect on POGs remain non-negative over all the centiles, however it is imprecisely estimated. It is reassuring to note that all the quantile treatment effects are non-negative which removes any concern related to the endowment effect.

Figure A.4: Three year quantile treatment effects

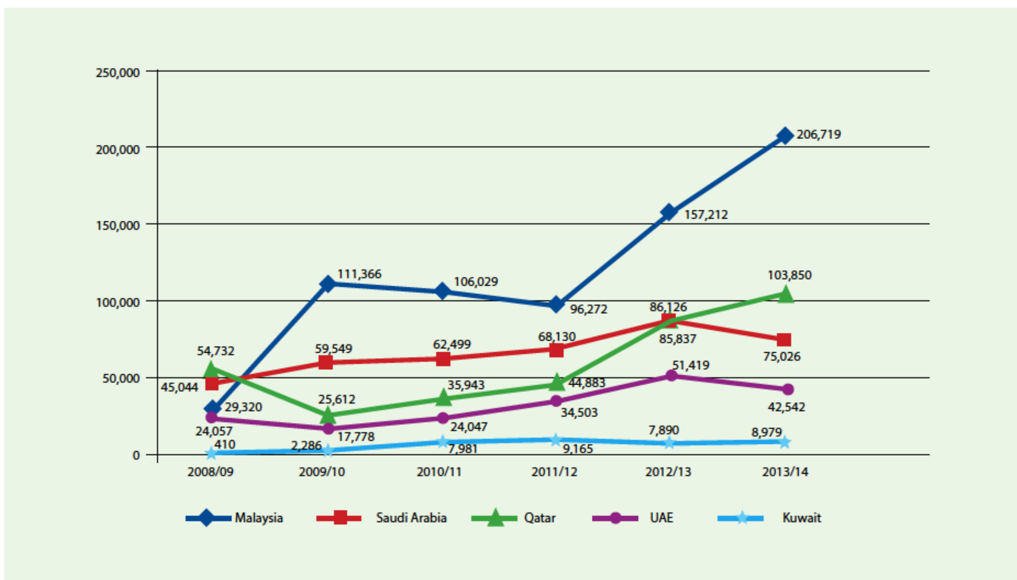


Notes: Quantile treatment effect (QTE) estimates of the differences in outcomes between three-year follow-up and baseline are presented in each panel. Bootstrapped 95% confidence intervals are using 400 replications. In Panel A, assets includes livestock, bicycle, radio, television, solar panel, motorbike, bed, hoes, sickle, shovel, slasher, pangas, mortar, sieve, wheel barrow, sprayer, maize sheller, grain mill, oil press, axe, ox yoke, ox plough, ox cart, livestock shed, feeder, chaff cutter, fencing, milking buckets and chairs, salt/mineral feeder and ripper/cultivato. In Panel B, consumption expenditures include both food (value of own production, purchased and gift in the last 7 days) and average weekly non-food expenditure (clothing, household durables, schooling, medical, alcohol-tobacco and other home expenditures).

APPENDIX B

SUPPLEMENTAL MATERIALS FOR CHAPTER 3

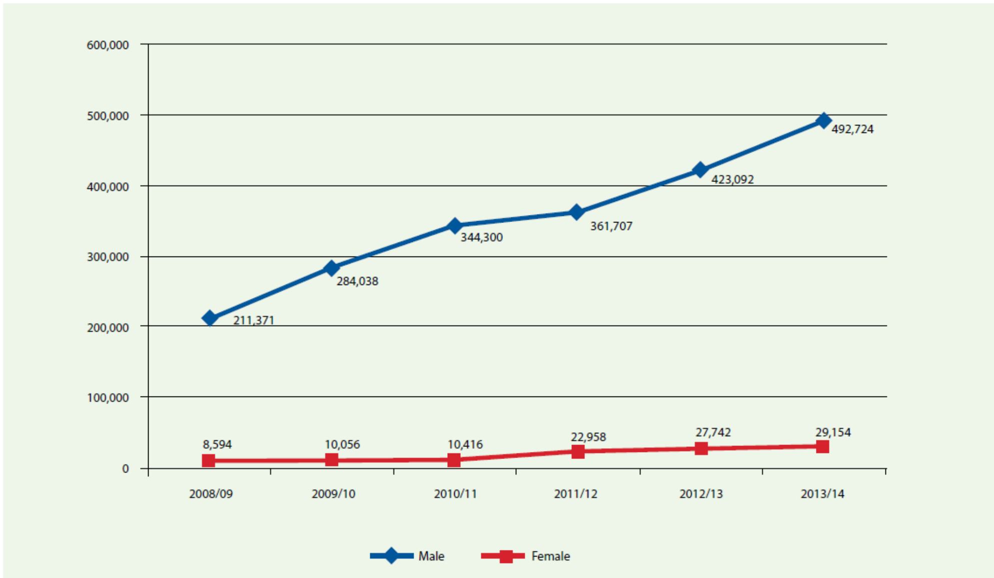
Figure B.1: Migration Trend for Labor-Employment to Top 5 Destination Countries



Source: Department of Foreign Employment.

