Essays in Social Networks and Development Economics

By

Ya-Ting Chuang

A dissertation submitted in partial fulfillment of

the requirements for the degree of

Doctor of Philosophy

(Agricultural and Applied Economics)

at the

UNIVERSITY OF WISCONSIN-MADISON

2015

Date of final oral examination: 05/04/2015

The dissertation is approved by the following members of the Final Oral Committee:

Jennifer Alix-Garcia, Associate Professor, Agricultural and Applied Economics

Laura Schechter, Associate Professor, Agricultural and Applied Economics

Daniel Phaneuf, Professor, Agricultural and Applied Economics

Corbett Grainger, Assistant Professor, Agricultural and Applied Economics

James R. Walker, Professor, Economics

To John Liu, for your 15-year love and unconditional support.

Acknowledgements

First and foremost, I would like to express my deepest appreciation to my advisor, Professor Jennifer Alix-Garcia, for her valuable guidance and generous support throughout my PhD studies. Professor Alix-Garcia has been such a wonderful mentor for my research, my career, and my life. Throughout the years, she has supported me academically and emotionally through the rough road to finishing my PhD degree. I remember she used to say, "you can do it" - her way of showing confidence in me - to give me positive energy and perseverance to jump through the hurdles. This dissertation would not have been possible without her intelligence, dedication, and enthusiasm in advising. I would also like to give special thanks to my extraordinary committee member, Professor Laura Schechter, for constantly guiding, encouraging, and supporting my research and me. I am grateful to have the opportunity to collaborate with her on one of the chapters. Thanks for being a brilliant and resourceful research collaborator, and at the same time for making the learning process joyful. I am also thankful for the rest of my committee members, Professor Daniel Phaneuf, Professor Corbett Grainger, and Professor James Walker for their valuable comments and suggestions.

I appreciate all my AAE colleagues, for supporting and enriching my life at UW-Madison. I acknowledge the financial support from TATA fellowship and Vilas Travel Grant from UW-Madison, Taiwan Ministry of Education. I am grateful to the following faculty members, Professor Jennifer Alix-Garcia, Professor Laura Schechter, Professor Corbett Grainger, Professor Daniel Phauneuf, Professor Xueli Wang, and Professor Ann Terlaak for providing me assistantship and enriching my academic experience. Special thanks to Indus Software, for providing me the opportunity to work in India, which started my first adventure in a developing country and inspires my career decision to work on development. I would like to thank the staff in ICRISAT, especially those at the Kandal and Shirapur villages in Maharashtra, India, for providing me local support in the field to conduct my own qualitative research in India. This qualitative research experience, though it is not the main part of the dissertation, provides intuition for my theoretical model set-up in my third chapter, and create emotional connections to my current and future research. I want to give my appreciation to the family who hosted me in Thailand, for providing me local contexts to link to my findings in my first chapter. I am also grateful to all the villagers I have met in the field for sharing their lives and experience with me. Those stories reinforce my academic devotion to work in the development field.

I would like to thank my parents, Tsun-Huang Chuang and Hui-Ying Yang, for supporting me in every way to pursue my degree in the U.S. Special thanks to my daughter, Mutien, for being such a cooperative baby and for making my last year at UW-Madison successful and joyful. Last but not least, I am thankful to my husband, John Chung-En Liu, for his unconditional support and love even in hardship. Without your encouragement, I would not had the faith to start this degree at the first place. This process would not have been so smooth without his support. Thanks for always being there for me, to lean on, to draw strength from, to share my life and research with, and to "walk" with at the end of this journey. I sincerely dedicate this dissertation to my husband, John Chung-En Liu.

Contents

Abstract

This dissertation is a collection of three essays on social networks and development economics. The first chapter examines the effect of peer networks on self-control problems. I construct a theoretical model to describe the way in which peer networks influence consumption behaviors through social norms, which guide individuals to conform to their friends' behavior. Using comprehensive data from a monthly survey conducted in 16 villages in Thailand from 1999 through 2004, I empirically examine peer effects on temptation consumption patterns, and test the mechanism underlying this relationship. Detailed social network information in the dataset allows the identification of impacts using a friend of a friend (excluded network) as the instrument. The empirical results provide evidence that peer decisions significantly impact individuals' temptation consumption such as alcohol and gambling, as well as savings. These peer effects are driven primarily by social norms, rather than by risk sharing.

In the second chapter, co-authored with professor Laura Schechter, we first conduct an extensive review of the disparate literature studying the stability of preferences measured in experiments. Then, we test the stability of individuals' choices in panel data from rural Paraguay, including both experimental and survey measures of risk, time, and social preferences collected over almost a decade. Answers to survey questions are quite stable, while experimental measures are less so. If choices made in experiments are not stable, it may be because these choices are influenced by shocks, or because they include high levels of noise. We find no evidence that real-world shocks influence play in games. We suggest that in a developing country context, researchers may want to design simpler experiments or make more use of survey questions to measure preferences.

The third chapter explores the impact of weather shocks on farmers' income diversification strategies. I combine historical weather data with household data in India to explore whether farmers employ different responses toward weather shocks in regions with different levels of historical variation. I find that weather shocks can negatively affect agricultural income, but this effect decreases in a riskier place where people have, over time, diversified their income into off-farm employment. I also find evidence that caste-networks can potentially determine people's income diversification strategies. Households who are within a different caste from the majority of their village peers will be more likely to seek for off-farm jobs, while households who are in a similar caste to the majority of the people within the village will seek agricultural wage jobs from others in the village.

Introduction

This dissertation began with my initial intellectual curiosity about microfinance. I started to work with microfinance institutions when I was pursing my Master in Public Policy degree at the University of Maryland - College Park. I found that the impact of microcredit is disappointing, let alone the high interest rate ranging from 30% to over 100%. In the summer of 2007, I worked on an impact evaluation project with a microfinance institution in Cambodia. I led a team to conduct a household survey for around 300 randomly sampled microfinance clients. This survey was literally door-to-door, for which we once travelled by boat—the only mode to reach a specific village. This was the time when the randomized controlled trials in microfinance started to become a fad. My evaluation project concluded with an unimpressive impact of microfinance[1]. The trend of the industry is to go with the "better" (a.k.a. wealthier) customers to improve financial sustainability. At a microfinance conference, I presented to donors and policymakers that the current practice of the industry may trade off its social goals for financial rewards. Had I changed the world? No. Otherwise, I would not have started my journey at AAE. Throughout the years, I have shifted my focus to saving products, as it may be easier for the poor to be self-sustainable. Later, I explored the literature in behavioral economics to help explain what hinders savings and other well-intentioned programs. In the end, I find my temporary satisfaction[2] in incorporating social network theories to better model human behaviors.

This dissertation is a collection of three chapters related to social networks and development economics. I aim to bring social network analysis and behavioral economics to understand important questions in the field of development economics. By modeling and testing indi-

[1]This result was without carefully taking care of the endogeneity.
[2]Of course, more future work and data collection are required.

viduals' decision-making in different aspects in life, we can improve our understanding about poverty and potentially provide insights on future policies. I first explore individual's consumption pattern by linking insights from social network theories and behavioral economics to show that the poor are constrained not only economically, but also mentally and socially. As experimental methods in behavioral economics become more prevalent and helpful in understanding people's decision-making process in developing countries, I exploit a unique dataset with my co-author to examine how stable risk, time and social preference measures are over time and to provide insights on using survey and experimental methods. As development and environmental issues are closely linked, part of my dissertation intersects with the field of environmental economics—I examine poor farmers' income diversification strategies toward climate change.

The first chapter, my job market paper, is to understand peer effects on the self-control problem, an important behavioral theory to explain why people do not save enough, procrastinate on work, and forgo profitable investments. These behavioral constraints can further create hurdles for escaping poverty. I first build a theoretical model that captures myopic behaviors and peer effects. To empirically test the existence and mechanism of peer effects on an individual's consumption behavior, I create social network information based on households' real transactions (ex: borrowing and lending, gift-giving, and labor-sharing relations) identified in the monthly panel data from Thailand. I then use an instrumental variable to identify peer effects on temptation consumption, such as alcohol, gambling, and smoking. My results suggest strong peer effects on temptation consumption, and these effects are more significant in more observable goods. My analysis shows that such peer effects likely stem from the mechanism of social norms, rather than from risk-sharing.

This study contributes to existing literature in three major ways. First, this is the first paper to extend the literature on time preferences by incorporating social interactions into the model and testing the model empirically. Second, in the current literature, researchers use exogenous identities, such as races, castes, and last names, as the social network unit. In contrast, I provide more refined social network information based on detailed real-life social relations. Finally, I identify peer effects through an innovative instrumental variable - excluded peers - that is, a household's friends of friends who are not directly linked with that household. The lagged consumption of excluded peers can fulfill the exclusion restriction needed for instrumental variable models. I also carefully address the identification issue, which has often been neglected in previous research.

The second chapter, co-authored with Professor Laura Schechter, is to understand whether preference measures by surveys and experiments are stable over almost a decade, and whether previous play in games or shocks can affect those preference measures. As theories and methods from experimental and behavioral economics have been brought into development economics to understand poverty, this chapter provides a new angle by observing those preference measures and providing potential future recommendations on future development research. Using a household dataset from Paraguay, where people repeatedly participate in surveys and experiments, we are able to examine risk, time and social preference stability over almost a decade. We also test the effects of shocks on these preference measures. The results indicate that social and time preferences are relatively more stable than risk preferences. Within the social preference measures, the survey measures perform much more consistently than experiment results. These results suggest that researchers should be more careful to use experiment measures when working with a population with lower education and higher uncertainties in life. In terms of shocks, we find that real-world shocks, such as income shocks or illness, appear to have no significant impacts on preferences. Compared to the current literature, our paper examines the stability of preference measures over a longer time span and more carefully addresses issues of attrition.

The third chapter examines farmers' climate change adaptation in India. As climate change can result in more erratic weather patterns, leading to distributional impact among the poor, this chapter examines how farmers react to weather shocks and provides insight on the heterogeneous impact of weather shocks. I build a model in which farmers form beliefs and employ adaptation strategies based on historical weather patterns. For empirical analysis, I combine historical weather information from the global spatial datasets with a long-term household survey from India to test empirical predictions from the model. In contrast to the literature, I identify income diversification strategies that differ with weather variations across regions. Farmers in riskier places are less responsive toward weather shocks because they have diversified their income sources over time, while, in less risky areas, farmers are under-prepared to cope with weather shocks. The income diversification strategy occurring in a region with lower historical weather variation makes farmers more vulnerable. I also find suggestive evidence that caste network potentially determines a household's income diversification strategy. This study suggests an additional angle from which to view climate change adaptation.

Chapter 1

Self-Control or Social Control?

Peer Effects on Temptation Consumption

1.1 Introduction

A common theme in the behavioral economics literature is that individuals have self-control problems. Individuals are tempted to do things that provide immediate satisfaction, rather than sacrificing now for the future. The more recent literature has emphasized the notion of imperfect self-control in an attempt to explain why individuals engage in deleterious behaviors such as smoking and drinking, consuming more than intended, not saving enough, borrowing money at high interest rates, and procrastinating.

Self-control problems are also found among the poor in developing countries. Poor households spend a considerable amount of disposable income on entertainment and indulgence goods, including temptation goods such as alcohol and tobacco. As shown by Banerjee and Duflo (2007), for example, poor crop farmers also have difficulty saving even small amounts of money upfront for fertilizers to be used later. Behaviors such as these, which impede economic success, are of utmost interest to policymakers, especially in light of the increasing promotion of credit to the poor. Individuals may over borrow when they do not recognize their preferences for immediate payoffs (Heidhues and Kőszegi, 2010).

Self-control theory, while useful in some settings, does not account for social influences on individual decision making. In this study, I address this gap by incorporating peer effects into self-control theory, and conducting empirical tests of the revised theory. The main research questions I address are: (1) Is individual-level temptation consumption affected by peers' consumption? (2) If so, what is the mechanism underlying this relationship?

I begin by including social interactions into the temptation model developed by Banerjee and Mullainathan (2010). I define temptation goods as alcohol, tobacco, and gambling, because data about the consumption of these goods can further inform the scholarly understanding of the potential negative consequences of the self-control problems. Temptation consumption, which is one embodiment of the self-control problem, may further perpetuate poverty, as demonstrated by (Mani et al., 2013). The peer effect that I incorporate into the model of self-control theory is derived from the idea that people want to follow social norms and thus suffer disutility when deviating from that of their peers. My model predicts that peers have a stronger effect on the consumption of temptation goods than on non-temptation goods, especially among observable goods. My model is also able to demonstrate that in the event of a shock at either the household or network level, poor households will consume more temptation goods. Both of these predictions have important implications for a larger range of phenomena, from saving and investment behaviors to poverty trap.

To test my model predictions empirically and examine spending behaviors, I use data from the Thai Townsend Monthly Project, which includes extensive information about household-level consumption and social relationships. I create household-level social network variables by exploiting the data associated real-world transactions (e.g., borrowing, lending, gift-giving, and labor sharing described in the survey). The extensive network information available in my data helps circumvent several identification challenges that are common concerns in the social interaction literature.

One major concern in the social interaction literature, for example, is the reflection problem, which refers to the inability of econometricians to identify the effects of the peer-group behavior on the actions of individuals because individuals, who comprise the network group, can also affect the group behavior (Manski, 1993). Another identification challenge in the social interaction literature is the inability to separate the effects of peer behavior from unobservable correlated shocks and omitted covariates.

To address the identification challenges, I apply an instrumental approach to identify peer effects using lagged consumption data from an excluded network—friends' of friends who are not linked directly with the focal individual. This approach eliminates the effects of the endogenous unobservable shocks that happen to individuals as well as their peers because these excluded peers do not directly interact with the focal individuals. Another benefit of my approach is that the instrumental variable is time-varying, and thus any time-invariant covariates will not hinder the identification after controlling for individual, village-year, and seasonal fixed effects. The lagged consumption variables prevent the problem of reverse causation or a joint consumption decision. The method is able to produce unbiased results even in the presence of measurement error in defining the network (De Giorgi et al., 2010).

Overall, I find that household-level temptation consumption, especially the consumption of more observable goods, is subject to strong peer effects. In particular, I find that one bhat increase in peers' temptation consumption leads to 1.3 bhat increase in individual's temptation consumption. The results also show that in the face of economic shocks, poor households consume a higher share of temptation goods than rich households—temptation consumption has a concave shape. This finding confirms the theoretical assertion that poor households are subject to greater cognitive constraints (Chemin et al., 2013; Mani et al., 2013). Further robustness tests reveal that temptation consumption decisions are more strongly influenced by social norms than risk-sharing. In sum, the results indicate that peer effects exacerbate myopic consumption behaviors and suggest that peer behavior is a previously omitted but important social element in models of individuals' consumption decisions.

My study contributes to the current literature in three ways. First, I enrich the behavioral economics literature by incorporating social network effects into models of self-control problems, which until now have focused on an individualistic perspective. This is the first paper to theorize and to empirically validate the social element in the self-control theory. Second, I construct refined social network information to produce empirical evidence that peer effects emerge as a result of social norms—people tend to conform with average temptation consumption behaviors among their peers. The empirical results contribute to development economics because the mechanism of peer effects has been largely overlooked in this literature, and researchers have often employed a relatively coarse definition of networks (e.g., ethnicity, last name, village). Third, within the policy discussion, these results deepen the understanding of consumption behavior among the poor and suggest policy applications for future financial instruments, as recent financial tools in the microfinance industry attempt to address the self-control problem. One example is a "commitment saving device," which has been shown to help people who are myopic save more (Ashraf et al., 2006). Another example is the establishment of local saving groups (e.g., self-help group[1] in India), which utilize a collective mechanism to overcome individual-level self-control limitations (Gugerty, 2007). The evidence in this paper suggests the need for caution when relying on peer effects to overcome moral hazard issues, because these effects may entail unintended consequences. Socializing with myopic peers can lead an individual to allocate his financial resources more myopically.

The organization of the paper is as follows: Section 1.2 provides a literature review, and highlights the gap in the existing literature. Section 1.3 describes the theoretical model and the testable predictions generated by the model. Section 3.4 outlines the data and the variables of interest. Section 1.5 explains the empirical strategy, while Section 3.6 discusses the empirical results. I rule out alternative risk-sharing model explanations, and discuss the results of several robustness checks in Section 1.7. Section 3.7 concludes.

[1] Self-help group (SHG) is an instrument employed to help villagers to save. The practice, originally promoted by local non-governmental organizations in India, has an anti-poverty agenda. SHGs usually comprise 10-20 people, and are mostly for women. Members make regular contributions to the group savings. When a group accumulates sufficient capital, members can borrow from the fund. SHGs aim to improve the financial situations of poor women and increase their economic mobility, especially in locations where formal financial institutions have little market penetration.

1.2 Literature Review

This paper contributes to the broader behavioral economics literature on time inconsistency and self-control. A number of studies apply hyperbolic or quasi-hyperbolic discounting to consumers' preferences to capture time inconsistency. This trade-off between near future and distant future accurately describes people's urge to consume items that give them immediate joy, rather than being patient and waiting for a greater reward in the future (Strotz, 1956; Phelps and Pollak, 1968; Thaler and Shefrin, 1981; Ainslie, 1992; Laibson, 1997; O'Donoghue and Rabin, 1999). Other researchers attribute this myopic behavior to a self-control problem—individuals are susceptible to different kinds of temptations. These models can also illustrate individuals' internal conflict between the present and the future self's interests, which causes them to behave as if they were myopic (Gul and Pesendorfer, 2001, 2004; Fudenberg and Levine, 2006; Banerjee and Mullainathan, 2010). However, the current literature on this myopic behavior is mainly based on individual psychological mechanisms. Battaglini et al. (2005) is one of the very few theoretical papers on self-control that models the influence of peers on individuals' self-control problem. Their model shows that individuals' self-control problem can be either worsened or improved by the peer effect depending on the type of person.[2] My research provides empirical evidence on this matter.

Another related strand of literature is on psychology and poverty. There is emerging research showing that poverty reduces cognitive resources and thus induces disadvantageous economic behaviors (Chemin et al., 2013; Haushofer, 2011; Haushofer et al., 2011; Haushofer and Fehr, 2014; Mani et al., 2013). For example, Chemin et al. (2013) find that rain deficits increase cortisol levels among farmers, especially those who are highly dependent on agriculture. Mani et al. (2013) also find shocking evidence that poor farmers' cognitive function decreases before the harvest cycle, as compared with the same farmers after the harvest, when they are rich. This is because poor farmers' mental resources are preoccupied with

[2] People who are the weak type (less resistant to self-control problems) are more susceptible to peer effects.

poverty-related concerns. Similar indications can be found in Shah et al. (2012), who show, through different experiments, that scarcity can consume mental resources. In this paper, I also find that in the face of negative income shocks, poor households' temptation consumption behaviors, which may be driven by their cognitive distress, are also more severe.

Until now, only a few studies try to add peer effects to individuals' financial behaviors in developing countries, mostly related to the usage of microfinance products. For example, Banerjee et al. (2013) worked with a local microfinance institution (MFI) in India to understand the effect of peers on microfinance take-up. They find that microfinance participation is highly influenced by information diffusion from the peers with higher eigenvalue centrality, which is a network theory-based measurement of the importance of a person. Breza (2011) used administrative data from an MFI in India. Her analysis indicates high peer loan repayment positively impacts individuals' loan repayment. Cai and Song (Cai and Song) study in China also shows a positive peer effect on insurance product take-up, mainly through information diffusion. Similar to Cai and Song (Cai and Song), Bursztyn et al. (2013) analyzed people's asset purchasing decisions by manipulating peers' asset purchasing information along with randomized purchasing opportunities to sort out social utility and social learning mechanisms underlying peer effects. These studies all provide insight into network effects on individuals' financial decisions. The mechanism of this observed peer effect, though, is mostly through information diffusion, which is not directly relevant for temptation goods. My research focuses on examining peer effects on consumption behavior through risk-sharing or peer pressure, and can add to this thread of literature by examining people's general problem in managing finances in developing countries. Previous studies, especially in the randomization setting, use limited peer definition for testing peer effects, while my study categorizes peer networks through different real transaction relationships throughout six years.[3]

[3] Only Banerjee et al. (2013) have collected network information using 13 dimensions, including people whom they go to their home, people whom they would borrow money from, people whom they would have lend material goods, etc. Breza (2011) refers to peers within the same microfinance group. Bursztyn et al. (2013) observe a pair of people who are previously socially connected, and investigate how investor 1's decision

The other related literature is peer effects on adolescence' risk taking behavior, such as smoking and alcohol usage (Alexander et al., 2001; Gaviria and Raphael, 2001; Duncan et al., 2005; Krauth, 2005; Nakajima, 2007; Kremer and Levy, 2008; Card and Giuliano, 2013; McVicar, 2012). My scope of analysis is to understand vulnerable populations in the developing countries, which may yield guidance on poverty reduction policies. My empirical strategy also differs from what is commonly applied in this literature by exploiting the excluded peer as an Instrumental Variable (IV).[4] Charles et al. (2009) and Chen et al. (2011) are the two papers that analyze social interaction on consumption behaviors among a broader population. Charles et al. (2009) find that consumption is a way for status seeking among the same racial group in the United States. They, however, use the same ethnic group as their network definition. Chen et al. (2011) also show status concern in rural China by analyzing people's gift-giving behaviors in special social occasions as weddings, childbirth ceremonies, and house-moving ceremonies. The contribution of my study is that I analyze detailed and diverse consumption categories with an important rural population in a developing country, and that I have social network data based on long-term real-world transactions.

This paper applies a methodologically innovative IV strategy proposed by Bramoullé et al. (2009) and De Giorgi et al. (2010). They prove that the information from the excluded network (i.e., people's friends' of friends, who are not directly linked with themselves) can be a feasible instrument to solve the reflection (Manski, 1993) and correlated effect (group shock) problems. Peer participation in certain activities is highly endogenous to individuals' participation—it is hard to rule out common unobservable shocks that may happen to a group. For example, in a classroom context, teacher quality is usually the group-level unobservable variable when studying peer effects on students' achievement. In my study,

may affect investor 2's. Cai and Song (Cai and Song) identify peers' learning effect through respondents' potentially listed five close friends.

[4]Among the literature on peer effects and youth's risk taking behaviors, only Duncan et al. (2005) and Kremer and Levy (2008) utilized a randomly assigned roommate situation to achieve a clean causal social interaction effect

as individuals do not directly interact with the excluded peers, they are not subject to any common group shocks. The only way the excluded peers can affect the individual's behavior is through the common peers they know. Another identification challenge, the reflection problem, rises when individuals are exactly the elements that compose the group—peer groups do not vary at the individual level. So the peer group's behavioral variable (e.g., consumption) cannot be separately identified from the exogenous covariates (e.g., average group characteristics). Studies using village, ethnicity, or race as the definition of network are not free from this criticism. With this regard, the non-overlapping network information across individuals in my study can prevent the reflection problem.

New studies take various approaches to the econometric problems. Banerjee et al. (2013) take advantage of a MFI's distributional algorithm information, which makes their identification less subject to this endogenous effect. In their study, since the MFI always targets the same type of people for distributing the initial information, this selection is independent from the social interaction within the village. Breza (2011) has an innovative identification strategy using the timing of the loan to instrument peer's incentive to repay. Cai and Song (Cai and Song) solve the endogeneity problem by conducting a randomized experiment where they offer a subset of farmers financial education and examine peer effects on those who are not treated. Bursztyn et al. (2013) also use randomization to understand how investors' asset purchasing decisions can be influenced by information and social utility. The present study does not benefit from randomization but presents a unique opportunity to apply the IV strategy using the information from the excluded network.

In conclusion, this study has several contributions: It adds an important piece to the behavioral economics literature on self-control problems by incorporating peer effects, which are often neglected. In addition, it is the first paper to empirically test peer effects on myopic consumption behaviors. There are very few studies looking at peer effects on individuals' spending behaviors in the developing countries; instead, most of them focus on behaviors in adopting/using microfinance products. My paper also analyzes diverse monthly consumption categories using social network information based on real long-term transactions. Previous studies either use a more coarse social network definition or analyze only a few consumption categories. Finally, the IV strategy plausibly resolves the endogeneity problem, helping to sort out one channel from the other.