Note: Migrants for labor-employment to foreign countries are required to obtain labor permits from the Department of Foreign Employment. Migrants can apply on their own or through a recruitment agency. Number reported in the figures are total labor permits issued to migrants who apply through the services of recruitment agencies.

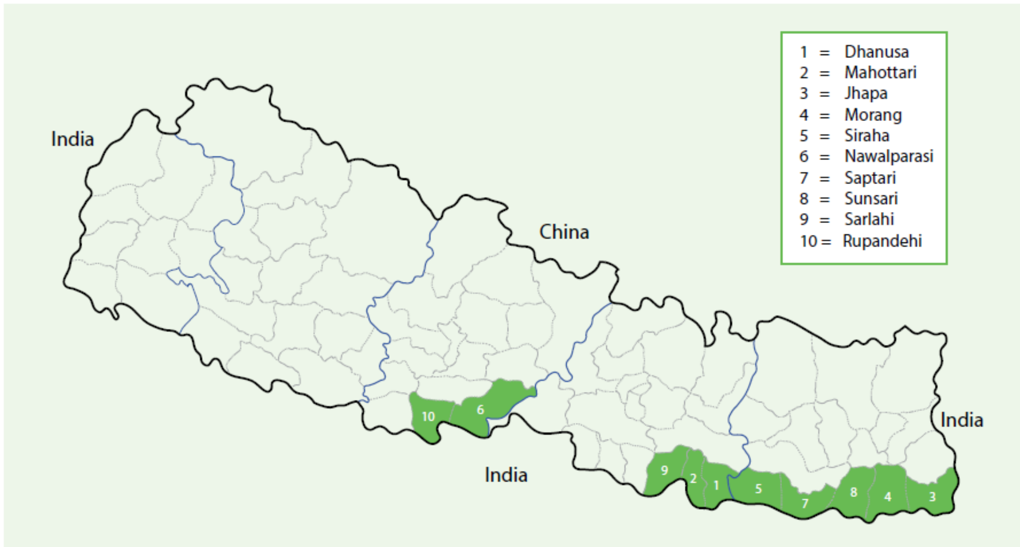
Figure B.2: Migration Trend by gender of Migrants



Source: Department of Foreign Employment.

Note: Source is Department of Foreign Employment (2014). Migrants for labor-employment to foreign countries are required to obtain labor permits from the Department of Foreign Employment. Migrants can apply on their own or through a recruitment agency. Number reported in the figures are total labor permits issued to migrants who apply through the services of recruitment agencies.

Figure B.3: Top-Ten Districts of Origin for Labor Employment Migration



Source for Base Map: Survey Department, Ministry of Land Reform and Management

Note: Source is Department of Foreign Employment (2014). Migrants for labor-employment to foreign countries are required to obtain labor permits from the Department of Foreign Employment. Migrants can apply on their own or through a recruitment agency. Number reported in the figures are total labor permits issued to migrants who apply through the services of recruitment agencies. These top ten districts account for 36.5% of the all labor-permits issued between 2008 and 2014.

Table B.1: Characteristics of Migrants

	Mean	Standard Deviation
Age	28.109	11.370
Male	0.844	0.363
Relation to HH head (Son/Daughter)	0.546	0.498
Education level		
Illiterate	0.172	0.377
1 to 10 grade	0.537	0.499
SLC/Intermediate	0.205	0.404
College or more	0.086	0.280
Migration to		
India	0.440	0.496
Malaysia	0.095	0.294
Middle east	0.247	0.432
Households with migrant	2212 (31.1%)	
Total households in the sample	7108	

Table B.2: Migration and Labor Supply - Full Model

	Migration (First-Stage)		OLS		IV1		IV2	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Migrant network 2001 (IV1)	2.088***							
	(0.253)							
Migrant network 2001 * GDP growth (IV2)		1.042***						
		(0.132)						
Household with migrant			-1.604***	-0.833*	-8.106***	-0.542	-7.625***	-0.717
			(0.282)	(0.426)	(2.521)	(3.422)	(2.438)	(3.495)
Age	-0.0250***	-0.0250***	1.152***	1.279***	0.988***	1.286***	1.000***	1.282***
	(0.00240)	(0.00240)	(0.119)	(0.110)	(0.139)	(0.137)	(0.137)	(0.139)
Age squared	0.000335***	0.000334***	-0.0158***	-0.0148***	-0.0136***	-0.0149***	-0.0137***	-0.0148***
	(2.95e-05)	(2.95e-05)	(0.00159)	(0.00141)	(0.00182)	(0.00177)	(0.00180)	(0.00181)
Sex: Female	0.134***	0.134***	-9.444***	-4.697***	-8.536***	-4.737***	-8.603***	-4.713***
	(0.00809)	(0.00812)	(0.409)	(0.598)	(0.518)	(0.781)	(0.516)	(0.801)
Years of education	-1.93e-05	4.40e-05	0.180***	-0.0878	0.180***	-0.0878	0.180***	-0.0878
	(0.00162)	(0.00162)	(0.0459)	(0.0642)	(0.0458)	(0.0644)	(0.0457)	(0.0642)
Married	-0.000824	-0.00157	-0.0914	4.051***	-0.114	4.052***	-0.112	4.051***
	(0.0112)	(0.0112)	(0.390)	(0.384)	(0.415)	(0.383)	(0.413)	(0.383)
HH head	0.0865***	0.0863***	1.835***	1.658***	2.422***	1.632***	2.379***	1.648***
	(0.00944)	(0.00944)	(0.430)	(0.381)	(0.489)	(0.474)	(0.486)	(0.487)
Share of children 0 to 6	0.141***	0.143***	-1.010	0.460	-0.0579	0.417	-0.128	0.443
	(0.0443)	(0.0443)	(1.190)	(1.220)	(1.278)	(1.320)	(1.264)	(1.319)
Share of children 7 to 15	-0.0341	-0.0337	-0.284	0.838	-0.472	0.846	-0.458	0.841
	(0.0309)	(0.0309)	(1.061)	(1.100)	(1.090)	(1.102)	(1.086)	(1.104)
Land own (Acres)	0.00676**	0.00669**	-0.519***	0.346***	-0.476***	0.344***	-0.479***	0.345***
	(0.00279)	(0.00279)	(0.0909)	(0.0770)	(0.0911)	(0.0819)	(0.0909)	(0.0822)
Owns a house	0.0878***	0.0838***	-3.403***	-2.119**	-2.844***	-2.144**	-2.885***	-2.129**
	(0.0163)	(0.0166)	(0.520)	(0.928)	(0.589)	(1.001)	(0.579)	(0.999)
Social caste: Brahmin/Chhetri	-0.0334**	-0.0345**	-0.876**	-1.352**	-1.050**	-1.344**	-1.037**	-1.348**
	(0.0160)	(0.0160)	(0.424)	(0.639)	(0.414)	(0.635)	(0.416)	(0.636)
Unemployment rate (VDC)	0.133	0.143	1.932	-26.93***	2.574	-26.96***	2.526	-26.94***
	(0.103)	(0.105)	(3.197)	(3.688)	(3.214)	(3.722)	(3.210)	(3.724)
Illiteracy rate (VDC)	0.201***	0.196***	1.525	-5.073**	2.410	-5.113**	2.344	-5.089**
	(0.0556)	(0.0555)	(1.444)	(2.034)	(1.537)	(2.068)	(1.527)	(2.065)
Inequality: Income Gini (VDC)	-0.114**	-0.126**	-1.409	2.480	-2.099	2.511	-2.048	2.492
	(0.0542)	(0.0550)	(1.515)	(1.934)	(1.534)	(1.947)	(1.530)	(1.958)
Poverty rate (District)	-0.168**	-0.200**	-4.378**	-2.990	-5.422**	-2.943	-5.345**	-2.972
	(0.0837)	(0.0849)	(2.066)	(2.659)	(2.121)	(2.747)	(2.109)	(2.750)
Constant	0.405***	0.431***	-1.005	1.396	2.159	1.254	1.925	1.339
	(0.0637)	(0.0636)	(2.482)	(2.514)	(2.875)	(2.936)	(2.846)	(3.011)
Observations	16,879	16,879	16,879	16,879	16,879	16,879	16,879	16,879
R^2	0.087	0.086	0.157	0.099	0.132	0.099	0.136	0.099
Wald χ^2					1996	1038	2014	1037
F-test 1stage					68.21	68.21	62.58	62.58
Region Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors clustered at VDC level. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Sample is working age (18 to 60) adults. Columns (1) and (2) are first stage for IV1 and IV2 respectively. Columns (3), (5), and (7) are hours in wage employment while Columns (4), (6), and (8) are hours in self employment.