1.3 Social Norm Model

This section presents individuals' consumption behaviors modified by a social norm model. In my model, individuals suffer from disutility when their temptation consumption deviates from the average peers' behavior. The model yields several predictions. First, an individual's temptation consumption is positively related with his peers'. Second, the observability of the goods matters in the social norm model. In addition, individuals' temptation consumption still comoves with their peers', even controlling for the total consumption of peers. Lastly, in the event of negative shocks, peers have positive effects on individuals' consumption.

1.3.1 Individual Maximization Problem

I assume that there is no information asymmetry within the network among different consumption goods because people in the same social network group have very close financial and social relationships. This assumption can be relaxed later by varying the observability of the goods.

The basic setup follows the model created by Banerjee and Mullainathan (2010). This model provides insights for understanding self-control problems through goods-specific preferences, and it yields similar predictions to a hyperbolic discounting model. Individual i maximizes a utility function that depends on two kinds of separable consumption—temptation goods (z_i) and goods without temptation (x_i). Temptations are consumption urges. For example, alcohol and tobacco are the type of goods that the present self would gain utility by consuming them, but do not gain utility from thinking about future self's consumption in them. This feature yields good-specific impatient behaviors biased toward the present since any temptation consumption left for the future would be viewed as a waste from the present self's point of view. Assuming a concave temptation function ($z(.)$), the model also implies different levels of myopia for the rich and the poor[5]—the poor behave as if they were more myopic than the rich.

To simplify the maximization problem, individual i lives for only two periods. There are no savings in the last period. The period 1 self maximizes $u(x_1)+v(z_1)+\delta u(x_2)$, where δ is the discount factor. The period 1 self gains utility from both goods consuming in the first period, but gets discounted utility from x goods consuming only in the second period. This setup fits the property of the temptation goods, which individuals cannot resist "now," but do not appreciate the future self to consume. The temptation goods generate utility only at the point of consumption. There is a disagreement of the composition of consumption between the current self and the future self. From period 1 self's point of view, any money left for temptation spending in the second period would be a waste.

Apart from utility gaining from consumption, individual i also cares about how he appears within a group. People worry about behaving differently than the majority. In other words, people gain "social rewards" by conforming with others. This conforming behavior is examined within the social group that people belong to. Thus, I use the deviation function, denoted as $\Phi(.)$, to capture the deviating payoff from the group behavior. The behavior of the majority can be viewed as a "social norm."

Therefore, individual i in a social network group g has the following maximization problem:

[5] I did not use the standard hyperbolic discounting model, or Battaglini et al.'s (2005) self-control model, because I do not have the direct behavioral variables to conduct relevant empirical tests derived from these models. In Battaglini et al.'s (2005) model, they separate people into different types—people with strong will who are less subject to self-control and people with weak willpower who more easily have self-control problems. They derive equilibrium group behavior by incorporating peer interactions into the model. This model is theoretically useful and related to my research question, but there is not enough information in these data to conduct empirical tests based on this model. At the same time, based on my fieldwork experience, the temptation framework is more reflective of the reality, which can also be viewed as an extreme version of hyperbolic preferences over temptation goods.

$$\max_{x_{1i},z_{1i}} u(x_{1i}) + v(z_{1i}) + \chi[\Phi(z_{1i}, \overline{z_{1-ig}})] + \delta u(x_{2i}(c_{2i})) \tag{1.1}$$

$$\text{s.t. } A_{2i} = (1+r)(\theta_{1i}y_{1i} - x_{1i} - z_{1i})$$

where $u'()$ and $v'() > 0$; $u''()$ and $v''() < 0$. At the same time, $v''()$ is assumed to be smaller than $u''()$. Both goods have a concave shape, but temptation goods have a more concave shape than non-temptation goods. It means that, as income/consumption increase, the marginal utility from temptation goods decreases much faster for temptation goods than non-temptation goods. This assertion indicates that the proportional spending on temptation goods over total spending should decrease as the total consumption increases. Temptation goods give people large marginal utility for the first few units (say, drinking sips of alcohol or eating a portion of a donut), but the marginal utility decreases drastically after the immediate urge is satiated.

In the constraint equation, A_{2i} is the savings available for the second period; r is the asset return; c_{2i} is the total consumption in the second period; y_{1i} denotes i's income at period 1; θ_{1i} represents exogenous idiosyncratic shock on i's income at period 1. In the second period, the period 2 self will maximize utility from consuming both goods and deviation payoff as defined before. At the last period, this consumption decision is subject to a budget constraint (i.e., $z_{2i}+x_{2i} = c_{2i}$, where $c_{2i} = A_{2i}+y_{2i}$). I can also write x_{2i} and z_{2i} into functions $x_{2i}(c_{2i})$ and $z_{2i}(c_{2i})$. χ describes the observability of the behavior, and is positive. The third term is associated with the payoff of self-image. $\overline{z_{1-ig}}$ is the average temptation consumption of i's group member at period 1 except individual i's. Here, I assume that people weight each member's behavior in the group equally. In other words, they would like to appear to be social by acting in line with the group expectation. Peer's temptation consumption is assumed to be exogenous, and depends on the income shock of the social network group. The assumption of this deviation function is that $\frac{\partial\Phi(z_i,\bar{z}_{-ig})}{\partial|z_i-\bar{z}_{-ig}|} < 0$—the more individual i deviates from the group behavior, the larger the disutility is.

To simplify the maximization problem, let $\Phi(z_i, \overline{z_{-ig}})$= - $\frac{1}{2}(z_i - \overline{z_{-ig}})^2$. This functional form is also used in Akerlof and Kranton (2002), where it captures student's utility loss from deviating from the predetermined ideal effort of the social category they belong. If the majority of group members consume a great deal of temptation goods, individual i will have an undesirable feeling about himself if he consumes a small amount. The quadratic form weights deviation above and below equally, and can be imagined as social distance. Thus, if the behavior is highly observable (χ is large), an individual's temptation consumption is expected to be in accordance with his peers' behavior. The maximization problem can be written as

$$\max_{x_{1i},z_{1i}} u(x_{1i}) + v(z_{1i}) + \chi \left[-\frac{1}{2}(z_{1i} - \overline{z_{1-ig}})^2 \right] + \delta u(x_{2i}(c_{2i})) \tag{1.2}$$

$$\text{s.t. } A_{2i} = (1+r)(\theta_{1i} y_{1i} - x_{1i} - z_{1i})$$

Because $x_{2i}(c_{2i}) = x_{2i}(A_{2i} + y_{2i}) = x_{2i}[(1+r)(\theta_{1i} y_{1i} - x_{1i} - z_{1i}) + y_{2i}]$, and at the same time, $z_{2i} + x_{2i} = c_{2i}$, the first-order conditions with respect to z_{1i} and x_{1i} are:

$$v'(z_{1i}) - \chi(z_{1i} - \overline{z_{1-ig}}) + \delta u'(x_{2i}) \left(\frac{\partial x_{2i}}{\partial c_{2i}} \right) \left(\frac{\partial c_{2i}}{\partial z_{1i}} \right) = 0 \tag{1.3}$$

$$u'(x_{1i}) + \delta u'(x_{2i}) \left(\frac{\partial x_{2i}}{\partial c_{2i}} \right) \left(\frac{\partial c_{2i}}{\partial x_{1i}} \right) = 0 \tag{1.4}$$

Assuming a constant absolute risk aversion (CARA) functional form helps clarify the comparative static. $u(x) = -\frac{1}{\theta_x} e^{-\theta_x x}$ and $v(z) = -\frac{1}{\theta_z} e^{-\theta_z z}$. In addition, since $\frac{\partial c_{2i}}{\partial z_{1i}} = -(1+r)$ and $\frac{\partial x_{2i}}{\partial c_{2i}} + \frac{\partial z_{2i}}{\partial c_{2i}} = 1$, equation 1.3 becomes

$$z_{1i} - \frac{1}{\chi} e^{-\theta_z z_{1i}} = \overline{z_{1-ig}} - \frac{1}{\chi}(1+r)\delta e^{-\theta_x x_{2i}} \left(1 - \frac{\partial z_{2i}}{\partial c_{2i}} \right) \tag{1.5}$$

1.3.2 Predictions

The model generates the following comparative statics, where the full proofs refer to Section 1.9.

Prediction 1: *An increase in peers' temptation consumption will lead to an increase in individual i's temptation consumption as long as the behavior is observable* $\left(\frac{\partial z_{1i}}{\partial \overline{z_{1-ig}}} > 0\right)$ *if* $\chi > 0$.

The main interest here is to analyze $\frac{\partial z_{1i}}{\partial \overline{z_{1-ig}}}$. The prediction is driven by the deviation function. As long as the consumption behaviors are observable, an increase in peers' temptation consumption will lead to an increase in individual i's temptation consumption because people suffer from behaving differently from their group norm.

Prediction 2: *Peer effect is stronger in temptation consumption, rather than in non-temptation consumption* $\left(\frac{\partial z_{1i}}{\partial \overline{z_{1-ig}}} > \frac{\partial x_{1i}}{\partial \overline{x_{1-ig}}}\right)$.

On the contrary, individuals' non-temptation consumption is not affected by their peers based on the implication of equation 1.4. This prediction is straightforward by my model construction. Suppose that peers' consumption on temptation ($\overline{z_{1-ig}}$) and non-temptation goods ($\overline{x_{1-ig}}$) are exogenous, individual's non-temptation consumption would not be affected by their peers.

Prediction 3: *Peer effects on temptation consumption are stronger when peers' consumption behaviors are more observable* $\left(\frac{\partial^2 z_{1i}}{\partial \overline{z_{1-ig}} \partial \chi} > 0\right)$.

This observability can be used to distinguish the magnitude of peer effects between consuming different types of goods. If peers' temptation consumption behaviors are more observable (higher χ), individuals' temptation consumption correlates more with their peers'. Based on the model prediction, social norms do not apply universally, but seem to be attached with the visibility of that behavior.

Prediction 4:

When individuals are poor, negative idiosyncratic shocks will increase total consumption ($\frac{\partial z_{1i}}{\partial \theta_{1i}} < 0$, and $\frac{\partial x_{1i}}{\partial \theta_{1i}} < 0$ as c is small);

If one poor peer encounters adverse shock, other things being equal, this negative peer's shock has a positive impact on temptation consumption.[6]

Another focus is the comparative static of consumption with respect to shocks – θ_{1i}. Assuming θ_{1i} is exogenous, it is possible that an individual would consume more temptation goods when encountering negative income shock. That said, $\frac{\partial z_{1i}}{\partial \theta_{1i}} < 0$ over a certain range of consumption. The reason for this property can be seen from equation 1.3 without applying any functional form in the mathematical appendix in Section 1.9.

The intuition can be viewed as increasing psychological barriers for the poor. The negative shock would make poor people be more desperate, and less patient in consuming more now, rather than saving for the future. Many studies have found that poverty (or broadly speaking, scarcity) is associated with higher stress level, leading to worse cognitive performances (Haushofer, 2011; Haushofer et al., 2011; Shah et al., 2012; Chemin et al., 2013; Mani et al., 2013).

Following a similar logic, if one poor peer encounters negative income shock, assuming other things being equal, this effect will push up peers' average temptation consumption. Based on prediction 1, this increase in peers' average temptation consumption will further increase own temptation consumption.

In conclusion, I will be able to distinguish the mechanisms using the following predictions (an alternative risk-sharing mechanism is presented in the robustness check section. The comparison of predictions is in Table 1.6): (1) Peer effects happen mainly through temptation consumption. After controlling for peers' total consumption, peer effects on temptation

[6] I can show this intuition based on specific assumptions, but the aggregate effect of peers' shock cannot be generally proved.

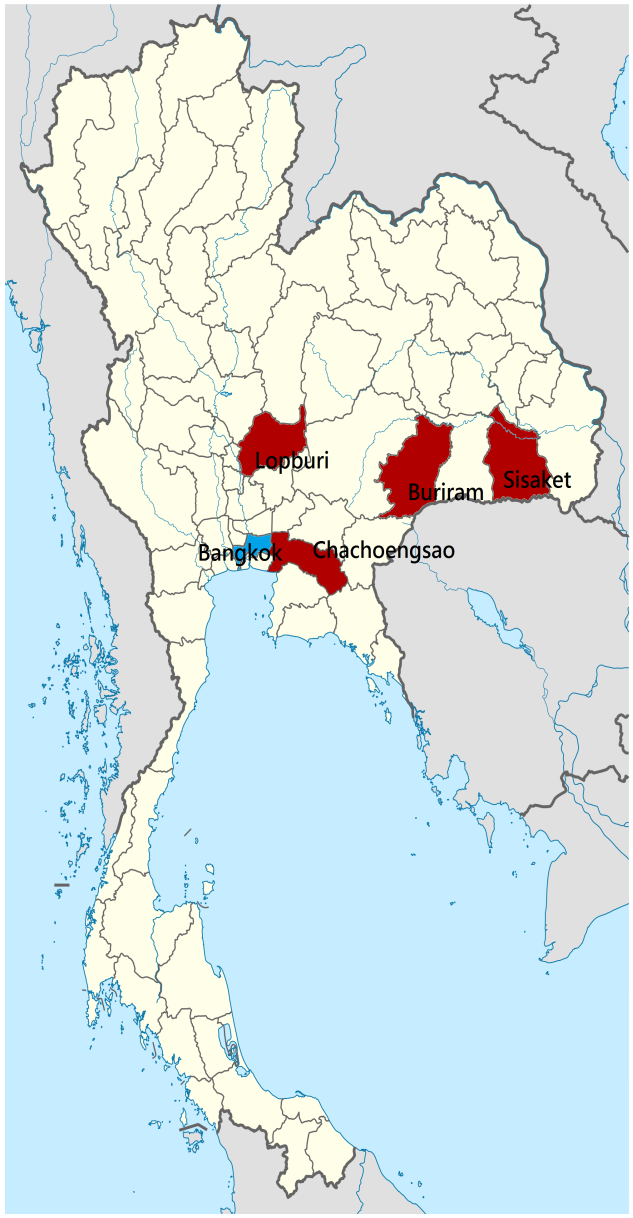
consumption should still be significant based on the social norm model. (2) The peer effect is stronger in temptation goods than in non-temptation goods. (3) The observability of consumption should matter if peer effects are through social norms. (4) Individuals' negative shock will have a counterintuitive positive effect on consumption because of the concave shape of temptation consumption among the poor. Poor peers encountering negative shocks should also create a similar positive effect on temptation consumption through social norm mechanism.

1.4 Dataset and Variables of Interest

1.4.1 Dataset Description

The study uses data from the 1999 to 2004 monthly waves of the Townsend Thai Monthly Survey. The continuously observed sample size is 480 in all 72 months. The survey was conducted in 16 villages, four in each of four separate provinces. As Figure 3.1 shows, two provinces (Chachoengsao and Lopburi) are close to Bangkok, and the other two (Buriram and Sisaket) are in the northeastern rural region close to the Cambodian border. The success rate of the survey (the number of households that were successfully surveyed out of the total number of households in each month) is at least 93%. However, because some households migrate permanently during the survey period, they are replaced by other randomly selected households in order to make the sample representative of the village.

Figure 1.1: Map of Thailand with Surveyed Provinces



The data include households' demographic characteristics, expenditure, and income. There is also detailed information on financial, gift exchange, and labor-sharing relationships. All these transactional relationships are time-varying. The monthly temporal scale is a valuable feature of the dataset since consumption data are difficult to recall, and the frequent data collection reduces measurement error. In addition, the expenditure information is comprehensive, including categories such as various food items, oil and fat, sugar and sweet, beverages, alcohol, tobacco, gambling, etc.

1.4.2 Social Network Data

One of the main strengths of this study is the actual, rather than a proxy for, social network with whom people truly interact. I categorize household-level social networks using their transactions, including borrowing and lending, gift-giving, and labor sharing[7] over a long period of time. Households who have had any of these relationships within the survey period are categorized as being connected. The social network is defined by the aggregation of all the transaction relations a household i has through financial relationships, gift exchange, and labor-sharing relationships over the survey period.

This time-invariant definition captures all the social relations people may have even though there is no transaction observed in a specific period. I construct a matrix called $\mathbf{G}$, where $\mathbf{G}_{ij} = 1$ if household i is linked with j, for any $j \neq$ i. Since there is no further information to establish the weight of the peers, I put the same weight on each linked pair. Here I assume symmetry ($\mathbf{G}_{ij} = \mathbf{G}_{ji}$). If a household is linked in one direction, I assume that they can be linked in the other way around. For example, i reports that he/she has borrowed from j, so j should be within i's social network ($\mathbf{G}_{ij} = 1$). However, it may happen that j did not report i in any of the social relations. It is very likely that i is indeed within j's social network as well, but j forgets to report his relationship with i. It is less possible that i lies about his relationship with j. Although Schechter and Yuskavage (2011) show empirically that social networks with reciprocated relationships may have different features from those with unreciprocated relationships, their result does not provide a prior on how this might affect temptation consumption. Even if a social network with unreciprocated relationships has weaker social norm effect among linked pairs, assigning pairs with unreciprocated relationships the same weight as that with reciprocated relationships will only underestimate

[7] According to the data, households exchange labor or offer free labor to others in different business activities. These labor-sharing activities happen between neighbors, relatives, and friends. This information helps to capture the peers with whom households have close relationship.

our peer effect.

1.4.3 Key Variables of Interest

The key outcome variable is the expenditure on temptation goods. Since the detailed monthly survey provides the possibility of separating consumption into different categories, I use household's expenditure on alcoholic beverages (at home), alcoholic beverages (consumed away from home), tobacco, lottery, and gambling.

The key explanatory variable is consumption spending of the people within the network. I calculate mean temptation consumption within household i's network ($\overline{z_{-ig}}$) as the proxy for this. The mean temptation consumption for household i's network is the aggregate household j's temptation consumption conditional on the information of $\mathbf{G}$ and divided by the network sample. Other explanatory variables, for example, peers' shock variable, are defined similarly. Peers' health shock, which is used as a proxy for income shock, is the aggregate household j's days of sickness per capita conditional on the information of $\mathbf{G}$ and divided by the network sample size.

1.4.4 Summary Statistics

Summary statistics from the Thai dataset are presented in Table 1. It is worth noting that households spend a significant amount on temptation goods, which consists of seven percent of total consumption on average. The yearly expenditure on temptation goods is equivalent to households' average yearly spending on education. Figure 1.3 shows that there is variation among different households in terms of their spending on temptation goods. Figure 1.4 further confirms that the assumption of a concave shape of temptation goods is reasonable (i.e., $z''(c) < 0$). The poor appear to be more myopic than the rich. The figure shows that the proportional spending on temptation goods over the total consumption is decreasing with respect to income level.

Among the total 480 observations, 374 people can be linked with at least one peer within the same tambon (an administrative level above village). On average, the network size is five, mostly neighbors and relatives.

Table 1.2 shows simple correlations of the characteristics between villagers and their peers. People within the same network have similar income level, household size, and percentage of their agricultural income. The correlation on the percentage of agricultural income is especially strong. This implies that people tend to have networks composed of individuals with the same occupation. This may be because people who have labor-sharing relationships are specialized in the same economic activity. In terms of idiosyncratic health shock, peers' health shock is much less correlated.

1.5 Empirical Strategy

1.5.1 General

The focus of the analysis is the relationship between peers' and individuals' spending on temptation goods. The equation of interest is

$$temp_{ivst} = \alpha_0 + \alpha_1 temp_{G_i vst} + \alpha_2 X_{ivst} + h_i + season_s + f_{vt} + \epsilon_{ivst} \tag{1.6}$$

$temp_{ivst}$ is the per capita monthly consumption of temptation goods of household i in village v season s at time t, with a peer group G_i, on alcoholic beverages (at home), alcoholic beverages (consumed away from home), tobacco, lottery, and gambling. $temp_{G_i vst} = \frac{\sum_{j \in G_i, j \neq i} temp_{jvst}}{N_{G_i}}$ is the average consumption of temptation goods of i's peer group net of i's spending; N_{G_i} is the number of peers of household i, which is a fixed composition over time. The group-level temptation consumption does not include self's consumption. Mace (1991) uses this same strategy to test risk-sharing theory.[8] Here because of CARA definition, as well as all the zeros in temptation consumption, all the consumption variables are in levels instead of logs.

[8] It is comparable at the end to rule out risk-sharing explanation using the same specification, but different empirical predictions (see more in Section 1.7.1).

X_{ivst} is a vector of controls for household characteristics. ϵ_{ivst} is the error term.

I further control for different fixed effects. h_i are household fixed effects in order to control for time-invariant household fixed demographic characteristics. $season_s$ are seasonal fixed effects, which can eliminate any seasonal consumption pattern that could be confounded with the peer effects of interest; for example, people may consume more alcohol during a certain festival that happens at a certain season of the year. Village-year fixed effects (f_{vt}) are also taken into account to prevent from capturing a systematic consumption pattern at the village-year level. After controlling for these necessary covariates, my identification comes from a household's peers' time-variant change in consumption within the same village-season-year.

The parameter of interest is α_1, which is expected to be greater than zero. However, α_1 may not be identified under this equation because of endogeneity. This reflection problem may happen when the endogenous effect $temp_{G_i}$ is a linear combination of all other regressors, and thus the endogenous effect is entangled with the exogenous effect[9] (Manski, 1993; Brock and Durlauf, 2001). For example, if people within a small village are peers, we will not be able to identify α_1 because the group characteristics cannot be distinguished from the endogenous group behavior. In that case, α_1 cannot be distinguished from α_2, and is not identified.

In addition, it is likely that people select their peers/friends. The peer effect may be subject to unobservable individual characteristics because individuals' decisions on peer selection will explain why they behave similarly to their peers. For example, if individuals with self-control problems like to be with people who consume a great deal of temptation goods, researchers may mistakenly think that peers' behaviors have perverse effects on individuals. The other issue is the omitted group-level unobservables. This can be viewed as a correlated effect. That is, the observed network effect may simply confound with the common group shock that network members encounter and cannot be observed by econometricians. For

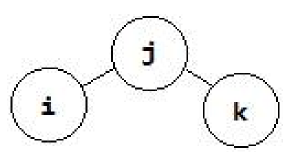
[9]The exogenous group mean effect is also called contextual effect in the literature.

example, if people within the same social network celebrate a special event that may affect everyone's consumption behavior. Econometricians failing to observe that common group shock may misinterpret this consumption comovement as peer effects. In addition, the decision of individuals and the peers' decision can be made simultaneously, which can also lead to the failure of identification.

1.5.2 Instrumental Approach

De Giorgi et al. (2010) and Bramoullé et al. (2009) propose an innovative approach to solve the problem of reflection and endogeneity. The main idea is to use household i's excluded peers as an IV strategy. As Figure 1.2 shows, households i and j interact with each other; households k and j interact with each other, but households k and i do not interact with each other. i's peer group (defined as G_i) includes all j. The excluded peer, household k, is in the network group with j, but not in the network group with i. Thus, i's excluded peer group (defined as K_i) includes all k, where k has to satisfy $k \in G_j$ and $k \notin G_i$. The information of the excluded peer group K_i can thus be used as the instrument since j's peer group does not coincide with i's peer group.

Figure 1.2: Network illustration



As long as individuals' relevant peer groups are not totally overlapped, I can identify peer effect using this strategy. If all social groups have the same size, or totally overlapped, peer effects are not identified (Bramoullé et al., 2009). Since in my analysis each individual has different peer groups and each group has different sizes, $temp_{G_i}$ cannot be a linear combination of all other regressors. This solves the reflection problem. In the final analysis, I assume that peers' characteristics do not affect the individual's consumption behavior because based on the theory, individuals care only about deviating from their peers' behaviors, and do not care about peers' type.

In addition, this strategy can eliminate the correlated effect because i and the excluded peer k do not directly interact with each other. The excluded peer will not be correlated with the unobservable shocks at the group level as the excluded group K_i is not subject to the unobservable effect within i's network. This property makes the excluded peer's information fulfill the exclusion restriction requirement. Even under the weaker assumption that i and j have a stronger interaction with each other than i and k, the peer effect can still be identified. De Giorgi et al. (2010) show that even with some extent of measurement error (i.e., k may in fact interact with i), the estimation is still unbiased.