Table B.3: 2SLS Estimation of Labor Supply by Gender- Full Model

	Female			Male		
	(1) Total Hours	(2) Work Hours (Wage + Self)	(3) Total Household Activity Hours	(4) Total Hours	(5) Work Hours (Wage + Self)	(6) Total Household Activity Hours
Household with migrant	-5.702	-4.538	-1.164	-17.65**	-18.34**	0.689
	(6.312)	(3.552)	(4.960)	(8.652)	(7.142)	(5.980)
Observations	9,597	9,597	9,597	7,282	7,282	7,282
Wald χ^2	2436	960.9	2287	1528	1085	1127
F-test 1stage	74.90	74.90	74.90	35.55	35.55	35.55
Controls						
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
VDC characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes

Note: 2SLS estimates are reported in the table. Instrument used for 2SLS is share of international migrants in a VDC in 2001 (IV1). Sample is working age (18 to 60) adults and standard errors are clustered at VDC level. Robust standard errors in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Individual controls are age age^2, years of education, household head identifier, and marital status. Household controls are share of children, amount of land-owned, house ownership and social caste. Similarly VDC level controls are, unemployment rate, poverty rate, illiteracy rate, and inequality (Gini).

Table B.4: 2SLS Estimation of Labor Supply by Women's Age and Household Head Status- Full Model

	(1)	(2)	(3)	(4)	(5)	(6)
	Total Hours	Work Hours (Wage + Self)	Total Household Activity Hours	Total Hours	Work Hours (Wage + Self)	Total Householdl Activity Hours
Panel A	Old			Young		
Household with migrant	-16.17**	-12.73***	-3.440	-0.0365	0.293	-0.330
	(7.662)	(4.652)	(5.720)	(7.072)	(4.345)	(5.395)
Observations	3,144	3,144	3,144	6,453	6,453	6,453
Wald χ^2	776.8	360.9	885.7	2287	958.8	2627
F-test 1stage	60.28	60.28	60.28	62.12	62.12	62.12
Panel B	HH Head			Other Members		
Household with migrant	-12.86	-12.72**	-0.146	-3.036	-1.939	-1.097
	(9.121)	(5.703)	(6.672)	(6.589)	(3.970)	(5.135)
Observations	1,593	1,593	1,593	8,004	8,004	8,004
Wald χ^2	428.1	204.5	1042	2298	921.2	2163
F-test 1stage	31.33	31.33	31.33	62.19	62.19	62.19
Controls						
Individual characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes	Yes
VDC characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Region Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes

Note: Young adults are ages between 18 and 40 while old adults are ages between 41 and 60. 2SLS estimates are reported in the table. Instrument used for 2SLS is share of international migrants in a VDC in 2001 (IV1). Sample is working age (18 to 60) adults and standard errors are clustered at VDC level. Robust standard errors in parentheses.* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Individual controls are age age^2, years of education, household head identifier, and marital status. Household controls are share of children, amount of land-owned, house ownership and social caste. Similarly VDC level controls are, unemployment rate, poverty rate, illiteracy rate, and inequality (Gini).

APPENDIX C

SUPPLEMENTAL MATERIALS FOR CHAPTER 4

Figure C.1: Administrative map of Nepal

Note: The map represents administrative areas before the 2015 constitution when Nepal was divided into 5 development regions, 75 districts and about 4000 rural (village development committees) and urban (municipalities) areas.

Figure C.2: Conflict intensity heterogeneity: Casualty count

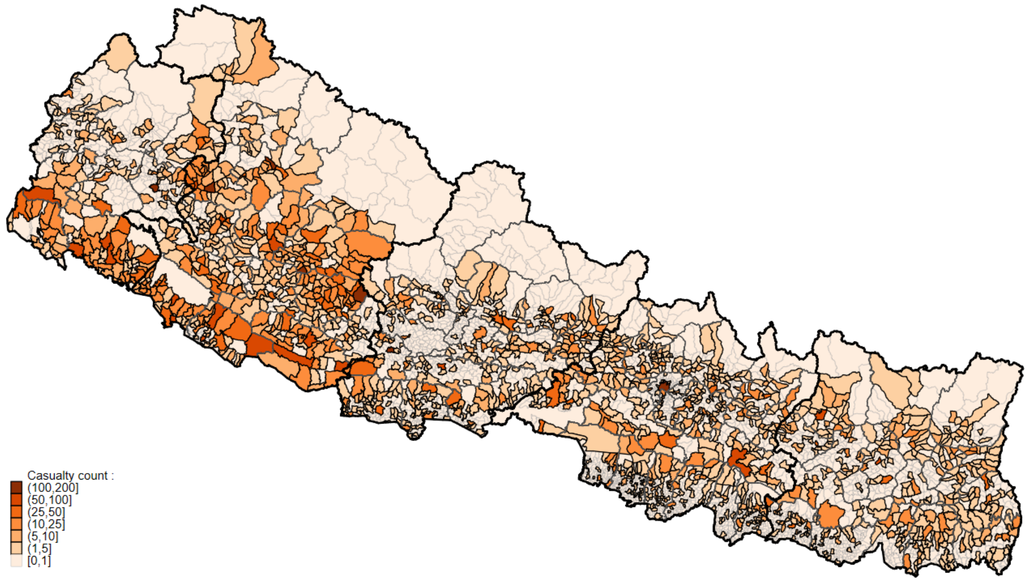


Figure C.3: Nepal Demographic Health Survey 2016 Coverage

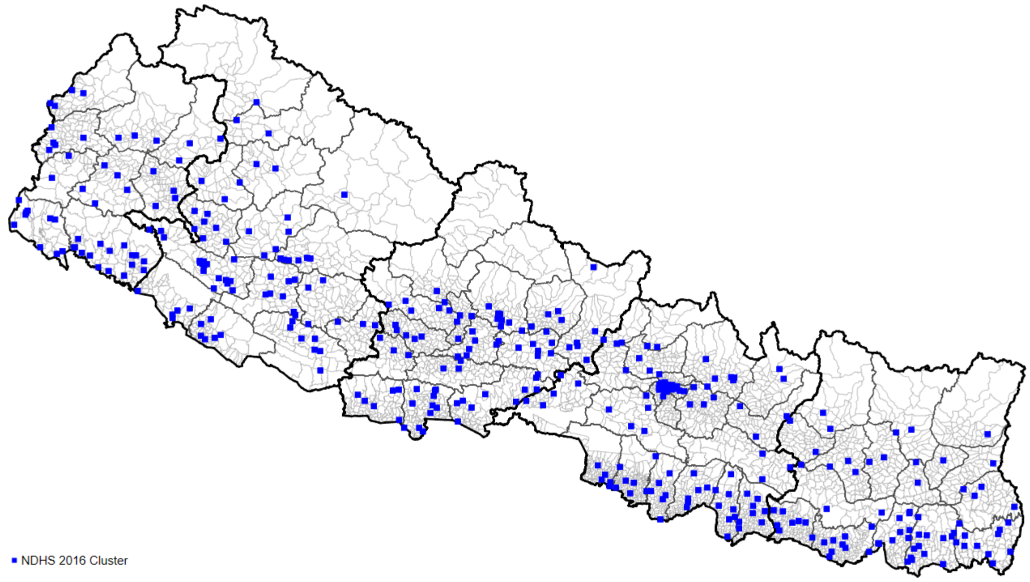


Figure C.4: Conflict intensity heterogeneity: Casualty count and NDHS 2016 clusters