To further address the simultaneity problem, I use lagged consumption behaviors as the instrument. It is plausible to assume that an individual's contemporary decision cannot affect peers' previous consumption. Also, for the lagged instrument to work, I need an assumption that this spillover effect of consumption behaviors take some time for one to adopt. The monthly lag is a reasonable assumption because empirical data shows that consumers' utility can exhibit some level of habit formation—a theory which captures the fact that current utility depends on current consumption relative to the lagged consumption, and thus cause the delay of consumption response to shocks (Fuhrer, 2000). I use habit formation to justify my empirical strategy, but do not explicitly incorporate it into the theoretical model because this part of modeling is beyond the scope of this paper. Nevertheless, I test this assumption using a more symmetric time frame in the robustness check section.

The first-stage regression for the peer group is

$$temp_{G_ivst} = \beta_0 + \beta_1 Z_{K_ivst-1} + \beta_3 X_{ivst} + h_i + season_s + f_{vt} + \eta_{G_ivst} \tag{1.7}$$

where $temp_{G_ivst}$ is the average spending amount on temptation goods of i's peer group G_i in village v season s at time t; Z_{k_ivst} is the average temptation consumption of individual i's excluded peer group K_i in village v season s at time $t-1$; X_{ivst} are appropriate household controls; h_i are household fixed effects; $season_s$ are seasonal fixed effects; f_{vt} are village-year fixed effects; and η_{G_ivst} is the error term.

The second-stage regression is

$$temp_{ivst} = \delta_0 + \delta_1 te\hat{mp}_{G_ivst} + \delta_2 X_{ivst} + h_i + season_s + f_{vt} + \varepsilon_{ivt} \tag{1.8}$$

$temp_{ivst}$ is the per capita monthly temptation consumption of household i in village v season s at time t. h_i are household fixed effects; $season_s$ are seasonal fixed effects; f_{vt} are village-year fixed effect. The rest of the variables are the same as in equation 1.9. The main interest is δ_1, which is hypothesized to be greater than zero.

1.5.3 Empirical Predictions for Social Norm Mechanism

In addition to the prediction on δ_1, the theory also generates several other predictions, which I reiterate in this section. All the regressions are estimated using the instrumental technique proposed in Section 1.5.2.

Peer effects on temptation: Based on Prediction 1, peers' temptation consumption should affect individual's. $\delta_1 > 0$ in equation 1.8. This peer effect should still be significant even after controlling for peers' total consumption. This property can be helpful to distinguish from the alternative mechanism: risk sharing. The predictions of the alternative risk-sharing theory will be presented in Section 1.7.1. For example, I estimate the following specification:

$$temp_{ivst} = \gamma_0 + \gamma_1 temp_{G_ivst} + \gamma_2 cons_{G_ivst} + \gamma_3 X_{ivst} + h_i + season_s + f_{vt} + \varepsilon_{ivst} \tag{1.9}$$

where $cons_{G_ivst}$ is the average per capita monthly total consumption of household i's peer group G_i in village v season s at time t. Therefore, $\gamma_1 > 0$.

Non-temptation consumption v.s. temptation consumption: Replacing temptation consumption with non-temptation consumption in equation 1.6 can also help distinguish motivations. Based on Prediction 3, the coefficient of peers' temptation consumption should be greater than that of peers' non-temptation consumption if the mechanism is through social norm. The logic here is that the social-norm model predicts that people imitate peers' temptation consumption, rather than regular (non-temptation) consumption. Run the following regression:

$$nontemp_{ivst} = b_0 + b_1 nontemp_{G_i vst} + b_3 X_{ivst} + h_i + season_s + f_{vt} + \xi_{ivst}$$

where $nontemp_{ivst}$ is the per capita monthly non-temptation consumption of household i in village v season s at time t, and $nontemp_{G_i vst}$ is the average per capita non-temptation consumption of household i's peer group G_i in village v season s at time t. b_1 is expected to be less than δ_1.

Observability: According to Prediction 3 from my model, peer effects are stronger for temptation goods that are more observable. Higher observability (χ) of peers' temptation consumption may induce a larger conformity effect on own temptation consumption because of the larger utility loss of deviating from others. For example, alcohol consumption outside is more observable than alcohol consumption at home.

$$alcoholTOTAL_{ivst} = \gamma_0 + \gamma_{temp_H} alcoholHOME_{G_i vst} + \gamma_3 X_{ivst} + h_i + season_s + f_{vt} + \varepsilon_{ivst}$$
$$alcoholTOTAL_{ivst} = \gamma_0 + \gamma_{temp_O} alcoholOUT_{G_i vst} + \gamma_3 X_{ivst} + h_i + season_s + f_{vt} + \varepsilon_{ivst}$$

where $alcoholHOME_{G_i vst}$ is the average per capita alcohol consumption at home of household i's peer group G_i in village v season s at time t; $alcoholOUT_{G_i vst}$ is the average per capita outside alcohol consumption of household i's peer group G_i in village v season s at time t; $alcoholTOTAL_{ivst}$ is household i's total alcohol consumption, including at home and outside, in village v season s at time t.

In the above equation, the coefficient of peers' temptation consumption outside should be greater than that of peers' temptation consumption at home because the former is more observable than the latter. Thus, γ_{temp_O} is expected to be greater than γ_{temp_H}.

I also run similar specification, but using $alcoholHOME_{ivst}$ as the dependent variable, where $alcoholHOME_{ivst}$ is household i's per capita alcohol consumption at home in village v season s at time t. This specification is to test whether this consumption norm has spillover effects on households' own alcohol consumption at home. I expect similar prediction that γ_{temp_O} is greater than γ_{temp_H}.

Shock event: Idiosyncratic shocks cause different effects on individual's consumption (Prediction 4 in Section 3.2). In the social norm model, the shape of the temptation would matter because people face trade-offs between the present and the future period. At the consumption level where individuals are myopic, positive (negative) shock would have a negative (positive) effect on consumption, especially for the poor (i.e., $\beta_{temp2} > 0$, $b_{nontemp2} > 0$). Here the larger the shock variable ($shock_{ivst}$), the worse the shock is. At the same time, poor peers' shock would have the same effect on temptation consumption through social norms mechanism (i.e. $\beta_{temp1} > 0$):

$$\begin{aligned} temp_{ivst} =& \beta_0 + \beta_{temp1} shock_{G_i vst} + \beta_{temp2} shock_{ivst} + \beta_{inc} poor_{ivst} \\ & + \beta_c poor_{ivst} shock_{ivst} + \beta_3 X_{ivst} + hi + season_s + f_{vt} + \epsilon_{ivst} \end{aligned}$$

$$\begin{aligned} nontemp_{ivst} =& b_0 + b_{nontemp1} shock_{G_i vst} + b_{nontemp2} shock_{ivst} + b_{inc} poor_{ivst} \\ & + b_c poor_{ivt} shock_{ivst} + b_3 X_{ivst} + hi + season_s + f_{vt} + \epsilon_{ivst} \end{aligned}$$

where $shock_{ivst}$ is per capita average days of health shock of household i in village v season s at time t, $shock_{G_i vst}$ is the aggregate days of health shock among household i's peers G_i who are under the poverty line in village v season s at time t, excluding household i's own shock, and $poor_{ivst}$ is household i's poverty status in village v season s at time t. Notice that I do not further control for the number of friends, because it does not change over time and

I have controlled for household fixed effects. But peers' poverty status can be different over time, so I further control for the time-varying number of poor peers as a comparison.

Since idiosyncratic shock has a positive impact on people's consumption when people are poor enough, the shock and poor interaction term should be positive ($\beta_c > 0$ and $b_c > 0$). Poor people appear to be more myopic so that shock would have a positive impact on their consumption.

1.6 Empirical Results

Almost all the results using the instrumented social network information support the theory of social norm. However, in some of the cases, the instrument, unfortunately, does not have very high F-statistics in the first stage. For example, in the table analyzing peer effects on temptation and non-temptation consumption. The F-statistics in the first stage are not high because peer effects do not happen in non-temptation consumption. With respect to the issue of weak instrument, I further use the Conditional Likelihood Ratio (CLR) test to report the robust confidence intervals under weak instrument. According to Andrews et al. (2008), CLR test is more optimal than Anderson and Rubin (AR) statistics and LM statistics, which are both robust statistics under weak instrument. The results using instrumental variables and CLR tests are similar to that using OLS. Even though some observations are missing using the excluded network as instruments, this consistency yields high confidence of the results.[10]

1.6.1 Peer Effects on Temptation and Non-temptation

Table 1.3 presents the OLS and IV results. The coefficient in column 3 of Table 1.3 indicates that own temptation consumption is affected by peers, and the magnitude of peer effects

[10]In order to use a friend of a friend as the instrument, there should exist such kind of third person k between two people, say, i and j. However, there is a missing instrument for the case when i is the only friend of j, and at the same time, j is the only friend of i.

on temptation consumption is also remarkable. One extra baht of peers' average monthly spending on temptation goods can lead to 1.5 bahts of individual's temptation consumption in the IV specification using clustered standard errors, wild clustered bootstrap adjustment, and robust standard errors without clustering (not shown here). In order to be consistent with the CARA utility function shown in the model, I use the level of consumption instead of log measures[11]. Because of the weak instrument, I further test the results using the Conditional Likelihood Ratio test, which reports reliable results under a weak instrument. The results remain robust as the CLR test suggests positive confidence intervals.

The coefficients in the IV specification are higher than the OLS coefficient. It means that the correlated effect (in the disturbance term) that OLS coefficients pick up actually runs in the opposite direction from the peer effect. The higher IV is not unique in this study as De Giorgi et al. (2010) also found this similar result. They explain that each unobservable common shock can have a different sign, so OLS coefficients are not unambiguously larger than the IV estimators. In addition, the peer group is not perfectly overlapped, so the simultaneity issue is much eliminated in the OLS case compared with using a totally overlapped social network definition[12]. Columns 1 to 4 show that the coefficient of peers' temptation consumption is higher than that of peers' non-temptation consumption. These results corroborate the social norm mechanism that individuals feel bad about deviating from the average temptation consumption of their peers.

Columns 5 and 6 of Table 1.3 present the consumption relationship between own and peer, but controlling for peers' total consumption. This test aims to rule out the alternative risk-sharing hypothesis where peer effects should go away once controlling for peers' total consumption (a detailed explanation of the prediction on alternative risk-sharing mechanism is presented in Section 1.7.1). The results serve as another piece of evidence to support social norm mechanism: peer effects on temptation consumption remain positive and sig-

[11] Using log measures yields qualitatively similar results.

[12] For example, if one uses village as the social network definition, then, within a network, everyone's social network overlaps entirely. The aggregation of each individual within the network group comprises the group itself.

nificant when controlling for peers' total consumption. The coefficient on peers' temptation consumption is around 1.6. The coefficient on peers' non-temptation consumption is much smaller and insignificant controlling for peers' total consumption. All the results in Table 1.3 are consistent with Predictions 1 and 2 in social norm theory.

1.6.2 Observability

Table 1.4 presents peer effects of alcohol consumption at home versus alcohol consumption outside. The result supports Prediction 3 in the social norm theory—the peer effect is much more significant among more observable consumption. Columns 1 to 4 show the effects of peers' alcohol consumption outside versus peers' alcohol consumption at home on household's total alcohol consumption. Columns 1 and 2 present the results from OLS specification, and columns 3 and 4 present the results from IV specification. The results indicate that the coefficients of peers' alcohol consumption outside are stronger than that of peers' alcohol consumption at home—consistent with the social norm theory. It is worth noting that the instrument on peers' alcohol consumption at home is relatively weak, and therefore the coefficient may be inflated. The weak instrument issue is not worrisome nonetheless because peers' alcohol consumption at home is less observable and thus generate smaller peer pressure. By comparing the OLS coefficients in columns 1 and 2, I am confident that peers' alcohol consumption outside have qualitatively stronger influence than peers' alcohol consumption at home. Columns 5 and 6 are the coefficients of peers' alcohol consumption on household's home consumption. As expected, columns 5 and 6 have similar results as in columns 3 and 4, given that this social norm of peers' drinking behavior should have spillover effect on household's home alcohol consumption. Columns 7 and 8 present similar analysis as in columns 3 and 4, but controlling for peers' total consumption. The coefficient on peers' alcohol consumption outside is qualitatively larger and more statistically significant than that at home after controlling for peers' total consumption. Overall, one extra baht of peers' monthly spending on alcohol outside is associated with 4.3 baht of individual's monthly spending on total alcohol. Since alcohol consumption outside is likely to be more observable than alcohol consumption at home, the results verify that the deviation function plays a more important role in maximizing individual utility when peers' behaviors are more observable.

1.6.3 Shock Event

Table 1.5 presents the effect of peers' idiosyncratic shock on consumption patterns. Here health shock is the proxy for income shock, and is measured as total days of sickness of the household[13]. So the larger the number, the more adverse the shock is. As income may be endogenous to the consumption pattern, health shocks can capture a more exogenous variation. Overall, people's consumption pattern in the event of health shocks also supports the predictions in the social norm theory. Since peers' shock variable is not subject to the simultaneity problem, I use the contemporaneous shock variable of i's excluded network to instrument peer effects (the signs and magnitude are the same using shock variables at period $t-1$ as instrument). As health shocks are idiosyncratic and people are less subject to correlated effect, I also present the non-instrumented OLS result as comparison.

According to Prediction 4 in the social norm theory, poor peers' negative shock should have positive effects on own temptation consumption through the conformity effect. The first row in columns 1 3 and 5 should be, in theory, positive and significant. As expected, both of these coefficients are positive. The coefficient in the IV specification is significantly different from zero. Notice that peers' adverse shock has much greater positive impact on own temptation consumption than that in household's non-temptation consumption. The difference between columns 3 and 4 and columns 5 and 6 is the extra control for time-varying number of poor peers. Although the number of poor peers may be endogenous, these results in columns 5 and 6 help me to validate that the results in row 1 are not mainly driven by those who have more poor friends in their network. In conclusion, one extra day of

[13] Health shock is significantly correlated with income. One percentage increase of sickness decreases income by three percent.

poor peer's sickness within a month can increase household's per capita monthly temptation consumption by one bhat.

Furthermore, own health shock should have a positive effect on both temptation and non-temptation consumption among the poor, meaning that the interaction term of poverty status and health shock in row 4 should be positive. Table 1.5 shows that poor households appear to be more myopic by consuming more temptation goods, relative to the rich. The positive effect of negative shocks on consumption would be more true among the poor than the rich. In the results using both OLS and IV, the coefficients on $poverty_{ivt} * shock_{ivt}$ in columns 1 and 3 are positive among temptation consumption; however, the coefficients on $poverty_{ivt} * shock_{ivt}$ in columns 2 and 4 are negative among non-temptation consumption. These results indicate that, in the event of negative shocks, the poor would choose to spend much less in non-temptation consumption relative to the rich, while spending more on temptation consumption compared with the rich. Poor households seem to be less resistant to temptation goods. If we view consuming temptation goods as a sign of impatience, the evidence slightly supports income heterogeneity of the myopic behavior. Take column 3 for example, one extra day of sickness can decrease rich households' temptation consumption by 0.175 bahts, while one extra day of sickness only decreases temptation by 0.0825 bahts among the poor households.

1.7 Robustness Check

1.7.1 Alternative Model: Risk-Sharing Model

This section contrasts the social norm explanation with an alternative mechanism that could explain the comovement of consumption: risk sharing. A household's social network provides risk-sharing function, which makes people borrow and lend from the same pool of money. There may be a risk of treating risk sharing as peer effects. I present the comparable model of risk sharing, and argue that the social norm model can better explain the observed consumption pattern.

I present here a modified version of Townsend (1994)'s model in order to contrast its predictions with that of the model predicted in Section 1.3. The individual utility function still depends on temptation goods (z_i) and goods without temptation (x_i). There are different states (s)—good income shock or bad income shock, which happens with probability Π_{st} at time t. λ_i is the weight associated with different individuals, and $\sum_i \lambda_i = 1$. β^t is the discount factor at time t. In addition, there is no saving available. There are N individuals ($i = 1, 2, \ldots, N$ within the risk-sharing network, where i denotes different individuals), and thus $\sum_{i=1}^{N} \lambda_i = 1$. The maximization problem is similar to a social planner's problem, and can be written as:

$$\max_{x_{ist}, z_{ist}} \sum_{s=1}^{S} \Pi_{st} \sum_{t=1}^{T} \beta^t \sum_{i=1}^{N} \lambda_i [u(x_{ist}) + v(z_{ist})] \tag{1.10}$$

$$\text{s.t. } \sum_{i=1}^{N} [x_{ist} + z_{ist}] \leq \sum_{i=1}^{N} \theta_{ist} y_{ist}$$

where β^t is the discount factor at time t; y_{ist} is i's income at state s time t; θ_{ist} represents i's shock on income at state s time t. The first-order condition from the maximization problem yields:

$$(x_{ist}) : \lambda_i \Pi_{st} \beta^t u'(x_{ist}) = \mu_{st}, \forall i, s, t \tag{1.11}$$

$$(z_{ist}) : \lambda_i \Pi_{st} \beta^t v'(z_{ist}) = \mu_{st}, \forall i, s, t \tag{1.12}$$

where μ_{st} is the Lagrange multiplier. The above solution further yields:

$$\frac{\lambda_j}{\lambda_i} = \frac{u'(x_{ist})}{u'(x_{jst})} = \frac{v'(z_{ist})}{v'(z_{jst})} = \frac{u'(x_{ist})}{v'(z_{jst})} = \frac{v'(z_{ist})}{u'(x_{jst})} \tag{1.13}$$

In the case of the CARA utility function, an individual's temptation consumption depends on the total consumption. Once controlling for peers' total consumption, peers' consumption in specific categories should not matter for individuals' temptation consumption. z_{ist} depends only on $\overline{z_{st}} + \overline{x_{st}}$ (i.e., $z^*_{ist} = \frac{1}{\theta_z}(\ln\lambda_i - \overline{\ln\lambda}) + \frac{1}{2}(\frac{1}{N}\sum_{i=1}^{N} \theta_{ist} y_{ist}) = z^*(\overline{z_{st}} + \overline{x_{st}})$).

The result is similar in the constant relative risk aversion (CRRA) utility function.[14]

To summarize, in the risk-sharing model, individuals' consumption comoves with their peers. This risk-sharing mechanism leads to similar peer effects on individuals' temptation consumption as the social norm model. However, this comovement happens not only for individuals' consumption in temptation goods, but also in non-temptation goods. If the utility function is CARA and individuals have similar risk-aversion levels in consuming non-temptation goods and in temptation goods, $\frac{\partial z_{ist}}{\partial(\overline{x_{st}}+\overline{z_{st}})} > 0$. Once we control for the total consumption of peers, this comovement between peers' and individuals' temptation consumption would no longer hold.

It is also informative to compare the impact of shocks within the two frameworks. Since $z^*_{ist} = \frac{1}{\theta_z}(ln\lambda_i - \overline{ln\lambda}) + \frac{1}{2}(\frac{1}{N}\sum_{i=1}^{N}\theta_{ist}y_{ist})$ in the CARA utility, two properties similar to Fafchamps and Lund (2003) can be concluded: (1) Shocks affecting network members (i.e., $\theta_{gst} = \frac{1}{N}\sum_{i=1}^{N}\theta_{ist}$) will decrease an individual's consumption (both temptation and non-temptation consumption). (2) Idiosyncratic shocks have no impact on individual's consumption (both temptation and non-temptation consumption) once controlling for network shocks.

In conclusion, the social norm model yields different results from the risk-sharing model in Predictions 2–4. Table 1.6 illustrates the differences. First, both models predict positive correlation between own and peers' temptation consumption. In the second prediction: risk-sharing model predicts that the coefficient on peers' temptation consumption is no longer significant after controlling for peers' total consumption. Third, the coefficient on peers' temptation consumption is the same as that on peers' non-temptation consumption in the risk-sharing model, while the coefficient on peers' temptation consumption is significantly larger than that on peers' non-temptation consumption in the social norm model. Fourth,

[14] Applying a CRRA utility function, the utility for consuming non-temptation goods is $u(c) = \frac{c^{-r_x}}{1-r_x}$; utility for consuming temptation goods is $v(c) = \frac{c^{-r_z}}{1-r_z}$. r_x and r_z are the coefficients of relative risk-aversion ($R(c) = cA(c) = \frac{-cu''(c)}{u'(c)}$). At the end, I will get $\ln z^*_{ist} = \frac{1}{r_z}(\ln\lambda_i - \overline{\ln\lambda}) + \frac{1}{2}(\overline{\ln z_{st}} + \overline{\ln x_{st}})$. Individual i's growth of temptation consumption depends on the growth of total consumption (including both temptation and non-temptation consumption) of the peers.

there is a significant difference between more observable consumption and less observable consumption in the social norm theory; peer effects would be stronger on alcohol consumption outside than alcohol consumption at home. The risk-sharing model does not distinguish those two consumption behaviors. With respect to the income shock in the fourth prediction, peers' shock will have negative effects on own temptation and non-temptation consumption in the risk-sharing model, but peers' negative income shock, in contrast, will increase own temptation consumption through the social norm mechanism. In the social norm model, idiosyncratic shocks will also have positive effects on the total consumption. In the risk-sharing model, idiosyncratic shocks will not play a role in own consumption if we control for peers' aggregate shock. Our results, consistent with the predictions from the social norm model, validate that social norms would be a more probable explanation than the risk-sharing theory.

1.7.2 Other Robustness Checks

The previous section contrasts the predictions between the risk-sharing and the social norm model. This section presents several robustness checks. My results support social norms. However, to make sure that I did not process the data differently than the previous literature using the same information, I use village as the social network definition to test the risk-sharing theory. Similar to Townsend (1994), I use the aggregate yearly data to run the analysis on household's idiosyncratic income against household's consumption. If risk sharing is in place and efficient, the coefficient on idiosyncratic income should be small and insignificant.

Table A-1 shows the relationship between own income and consumption. The results in columns 1 and 2 indicate the existence of risk sharing at the village level. The coefficient in column 1, although significant, is quite small. The coefficient in column 2 using first difference specification is small and insignificant. Idiosyncratic income is not correlated with consumption. Yet village is a very crude definition for social network. When it comes to people's consumption behaviors, it is more important to understand the peer groups with whom people have close interaction. Social norm strongly affects villagers' temptation consumption when observing the behaviors of individuals' peer groups.

I further conduct robustness check using variables with a different time frame. This alternative analysis sheds additional light on the mechanism because the lagged instrument may require a habit formation assumption in addition to peer effects. One may also worry about the asymmetry of the timing that I use lagged consumption to instrument peers' current-period consumption at the first stage[15], while using both peers' and own consumption variables at the current period. To test whether the results are still robust with a symmetric time frame, I use consumption at time $t-2$ to instrument peers' consumption at time $t-1$ in the first stage, and then use this predicted $t-1$ variable on own consumption variable at time t. I expect the results to be similar using this symmetric specification because there can be a delay in response to peers' temptation consumption, assuming habit formation in consumers' utility function. Table A-2 shows that using variables with a different time frame, we observe similar peer effects on temptation consumption, and the results are weaker compared to the previous results using instruments at $t-1$ in table 1.3. In column 3, for example, the coefficient is at the border line of significance. Table A-3 includes results using alcohol consumption with the similar time frame as explained above. The results in this table are consistent and robust as well.

Another caveat of the analysis is that the data are sampled within the village. Identification may be compromised by using sampled networks (Chandrasekhar and Lewis, 2011). They show that even if network members are sampled randomly, this partial sampling will lead to nonclassical measurement errors, and can bias the estimation. Because of the concern of mis-measured social network, I sampled 50 percent of my observations to re-run the analysis. Although I cannot recover all the non-sampled network information, this robustness check can gauge whether the result is strong and stable enough even with some level

[15]Initially, I use a lagged variable to eliminate the simultaneous decision making of own and peers.

of missing network information. The results are presented in Table A-4 to Table A-6. All results stay the same. The robustness of the results from 50 percent of the sample reduces the concern of measurement errors of the sampled social network.

Some may challenge the observability test between "alcohol consumption at home" and "alcohol consumption outside"; people may gain individual utility by simply "drinking with their friends." This alternative can contradict with the definition of "temptation" good that people do not gain utility from thinking about future consumption at present. To address this concern, I verify the result using temptation consumption excluding alcohol consumption. The specification I can use is similar to the test in observability. Instead of alcohol consumption, I use $tempExAlcohol_{ivt} = \delta_0 + \delta_{temp_O} tempExAlcohol_{G_i vt} + \delta_3 X_{ivt} + f_{vt} + \xi_{ivt}$, where $tempExAlcohol_{ivt}$ represents an individual's monthly temptation consumption excluding alcohol consumption, and $tempExAlcohol_{G_i vt}$ is i's peers' average monthly temptation consumption excluding alcohol consumption. Then I use the same specification controlling for peers' average total monthly consumption.

Table A-7 presents the result of peer effects on temptation consumption excluding alcohol consumption. Column 1 indicates that peers' temptation consumption excluding alcohol consumption has a significant impact on an individual's. The coefficient on peers' temptation consumption (excluding alcohol) is around 1.6. The positive sign still holds in column 2 even after controlling for peers' total consumption, although it is only close to 10% significance level. Assuming that people do not gamble or buy lotteries together, the significance of the result using temptation consumption on gambling/lottery buying verifies the social norm hypothesis. Based on the anecdotal evidence, people in those Thai villages usually go gambling by themselves. There are also multiple types of informal gambling, such as buying lotteries, betting on stock prices and fish/chicken fights. Individuals usually give a bet at the local stores. The result further confirms that the peer effects of alcohol consumption are not simply driven by the joy of consuming together.

Temptation spending captures people's myopic consumption allocation. Based on

Banerjee and Mullainathan (2010), the concave shape of temptation will have an impact on an individual's saving. So I further test whether peer effects on temptation spending would affect saving behaviors. Based on the availability of the data,[16], I use whether any household members have a saving account to approximate saving behaviors. Table A-8 shows that peers' temptation spending further hinders an individual's saving behavior. The confidence interval using the CLR test falls entirely in the negative range. Although the IV coefficient is not significant, the CLR test gives a more robust result under weak instrument.

1.8 Conclusion

Self-control problems lead individuals to consume multiple types of temptation goods, and this consumption behavior is primarily influenced by peers; thus, the "self-control" problem is, in essence, a "group-control" problem. To examine peer effects on temptation consumption, I developed a social norm model of individuals-level consumption behaviors. The social norm model asserts that people have a tendency to emulate the temptation consumption of the majority. The extent of this conforming behavior varies with the observability of the consumption. The analysis revealed that even when peers' total consumption is controlled, peer effects can still be found on temptation consumption.

Using comprehensive survey data from Thailand, I created instrumental variables to overcome endogeneity and test peer effects on temptation consumption. The data, which were collected on a monthly basis, include important information on social relations, a variety of sources of income, and several types of consumption. The empirical results show that peer effects on temptation consumption are driven mainly by social norms: people's temptation consumption varies with the consumption of their peers because people tend to conform with the behavior of the majority of members of their social networks. The covariation of group members' consumption is significantly more prevalent for temptation goods than for

[16] Some households have negative income, so it is not clear whether simply using income minus consumption would yield meaningful results.

non-temptation goods even when peers' total consumption is controlled. In addition, results differed for goods that are more observable and those that are less observable—individual's public alcohol consumption was more affected by peer pressure than alcohol consumption at home. The data also show that the social norm mechanism is weaker when village-year fixed effects are not accounted for, which implies that there may be time-variant village factors associated with certain consumption patterns. In conclusion, risk sharing is found at the village level (as shown in prior literature), but is only one part of the explanation of the covariation of people's consumption. Social norm theory provides an essential and previously overlooked supplement to the explanation of myopic consumption behavior.

These results raise concerns about group-based financial products in which policymakers use peer pressure to encourage loan repayment and saving commitment. Peer effects may have undesirable consequences for these products. Socializing with peers who engage in undesirable financial behavior can make individuals behave more myopically by consuming more temptation goods, saving less money than they desire, and missing profitable investment opportunities. These outcomes may have particularly negative consequences for vulnerable households. While these group-based microfinance innovations have significant merits, financial institutions should require institutional monitoring of group dynamics and the effects of these dynamics on individual spending behaviors.

1.9 Mathematical Appendix

Prediction 1: *An increase in peers' temptation consumption will lead to the increase of individual i's temptation consumption as long as the behavior is observable* ($\frac{\partial z_{1i}}{\partial \overline{z_{1-ig}}} > 0$) *if* $\chi > 0$.

The main interest here is to analyze $\frac{\partial z_{1i}}{\partial \overline{z_{1-ig}}}$. Take partial derivative with respect to $\overline{z_{1-ig}}$ from equation 1.5:

$$\frac{\partial z_{1i}}{\partial \overline{z_{1-ig}}} + \frac{\theta_z}{\chi} e^{-\theta_z z_{1i}} \frac{\partial z_{1i}}{\partial \overline{z_{1-ig}}} = 1$$

$$\Longrightarrow \frac{\partial z_{1i}}{\partial \overline{z_{1-ig}}} = \left[1 + \frac{\theta_z}{\chi} e^{-\theta_z z_{1i}}\right]^{-1}$$

As long as $\chi > 0$, $\frac{\partial z_{1i}}{\partial \overline{z_{1-ig}}} > 0$

Prediction 3: *Peer effects on temptation consumption are stronger when the behavior is more observable* ($\frac{\partial^2 z_{1i}}{\partial \overline{z_{1-ig}} \partial \chi} > 0$).

Since we know that:

$$\frac{\partial z_{1i}}{\partial \overline{z_{1-ig}}} = \left[1 + \frac{\theta_z}{\chi} e^{-\theta_z z_{1i}}\right]^{-1}$$

So,

$$\frac{\partial^2 z_{1i}}{\partial \overline{z_{1-ig}} \partial \chi} = \left[1 + \frac{\theta_z}{\chi} (e)^{-\theta_z z_{1i}}\right]^{-2} \left[\frac{\theta_z}{\chi^2} (e)^{-\theta_z z_{1i}}\right]$$

This is positive because $\left[1 + \frac{\theta_z}{\chi} (e)^{-\theta_z z_{1i}}\right]^{-2} > 0$, and $\frac{\theta_z}{\chi^2} (e)^{-\theta_z z_{1i}} > 0$

The results are very similar in CRRA utility function: Assume $u(x) = \frac{x^{1-\gamma_x}}{1-\gamma_x}$ and

$v(z) = \frac{z^{1-\gamma_z}}{1-\gamma_z}$. Equation 1.5 becomes

$$z_{1i} - \frac{1}{\chi}(z_{1i})^{-\gamma_z} = \overline{z_{1-ig}} - \frac{1}{\chi}(1+r)\delta(x_{2i})^{-\gamma_x}\left(1 - \frac{\partial z_{2i}}{\partial c_{2i}}\right) \tag{1.14}$$

Thus, as long as χ is greater than zero, the left-hand side of the equation is an increasing function in z_{1i}. Increasing peers' temptation consumption will lead to the increase of individual i's temptation consumption.

Prediction 4:

When individuals are poor, negative idiosyncratic shocks will increase total consumption ($\frac{\partial z_{1i}}{\partial \theta_{1i}} < 0$, and $\frac{\partial x_{1i}}{\partial \theta_{1i}} < 0$ as c is small);
If one poor peer encounters adverse shock, other things being equal, this negative peer's shock has a positive impact on temptation consumption.

From equation 1.3, we have:

$$v'(z_{1i}) = \chi(z_{1i} - \overline{z_{1-ig}}) + \delta u'(x_{2i})\left(\frac{\partial x_{2i}}{\partial c_{2i}}\right)(1+r) \tag{1.15}$$

First, look at the right-hand side of equation 1.15. Higher θ_{1i} (positive income shock) will lead to smaller $u'(x_{2i})$, but larger $(1 - \frac{\partial z_{2i}}{\partial c_{2i}})$ (which is equal to $\frac{\partial x_{2i}}{\partial c_{2i}}$). These two countervailing effects result from the initial assumptions of the model: $u'(x_{2i})$ decreases along with the higher θ_{1i} because x_{2i} is a function of c_{2i}, where $c_{2i} = (1+r)(\theta_{1i}y_{1i} - x_{1i} - z_{1i}) + y_{2i}$. Because of the diminishing return of utility, $u'(x_{2i})$ will decrease when c_{2i} is higher. At the same time, this positive shock will increase $(1 - \frac{\partial z_{2i}}{\partial c_{2i}})$ because of the concave shape of temptation goods (i.e. $z''(c) < 0$). Thus, when the second effect dominates, the right-hand side of equation 1.3 will increase with respect to an increase in θ_{1i} For the left-hand side ($v'(z_{1i})$) to increase, z_{1i} has to decrease. To conclude, $\frac{\partial z_{1i}}{\partial \theta_{1i}} < 0$ when c_{2i} is small.

To see why, among poorer individuals, the second effect ($(1 - \frac{\partial z_{2i}}{\partial c_{2i}})$) dominates the first ($u'(x_{2i})$) on the right-hand side: $\frac{\partial z_{1i}}{\partial \theta_{1i}} < 0$ as long as $[u'(x_{2i})(1 - \frac{\partial z_{2i}}{\partial c_{2i}})]$ is an increasing function of c_{2i}. Suppose $\frac{\partial^2 z_{2i}}{\partial c_{2i}^2}$ is monotone, and $\frac{\partial^3 z_{2i}}{\partial c_{2i}^3} > 0$, there exists a sufficiently low c_{2i}, which makes $[u'(x_{2i})(1-\frac{\partial z_{2i}}{\partial c_{2i}})]$ an increasing function in c_{2i}. Use the previous functional form to illustrate. $\frac{\partial z_{1i}}{\partial \theta_{1i}} = \frac{-(1+r)\delta}{\chi+\theta_z e^{-\theta_z z_{1i}}}(1+r)y_{1i}[-\theta_x e^{-\theta_x x_{2i}} - \frac{\partial^2 z_{2i}}{\partial c_{2i}^2}]$ Therefore, $\frac{\partial z_{1i}}{\partial \theta_{1i}} < 0$ when $-\theta_x e^{-\theta_x x_{2i}} - \frac{\partial^2 z_{2i}}{\partial c_{2i}^2} > 0$ (that said, $\frac{\partial^2 z_{2i}}{\partial c_{2i}^2} < -\theta_x e^{-\theta_x x_{2i}}$). Since $\frac{\partial^3 z_{2i}}{\partial c_{2i}^3} > 0$, $c < max\{\frac{\partial^2 z_{2i}}{\partial c_{2i}^2} + \theta_x e^{-\theta_x x_{2i}}\}$.

Similarly, from equation 1.4, we have:

$$u'(x_{1i}) = \delta u'(x_{2i})\left(\frac{\partial x_{2i}}{\partial c_{2i}}\right)(1+r) = 0 \tag{1.16}$$

Positive income shock will lead to smaller $u'(x_{2i})$, and larger $(1-\frac{\partial z_{2i}}{\partial c_{2i}})$ $(= \frac{\partial x_{2i}}{\partial c_{2i}})$. The left-hand side of equation 1.16 will increase when the positive shock leads to a much larger $(1-\frac{\partial z_{2i}}{\partial c_{2i}})$. Similar conclusion can be achieved for x good: $\frac{\partial x_{1i}}{\partial \theta_{1i}} < 0$ when c_{2i} is small.

Following the same logic, a poor enough peer can also increase his temptation consumption when encountering negative income shock. Here I want to show the intuition that a poor peers' negative shock can lead to an increase in household's own temptation consumption if holding all other peers' shock constant. Suppose that there is a household $j' \in \{$ poor & i's peer group $\}$, who encounters negative income shock (smaller $\theta_{1j'}$). Household j' will increase temptation consumption (i.e. $\frac{\partial z_{1j'}}{\partial \theta_{1j'}} < 0$) because the second effect $(1-\frac{\partial z_{2j'}}{\partial c_{2j'}})$ dominates the first $(u'(x_{2j'}))$ on the right-hand side of equation 1.16. An increase in $z_{1j'}$ responding to a smaller $\theta_{1j'}$ will lead to an increase in the peers' average temptation consumption $(\overline{z_{1-ig}})$ because $j' \in \{$ i's peer group $\}$. Based on prediction 1, an increase in peers' average temptation consumption will result in an increase in individual's own temptation consumption. Similar logic applies if more than one poor peers encounter negative shock event.

Table 1.1: Summary Statistics

	mean	sd	min	max	N
Temptation consumption	94	211.9145	0	7433	26928
Non temptation consumption	1,393	3482.114	37	287815	26928
Total consumption	1,487	3528.544	37	287815	26928
Alcohol consumption at home	31	158.2397	0	6687	26928
Alcohol consumption outside	12	51.46327	0	1680	26928
Sickness	6.36	15.52159	0	686	26928
Temptation spending among total consumption	0.068	0.081228	0	0.7208	26928
Household per-capita monthly income	2,872	11765.49	-301900	430397	26928
Household Size	4.37	1.9368	1	15	26928

Note: All the consumption are per capita monthly spending

Figure 1.3: Histogram of Proportional Spending on Temptation Goods

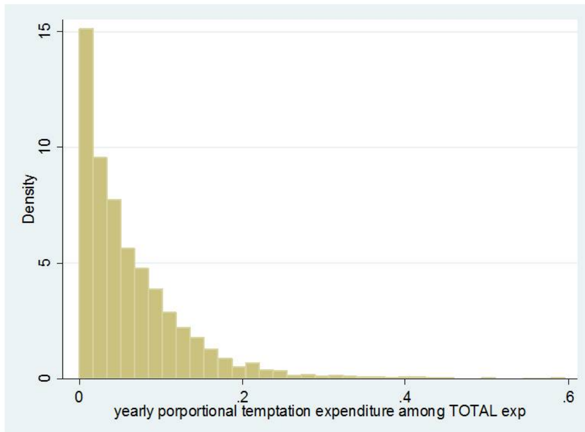


Figure 1.4: Proportional Spending on Temptation Goods across Income

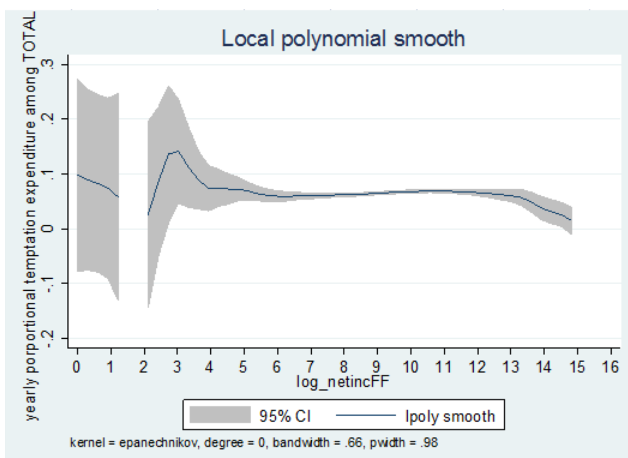


Table 1.2: Correlation of Social Network

Income	0.1470***
Household size	0.1342***
Percentage of ag income (differed by year)	0.5286***
Percentage of ag income (average throughout years)	0.3802***
Days of health shock	0.0207***

Note: *** p<0.01, ** p<0.05, * p<0.1

Table 1.3: Consumption Relationship between Own and Peer

	temp	non-temp	temp	non-temp	temp	non-temp
	OLS		IV		IV	
	(1)	(2)	(3)	(4)	(5)	(6)
Peer's temptation consumption	0.0439**		1.516*		1.636*	
	(0.0158)		(0.784)		(0.883)	
	[0.005]***		[0.0000]***		[0,0000]***	
Peer's non-temptation consumption		0.0190		1.153		-34.24*
		(0.0128)		(0.812)		(20.23)
		[0.1178]		[0.0599]*		[0.1238]
Peer's consumption					-0.0148	33.94*
					(0.0138)	(20.03)
					[0.2635]	[0.0918]*
Household size	-10.86***	-136.2**	-10.63***	-140.8**	-10.57***	-128.5**
	(3.031)	(47.53)	(3.276)	(61.27)	(3.211)	(53.10)
	[0.002]***	[0.004]***	[0.004]***	[0.012]**	[0.004]	[0.016]
Village-year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	26,928	26,928	24,353	24,353	24,353	24,353
F-stat of 1st Stage			7.206	2.874	6.423	1.520
CI of IV coefficient using CLR			[.4682, 5.9437]		[.4980, 7.5359]	

Note: Robust standard errors clustered at the village level in parenthesis; p value using wild cluster bootstrap reported underneath the robust standard errors

*** p<0.01, ** p<0.05, * p<0.1

All dependent variables are the level of household's per capita monthly consumption. Peer's consumption is calculated as the average level of per capita consumption excluding own household's consumption. Peer's consumption is instrumented using lagged consumption of individual i's friends of friends who are not directly linked with i. Conditional Likelihood Ratio (CLR) Test is developed by Moreira (2002). Similar to Anderson-Rubin (AR) test, CLR test gives robust confidence set under weak instrument. Yet, CLR test outperform AR test in power simulations (Andrews et al 2006).

Table 1.4: Alcohol Consumption at Home and Outside

	Dependent Variable: Household's alcohol consumption							
	Total				At home		Total	
	OLS	OLS	IV	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Peer's alcohol consumption at home	0.00239		3.098		2.406		3.602	
	(0.00747)		(4.524)		(3.570)		(5.978)	
	[0.7342]		[0.5039]		[0.5108]		[0.5558]	
Peer's alcohol consumption outside		0.193**		4.316***		2.169*		4.317***
		(0.0839)		(1.472)		(1.223)		(1.474)
		[0.0394]**		[0.0103]**		[0.0963]*		[0.0103]**
Peer's total consumption							-0.0293	-0.000110
							(0.0553)	(0.000561)
							[0.6048]	[0.8470]
Household size	-6.166***	-6.197***	-4.738	-8.797***	-2.807	-5.431**	-4.334	-8.798***
	(1.924)	(1.932)	(5.201)	(3.049)	(3.775)	(2.421)	(6.071)	(3.049)
	[0.0281]**	[0.0268]**	[0.3766]	[0.0113]**	[0.4687]	[0.0403]**	[0.4862]	[0.0039]***
Village-year fixed effect		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	26,928	26,928	24,353	24,353	24,353	24,353	24,353	24,353
F-stat of 1st Stage			2.345	21.52	2.345	21.52	2.064	21.47

Robust standard errors clustered at the village level in parenthesis; p value using wild cluster bootstrap reported underneath the robust standard errors

*** p<0.01, ** p<0.05, * p<0.1

Peer's consumption is calculated as the average level of per capita monthly consumption excluding own household's consumption. Peer's consumption is instrumented using lagged consumption of individual i's friends of friends who are not directly linked with i.

Table 1.5: Shock on Consumption Pattern with Income Interaction

	temp OLS (1)	non-temp OLS (2)	temp IV (3)	non-temp IV (4)	temp IV (5)	non-temp IV (6)
Poor peer's total days of health shock	0.0272	0.155	1.223***	-7.188	1.172***	-7.088
	(0.0505)	(0.330)	(0.347)	(10.19)	(0.313)	(10.00)
	[0.5980]	[0.6451]	[0.0062]***	[0.4729]	[0.0025]***	[0.4787]
Individual's days of health shock	-0.117	5.030	-0.175	5.388	-0.182	5.401
	(0.230)	(3.291)	(0.238)	(3.292)	(0.237)	(3.302)
	[0.6187]	[0.14725]	[0.3446]	[0.1715]	[0.3321]	[0.1717]
Poverty	-83.78***	-1,204***	-85.09***	-1,196***	-83.17***	-1,200***
	(11.07)	(90.62)	(10.61)	(85.59)	(10.53)	(88.42)
	[0.0000]***	[0.0000]***	[0.0000]***	[0.0000]***	[0.0000]***	[0.0000]***
Poverty*individual's health shock	0.0572	-6.208*	0.0925	-6.425**	0.0946	-6.429**
	(0.228)	(3.169)	(0.246)	(3.071)	(0.244)	(3.070)
	[0.8050]	[0.0689]*	[0.5934]	[0.0955]*	[0.6227]	[0.0955]*
Household size	-6.593*	-78.73	-6.298**	-80.54*	-6.412**	-80.32*
	(3.130)	(45.56)	(3.165)	(43.44)	(3.115)	(43.60)
	[0.0524]	[0.1045]	[0.0665]	[0.0171]	[0.0632]*	[0.0173]**
Village-year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Additional control for # of poor peers					Yes	Yes
Observations	28,008	28,008	28,008	28,008	28,008	28,008
F-stat of 1st Stage			114.8	114.8	125.7	125.7

Robust standard errors clustered at the village level in parenthesis; p value using wild cluster bootstrap reported underneath the robust standard errors

*** p<0.01, ** p<0.05, * p<0.1

Peer's consumption is calculated as the average level of per capita monthly consumption excluding own household's consumption. Peer's health shock is instrumented using contemporaneous shock information of individual i's friends of friends who are not directly linked with i.

Table 1.6: Predictions from Social Norm and Risk-sharing Model

	Specification	Social Norm	Risk-sharing
1: Own and peer	$temp_{it} = \alpha_0 + \alpha_1 temp_{G_{it}} + \alpha_2 X_i + f_{vt} + \epsilon_{it}$	$\alpha_1 > 0$	$\alpha_1 > 0$
2: Extra Control	$temp_{ivt} = \gamma_0 + \gamma_1 temp_{G_ivt} + \gamma_2 cons_{G_ivt} + \gamma_3 X_{ivt} + f_{vt} + \varepsilon_{ivt}$	$\gamma_1 > 0$	$\gamma_1 = 0$, $\gamma_2 > 0$
3: Non-temp vs temp	$temp_{ivt} = \gamma_0 + \gamma_{temp} temp_{G_ivt} + \gamma_3 X_{ivt} + f_{vt} + \varepsilon_{ivt}$ $nontemp_{ivt} = \gamma_0 + \gamma_{nontemp} nontemp_{G_ivt} + \gamma_3 X_{ivt} + f_{vt} + \varepsilon_{ivt}$	$\gamma_{temp} > \gamma_{nontemp}$	$\gamma_{temp} = \gamma_{nontemp}$
4: Observability	$alcoholTOTAL_{ivt} = \gamma_0 + \gamma_{temp_H} alcoholHOME_{G_ivt} + \gamma_3 X_{ivt} + f_{vt} + \varepsilon_{ivt}$ $alcoholTOTAL_{ivt} = \gamma_0 + \gamma_{temp_O} alcoholOUT_{G_ivt} + \gamma_3 X_{ivt} + f_{vt} + \varepsilon_{ivt}$	$\gamma_{temp_O} > \gamma_{temp_H}$	$\gamma_{temp_O} = \gamma_{nontemp_H}$
5: Shock event	$temp_{ivt} = \beta_0 + \beta_{temp1} healthshock_{G_ivt} + \beta_{temp2} healthshock_{ivt} +$ $\beta_3 X_i + f_{vt} + \epsilon_{ivt}$ $nontemp_{ivt} = b_0 + b_{nontemp1} healthshock_{G_ivt} + b_{nontemp2} healthshock_{ivt}$ $+ b_3 X_i + f_{vt} + \epsilon_{ivt}$	$\beta_{temp1} > 0$, $\beta_{temp2} > 0$ $b_{nontemp2} > 0$	$\beta_{temp1} < 0$, $\beta_{temp2} = 0$; $b_{nontemp1} < 0$ $b_{nontemp2} = 0$

1.10 Appendix

Table A-1: Risk-sharing at the Village

	Household's consumption per capita	
	level	first difference
Net income per capita	0.0300***	
	(0.00340)	
Net income per capita (first difference)		0.0237
		(0.0230)
Observations	3,804	3,170
R-squared	0.095	0.033

Note: Standard errors in parenthesis

*** p<0.01, ** p<0.05, * p<0.1

All dependent variables are at the level of household's per capita yearly consumption.

People within the same village are categorized as in the same social network.

Table A-2: Consumption Relationship between Own and Peer (Different Time Frame)

	temp	non-temp	temp	non-temp
	IV t-2 instrument on t-1			
	(1)	(2)	(3)	(4)
Peer's temptation consumption at $t-1$	1.154*		1.264	
	(0.695)		(0.822)	
Peer's non-temptation consumption at $t-1$		1.146		-45.52
		(0.726)		(30.70)
Peer's consumption at $t-1$			-0.0121	45.10
			(0.0127)	(30.38)
Household size	-12.37***	-146.3**	-12.40***	-186.1**
	(4.003)	(60.25)	(4.041)	(66.29)
Village-year fixed effect	Yes	Yes	Yes	Yes
Seasonal fixed effect	Yes	Yes	Yes	Yes
Household fixed effect	Yes	Yes	Yes	Yes
Observations	24,010	24,010	24,010	24,010
F-stat of 1st Stage	7.549	3.350	6.598	1.026

Robust standard errors clustered at the village level in parenthesis

*** p<0.01, ** p<0.05, * p<0.1

All dependent variables are at the level of household's per capita monthly consumption. Peer's consumption is calculated as the average level of per capita monthly consumption excluding own household's consumption. In columns (1) to (4), peer's t-1 consumption is instrumented using 2-period lagged consumption of individual i's friends of friends who are not directly linked with i.