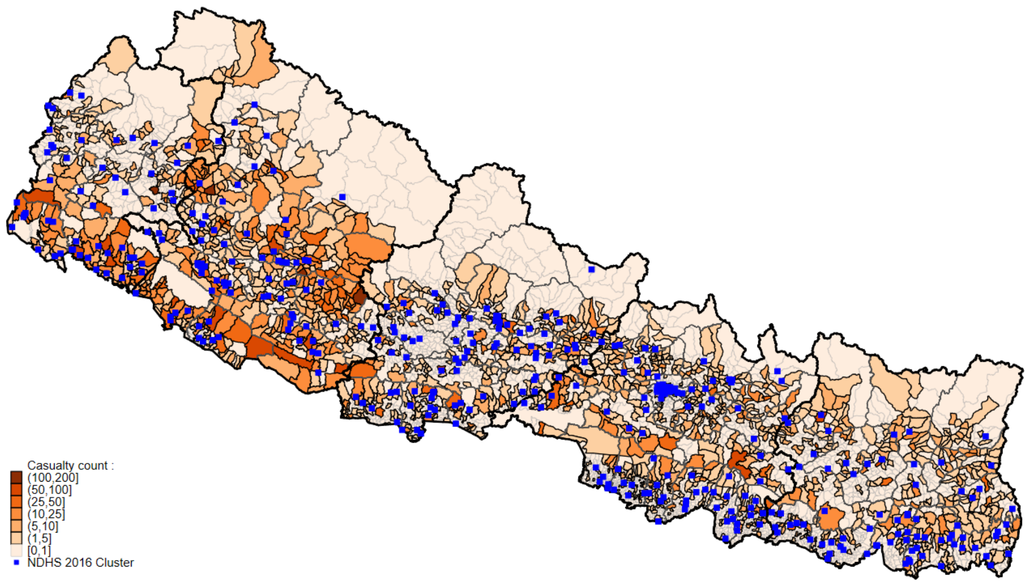
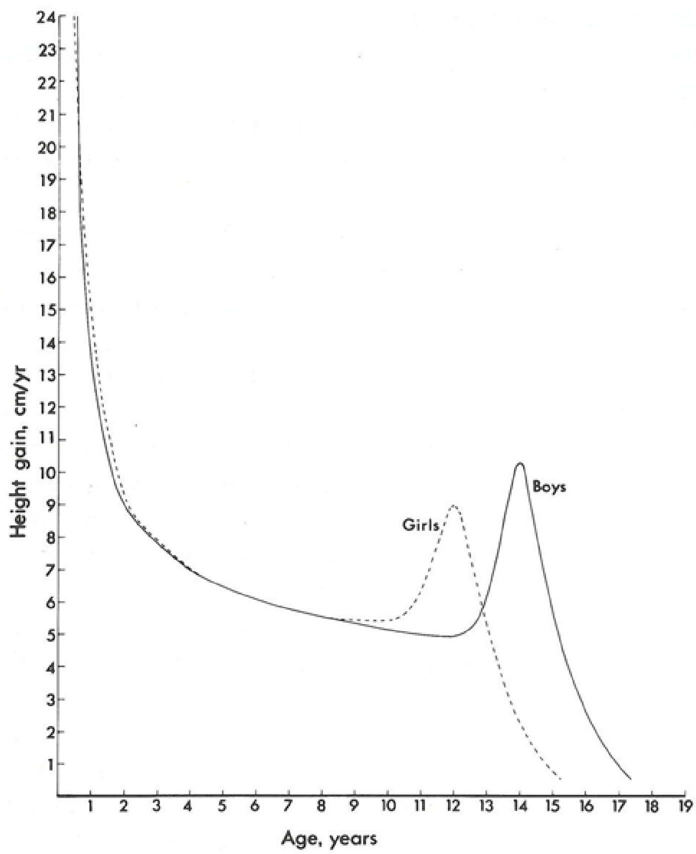


Figure C.5: Typical growth velocity curve



Source: Adopted from Tanner, Whitehouse, and Takaishi (1966). *Archives of disease in childhood* vol. 41,220 (1966): 613-35.

Table C.1: Impact on first generation adult stature using alternative specification

	Height in cm			Height for age sd		
	(1)	(2)	(3)	(4)	(5)	(6)
Months of war during 0 to 3 years	-0.026	-0.011	-0.103	-0.008	-0.007	-0.021
	(0.334)	(0.338)	(0.361)	(0.056)	(0.057)	(0.061)
Months of war during 4 to 8 years	-0.130**	-0.171***	-0.173***	-0.022**	-0.029***	-0.029**
	(0.056)	(0.064)	(0.067)	(0.009)	(0.011)	(0.011)
Months of war during 9 to 15 years	0.009	-0.052*	-0.060**	0.002	-0.009*	-0.010**
	(0.024)	(0.028)	(0.028)	(0.004)	(0.005)	(0.005)
Observations	4,421	4,421	4,421	4,418	4,418	4,418
Adjusted R-squared	0.027	0.029	0.041	0.027	0.029	0.041
Birth year and month fixed effects		Yes	Yes		Yes	Yes
Regional trends and other controls			Yes			Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	383	383	383	383	383	383
Outcome mean	151.6	151.6	151.6	-2.034	-2.034	-2.034