Table A-3: Alcohol Consumption at Home and Outside (Different Time Frame)

	Dependent Variable: Household's alcohol consumption					
	Total		At home		Total	
	IV					
	$t-2$ instrument on $t-1$					
	(1)	(2)	(3)	(4)	(5)	(6)
Peer's alcohol consumption at home at $t-1$	1.263		0.339		1.563	
	(1.955)		(1.005)		(2.850)	
Peer's alcohol consumption outside at $t-1$		4.684**		2.262		4.698**
		(2.054)		(1.628)		(2.063)
Peer's total consumption at $t-1$					-7.513**	-8.378***
					(3.187)	(3.235)
Household size	-7.483**	-8.378***	-4.694**	-5.244**	-7.484**	-8.377***
	(2.990)	(3.234)	(2.236)	(2.565)	(2.971)	(3.236)
Village-year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	24,010	24,010	24,010	24,010	24,010	24,010
F-stat of 1st Stage	0.530	19.57	0.530	19.57	0.365	19.50

Robust standard errors clustered at the village level in parenthesis

*** p<0.01, ** p<0.05, * p<0.1

Peer's consumption is calculated as the average level of per capita monthly consumption excluding own household's consumption. Peer's t-1 consumption is instrumented using 2-period lagged consumption of individual i's friends of friends who are not directly linked with i.

Table A-4: Consumption Relationship between Own and Peer (Sub-sample)

	temp OLS (1)	non-temp OLS (2)	temp IV (3)	non-temp IV (4)	temp IV (5)	non-temp IV (6)
Peer's temptation consumption	0.0117		1.339**		1.356**	
	(0.0177)		(0.639)		(0.644)	
Peer's non-temptation consumption		-0.0120***		-1.082		-55.65
		(0.00247)		(1.244)		(51.32)
Peer's consumption					-0.00487	55.44
					(0.00477)	(51.16)
Household size	-11.35**	-111.3	-7.904	-42.40	-7.798	-37.67
	(4.078)	(78.22)	(4.885)	(96.05)	(4.806)	(184.0)
	(3.031)	(47.53)	(3.276)	(61.27)	(3.211)	(53.10)
Village-year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,304	11,304	8,946	8,946	8,946	8,946
F-stat of 1st Stage			10.36	3.007	10.21	1.040

Note: Robust standard errors clustered at the village level in parenthesis

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

All dependent variables are at the level of household's per capita monthly consumption. Peer's consumption is calculated as the average level of per capita monthly consumption excluding own household's consumption. Peer's consumption is instrumented using lagged consumption of individual i's friends of friends who are not directly linked with i

Table A-5: Alcohol Consumption at Home and Outside (Sub-sample)

	Dependent Variable: Household's alcohol consumption							
	Total				At home		Total	
	OLS	OLS	IV	IV	IV	IV	IV	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Peer's alcohol consumption at home	0.00364		2.297***		1.889***		2.332***	
	(0.0138)		(0.799)		(0.665)		(0.830)	
Peer's alcohol consumption outside		-0.0155		3.187***		1.178**		3.163***
		(0.0409)		(0.677)		(0.505)		(0.685)
Peer's total consumption							-0.00809	0.000513***
							(0.00787)	(0.000178)
Household size	-7.060***	-7.077***	-7.740***	-3.822	-5.985**	-3.875	-7.599***	-3.850
	(2.299)	(2.314)	(2.636)	(2.988)	(2.575)	(2.837)	(2.450)	(2.992)
Village-year fixed effect		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Seasonal fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,304	11,304	8,946	8,946	8,946	8,946	8,946	8,946
F-stat of 1st Stage			1.745	36.79	1.745	36.79	1.720	37.19

Robust standard errors clustered at the village level in parenthesis

*** p<0.01, ** p<0.05, * p<0.1

Peer's consumption is calculated as the average level of per capita monthly consumption excluding own household's consumption. Peer's consumption is instrumented using lagged consumption of individual i's friends of friends who are not directly linked with i.

Table A-6: Shock on Consumption Pattern with Income Interaction (Sub-sample)

	temp	non-temp	temp	non-temp
	OLS		IV	
	(1)	(2)	(3)	(4)
Log peer's days of health shock	-3.961	-21.58	104.3	-568.2
	(2.787)	(31.89)	(123.9)	(545.0)
Log individual's helth shock	4.431	180.9	10.52	224.7*
	(6.059)	(107.1)	(10.06)	(131.6)
Log net income	3.485***	24.08	4.005***	12.87
	(0.975)	(17.27)	(1.549)	(20.26)
log (Income)*log (individual's helth shock)	-0.529	-24.70	-1.052	-31.09
	(0.654)	(15.49)	(1.004)	(18.95)
Household size	-14.13**	-117.8	-11.47	-67.42
	(5.936)	(123.4)	(7.435)	(131.5)
Village-year fixed effect	Yes	Yes	Yes	Yes
Seasonal fixed effect	Yes	Yes	Yes	Yes
Household fixed effect	Yes	Yes	Yes	Yes
Observations	7,284	7,284	5,654	5,654
F-stat of 1st Stage			24.75	24.75

Note: Robust standard errors clustered at the village level in parenthesis

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Peer's consumption is calculated as the average level of per capita monthly consumption excluding own household's consumption. Peer's health shock is instrumented using lagged consumption of individual i's friends of friends who are not directly linked with i.

Table A-7: Temptation Consumption excluding Alcohol Consumption

	Household's temptation consumption excluding alcohol consumption	
	(1)	(2)
Peer's temptation consumption (except alcohol)	1.635*	1.652
	(0.992)	(1.009)
	[0.41916]	[0.37924]
Peer's total consumption		-0.00154
		-0.00113
		[0.19162]
Household size	-4.128**	-4.124**
	(1.638)	(1.657)
	[0.01597]**	[0.02794]**
Village-year fixed effect	Yes	Yes
Seasonal fixed effect	Yes	Yes
Household fixed effect	Yes	Yes
Observations	24,353	24,353
F-stat of 1st Stage	28.54	27.97

Note: Robust standard errors clustered at the village level in parenthesis; p value using wild cluster bootstrap reported underneath the robust standard errors

*** p<0.01, ** p<0.05, * p<0.1

All dependent variables are at the level of household's per capita monthly consumption. Peer's consumption is calculated as the average level of per capita monthly consumption excluding own household's consumption. Peer's consumption is instrumented using lagged consumption of individual i's friends of friends who are not directly linked with i. The social network is defined using people's financial, gift-giving, and labor-sharing relations.

Table A-8: Peers' Temptation on Saving

	Dependent variable: Whether household opens a saving account in the given month
Peer's temptation consumption	-0.00224
	(0.00197)
Household size	0.0135*
	(0.00724)
Village-year fixed effect	Yes
Seasonal fixed effect	Yes
Household fixed effect	Yes
Observations	24,346
F-stat of 1st Stage	6.84
CI of IV coefficient using CLR	[-.0093, -.0009]

Note: Robust standard errors clustered at the village level in parenthesis

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Saving captures whether any household member has opened saving account in the past month. Peer's consumption is calculated as the average level of per capita monthly consumption excluding own household's consumption. Peer's consumption is instrumented using lagged consumption of individual i's friends of friends who are not directly linked with i. The social network is defined using people's financial, gift-giving, and labor-sharing relations. Conditional Likelihood Ratio (CLR) Test is developed by Moreira (2002). Similar to Anderson-Rubin (AR) test, CLR test gives robust confidence set under weak instrument. Yet, CLR test outperform AR test in power simulations (Andrews et al 2006).

Chapter 2

Stability of Experimental and Survey Measures of Risk, Time, and Social Preferences Over Multiple Years

2.1 Introduction

Time, risk, and social preference parameters are crucial inputs into many economic models. They have important impacts on outcomes in models of technology adoption, migration, savings, and risk-sharing. Over the past decades experimental economists have worked on perfecting methods for measuring these parameters. These preferences are assumed constant by theory, so the experiment and survey-based constructs which purport to measure them should also remain constant. If preferences do vary over time, theory would suggest that this is due to some shock in the environment the individuals are facing (e.g., a monetary windfall or a recently experienced theft).

In this paper we study the stability over time of preferences as measured by both survey questions and experiments. We also study whether these measures are affected by real world shocks or experiences in experiments in previous years. Because the literature on this topic is spread across many journals in many different disciplines including economics, psychology, management, and marketing, the first contribution of this paper is an extensive cross-disciplinary review of the literature regarding the stability of experimentally-measured preferences over time. Most of the existing literature is focused on developed countries and more educated populations, though that has been changing more recently. We compile the correlations of preferences over time found in each paper, which can serve as a baseline to compare the correlations found in our own data and those found in future studies.

Next, we make use of a unique dataset which follows households in rural Paraguay, running surveys and experiments with them in 2002, 2007, 2009, and 2010. This data contains experimental measures of social, risk, and time preferences as well as survey measures of social preferences. The contribution of our paper compared to the previous work is that we look at all three types of preferences,[1] over relatively long periods of time, and with a relatively large and diverse population in a developing country.

We should note that we do not observe preferences themselves, we observe choices made in experiments and answers given to survey questions. These choices will typically depend on both underlying preferences and the environment. If there are aggregate changes (e.g., a recession) this should affect the average level of preferences, but an individual's relative position in the distribution should not change. Our results account for the environment by controlling for idiosyncratic characteristics of the environment (e.g., household-level income and village fixed effects) when measuring the stability of choices.

We find that in our data survey measures of social preferences are extremely stable over long periods of time. None of the experimental measures approach such a level of stability. Experimental measures of time preferences are highly stable over time while risk preferences are not. We find some weak evidence of stability in experimental measures of social preferences, but the data seems too noisy to estimate this relationship with much precision.

One potential explanation for the fact that we find experimental measures of preferences to not be stable over time might be that households are facing shocks which cause their preferences to change. For rural Paraguayans who don't have access to crop insurance or health insurance, such shocks may take a large toll on the household's financial situation. We

[1]We do not look at correlations of preferences across domains. Nevertheless, an advantage of looking at all three domains (risk, time, and social preferences) with one subject pool is that it slightly lessens the potential for data-mining, picking and choosing the subject pools or the preferences which are or are not significantly correlated.

test whether income, health, and theft shocks are correlated with play in games or answers to survey questions. We find no evidence that these shocks have any impact on the choices people make in experiments or their answers to the survey preference measures. We do find some suggestive evidence that experiences in earlier experiments impact play in future games; e.g., that being linked with a more generous partner in the past makes a player more generous himself in future games and more likely to punish ungenerous players. But, it seems that these effects have more to do with learning about the game rather than actual changes in preferences.

In sum, in our dataset *survey* measures of social preferences are quite consistent over time; on the other hand, *experimental* measures of social preferences are only weakly correlated over time. Neither income, theft, nor health shocks impact answers to survey questions or choices made in experiments.

The greater stability of preferences measured by surveys may be due to many reasons (see Section 2.4). One reason is that many of the theoretically elegant experiments designed by experimental economists may be better suited for the student populations found in university labs who are highly educated and non-poor.

Recent research suggests that economic scarcity consumes attentional resources and interferes with cognitive function which may then lead to errors and biases in decision making (Shah et al., 2012). Benjamin et al. (2013) and Burks et al. (2009) give evidence that individuals with lower cognitive ability exhibit more behavioral biases in risk and time experiments and play less consistently in risk games. The scarcity experienced by poor individuals in rural areas of developing countries may lead the decisions they make in experiments to contain more errors than decisions made by individuals who do not experience such need. This may then explain why the decisions they make in experiments are not correlated over time.

In addition to the effect of poverty on cognitive function and decision making, the low levels of education in our rural Paraguayan sample may make the experiments significantly harder for them to understand than the survey questions. In our data the median level of education is only five years and the maximum is twelve. Using a representative sample in the Netherlands, Choi et al. (2014) find that less educated respondents play less consistently in risk and time preference games. Using a sample of Canadians, Dave et al. (2010) find that simpler experiments measuring risk aversion work better among less numerate subjects, as more complex experiments lead to much noise in decision-making. Charness and Viceisza (2014) measure the risk preferences of a population of rural Senegalese individuals and find that more complex tasks lead to lower levels of understanding and more noisy responses.

This highlights the difficulties involved in conducting economic experiments in developing countries among populations with high levels of poverty and low levels of education. Although economists tend to dismiss stated survey measures of preferences, they may want to reconsider the usefulness of non-incentivized survey measures or more simple experiments when working with such populations.

2.2 Previous Literature

While Loewenstein and Angner (2003) ponder whether underlying preferences are stable over time from a philosophical and theoretical perspective, we empirically examine whether preferences measured from surveys and experiments are stable. Below we review three related strands of the literature. The empirical literature on preference stability over time is most directly related to our paper and we review those papers first and most thoroughly. Our paper can also be related to the growing number of studies looking at how shocks such as illness, income shocks, civil wars, and natural disasters lead to changes in measures of preferences.[2] Finally, we briefly review the literature on the stability of preferences as measured in different games but played on the same day, which is relevant for this paper since in many cases we do not play exactly the same game in each year.

[2] Jamison et al. (2012) conduct a nice, more general, overview of the literature on the measurement of risk and time preferences, including experimental design, incentivized versus hypothetical experiments, correlations with real-world outcomes, and stability over time.

2.2.1 Stability over Time of Preferences Measured in Experiments

First we review papers studying the stability of measures of risk, time, and social preferences over time. Compared with our data, much of this literature either uses a smaller sample, a shorter period of time, or both. These papers only focus on one type of preference with the large majority studying only risk preferences. More recently the number of papers looking at time preferences has been growing, while there are still very few that look at the stability of social preferences. Very few of these papers discuss attrition, although the sample sizes reported often suggest that many fewer individuals participate in later rounds compared to earlier rounds of the experiments.[3] In addition many focus on student populations, which leads to worries about selection and external validity. The majority use better-educated samples from developed countries, although that has been changing more recently. Most of the papers which report correlations find that measured preferences are significantly correlated over time, though there may be some publication (and pre-publication) bias in this finding.

Risk Preferences: The results regarding stability of risk preferences are summarized in Table A-1. The evidence suggests that risk preferences are relatively stable over time, with reported correlations ranging from a low of -.38 to a high of 0.68. If one excludes the studies with fewer than 100 observations, the range is 0.13 to 0.55.[4][5] Interestingly, there does not appear to be much systematic difference in the correlations reported by studies which measure risk preferences over shorter versus longer periods of time. Likewise, there doesn't appear to be much difference in the the stability of hypothetical

[3]Some welcome exceptions which do discuss attrition include Andersen et al. (2008), Beauchamp et al. (2012), Meier and Sprenger (2015), and Wölbert and Riedl (2013).

[4]One of the authors has collected data from 17 Wisconsin farmers over 2 years and finds a significant correlation of 0.46 when looking at number of risky choices but an insignificant correlation of 0.11 when looking at coefficients of relative risk aversion. See Barham et al. (2014) for information about the first round of data collected.

[5]There are also papers which look at the stability of prospect theory risk preferences including Baucells and Villasís (2010) (141 students over three months, correlation of 0.32), Glöckner and Pachur (2012) (64 students over one week, correlations between 0.22 and 0.59), and Zeisberger et al. (2012) (73 German undergraduates over one month).

Table A-1: Stability of Risk Preferences

Paper	Population	Time	Corr	Sig	Inc
Menkhoff and Sakha (2014)	384 rural Thai	5 years	?	yes	inc
Levin et al. (2007)	124 US children/parents	3 years	0.20 - 0.38	yes	inc
Guiso et al. (2011)	666 Italian investors	2 years	0.13[1]	yes	hyp
Kimball et al. (2008)	700 older Americans	2 years	0.27	?	hyp
Love and Robison (1984)	23 US farmers	2 years	-0.38 - 0.23[1]	no[2]	hyp
Sahm (2012)	12000 older Americans	multiple years	0.18	yes	hyp
Beauchamp et al. (2012)	489 Swedish twins	1 year	0.48[3]	yes	hyp
Goldstein et al. (2008)	75 Americans	1 year	0.43	yes	hyp
Lönnqvist et al. (2014)	43 German students	1 year	0.21	no	inc
Smidts (1997)	205 Dutch farmers	1 year	0.44	yes	hyp
Wehrung et al. (1984)	84 N. American businessmen	1 year	0.36	yes	hyp
Andersen et al. (2008)	97 Danes	3- 17 months	?	yes	inc
Harrison et al. (2005)	31 US students	6 months	?	yes	inc
Vlaev et al. (2009)	69 British students/adults	3 months	0.20-0.63[4]	yes	hyp
Horowitz (1992)	66 US students & 23 PTA	2 months	?	no	inc
Wölbert and Riedl (2013)	53 Dutch students	5-10 weeks	0.36-0.68	yes	inc
Schoemaker and Hershey (1992)	109 US MBA students	3 weeks	0.55	yes	hyp
Hey (2001)	53 British students	a few days	?	yes	inc

Corr - the correlation of risk preferences over time. Sig - whether risk preferences are significantly related over time. Inc - whether the experiment was incentivized rather than hypothetical.

[1] Our own calculation, from the raw data reported in the original paper.

[2] In fact, the negative correlation of -0.38 is significant at the 10% level.

[3] This is a polychoric correlation, which may be larger because it suffers from less attenuation bias.

[4] Interestingly, the one insignificant correlation of 0.20 was for the risk question which used the Multiple Price List (MPL) mechanism over gains.

versus incentivized measures, or from student versus non-student populations.

Time Preferences: The results regarding stability of experimentally measured time preferences can be found in Table A-2. Of the papers looking at the stability of time preferences, the reported correlation coefficients range from 0.004 to 0.75. Most numbers seem to be slightly higher than the range of risk preference correlation coefficients mentioned above, although there are fewer observations. If one excludes the studies with less than 100 observations four studies remain, with correlations (for the comparisons with over 100 observations) ranging from 0.09 to 0.68. This is similar to the range of the correlations in the risk aversion studies.[6]

Table A-2: Stability of Time Preferences

Paper	Population	Time	Corr	Sig	Inc
Meier and Sprenger (2015)	250 US low-income	2 years	0.40	yes	inc
Krupka and Stephens Jr (2013)	1194 Americans	1 year	?	?	hyp
Harrison et al. (2006)	97 Danes	3 - 17 months	?	yes	inc
Kirby et al. (2002)	95-123 Bolivian Amerindians	3 -12 months	0.004-0.46	yes	inc
Kirby (2009)	46-81 US students	1 - 12 months	0.57-0.75	yes	inc
Li et al. (2013)	336-516 Americans	1 week - 14 months	0.33-0.68	yes	hyp
Wölbert and Riedl (2013)	53 Dutch students	5-10 weeks	0.61-0.68	yes	inc
Dean and Sautmann (2014)	961 peri-urban Malians	1 week	0.61-0.67	yes	inc

Corr - the correlation of time preferences over time. Sig - whether time preferences are significantly related over time. Inc - whether the experiment was incentivized rather than hypothetical.

Social Preferences: The results regarding stability of experimentally-measured social preferences can be found in Table A-3. In this case, correlation coefficients range from -0.15 to 0.69, similar to the range for time and risk preferences. If one excludes the studies with less than 100 observations then only one study remains, with a range of 0.12 to 0.28.[7]

[6] McLeish and Oxoby (2007) (86 Canadian students over seven weeks) and Halevy (2015) (117 Canadian students over four weeks) seem to have the data available to test stability of time preferences, but do not do so.

[7] Two additional papers, Volk et al. (2012), and Sass and Weimann (2012), study the aggregate decay of cooperation over time and/or categorize individuals into different types based on their play and look at the stability of their type over time. It is not clear whether or not these two papers find evidence of stable decisions in the same game repeated over time at the individual level.

Table A-3: Stability of Social Preferences

Paper	Population	Time	Corr	Sig	Inc	game/preference
Carlsson et al. (2014)	196 Vietnamese	6 years	0.12 - 0.28	yes	inc	public good
Lönnqvist et al. (2014)	22 German students	1 year	0.69	yes	inc	trust
Brosig et al. (2007)	40 German students	3 months	0.09-0.48[1]	no/yes	inc	altruism
Brosig et al. (2007)	40 German students	3 months	-0.15-0.56[1]	no/yes	inc	sequential PD

Corr - the correlation of social preferences over time. Sig - whether social preferences are significantly related over time. Inc - whether the experiment was incentivized rather than hypothetical.
[1] Our own calculation, from the raw data reported in the original paper.

Scanning all three tables, no obvious patterns appear. The results are similar for games played at longer and shorter intervals, for incentivized and hypothetical games, and for games played with student and non-student populations. While we see that there are many papers which study the stability of measures of preferences over time, the contribution of our paper is that we look at both experimental and survey measures of all three (risk, time, and social preferences) in a relatively large dataset over relatively long periods of time with a diverse population in a developing country, and that we also have data on other real-world outcomes for these same individuals.

2.2.2 Impact of Events on Preferences: Economic Shocks, Natural Disasters, and Conflict

Research studying how shocks affect preferences usually starts from the underlying implicit assumption that, in the absence of the shock, preferences would have changed less. Originally researchers were most interested in studying how job market shocks cause changes in preferences. Many papers find that changes in income, unemployment, health status, and family composition do not lead to changes in risk preferences (with direct evidence in Sahm (2012) or indirect evidence in Brunnermeier and Nagel (2008) and Chiappori and Paiella (2011)) or time preferences (Harrison et al., 2006; Meier and Sprenger, 2015).[8] In a developing country context in rural Malawi, Giné et al. (2014) find no impact of household shocks such as a death

[8]Guiso et al. (2011) do find their entire sample to become more risk averse after the financial crisis, but find no time-varying variables at the individual level which impact levels of risk aversion.

in the family or shortfalls in expected income on time preferences. This suggests that risk and time preferences may be relatively unaffected by such shocks. Carvalho et al. (2014) look at low-income Americans and find that individuals are more present-biased regarding money before payday than after, but they hypothesize that this is most likely due to differences in liquidity constraints rather than differences in preferences since they behave similarly for inter-temporal decisions regarding non-monetary rewards and play no differently in risk experiments. Meier and Sprenger (2015) suggest that changes in measured preferences over time thus may either be noise, or may be orthogonal to socio-demographics.

Another slightly more recent group of papers disagrees with this assessment and does find that preferences are affected by shocks. Krupka and Stephens Jr (2013) show that economic shocks are correlated with changes in time preferences, while Fisman et al. (2013) find that economic shocks increase selfishness.[9] Booth et al. (2014) find that randomly assigning female college students to single-sex discussion sections causes them to make more risky decisions in games. In a developing country context Dean and Sautmann (2014) find, using weekly data, that individuals become more patient when they have positive income or savings shocks, and more impatient when they face consumption shocks (increased expenditures due to damage or loss of property, or due to illness of a family member). Menkhoff and Sakha (2014) also look in a developing country and find that agricultural shocks (drought and flood) and economic shocks (an increase in the price of inputs or the collapse of a business) causes rural Thai respondents to become more risk averse. Finally, using a randomized controlled trial Carvalho et al. (2014) find that Nepali women who are randomly offered savings accounts are more risk-taking and more patient than those who are not.

There is a new burgeoning literature looking at how preferences are impacted by extreme events such as civil wars or natural disasters. While this literature is fascinating, authors tend to face two main difficulties. First, data on preferences is usually only available

[9] Alesina and La Ferrara (2002) and Malmendier and Nagel (2011) use survey rather than experimental data and find that individuals who have experienced economic shocks are less trusting and less willing to take risks respectively.

after the event and not before. Second, it is hard to construct a control group, since these events affect different populations differentially.

These papers find divergent results, which have not yet been reconciled with one another. The research on natural disasters (including earthquakes, hurricanes, and tsunamis) suggests that such shocks make people either more risk averse (Cameron and Shah, 2015; Cassar et al., 2011; van den Berg et al., 2009), less risk averse (Bchir and Willinger, 2013; Willinger et al., 2013; Eckel et al., 2009; Hanaoka et al., 2014), or have no effect on risk preferences (Becchetti et al., 2012); either more impatient (Bchir and Willinger, 2013; Cassar et al., 2011), less impatient (Callen, 2011), or no consistent effect on time preferences (Willinger et al., 2013); either more trusting (Cassar et al., 2011) or no difference in level of trust (Andrabi and Das, 2010); less trustworthy (Fleming et al., 2014); and more altruistic (Becchetti et al., 2012).[10]

The research on conflict (including civil wars and political violence) likewise shows contradictory results. Findings suggest that conflict may decrease risk aversion (Voors et al., 2012) or increase risk aversion (Moya, 2011; Callen et al., 2014; Kim and Lee, 2014); decrease patience (Voors et al., 2012); lower trust (Cassar et al., 2013); increase initial trustworthiness but lower subsequent trustworthiness (Becchetti et al., 2014); increase altruism (Voors et al., 2012); and increase egalitarianism (Bauer et al., 2014).

Finally, there is a bit of evidence regarding how experiences in one experiment may impact play in later experiments and real-world decisions. Cai and Song (2014) find that experiencing more disaster in an insurance game increases uptake of a real insurance product but does not influence risk aversion as measured in a subsequent game. Jamison et al. (2008) find that being deceived in one experiment leads participants to behave more inconsistently (exhibit more multiple-switching behavior) in future experiments measuring risk aversion. Matthey and Regner (2013) find that individuals who have participated in experiments in the past behave more selfishly in subsequent experiments.

[10] Castillo and Carter (2011) find non-linear effects of weather shocks on trust and trustworthiness, with small shocks increasing both but larger shocks decreasing both.

2.2.3 Stability of Preferences Measured in Different Games

There are many papers which look at the stability of preferences measured in different games and we only mention a few here. This subsection of the literature review is not as comprehensive as the subsection reviewing the literature on stability measured over time. Most research shows that risk preferences are not stable across different settings or different games (Binswanger, 1980; Isaac and James, 2000; Kruse and Thompson, 2003; Eckel and Wilson, 2004; Berg et al., 2005; Anderson and Mellor, 2009; Vlaev et al., 2009; Dulleck et al., 2015). On the other hand, Choi et al. (2007) find that risk preferences are stable across games when the games in question are quite similar to one another. Reynaud and Couture (2012) find that risk preferences are stable both across games and within the same game at different levels of stakes, while Dulleck et al. (2015) using a similar design only find stability across levels of stakes for the same game but not across different games.

Time preferences have been found to be correlated across goods (Reuben et al., 2010; Ubfal, 2014) and across lengths of time (McLeish and Oxoby, 2007; Halevy, 2015). Social preferences have been found to be rather stable both using variants of the same game (Andreoni and Miller, 2002; Fisman et al., 2007) and using very different games (Ackert et al., 2011; de Oliveira et al., 2012), although Blanco et al. (2011, Table 3) find a low correlation across most games in their sample.

Although our review of previous research suggests that risk preferences may be less stable across different games than time and social preferences are, our review in this subsection of the literature on stability across games is not exhaustive nor does it take into account publication bias. One message that seems to be supported by the majority of papers is that preferences tend to be more stable when measured in similar games or games which are variants of one another. When preferences are measured in very different games, they are less likely to be consistent. This conclusion was reached long ago by Slovic (1972) with regards to experiments on risk taking and seems to more generally continue to be verified.

2.3 Datasets

We have survey and experimental data from 2002, 2007, 2009, and 2010 for different subsets of the same sample. As the original purpose of this data collection was not to look at the stability of preferences over time, both the samples and the experiments varied across rounds. Summary statistics of all the variables analyzed here can be found in Table 2.1.

From looking at the summary statistics table, we note something which may hint at the results we will find below. The means of the survey questions (for example, questions such as what share of people in the world do you trust) appear to be much more stable over time than the experimental data (for example, the amounts sent in the dictator games). On the other hand, we do see a similar pattern in both the 2007 and 2009 dictator data, where the least is sent in the anonymous game; the most is sent in the chosen-revealed game; and the amounts sent in the revealed and chosen non-revealed games lie somewhere in between. A discussion of and explanation for this pattern in the 2007 data can be found in Ligon and Schechter (2012).

2.3.1 Sample Selection

In 1991, the Land Tenure Center at the University of Wisconsin in Madison and the Centro Paraguayo de Estudios Sociológicos in Asunción worked together in the design and implementation of a survey of 285 rural Paraguayan households in fifteen randomly chosen villages in three departments (comparable to states) across the country. The households were stratified by land-holdings and chosen randomly.[11] The original survey was followed by subsequent rounds of survey data collection in 1994 and 1999. Subsequent rounds including both survey and experimental data were conducted in 2002, 2007, 2009, and 2010.

In 2002 the survey began to include questions measuring trust and economic experi-

[11] There was a sixteenth community which was not chosen randomly and which consisted of Japanese farmers. After the 2002 survey these households were dropped and their data is not analyzed in this paper.

ments. By that point it was only possible to interview 214 of the original 285 households. Of the 214 households interviewed, 188 also sent a household member to participate in the economic experiments.

In 2007, new households were added to the survey in an effort to interview 30 households in each of the fifteen villages. (This meant adding between 6 and 24 new households in any village in addition to the original households.) Villages ranged in size from around 30 to 600 households. In total 449 households were interviewed, of which 371 sent a household member to participate in the economic experiments. We interviewed 195 of the previous 214 households and added 254 new households.

In 2009, we returned only to the two smallest villages. Of the 59 households interviewed in these two villages in 2007, we interviewed someone from 52 of those households in 2009. In this round, the experiments were conducted as part of the survey and so all surveyed households also participated in the experiments.

Finally, in 2010 we returned to the ten villages in the two more easily accessible departments, excluding the five villages in the more distant department. These ten villages included the two villages interviewed in 2009. In this round, we played a reciprocity experiment with 119 of the 299 individuals interviewed in 2007 in those ten villages. The households who participated were those chosen by political middlemen.

The numbers cited in the previous paragraphs refer to the number of households followed over time. As preferences are individual characteristics, rather than household characteristics, we also care about whether the same individual responded to the survey or participated in the experiments. In 2002 we told individuals that we preferred to run the surveys and games with the household heads but allowed other household members to participate if the head was not available. For the 2007 *survey* we tried to follow up with the 2002 game player if possible. The 2007 *games* were with the household head if possible. In 2009 we interviewed all adults in each household. In 2010 we followed up with the 2007 survey respondent. For more details on the 2002 dataset see Schechter (2007a), for the 2007 dataset see Ligon and Schechter (2012), and for the 2010 dataset see Finan and Schechter (2012).

In sum, while 214 households were interviewed in 2002, and the sample was expanded to cover 449 households in 2007, the number of observations we can compare across rounds is smaller. Table 2.2 presents the number of observations in each survey and the number of observations for the same person across rounds.

The individuals we follow over time may be a rather select sample. They are still alive and haven't migrated away. They also include those households in which we were consistently able to contact the same individual in each round. One could conduct a selection analysis if one had access to a variable which affected the likelihood of staying in our sample, but not affect social, time, or risk preferences. Though we can think of no such variable, we believe that our sample consists of precisely those people whose preferences are most likely to be consistent over time. They are the most stable individuals: people who continue to live in the same village and be available to be interviewed over a period of many years. So, we believe that our estimates of the stability of preferences over time should overstate the stability of preferences over time. Given that we find that experimental measures of preferences are not very stable in our sample, one would expect that a sample without such attrition problems might be even less stable.[12]

We do test whether the people who remain in our survey are different from those who attrit. Table 2.3 shows average characteristics (as measured in 2002) for two mutually exclusive subsets of the individuals who participated in 2002: those how remained in later rounds and those who attrited. Table 2.4 shows average characteristics (as measured in 2007) for the individuals who participated in 2007 and did or did not attrit in later rounds. To distinguish how much selection there is at the village versus the individual level (since not all villages were visited in each survey round) we show unconditional tests for significant

[12]Of course, even if one could measure preferences over time without attrition in a university lab, there is selection in terms of what type of person signs up to participate in university experiments. In this case, we start out with a more representative sample of the Paraguayan countryside.

differences in characteristics and also show such tests conditional on village fixed effects.

Looking across the two tables, the number of significant differences we find is not too much higher than what we would have expected by chance (17 out of 124). It appears that those individuals who remain in our sample over time may be slightly older and less educated, as well as slightly more trusting. We control for village fixed effects, age, education, sex, and household size in our regressions to help account for some of this selection.

2.3.2 Survey Data

The data for which we have the most continuity across rounds is the survey trust data. In 2002 the survey asks what share of people in the world, people in the village, and close neighbors they trust. The survey also asks what share of their village-mates would take advantage of them if given the opportunity. Possible answers for both sets of questions are 5-all, 4-more than half, 3-half, 2-less than half, and 1-none. While the correct cardinality is approximately 1, .75, .50, .25, and 0, as this is just a linear transformation of the 1-5 scale we have left the variable in its original form for regression analysis. Respondents are also asked if they think it is bad if somebody buys something knowing it is stolen (The variable equals 1 if they say it is very bad and 0 if they say it is a little bad or not bad at all.)

These same questions all reappear in the 2007 survey, although for the final question it is made more specific asking if they think it is bad if somebody buys a radio knowing it is stolen. In this round a new negative reciprocity question was added asking if someone put them in a difficult position would they to do the same to that person (1 equals always or sometimes, 0 equals never). All of these questions were continued in the 2009 survey. The 2010 survey was shortened so that it only asks about trust in villagemates and negative reciprocity.

We include measures of trust (from both surveys and experiments) in our analysis of social preferences although one might argue that trust measures beliefs about others rather than an underlying social preference (Fehr, 2009; Sapienza et al., 2013). For example, if one sees that trust is going down over time, as measured either by a decrease in the amount sent in a trust game (described later) or an increase in the share of one's village-mates one believes would take advantage if given the opportunity, this may be due to one of many different reasons. It may be because the respondent's social preferences are changing, for example the respondent has become inherently more trusting; it may be because social preferences in the village are changing, for example trustworthiness in the village is going down causing the individual to be more trusting; or it may be because the respondent's beliefs have changed, for example the respondent experienced a robbery and so now believes his village-mates to be less trustworthy. Given that some piece of trust is due to underlying preferences, and that we can control for village fixed effects which capture the environment of trustworthiness in the village, we include trust in our analysis.

2.3.3 Experimental Data

Because each round of data collection was conducted to answer different questions, rather than with the express purpose of looking at the stability of preferences over time, some of the experiments conducted in each round differ while some are repeated.

Risk Preferences

In 2002 we conducted an incentivized risk experiment in which players were given 8,000 Gs and chose how much of that to bet on the roll of a die (Schechter, 2007b). At that point in time the exchange rate was approximately 4,800 Gs to the dollar, and a day's wages was 12,000 Gs. They could choose to bet 0, 2, 4, 6, or 8 thousand Gs, and different rolls of the dice led to losing all their bet, half their bet, keeping their bet, or earning an extra 50%, 100%, or 150% of their bet. The measure of risk preferences is the amount of money bet on the die.

The 2007 and 2009 surveys both contained hypothetical risk questions based on the question asked in the Mexican Family Life Survey (MxFLS). The respondent is asked if he prefers 50,000 Gs for sure or a 50-50 chance of 50 or 100 thousand Gs. If he prefers the lottery, he is then asked if he prefers 50 thousand Gs to a 40/100 lottery. Respondents who still prefer the lottery are then offered 30/100, 20/100, and finally 10/100 lotteries. The measure of risk preferences is the number of risky choices preferred. The 2010 survey does not measure risk preferences.

Time Preferences

The 2007 and 2009 surveys ask hypothetical questions measuring time preferences, while the 2002 and 2010 surveys do not broach this topic. Respondents are asked whether they prefer 50,000 Gs today or 75,000 Gs a month from today. For those who prefer the money today, they are asked if they would prefer 50,000 Gs today or 100,000 Gs a month from today. If the person still prefers the money today, he is asked how much one would have to offer him to convince him to wait for a month. The measure of time preferences is the amount of money he would need one month later.

Social Preferences

Every round of survey collection conducted at least one incentivized experiment measuring social preferences, although some of the experiments differed across rounds.

2002 Trust Game: We conducted an incentivized trust game in which every participant played in both the role of trustor and trustee (Berg et al., 1995). The trustor was given 8,000 Gs and decided how much of that to keep and how much to send to the trustee. The trustee was randomly chosen from the group of players and received the amount sent by the trustor, tripled. The trustee then decided how much of that to keep and how much to send back to the trustor, if any. These games were anonymous as neither player knew with whom he was matched. The amount sent by the trustor is often used to measure trust (although it also measures risk aversion and altruism), while the amount sent by the trustee is often used to measure trustworthiness or reciprocity (although it also measures altruism).

2007 Variants of Dictator Game: Participants were asked to play four distinct dictator games. For more details see Ligon and Schechter (2012). The games varied in whether or not the dictator was anonymous, and in whether the recipient was randomly selected from the set of households in the village, or was chosen by the dictator. In each of the games the dictator was given 14,000 Gs and decided how to divide it between herself and another household. The other household could be any household in the village, not just those participating as dictators in the experiment. We doubled any money shared by the dictator and also added a random component with mean 5,000 Gs before passing it on to the recipient at which point the game ended.

In the 'Anonymous-Random' (AR) game, the dictator decided how much to share with a randomly selected household in the village, and neither the dictator nor the recipient ever learned who the other was. This is the canonical dictator game and it measures undirected altruism or benevolence. In the 'Revealed-Random' (RR) game, the dictator once more chose how much to send to a randomly selected household. It was known that the identities of the dictator and recipient would subsequently be revealed to each other. The amount sent measures a combination of undirected altruism and fear of sanctions.

In the two 'Chosen' games the dictator chose a single, common recipient. In the 'Anonymous-Chosen' game, the recipient never learned the identity of the dictator. The amount sent measures a combination of undirected and directed altruism. In the 'Revealed-Chosen' game, the dictator's identity is revealed to the recipient. The amount sent in this final game measures a combination of undirected and directed altruism, fear of sanctions, and reciprocity.

2009 Variants of Dictator Game: Participants played in games which were quite similar to those in 2007. The difference is that in 2009 no random component was added to the amount received by the recipient. And, some (but not all) of them played the two 'Chosen' games.

2010 Reciprocity Game Respondents in 2010 were asked to play a reciprocity game similar to that designed by Andreoni et al. (2003) with the political middlemen who chose them. The respondent was told that the middleman was given 12,000 Gs and could choose to send 2, 4, 6, 8, or 12,000 Gs to the respondent. The respondent could take the money and choose to do nothing, or he could choose to reward or fine the middleman. For every 100 Gs the respondent put toward the reward or fine, the middleman received an extra 500 Gs, or had 500 Gs taken away from him. Neither player could earn less than 0 Gs total.

We used the strategy method, asking the respondent what he would do if he were to receive each potential amount. Here we define a negative reciprocal individual as one who would fine the middleman if he sent 2,000 Gs (the lowest possible amount to send) and a positive reciprocal individual as one who would reward him if he sent all 12,000 Gs.

Experiences while Playing

Not only will we look at the stability of preferences over time, but we will also look at whether and how experiences in the experiments in one year impact play in later years of the experiments. The different experiences we will look at include the following:

- In the 2002 risk games the player's winnings were determined by the roll of the die. The die was rolled in front of the player. The higher the roll of the die, the luckier the player was and the more he won.

- In the 2002 trust game, the trustee received a certain amount from the trustor. Receiving more can change the trustee's perception of the trust and generosity of his village-mates.

- In the 2002 trust game, the trustor chose how much to send to the trustee. The trustee then decided how much of that, if any, to return. Receiving back a higher share of the amount sent can change the trustor's perception of the generosity and reciprocity of his village-mates.

2.3.4 Shock Data

Finally, we study how preferences react to household experiences. We have data on income, health, and theft shocks which are arguably some of the most important shocks faced by agricultural households who do not have access to crop insurance or health insurance. Specifically, the 2002, 2007, and 2009 surveys contain information on household income and theft experienced in the past year. Those rounds also asked the days of school or work lost to illness in the past year.

2.4 Analysis and Results

Our hypothesis is that preferences as measured by experiments and surveys are significantly correlated over time. In the first stage of our analysis we look at the stability of preferences (as measured in experiments and surveys) over time. We show the correlation coefficient between the two variables of interest and evaluate whether it is statistically significantly different from 0. In addition, we run regressions of the later variable on the earlier variable with village fixed effects while also controlling for log income, sex, age, and education level as measured in the later year.[13] In results not shown here, we re-run all of the analysis dividing our sample into those below and above the median in terms of age (50 years) or education (5 years) but do not find any consistent differences.

[13] The only exception is that we do not have a measure of income in 2010 and so we use 2007 income.

In all tables of results we show asterisks (*) to represent levels of significance according to traditional p-values which treat each test as a lone independent test. Given that we are testing multiple hypotheses at the same time, these traditional p-values may over-reject the hypothesis of zero correlation of measures over time. To account for this, we additionally show plus signs (+) to represent levels of significance according to q-values for False Discovery Rates (FDR) to correct for multiple comparisons. We use the calculation designed by Benjamini and Hochberg (1995) and explored in detail by Anderson (2008).

Theory would suggest that if these measures of preferences are not highly correlated over time, it is because the environment has changed. For example, a monetary windfall may cause a person to make less risk averse choices. Thus, our secondary null hypothesis is that certain major shocks in a person's life will impact their preferences as measured by these games and questions. Thus, in the second stage of analysis we look at how experiences in previous experiments affect play in later experiments; and in the third stage we look at how income, theft, and health shocks impact preferences.

Overall, we find that experimental measures are less stable than the survey measures. This may be because some of the experiments varied across years, while the survey questions remained the same in each survey. Semantically, when we compare play in the same game at different points in time we are running "test-retest" analysis, while when we compare play in different games purportedly measuring the same preferences at different points in time we are running a "construct validity" analysis. That said, even when we do have experimental measures for the exact same game at two points in time, for example the variants of the dictator games or the repeated risk game, the choices are not any more stable than measures of the same preferences from different games.

Even for those cases where the games played in both rounds are exactly the same, we might not necessarily expect these measures to be stable. Although underlying preferences may be stable, the choices made in experiments depend on preferences as well as on the environment, and the latter may have changed over time.

We think this is not a serious issue in our study for three reasons. First, if everyone is getting poorer or wealthier across the board at the same rate, then we would still imagine that the people who were most risk averse in earlier years would continue to be the people who were most risk averse in later years. Second, we run regressions controlling for income, education, sex, age, and village fixed effects and so our results are conditional on the environment. Finally, if the environment played a large role, then we would expect to find significant results in the third stage of analysis looking at how income, health, and theft shocks are related to play in games. But we do not find any correlation between shocks and preferences.

The decisions made in the experiments are not correlated over time, nor are they correlated with shocks to the environment, suggesting that individuals' decisions may contain substantial noise. This may be more likely in a developing country context where participants experience high levels of poverty and scarcity and have low levels of education.

With so much data from so many different experiments and survey questions, in so many different rounds of data collection, it would be possible to look at thousands of correlations. We limit ourselves by only showing results which fit the following criteria. First, given that we have much incentivized experimental social preference data, we do not analyze the data from the hypothetical social preference experiments.[14] We do analyze the hypothetical risk and time preference data, since we have less data on these preferences.

Second, we do not include comparisons between the 2002 and 2009 data, since the sample size in those comparisons is only 21, nor do we include comparisons between the 2009 and 2010 data, since that sample size is only 23. The comparisons we do include are 2002 vs 2007, 2002 vs 2010, 2007 vs 2009, and 2007 vs 2010.

Third, we only compare variables which are measures of the same thing at different points in time. For example, we look at how survey measures of trust are correlated over time, and we look at how experimental measures of trust are correlated over time. But, we do not look at how *survey* measures of trust are correlated with *experimental* measures of trust over

[14] We have analyzed this data, and most of the time the hypothetical social preference data are not stable over time.

time, or at how experimental measures of trust are correlated with experimental measures of risk aversion over time. Although there are definitely many interesting comparisons to be made, we avoid data mining by setting these rules for ourself in advance.

2.4.1 Stability of Preferences

Risk and Time Experiments

Table 2.5 shows the results for risk and time preferences. The 2002 game was incentivized. The same hypothetical risk game was played in both 2007 and 2009. The research cited in Section 2.3 has shown that players do not have stable preferences across different risk games played at the same point in time. This makes it less likely that we will find risk preferences are correlated between 2002 and 2007, but this critique should not affect the 2007-2009 comparison. Still, the table shows no significant correlation between risk preferences measured at different points in time. If we run the analysis using coefficients of relative risk aversion rather than the number of risky choices, we again find insignificant results of similar magnitudes. The magnitudes of the correlations in measures of risk preferences over time we find in Table 2.5 are far below those magnitudes cited in Section 2.1.

On the other hand, time preferences (measured from the answers to the same series of hypothetical questions in 2007 and 2009) are remarkably stable over time. We cannot reject that the coefficient on time preferences in 2007 predicting time preferences in 2009 is 1. This is in stark contrast to the results for the hypothetical risk games in 2007 and 2009 where we cannot reject that the coefficient is 0. This may be because time preferences are more stable over time than risk preferences, or because people understood the time questions better and so they contain less noise.

One indication that the 2007/2009 risk preference data contains much noise is the fact that 19% of respondents in 2007 and 27% in 2009 preferred 50,000 Gs for sure to a 50/50 chance of winning 50,000 or 100,000 Gs, even after it was pointed out to them that this was a dominated option. This is not unusual. In the MxFLS, upon which the questions we used in our survey were based, nearly a quarter of the individuals chose the dominated option even after having it explained a second time (Hamoudi, 2006). In a similar set of questions in the 2007 Indonesian Family Life Survey (IFLS), 41% of the almost 30,000 respondents chose a dominated option even after it was pointed out to them (Caruthers, 2013).

This is reminiscent of Gneezy et al. (2006) who find that many individuals value a risky prospect less than its worst possible realization. Subsequent work argued however that the effect is eliminated either when focussing on the subset of the subject pool which passes a comprehension test or when making the instructions more clear (Keren and Willemsen, 2009; Rydval et al., 2009). Choices made by individuals who do not fully understand the experiment may be filled with much noise, and thus these choices may not be significantly correlated over time.

This suggests that one reason we find such low correlations for measures of risk aversion compared to those found in the rest of the literature may be that our sample has a much lower level of education than most of the other experiments. Choi et al. (2014) find that younger, more educated, and wealthier respondents behave more consistently across multiple games measuring risk and time preferences played at the same point in time.

This might suggest that we would find higher correlations among the more educated individuals in our sample. But, if we divide our sample into those below and above the median in terms of age or education, we find a similar lack of significance in all groups for risk preferences. Given that the median in our sample is 5 years of education and the maximum is 12, our more educated half of the population still has much lower education levels than most other samples studied in the literature. For time preferences, the strong correlation seems to be coming entirely from the younger and more educated halves of the population. The correlation in the older and less educated halves are insignificant.

Looking forward, in the next subsection we show evidence that the more specific a trust question is, the more stable its answer is. While some risk games conducted in developing countries are framed in terms of crop choice or other real-world investment decisions, our risk games were very abstract. It would be interesting for future research to study whether decisions made in framed experiments are more stable than those made in artefactual (not framed) experiments.

Social Preference Surveys

Table 2.6 shows the results for the survey questions which measure social preferences and were repeated across years.[15] Many of the variables show a surprising amount of stability over time. Almost all of the comparisons are significant. On the other hand, when looking at the regression coefficients we can also reject that the relationship is one-to-one.

Imagine a model in which observed trust is equal to an underlying trust parameter plus two additional pieces: recent experiences and measurement error. If most of the variation around the underlying trust parameter is due to recent experiences, we might expect the correlation to go down as more time passed. On the other hand, if most of the variation around the underlying trust parameter were due to measurement error, then we might expect the size of the correlations to be similar over time.

The correlation and regression coefficients are almost always highest for the 2007 vs 2009 comparison, which is also the comparison over the shortest time span. Unfortunately, this is also the only comparison which involves the 2009 data. Because the 2009 data only comes from the two smallest villages, there is very little overlap between either 2002 or 2010 and 2009. Thus, we cannot test whether the high correlation between 2007 and 2009 is due to the select sample in 2009 or if it is due to the shorter time span.[16] But, given the large magnitude of the coefficients for the one comparison shown between 2002 and 2010, it suggests that it is not the case that the correlation goes down significantly over long periods of time. This may imply that most variation is due to measurement error rather than the influence of recent experiences, which may in turn imply that these are innate preferences

[15]Not all questions were asked in all years.
[16]There are only 14 individuals who participated in the 2002, 2007, and 2009 surveys.

which are not strongly affected by shocks.