Note: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Standard errors are clustered at village level. All ages in the table refer to age at the start of the war. Other controls include an indicator for high caste and month of interview fixed-effects. Specification used is $Y_{imntvdr} = \beta_0 + \beta_1 \times$ Exposure during 0 to 3 years $+ \beta_2 \times$ Exposure during 4 to 8 years $+ \beta_3 \times$ Exposure during 9 to 15 years $+ \alpha_t + \eta_m + \delta_v + \gamma_r^T + X_i + \omega_n + \varepsilon_{imntvdr}$, where Y is a height of a woman i born in a month m and year t, and residing in village v. district d and development region r. Conflict variables are defined as women's exposure to conflict during 0 to 3 years, 4 to 8 years, and 9 to 15 years of age. All other variables have same meaning as the main specification equation 7. Reported in the table are the estimated β_1, β_2, and β_3 coefficients.

Table C.2: Impact on first generation adult height (cm) using alternative specification and alternative measure of conflict

Conflict variable =	Casualty count	Including contiguous villages	
	(1)	(2)	(3)
	Own village	Months of war	Casualty count
Months of war during 0 to 3 years	-0.033	-0.003	-0.007
	(0.148)	(0.084)	(0.040)
Months of war during 4 to 8 years	-0.069***	-0.101***	-0.027***
	(0.019)	(0.034)	(0.010)
Months of war during 9 to 15 years	-0.021**	-0.019	-0.006
	(0.010)	(0.018)	(0.005)
Observations	4,421	4,421	4,421
Adjusted R-squared	0.041	0.041	0.041
Birth year and month fixed effects	Yes	Yes	Yes
Regional trends and other controls	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes
Number of clusters	383	383	383
Outcome mean	151.6	151.6	151.6

Note: *** (**) (*) indicates significance at the 1% (5%) (10%) level. Standard errors are clustered at village level. All ages in the table refer to age at the start of the war. Other controls include an indicator for high caste and month of interview fixed-effects. Specification used is $Y_{imtvdr} = \beta_0 + \beta_1 \times$ Exposure during 0 to 3 years $+$ $\beta_2 \times$ Exposure during 4 to 8 years $+ \beta_3 \times$ Exposure during 9 to 15 years $+ \alpha_t + \eta_m + \delta_v + \gamma_r^T + X_i + \varepsilon_{imtvdr}$, where Y is a height of a woman i born in a month m and year t, and residing in village v. district d and development region r. Conflict variables are defined as women's exposure to conflict during 0 to 3 years, 4 to 8 years, and 9 to 15 years of age. All other variables have same meaning as the main specification equation 6. Reported in the table are the estimated β_1, β_2, and β_3 coefficients.

Table C.3: Impact on first generation adult height (cm) - women living in the same district as birth

	Height in cm			Height for age sd		
	(1)	(2)	(3)	(4)	(5)	(6)
Age 22 to 29 × Months of war	-0.026	-0.028	-0.023	-0.004	-0.005	-0.004
	(0.047)	(0.046)	(0.047)	(0.008)	(0.008)	(0.008)
Age 9 to 15 × Months of war	-0.003	-0.001	-0.004	-0.000	-0.000	-0.001
	(0.039)	(0.039)	(0.039)	(0.007)	(0.007)	(0.007)
Age 4 to 8 × Months of war	-0.069*	-0.066	-0.074*	-0.011*	-0.011	-0.012
	(0.041)	(0.043)	(0.044)	(0.007)	(0.007)	(0.007)
Age0 to 3 × Months of war	-0.145***	-0.148***	-0.162***	-0.024***	-0.025***	-0.027***
	(0.046)	(0.046)	(0.046)	(0.008)	(0.008)	(0.008)
Observations	3,243	3,243	3,243	3,242	3,242	3,242
Adjusted R-squared	0.024	0.023	0.032	0.025	0.023	0.033
Birth year and month fixed effects		Yes	Yes		Yes	Yes
Regional trends and other controls			Yes			Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	380	380	380	380	380	380
Control mean (Age 16 to 21)	151.3	151.3	151.3	-2.089	-2.089	-2.089

Note: Sample is limited to women living in the same district as their birth. *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference specification. Standard errors are clustered at village level. All ages in the table refer to age at the start of the war. Comparison cohort is age 16 to 21 and cohort 22 to 29 is a second comparison group that serves as a placebo test. Other controls include an indicator for high caste and month of interview fixed-effects.