There also seems to be some suggestive evidence that as the questions become more specific, the responses exhibit more stability. For example, the correlation and regression coefficients regarding trusting people in the world are rather low and often not significant. The coefficients regarding trusting people in the village are higher, while the coefficients regarding trusting close neighbors are usually even higher, although most of these differences are not significant.

Social Preference Experiments

Table 2.7 shows the results for the experimental measures of social preferences. The results are loosely divided up by social preference into altruism, trust, and reciprocity. Many of the decisions made in the games measure multiple preferences at the same time. For example, the amount sent by the trustor in the trust game is a combination of both trust and altruism. The amount returned by the trustee in the trust game combines altruism, trust, and reciprocity. The amounts sent by the dictator in the two non-revealed games measure altruism. The amounts sent in the two revealed games additionally measure trust since a person who trusts his village-mates more will send more in the hopes that the recipient will return more to the dictator once the recipient learns the dictator's identity. Finally, the reciprocity experiments measure reciprocity.

When looking at the stability of different social preferences over time we look at choices made in different games, some of which are more pure measures of the preference than others. For example, in 2007 and 2009 we have measures of altruism from anonymous dictator games which should arguably be the best measures of altruism. In 2002, we only have the amounts sent as trustor and trustee in the trust game. Although altruism is sure to influence those decisions, trust and reciprocity will influence them as well. We look at whether these decisions are correlated over time not because we believe that there is a single latent trait which drives behavior across all the experiments. We look at them because we believe that all three of those decisions involve altruism, and if altruism is a stable preference over time then those decisions should be correlated.

With regards to altruism, we find a strong correlation between the amount sent as trustor in 2002 and the amount sent in the anonymous game in 2007. We also find a strong correlation between the two measures of trust in 2002 and 2007. Individuals who give generously in one game at one point in time are more likely to be generous in a different game at a different point in time.

We find no correlation between the share returned in 2002 and later measures of reciprocity or later measures of altruism, but we do find correlation between the share returned in 2002 and our 2007 measure of trust. Although this correlation is relatively strong, interpreting the share returned as a measure of trust (rather than of trustworthiness) is a bit tenuous.

Finally, within the trust and altruism sections, we look at whether the amounts sent in the various dictator games played in 2007 and 2009 are correlated. Given the high degree of similarity in the games, it is surprising that not a single one of the four comparisons is significant.

According to our loose categorization of preferences measured by different games, we see the most stability in trust preferences, weak stability in altruistic preferences, and no stability in reciprocity. What is perhaps more noticeable than the stability of experimental measures of social preferences, may actually be their lack of stability. In comparing Table 2.6 with Table 2.7, the higher relative stability of survey-based measures of social preferences compared with experiment-based measures is evident.

Graphical Representation of Standardized Results

It is difficult to compare the magnitudes of the stability of preferences shown in Tables 2.5, 2.6, and 2.7 as they are spread across multiple tables and the variables are not standardized. Thus, we condense all of the previous results in Figure 1. In this figure we run similar regressions to those in our other tables (controlling for log income, sex, age, education, and village fixed effects) and we show whisker plots of the coefficients and 90% confidence intervals.

The difference between the results in the figure and those in the tables is that in this figure we standardize the preference variables on both the right and left-hand side so that they have a mean of 0 and standard deviation of 1. One thing to keep in mind when looking at this figure is that the explanatory and dependent variables were standardized separately. For example, in Table 2.5 the regression coefficient on the time variable was approximately 1. But, in the figure we see that it is closer to 0.4. This is because the mean and standard deviation of that variable is much higher in 2007 compared to 2009.

What becomes clear from this figure is that the survey measures of trust are quite strongly correlated over time. On the other hand, it also becomes clear that this relationship is far from being one-to-one. When looking at the experimental measures of social preferences, they do tend to be positively correlated over time, but the confidence intervals on the experimental measures are much wider than those on the survey measures. Finally, the confidence intervals around the measures of risk preferences are rather narrow, and that relationship seems like a more precisely estimated zero.

2.4.2 Impact of Outcomes in Previous Games on Later Games

Impact of Experience in the Risk Game

In the 2002 risk experiment the players' payoffs depended on the roll of a die. For those most risk averse people who didn't bet anything, we didn't roll the die, but we have an observation for all others. Appendix Table A-1 explores whether the roll of the die has an impact on play in the later risk experiment. We additionally control for the baseline level of risk aversion as measured in the earlier experiment.

We find weak evidence that having a good roll of the die in 2002 leads people to choose more risky choices in 2007. The magnitude of this effect is such that a roll of the die that is higher by 1, leads to 0.2 extra risky decisions being made out of 5 in 2007. If we look instead at coefficients of relative risk aversion (CRRA), we find that a roll of the die that is higher by 1 leads to a decrease of 0.16 in the individual's estimated CRRA in 2007.

A good roll of the die might lead players to think they will be more lucky in games played with us in the future. In results not shown here we do test if the roll of the die had an impact on subsequent risk-related decisions made in the real-world, such as the number of different crops planted, and we find no impact. This suggests that the impact only involves learning about the experimental setting, and does not change the players' underlying risk preferences.

Impact of Experience in the Trust Game

In 2002, the payoffs of trustors and trustees depended both on the choices they themselves made, as well as on the actions of their partners. The payoffs of a trustor who sent a positive amount depended on how much (if anything) the trustee with whom he was paired decided to return. This experience could give a player new information regarding how trustworthy and how altruistic his village-mates were within the experimental setting, and could then potentially impact how he played in games in later years. This impact could either be due to a change in how he expected others to behave, or due to a change in how he thought he himself ought to behave given his new signal regarding local experimental norms.

Appendix Table A-2 contains the results of this analysis. Because it might be the case that trustees return a different share depending on how much they received from the trustor, when looking at impacts on generosity in 2007 we also control for the amount sent by the respondent as trustor in 2002 in all regressions in this table. This also helps to control for baseline levels of social preferences, and we additionally control for the average share the respondent returned as trustee in 2002.

We find that receiving a higher share back as trustor in 2002 leads to an increase in altruism in 2007. The impact of going from receiving back all your money in the trust game, to doubling your money in the trust game is to increase the amount sent in 2007 by almost 1,000 Gs (out of a potential total of 14,000 Gs).[17] This is in line with Matthey and Regner (2013) who find that individuals who have participated in more experiments keep more money for themselves, with suggestive evidence that this is due to them receiving less than they had expected in the previous experiment.

We see a similar magnitude increase in the amount sent in all versions of the 2007 dictator games (anonymous and revealed, and randomly assigned and explicitly chosen). Thus, it seems likely that this impact is due to changes in the individual's sense of what the norm he should be following is, rather than a change in how he expects his village-mates will respond. Finally, receiving more as trustor in 2002 leads individuals to be significantly more negative reciprocal in 2010. If they receive more money from their partner in 2002, they are more likely to punish bad behavior from partners in 2010, enforcing norms of good behavior. This last result is the only one which stands up when considering that we are testing multiple hypotheses at once.

In 2002, trustees also receive different amounts of money depending on the play of their partner. Similar to the discussion above, this could change the individual's perception of how generous his village-mates are, and could also change his perception of the social norm in the village. Appendix Table A-3 looks at the impacts the amount received as trustee in 2002 may have on play in later games. Similarly to the previous table, we see that individuals who receive more as trustee in 2002 send more as dictator in all four versions of the dictator game. So, again, we do see that altruism in 2007 is increased if people receive more money from their village-mates in 2002.

We think that these changes are due to learning about the within-game sharing norms. We have tested whether different experiences as trustor or trustee lead to differences in real-world outcomes such as future gift-giving, and find no impact. This suggests that

[17] Since we tripled the money sent by the trustor, he could potentially receive back anywhere between none of the money he sent and three times the money he sent.

these experiences do not change underlying preferences but only change behavior within the experimental setting.

2.4.3 Impact of Real-World Shocks on Preferences

Finally we study the correlations between preferences and changes in income, changes in theft, and sickness in the past year. In the population we are studying, these are the most common and most relevant shocks. Income shocks are not exogenous, and so such an analysis should be taken with a grain of salt. Theft and health shocks are more plausibly exogenous than income shocks. Because we did not measure income, theft, or sickness in 2010, this analysis will only look at 2002, 2007, and 2009 data. Results can be found in Appendix B.

For income and theft, we regress levels of preferences on changes in income or changes in theft experienced while controlling for levels of preferences in the previous round.[18] One might prefer to run regressions of changes in preferences on changes in income or theft. But, since our preference variables are often not exactly the same in each survey round, it is problematic to take their difference. Instead, of putting the difference in preferences on the left hand side, we control for past preferences on the right hand side.

Appendix Tables B-1, B-2, and B-3 basically show no correlation whatsoever between preferences and income or theft shocks. Although the results are far from significant, the results regularly show that survey measures of trust are negatively correlated with increases in income and increases in theft. The results for the effect of health shocks on social preferences shown in Tables B-4 and B-5 are similarly significant. Appendix Table B-5 shows some suggestive evidence that health shocks induce individuals to be less generous in experiments. Only two variables have significant negative correlations, but the majority of rows show negative correlations. None of the coefficients in any of the tables shown in Appendix B

[18]In a previous draft of the paper we ran this analysis without controlling for preferences in the previous round and the results are similarly unexciting. The advantage of not controlling for previous measures of preferences is that sample sizes are larger when we can include any individual whose household appeared in the previous round, no matter which person responded in that previous round. We have also tried regressing changes in preferences on changes in income or theft, and similarly find no strong results.

retain significance when taking into account multiple hypothesis testing.

In contrast to our results, Dean and Sautmann (2014) do find an impact of income and expenditure shocks (which includes, for example, expenditure on family illness) on time preferences in a developing country context - peri-urban Mali. One difference between their setting and ours is that their shock and preference data was taken weekly over a period of 3 weeks. Family illness may influence choices made in experiments in that very week, but months later that effect may wear off. Thus if one uses yearly data, as we do here, it may be less likely to find impacts of shocks on preferences. Additionally, Dean and Sautmann (2014) find no impact of labor income on preferences, and hypothesize that this is due to its endogeneity. They only find an impact of non-labor earnings which are more likely to be exogenous. In our rural Paraguayan sample there are almost no sources of non-labor income, so we are not able to look at this type of income. Giné et al. (2014) find no impact of shocks on time preferences among rural Malawians over six to eight weeks, but the households in their sample suffer very few shocks over this time period, so their results do not exhibit much power. Overall, the general message of these tables is that shocks of the size measured in this paper do not significantly impact our measures of risk, time, or social preferences.

2.5 Conclusion

The results presented here suggest that social preferences measured by simple survey questions are quite stable over time, while social preferences measured by more complicated experiments are much less stable. Risk preferences exhibit a relatively tight 0 correlation over time. Time preferences on the other hand are quite stable. This may suggest that some underlying preferences, for example risk preferences, are not stable. Or, underlying preferences may be stable, but the measures we get from experiments contain significant amounts of noise. One implication of our results is that researchers should feel encouraged to use more survey measures of preferences, especially in low-education settings.

This is in accord with the findings of Dohmen et al. (2011) and Lönnqvist et al. (2014) that survey measures of risk aversion are more correlated with real world outcomes and more stable over time than experimental measures of risk aversion. On the other hand, Burks et al. (2012) find that a survey measure of time preference does not perform as well as experimental measures of time preferences.

Given that our measures of preferences are not very stable over time, we then also explore whether they are affected by observable shocks. There is some evidence that being matched with a generous partner in a trust game in 2002 leads individuals to send more in dictator games in 2007. We surmise that this may be due to a change in the players' perceptions of social norms within the experimental setting in their village. On the other hand, we find no evidence that income, theft, or health shocks are correlated changes in preferences.

Overall, these results are thought-provoking for researches who use experiments to measure both preferences and their impact on real-world outcomes, especially in a developing country setting with low levels of education and high levels of poverty. Similarly to Meier and Sprenger (2015), our results suggest that variability in preference measures may be mostly due to noise. The fact that the experimental measures show such low levels of stability may encourage researchers working in developing countries both to design simpler experiments which capture less noise, and to make more use of survey measures of preferences.

Table 2.1: Summary Statistics

	Mean	Sd	Min	Max	# Obs.
bet in 2002	3.43	2.05	0	8	189
roll of die in risk game in 2002	3.37	1.69	1	6	172
# risky choices in 2007 (hyp)	2.09	1.77	0	5	449
# risky choices in 2009 (hyp)	1.55	1.53	0	5	176
time preference in 2007 (hyp)	199.35	561.22	75	5000	449
time preference in 2009 (hyp)	111.99	116.61	75	1000	176
sent as trustor in 2002	3.74	1.99	0	8	188
sent as dictator in anonymous game in 2007	5.08	2.69	0	14	371
sent as dictator in chosen non-revealed game in 2007	5.39	2.68	0	14	371
sent as dictator in revealed game in 2007	5.47	2.69	0	14	371
sent as dictator in chosen revealed game in 2007	5.93	2.84	0	14	371
sent as dictator in anonymous game in 2009	2.51	1.94	0	10	176
sent as dictator in chosen non-revealed game in 2009	2.72	2.02	0	13	129
sent as dictator in revealed game in 2009	3.01	2.18	0	12	176
sent as dictator in chosen revealed game in 2009	3.69	2.71	0	14	129
share returned as trustee in 2002	0.44	0.20	0	1	188
amount received as trustee in 2002	11.23	5.97	0	24	188
proportion received back as trustor in 2002	1.30	0.61	0	3	175
trust people in the world in 2002 (survey)	2.40	0.64	1	5	214
trust people in the world in 2007 (survey)	2.58	0.92	1	5	449
trust people in the world in 2009 (survey)	2.84	1.15	1	5	176
trust people in the village in 2002 (survey)	3.14	1.13	1	5	214
trust people in the village in 2007 (survey)	3.22	1.09	1	5	449
trust people in the village in 2009 (survey)	3.40	1.22	1	5	176
trust people in the village in 2010 (survey)	3.40	1.11	1	5	119
trust closest neighbors in 2002 (survey)	3.96	1.27	1	5	214
trust closest neighbors in 2007 (survey)	3.97	1.30	1	5	449
trust closest neighbors in 2009 (survey)	3.96	1.34	1	5	176
would villagemates take advantage if had opportunity in 2002 (survey)	2.90	1.15	1	5	214
would villagemates take advantage if had opportunity in 2007 (survey)	2.44	1.09	1	5	449
would villagemates take advantage if had opportunity in 2009 (survey)	2.56	1.14	1	5	176
bad to buy something you know is stolen in 2002 (survey)	0.90	0.30	0	1	214
bad to buy something you know is stolen in 2007 (survey)	0.87	0.34	0	1	449
bad to buy something you know is stolen in 2009 (survey)	0.85	0.36	0	1	176
negative reciprocity in 2007 (survey)	0.25	0.43	0	1	449
negative reciprocity in 2009 (survey)	0.15	0.36	0	1	176
negative reciprocity in 2010 (survey)	0.16	0.37	0	1	119
positive reciprocity in 2010	0.71	0.46	0	1	119
negative reciprocity in 2010	0.18	0.39	0	1	119

Note: Hyp denotes that the game was hypothetical. All data is from experiments unless otherwise stated.

Table 2.2: Sample Sizes

	02 survey	02 game	07 survey	07 game	09 participate	10 participate
02 survey	214	142	123	79	21	39
02 game		188	139	103	17	43
07 survey			449	282	49	119
07 game				371	41	81
09 participate					176	23
10 participate						119

Note: Each cell contains the number of individuals participating in both the row and the column data.

Table 2.3: Tests for Sample Attrition from 2002

	2002 survey and 2007 survey				2002 survey and 2010 survey				2002 game and 2007 game				2002 game and 2010 game			
	in both	attrit	diff1	diff2	in both	attrit	diff1	diff2	in both	attrit	diff1	diff2	in both	attrit	diff1	diff2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
hhd income	19,611	18,830	780	1,724	10,323	21,275	-10,952	787	13,821	23,167	-9,346	-8607	10,348	20,330	-9,981	1,472
	(47,977)	(50,209)	(6,767)	(6,455)	(8,840)	(53,656)	(8,633)	(8,761)	(18,475)	(69,961)	(7,175)	(7244)	(8,396)	(55,512)	(8,510)	(9,043)
hhd size	5.72	5.48	0.23	0.34	5.59	5.62	-0.03	0.59	5.67	5.48	0.19	0.23	5.53	5.60	-0.07	0.39
	(2.51)	(2.31)	(0.34)	(0.32)	(2.09)	(2.49)	(0.43)	(0.43)	(2.51)	(2.44)	(0.36)	(0.36)	(2.13)	(2.57)	(0.43)	(0.45)
male	0.76	0.77	-0.01	-0.01	0.79	0.76	0.03	0.13	0.73	0.65	0.08	0.10	0.74	0.68	0.07	0.22**
	(0.43)	(0.42)	(0.06)	(0.06)	(0.41)	(0.43)	(0.08)	(0.08)	(0.45)	(0.48)	(0.07)	(0.07)	(0.44)	(0.47)	(0.08)	(0.09)
age	52.89	50.30	2.59	2.39	53.85	51.33	2.52	0.61	50.74	46.21	4.53*	4.03	52.56	47.54	5.01	4.09
	(13.25)	(16.60)	(2.04)	(2.00)	(12.75)	(15.20)	(2.62)	(2.73)	(15.15)	(20.05)	(2.57)	(2.71)	(14.62)	(18.32)	(3.05)	(3.38)
ed	4.54	4.98	-0.43	-0.36	4.38	4.81	-0.42	-0.51	4.55	5.09	-0.54	-0.45	4.26	4.96	-0.70	-0.79
	(2.38)	(2.38)	(0.35)	(0.35)	(1.99)	(2.66)	(0.45)	(0.48)	(2.37)	(2.78)	(0.38)	(0.40)	(1.88)	(2.73)	(0.44)	(0.50)
bet									3.57	3.27	0.30	0.28	3.77	3.34	0.43	0.81**
									(2.20)	(1.85)	(0.30)	(0.32)	(1.91)	(2.08)	(0.36)	(0.39)
sent as trustor									3.73	3.76	-0.04	-0.19	3.95	3.68	0.27	0.70*
									(2.10)	(1.86)	(0.29)	(0.31)	(2.11)	(1.95)	(0.35)	(0.38)
trust in village	3.13	3.15	-0.02	-0.05	3.15	3.14	0.02	-0.11	3.17	3.19	-0.02	-0.16	3.12	3.19	-0.08	-0.35
	(1.15)	(1.09)	(0.16)	(0.16)	(1.09)	(1.14)	(0.20)	(0.21)	(1.16)	(1.11)	(0.17)	(0.18)	(1.10)	(1.15)	(0.20)	(0.22)
# Obs.	123	91			39	175			103	85			43	145		

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Standard deviations are in parentheses for means while standard errors are in parentheses for the tests of differences. Columns 1, 5, 9, and 13 show information for the non-attriters while Columns 2, 6, 10, and 14 show information for the attriters. Diff1 is coefficient in regression controlling only for participation at the later date. Diff2 is coefficient in regression aditionally controlling for village fixed effects. Income is in thousands of Guarani. All variables are measured in 2002.

Table 2.4: Tests for Sample Attrition from 2007

	2007 survey and 2009 survey				2007 survey and 2010 survey				2007 game and 2009 game				2007 game and 2010 game			
	in both	attrit	diff1	diff2	in both	attrit	diff1	diff2	in both	attrit	diff1	diff2	in both	attrit	diff1	diff2
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
hhd income	14,614	24,545	-9,931*	-4,161	17,510	25,607	-8,097**	243	14,743	21,600	-6,858	1,633	15,922	22,216	-6,294	320
	(11,564)	(39,286)	(5,647)	(12,959)	(13,006)	(42,735)	(3,985)	(4,255)	(11,728)	(31,981)	(5,041)	(11,432)	(8,752)	(34,051)	(3,822)	(4019)
hhd size	4.88	4.91	-0.03	-0.24	4.98	4.88	0.10	0.29	4.78	5.00	-0.22	-1.05	4.75	5.03	-0.28	-0.08
	(2.63)	(2.36)	(0.36)	(0.82)	(2.58)	(2.32)	(0.26)	(0.27)	(2.49)	(2.37)	(0.39)	(0.86)	(2.29)	(2.41)	(0.30)	(0.30)
male	0.63	0.68	-0.04	-0.04	0.64	0.68	-0.04	0.06	0.49	0.65	-0.16**	-0.01	0.65	0.63	0.03	0.13**
	(0.49)	(0.47)	(0.07)	(0.17)	(0.48)	(0.47)	(0.05)	(0.06)	(0.51)	(0.50)	(0.08)	(0.18)	(0.48)	(0.48)	(0.06)	((0.06)
age	48.49	50.15	-1.66	7.71	51.39	49.46	1.93	1.72	48.17	47.95	0.23	16.93***	51.95	46.86	5.09**	6.32***
	(17.56)	(15.30)	(2.35)	(5.50)	(15.62)	(15.51)	(1.66)	(1.81)	(18.80)	(16.14)	(2.72)	(6.24)	(15.09)	(16.64)	(2.05)	(2.19)
ed	5.12	5.05	0.08	-1.94*	5.05	5.05	-0.00	-0.23	4.90	5.21	-0.31	-1.91*	4.89	5.26	-0.37	-0.78**
	(2.50)	(3.03)	(0.45)	(1.04)	(2.63)	(3.09)	(0.32)	(0.34)	(2.36)	(2.98)	(0.48)	(1.09)	(2.10)	(3.11)	(0.37)	(0.38)
risky choices	1.71	2.13	-0.42	-0.87	2.04	2.10	-0.06	-0.21	1.90	2.13	-0.23	-0.45	2.15	2.09	0.06	-0.13
	(1.68)	(1.78)	(0.27)	(0.64)	(1.78)	(1.77)	(0.19)	(0.21)	(1.67)	(1.79)	(0.29)	(0.70)	(1.80)	(1.77)	(0.22)	(0.24)
time preference	120	209	-89	8	156	215	-58	-23	117	330	-79	19	182	188	-7	59
	(67)	(593)	(85)	(197)	(456)	(594)	(60)	(65)	(59)	(196)	(89)	(203)	(550)	(536)	(68)	(71)
sent as dictator									4.59	5.15	-0.56	-0.84	4.79	5.17	-0.38	0.17
									(2.33)	(2.73)	(0.45)	(1.01)	(2.46)	(2.75)	(0.34)	(0.35)
trust in village	3.51	3.18	0.33**	0.61	3.35	3.17	0.19	0.12	3.71	3.22	0.49***	0.66	3.47	3.21	0.26*	0.14
	(1.19)	(1.08)	(0.17)	(0.39)	(1.15)	(1.07)	(0.12)	(0.13)	(1.19)	(1.07)	(0.18)	(0.42)	(1.10)	(1.09)	(0.14)	(0.15)
# Obs.	49	400			119	330			41	330			81	290		

Note: *** p<0.01, ** p<0.05, * p<0.1. Standard deviations are in parentheses for means while standard errors are in parentheses for the tests of differences. Columns 1, 5, 9, and 13 show information for the non-attriters while Columns 2, 6, 10, and 14 show information for the attriters. Diff1 is coefficient in regression controlling only for participation at the later date. Diff2 is coefficient in regression aditionally controlling for village fixed effects. Income is in thousands of Guarani. All variables are measured in 2007.

Table 2.5: Stability of Risk and Time Preferences

Explanatory variable	Dependent variable	Correlation coefficient	Regression coefficient	# Obs.
bet in 2002	# risky choices in 2007 (hyp)	0.070	0.0662	140
			(0.0858)	
# risky choices in 2007 (hyp)	# risky choices in 2009 (hyp)	-0.059	-0.0120	49
			(0.127)	
Time preference in 2007 (hyp)	Time preference in 2009 (hyp)	0.432***	1.036**	49
		+++	(0.419)+	

Notes: Per-comparison p-values: ***p <0.01, ** p <0.05, * p <0.10. FDR q-values: +++ q <0.01, ++ q <0.05, + q <0.10 calculated for 3 hypotheses within table and column. Standard errors are in parentheses. Controls in regressions include log income, sex, age, education, and village fixed effects. Hyp denotes that the game was hypothetical.