Table C.4: Control experiment of district birth rates (yearly births)

	Conflict: Months of war in district			Conflict: Casualties per 1000 district 1991 population		
	(1)	(2)	(3)	(4)	(5)	(6)
Births between 1966 to 1974 × Conflict	-0.009	-0.009	-0.009	-0.016	-0.016	-0.016
	(0.006)	(0.006)	(0.006)	(0.136)	(0.138)	(0.139)
Births between 1981 to 1987 × Conflict	-0.007	-0.007	-0.007	-0.142	-0.142	-0.142
	(0.011)	(0.011)	(0.011)	(0.179)	(0.182)	(0.183)
Births between 1988 to 1992 × Conflict	0.026	0.026	0.026	0.426	0.426	0.426
	(0.022)	(0.022)	(0.022)	(0.397)	(0.403)	(0.406)
Births between 1993 to 1996 × Conflict	0.043	0.043	0.043	0.732*	0.732	0.732
	(0.026)	(0.027)	(0.027)	(0.434)	(0.441)	(0.444)
Observations	2,325	2,325	2,325	2,325	2,325	2,325
Adjusted R-squared	0.447	0.437	0.809	0.446	0.436	0.808
District fixed effects		Yes	Yes		Yes	Yes
Birth year fixed effects			Yes			Yes
Number of clusters	75	75	75	75	75	75
Control mean (Births between 1975 to 1980)	15.69	15.69	15.69	15.69	15.69	15.69

Note: Yearly births are calculated using individuals observed in the 2001 Nepal population census and their year of birth normalized to 1000 districts inhabitants in 2001. *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference specification. Standard errors are clustered at district level.

Table C.5: Second generation health impact using alternative measure of conflict

Conflict variable =	Casualty count own village			Months of war including contiguous villages			Casualty count including contiguous villages		
	(1) HAZ	(2) WHZ	(3) BMIZ	(4) HAZ	(5) WHZ	(6) BMIZ	(7) HAZ	(8) WHZ	(9) BMIZ
Mother's age 22 to 29 ×	0.028	-0.009	-0.015	0.063***	-0.003	-0.015	0.023***	-0.003	-0.007
Mother's exposure to conflict	(0.024)	(0.019)	(0.019)	(0.022)	(0.016)	(0.016)	(0.007)	(0.006)	(0.006)
Mother's age 9 to 15 ×	-0.009	-0.010***	-0.011***	-0.001	-0.018*	-0.020*	-0.002	-0.006**	-0.007**
Mother's exposure to conflict	(0.009)	(0.004)	(0.004)	(0.014)	(0.010)	(0.010)	(0.005)	(0.003)	(0.003)
Mother's age 4 to 8 ×	-0.005	-0.011*	-0.010*	-0.004	-0.018*	-0.020**	-0.001	-0.007**	-0.007**
Mother's exposure to conflict	(0.008)	(0.006)	(0.006)	(0.013)	(0.010)	(0.010)	(0.004)	(0.003)	(0.003)
Mother's age 0 to 3 ×	-0.008	-0.005	-0.005	-0.000	-0.013	-0.016	-0.001	-0.005*	-0.005*
Mother's exposure to conflict	(0.006)	(0.005)	(0.005)	(0.013)	(0.010)	(0.010)	(0.004)	(0.003)	(0.003)
Observations	2,165	2,163	2,164	2,165	2,163	2,164	2,165	2,163	2,164
Adjusted R-squared	0.229	0.119	0.116	0.231	0.119	0.116	0.232	0.120	0.117
Mother and household controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Child controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of clusters	373	373	373	373	373	373	373	373	373
Control mean (Age 16 to 21)	-1.836	-0.580	-0.423	-1.836	-0.580	-0.423	-1.836	-0.580	-0.423

Note: Sample is children aged 0 to 59 months at the time of the survey. *** (**) (*) indicates significance at the 1% (5%) (10%) level. Treatment on the treated estimates are reported based on a difference-in-difference specification. Standard errors are clustered at village level. Mother's age in the table refers to mother's age at the start of the war. Comparison cohort is children born to mothers whose age was 16 to 21 at the start of the war. Children born to mother's cohort 22 to 29 is a second comparison group that serves as a placebo test. Mother's controls are mother's years of birth fixed effects, mother's month of birth fixed effects, region specific trends. Household controls are indicator for high caste, female headed households, and whether residing in a rural area. Child controls are indicator if child is a girl, a twin, birth order fixed effect, and fixed effects for child years of birth, month of birth, and month of anthropometric measurements. Reported outcomes are z-scores based on the WHO anthropometric measurement standards.