Table 2.6: Stability of Social Preferences in Surveys

Years	Variable	Correlation coefficient	Regression coefficient	# Obs.
2002 vs 2007	trust people in the world	0.064	0.0677 (0.135)	123
2007 vs 2009	trust people in the world	0.284** +	0.339* (0.190)+	49
2002 vs 2007	trust people in the village	0.137	0.162* (0.0937)+	123
2002 vs 2010	trust people in the village	0.440*** ++	0.425** (0.174)+	39
2007 vs 2009	trust people in the village	0.525*** +++	0.525*** (0.149)+++	49
2007 vs 2010	trust people in the village	0.254*** ++	0.206** (0.0872)+	119
2002 vs 2007	trust closest neighbors	0.273*** ++	0.275*** (0.0920)++	123
2007 vs 2009	trust closest neighbors	0.463*** +++	0.545*** (0.153)+++	49
2002 vs 2007	bad to buy something you know is stolen	0.141	0.226** (0.102)+	123
2007 vs 2009	bad to buy something you know is stolen	0.372*** ++	0.353*** (0.120)++	49
2002 vs 2007	would villagemates take advantage if had opportunity	0.251*** ++	0.167* (0.089)+	123
2007 vs 2009	would villagemates take advantage if had opportunity	0.355** ++	0.420* (0.209)+	49
2007 vs 2009	negative reciprocity	0.360** ++	0.236* (0.128)+	49
2007 vs 2010	negative reciprocity	0.245*** ++	0.176** (0.080)+	119

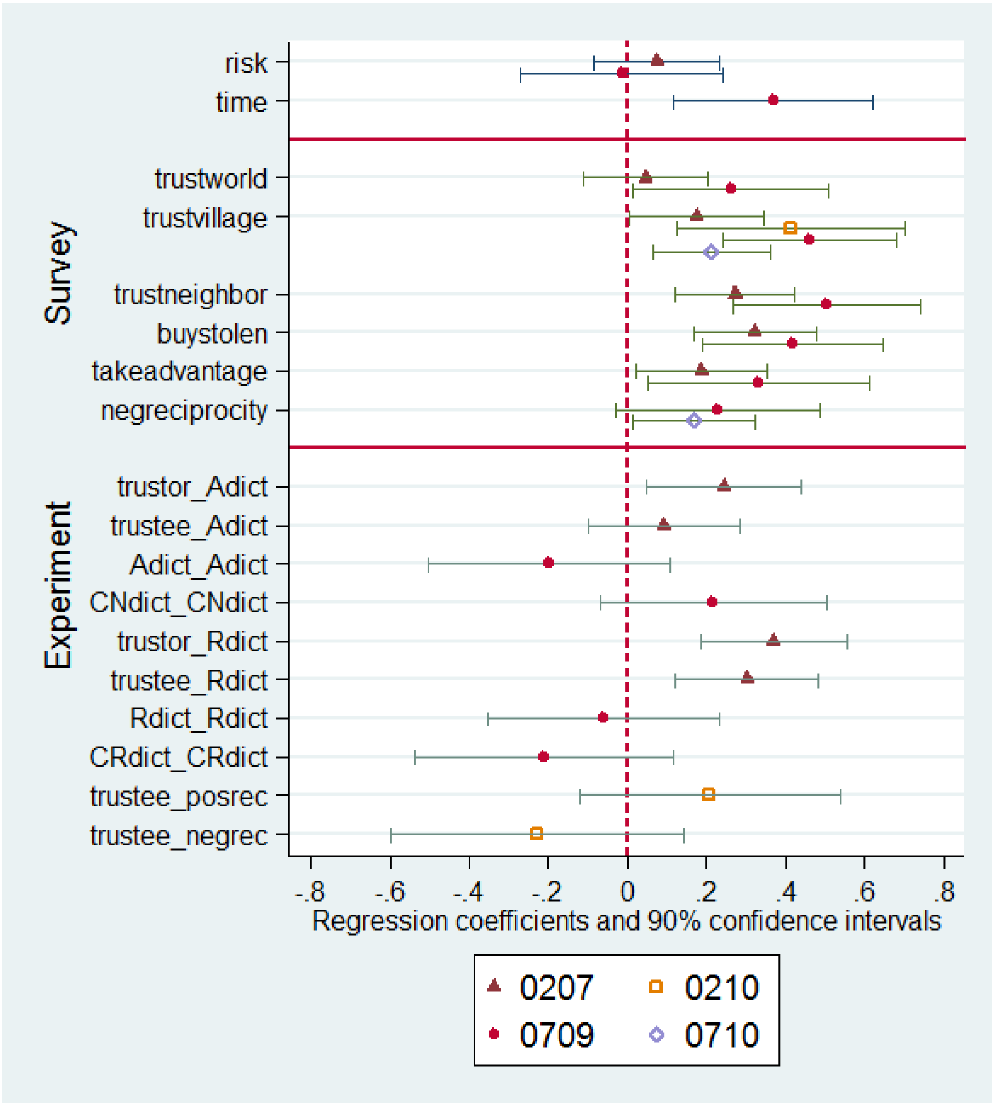
Notes: Per-comparison p-values: ***p <0.01, ** p <0.05, * p <0.10. FDR q-values: +++ q <0.01, ++ q <0.05, + q <0.10 calculated for 14 hypotheses within table and column. Standard errors are in parentheses. Controls in regressions include log income, sex, age, education, and village fixed effects.

Table 2.7: Stability of Social Preferences in Games

Explanatory variable	Dependent variable	Correlation coefficient	Regression coefficient	# Obs.
ALTRUISM				
sent as trustor in 2002	sent as dictator in anonymous game in 2007	0.297***	0.298**	103
		++	(0.143)	
share returned as trustee in 2002	sent as dictator in anonymous game in 2007	0.132	1.171	103
			(1.457)	
sent as dictator in anonymous game in 2007	sent as dictator in anonymous game in 2009	-0.107	-0.180	41
			(0.165)	
sent as dictator in chosen non-revealed game in 2007	sent as dictator in chosen non-revealed game in 2009	0.138	0.126	33
			(0.0976)	
TRUST				
sent as trustor in 2002	sent as dictator in revealed game in 2007	0.354***	0.513***	103
		+++	(0.154)$^{++}$	
share returned as trustee in 2002	sent as dictator in revealed game in 2007	0.283***	4.335***	103
		++	(1.556)$^{++}$	
sent as dictator in revealed game in 2007	sent as dictator in revealed game in 2009	0.049	-0.0496	41
			(0.140)	
sent as dictator in chosen revealed game in 2007	sent as dictator in chosen revealed game in 2009	-0.118	-0.236	33
			(0.215)	
RECIPROCITY				
share returned as trustee in 2002	positive reciprocity in 2010	0.009	0.473	43
			(0.439)	
share returned as trustee in 2002	negative reciprocity in 2010	0.123	-0.430	43
			(0.408)	

Notes: Per-comparison p-values: ***p <0.01, ** p <0.05, * p <0.10. FDR q-values: +++ q <0.01, ++ q <0.05, + q <0.10 calculated for 10 hypotheses within table and column. Standard errors are in parentheses. Controls in regressions include log income, sex, age, education, and village fixed effects.

Figure 2.1: Standardized effects with 90% confidence intervals



This figure shows coefficients from a regression of the standardized preference measured in the later period on the standardized preference measured in the earlier period. Preference variables are standardized to have mean 0 and sd 1. Controls include log income, sex, age, education, and village fixed effects. The top panel looks at risk and time preferences; the middle panel looks at social preferences measured using survey questions; and the bottom panel looks at social preferences measured using experiments. The survey measures of social preferences are: trust world - share of people you trust in the world, trust village - share of people you trust in the village, trust neighbor - share of your neighbors you trust, buy stolen - is it bad to buy something you know is stolen, take advantage - would villagemates take advantage if they had the opportunity, and negative reciprocity - if someone put you in a difficult position would you do the same to that person. The experimental measures of social preferences are: trustor - sent as first mover in trust game, trustee - share returned as second mover in trust game, Adict - amount sent in anonymous dictator game, CNdict - amount sent in chosen non-revealed dictator game, Rdict - amount sent in revealed dictator game, CRdict - amount sent in chosen revealed dicator game, posrec - would reward a middleman who sent him the highest amount, and negrec - would fine a middleman who sent him the lowest amount. If two variables are listed, the first one is from the earlier period and the second one is from the later period.

2.6 Appendix A: Impact of Previous Games on Play

Table A-1: Impact of Die Roll in 2002 Risk Game

Dependent variable	Correlation coefficient	Regression coefficient	# Obs.
# risky choices in 2007 (hyp)	0.119	0.184* (0.108)	126

Notes: *** p<0.01, ** p<0.05, * p<0.10. Standard errors are in parentheses. Controls in regressions include log income, sex, age, education, bet in 2002, and village fixed effects. Hyp denotes that the game was hypothetical.

Table A-2: Impact of Amount Trustor Received Back Divided by Amount He Sent in 2002

Dependent variable	Correlation coefficient	Regression coefficient	# Obs.
ALTRUISM			
sent as dictator in anonymous game in 2007	0.207**	0.916*	95
		(0.511)	
sent as dictator in chosen non-revealed game in 2007	0.143	0.684	95
		(0.538)	
TRUST			
sent as dictator in revealed game in 2007	0.159	1.000*	95
		(0.528)	
sent as dictator in chosen revealed game in 2007	0.146	0.800	95
		(0.531)	
RECIPROCITY			
positive reciprocity in 2010	0.175	0.126	42
		(0.131)	
negative reciprocity in 2010	0.421***	0.286**	42
	++	(0.110)+	

Notes: Per-comparison p-values: ***p <0.01, ** p <0.05, * p <0.10. FDR q-values: +++ q <0.01, ++ q <0.05, + q <0.10 calculated for 6 hypotheses within table and column. Standard errors are in parentheses. Controls in regressions include log income, sex, age, education, amount respondent sent as trustor in 2002, share returned by respondent as trustee in 2002, and village fixed effects.

Table A-3: Impact of Amount Received as Trustee in 2002

Dependent variable	Correlation coefficient	Regression coefficient	# Obs.
ALTRUISM			
sent as dictator in anonymous game in 2007	0.204**	0.098*	103
		(0.052)$^{+}$	
sent as dictator in chosen non-revealed game in 2007	0.162	0.123**	103
		(0.056)$^{+}$	
TRUST			
sent as dictator in revealed game in 2007	0.179*	0.116**	103
		(0.054)$^{+}$	
sent as dictator in chosen revealed game in 2007	0.179*	0.144**	103
		(0.057)$^{+}$	
RECIPROCITY			
positive reciprocity in 2010	-0.094	0.003	43
		(0.013)	
negative reciprocity in 2010	0.032	-0.002	43
		(0.013)	

Notes: Per-comparison p-values: ***p <0.01, ** p <0.05, * p <0.10. FDR q-values: +++ q <0.01, ++ q <0.05, + q <0.10 calculated for 6 hypotheses within table and column. Standard errors are in parentheses. Controls in regressions include log income, sex, age, education, amount respondent sent as trustor in 2002, share returned by respondent as trustee in 2002, and village fixed effects.

2.7 Appendix B: Impact of Real-World Shocks on Play

Table B-1: Impact of Shocks on Risk and Time Preferences

Explanatory variable	Dependent variable	Correlation coefficient	Regression coefficient	# Obs.
Δ log income 2002 to 2007	# risky choices in 2007 (hyp)	-0.003	-0.150 (0.241)	140
Δ log income 2007 to 2009	# risky choices in 2009 (hyp)	-0.027	0.013 (0.371)	49
Δ log theft 2002 to 2007	# risky choices in 2007 (hyp)	0.095	0.091* (0.050)	140
Δ log theft 2007 to 2009	# risky choices in 2009 (hyp)	-0.193	-0.067 (0.071)	49
days sick 2007	# risky choices in 2007 (hyp)	-0.150*	-0.006 (0.004)	140
days sick 2009	# risky choices in 2009 (hyp)	0.292**	0.013** (0.006)	49
Δlog income 2007 to 2009	time preference in 2009 (hyp)	0.001	1.502 (45.47)	49
Δ log theft 2007 to 2009	time preference in 2009 (hyp)	-0.014	-2.384 (8.826)	49
days sick 2009	time preference in 2009 (hyp)	-0.100	-0.164 (0.837)	49

Notes: Per-comparison p-values: ***$p<0.01$, ** $p<0.05$, * $p<0.10$. FDR q-values: +++ $q<0.01$, ++ $q<0.05$, + $q<0.10$ calculated for 9 hypotheses within table and column. Standard errors are in parentheses. Hyp denotes that the game was hypothetical. Controls in regressions include sex, age, education, and village fixed effects. Additionally the risk regressions control for risk aversion in the previous round, while the time regressions control for time preferences in the previous round. Days sick - number of days non-disabled person aged 11-74 couldn't work due to illness. Days sick regressions additionally control for number of non-disabled persons aged 11-74. Days sick and theft regressions additionally control for log income.

Table B-2: Impact of Income Shocks on Social Preferences

Explanatory variable	Dependent variable	Correlation coefficient	Regression coefficient	# Obs.
	Outcome in 2007			
	sent as dictator in anonymous game	-0.086	-0.403	103
			(0.360)	
	sent as dictator in chosen non-revealed game	0.033	-0.087	103
			(0.394)	
	sent as dictator in revealed game	-0.100	-0.573	103
			(0.370)	
	sent as dictator in chosen revealed game	-0.075	-0.566	103
			(0.400)	
Δ log income	trust people in the world	-0.097	-0.140	122
2002 to 2007			(0.120)	
	trust people in the village	-0.016	-0.056	122
			(0.138)	
	trust closest neighbor	-0.047	-0.069	122
			(0.153)	
	bad to buy something you know is stolen	0.001	0.019	122
			(0.041)	
	would villagemates take advantage if had opportunity	0.045	0.104	122
			(0.134)	
	Outcome in 2009			
	sent as dictator in anonymous game	-0.204	-0.409	41
			(0.595)	
	sent as dictator in chosen non-revealed game	-0.151	-0.277	33
			(0.513)	
	sent as dictator in revealed game	-0.100	-0.123	41
			(0.548)	
	sent as dictator in chosen revealed game	-0.054	0.364	33
			(0.924)	
Δ log income	trust people in the world	-0.102	-0.125	49
2007 to 2009			(0.302)	
	trust people in the village	-0.005	0.030	49
			(0.307)	
	trust closest neighbor	-0.076	-0.270	49
			(0.320)	
	bad to buy something you know is stolen	-0.113	-0.030	49
			(0.087)	
	would villagemates take advantage if had opportunity	-0.004	0.027	49
			(0.279)	
	negative reciprocity	0.239*	0.103	49
			(0.096)	

Notes: Per-comparison p-values: ***$p < 0.01$, ** $p < 0.05$, * $p < 0.10$. FDR q-values: +++ $q < 0.01$, ++ $q < 0.05$, + $q < 0.10$ calculated for 9 or 10 hypotheses within table, panel, and column. Standard errors are in parentheses. Controls in regressions include sex, age, education, and village fixed effects. Each regression additionally controls for social preferences in the previous round. In the top panel, the first four rows control for the amount sent as trustor and the average share returned as trustee in 2002. In the remaining rows of the first panel, the 2002 version of the 2007 outcome variable is included as a control variable. In all rows of the second panel, the 2007 version of the 2009 outcome is included as a control variable.

Table B-3: Impact of Theft Shocks on Social Preferences

Explanatory variable	Dependent variable	Correlation coefficient	Regression coefficient	# Obs.
	Outcome in 2007			
	sent as dictator in anonymous game	-0.030	-0.024	103
			(0.092)	
	sent as dictator in chosen non-revealed game	-0.027	-0.013	103
			(0.099)	
	sent as dictator in revealed game	0.055	0.073	103
			(0.094)	
	sent as dictator in chosen revealed game	0.002	0.083	103
			(0.101)	
Δ log theft	trust people in the world	-0.116	-0.059**	123
2002 to 2007			(0.025)	
	trust people in the village	-0.079	-0.064**	123
			(0.029)	
	trust closest neighbor	0.037	0.007	123
			(0.033)	
	bad to buy something you know is stolen	0.057	0.008	123
			(0.008)	
	would villagemates take advantage if had opportunity	-0.075	-0.007	123
			(0.028)	
	Outcome in 2009			
	sent as dictator in anonymous game	0.117	0.080	41
			(0.115)	
	sent as dictator in chosen non-revealed game	0.078	-0.015	33
			(0.086)	
	sent as dictator in revealed game	0.156	0.070	41
			(0.105)	
	sent as dictator in chosen revealed game	0.122	0.068	33
			(0.175)	
Δ log theft	trust people in the world	-0.025	-0.001	49
2007 to 2009			(0.058)	
	trust people in the village	0.035	-0.013	49
			(0.057)	
	trust closest neighbor	-0.077	-0.083	49
			(0.060)	
	bad to buy something you know is stolen	0.022	0.005	49
			(0.016)	
	would villagemates take advantage if had opportunity	0.064	0.066	49
			(0.052)	
	negative reciprocity	-0.008	-0.002	49
			(0.018)	

Notes: Per-comparison p-values: ***p <0.01, ** p <0.05, * p <0.10. FDR q-values: +++ q <0.01, ++ q <0.05, + q <0.10 calculated for 9 or 10 hypotheses within table, panel, and column. Standard errors are in parentheses. Controls in regressions include log income, sex, age, education, and village fixed effects. Each regression additionally controls for social preferences in the previous round. In the top panel, the first four rows control for the amount sent as trustor and the average share returned as trustee in 2002. In the remaining rows of the first panel, the 2002 version of the 2007 outcome variable is included as a control variable. In all rows of the second panel, the 2007 version of the 2009 outcome is included as a control variable.

Table B-4: Impact of Health Shocks on Survey Social Preferences

Explanatory variable	Dependent variable	Correlation coefficient	Regression coefficient	# Obs.
	Outcome in 2007			
	trust people in the world	-0.056	-0.002	123
			(0.003)	
	trust people in the village	0.072	0.004	123
			(0.003)	
days sick 2007	trust closest neighbor	-0.061	-0.001	123
			(0.003)	
	bad to buy something you know is stolen	-0.015	-0.000	123
			(0.001)	
	would villagemates take advantage if had opportunity	0.049	0.002	123
			(0.003)	
	Outcome in 2009			
	trust people in the world	-0.111	-0.006	49
			(0.005)	
	trust people in the village	0.173	0.007	49
			(0.005)	
days sick 2009	trust closest neighbor	0.109	0.004	49
			(0.006)	
	bad to buy something you know is stolen	0.217	0.002	49
			(0.001)	
	would villagemates take advantage if had opportunity	-0.219	-0.009*	49
			(0.005)	
	negative reciprocity	-0.004	-0.000	49
			(0.002)	

Notes: Per-comparison p-values: ***p <0.01, ** p <0.05, * p <0.10. FDR q-values: +++ q <0.01, ++ q <0.05, + q <0.10 calculated for 11 hypotheses within table and column. Standard errors are in parentheses. Controls in regressions include log income, sex, age, education, number of non-disabled persons aged 11-74, and village fixed effects. Days sick - number of days non-disabled person aged 11-74 couldn't work due to illness. Each regression additionally controls for social preferences in the previous round. In the first panel, the 2002 version of the 2007 outcome variable is included as a control variable. In the second panel, the 2007 version of the 2009 outcome is included as a control variable

Table B-5: Impact of Health Shocks on Experimental Social Preferences

Explanatory variable	Dependent variable	Correlation coefficient	Regression coefficient	# Obs.
	Outcome in 2007			
days sick 2007	sent as dictator in anonymous game	-0.052	-0.001	103
			(0.007)	
	sent as dictator in chosen non-revealed game	-0.035	0.004	103
			(0.008)	
	sent as dictator in revealed game	-0.242**	-0.015**	103
			(0.007)	
	sent as dictator in chosen revealed game	-0.110	0.000	103
			(0.008)	
	Outcome in 2009			
days sick 2009	sent as dictator in anonymous game	-0.126	-0.009	41
			(0.008)	
	sent as dictator in chosen non-revealed game	-0.296*	-0.006	33
			(0.005)	
	sent as dictator in revealed game	-0.236	-0.009	41
			(0.007)	
	sent as dictator in chosen revealed game	-0.250	-0.013	33
			(0.010)	

Notes:Per-comparison p-values: ***p <0.01, ** p <0.05, * p <0.10. FDR q-values: +++ q <0.01, ++ q <0.05, + q <0.10 calculated for 8 hypotheses within table and column. Standard errors are in parentheses. Controls in regressions include log income, sex, age, education, number of non-disabled persons aged 11-74, and village fixed effects. Days sick - number of days non-disabled person aged 11-74 couldn't work due to illness. Each regression additionally controls for social preferences in the previous round. In the top panel the amount sent as trustor and the average share returned as trustee in 2002 are included as control variables. In the second panel, the 2007 version of the 2009 outcome is included as a control variable.

Chapter 3

Climate Variability and Farmers' Income Diversification in India

3.1 Introduction

The impact of weather shocks is an important concern in developing countries because the poor often lack formal resources to deal with risk. In the presence of imperfect credit markets, people often manage risk through savings as a self-insurance scheme (Deaton, 1991). Households can also mitigate shocks by engaging in income diversification activities, such as construction work, cattle-rearing, or shop-keeping (Dercon, 2002). Risk sharing through long distance migration-marriage can be another way to smooth consumption (Rosenzweig and Stark, 1989). In a worst case scenario, child labor may be used to cope with unexpected shocks when a household has no other choice but to withdraw children from school (Jacoby and Skoufias, 1997; Jensen, 2000). The current literature on risk-sharing and risk-coping largely focuses on idiosyncratic shocks, as opposed to aggregate shocks (Dercon, 2008). Because systematic risk, which can affect the whole community, is more difficult to insure (Townsend, 1994), it is important to improve our understanding of household's coping mechanisms in the context of aggregate shocks.

This paper aims to examine farmers' income diversification strategies as a coping strategy to aggregate weather shocks. I ask two questions in this paper. 1) How do weather shocks affect households' sources of income? 2) Do farmers' income diversification strategies vary in regions with different historical weather patterns? To answer these questions, I first build a non-separable household decision model to explain farmers' income diversification strategies between on-farm and off-farm income sources. The model predicts that households in riskier places are less exposed to weather shocks because historical risk causes them to depend more heavily on off-farm work. To test these theories empirically, I use a dataset that spans three decades in 242 villages in India. I also compile various historical spatial data with the household data to examine weather patterns. The scope of the data has sufficient observable temporal and spatial variation that allows inspection of the effectiveness of adaptation through different income-generating activities over a long period of time. This is important because it may take time for households to adjust their engagement with the off farm sector. The identification comes from the fact that rainfall deviation from the mean is random across space. At the same time, I control for important household-level characteristics, and state fixed effects that may be confounded with households' adaptation strategies.

Results indicate that rainfall shocks negatively affect farmers' agricultural income. In response to shocks, farmers diversify their income through other wage jobs. Households in places with higher weather variation have a long history of such labor diversification. Therefore, reaction to contemporaneous rainfall shocks are smaller in a riskier place compared to in a less risky place. I also find weak evidence that households that differ from their peers' caste in the village may be more likely to look for off-farm jobs outside the village. These results of heterogeneous adaptation between regions indicate that policymakers should pay more attention to places that are less prepared for climate change. For research focusing on the impact of climate change, my results suggest the necessity to incorporate agents' ex-ante response into the model.

This paper speaks to the large literature on risk-coping and risk-sharing in development economics, and focuses on systematic weather risk, which is difficult to mitigate. This perspective is particularly pressing given the mounting evidence that anthropogenic climate change will lead to increased weather shocks in the future. I focus on examining the role of off-farm work as a method of coping with climate change because previous studies have found that access to off-farm jobs is a major source of long-term poverty reduction (Cherdchuchai and Otsuka, 2006; Escobal, 2001; Estudillo et al., 2008; Lanjouw and Lanjouw, 2001; Reardon, 1997). Following this stream of literature, Foster and Rosenzweig (2003) developed a general equilibrium model calibrated using data from India to emphasize the important role of the non-farm tradable sector in contrast to the non-farm non-tradable sector. They concluded that the tradable non-farm sector is negatively correlated with agricultural sector growth, while non-farm non-tradable activity is complementary to agricultural growth. In the short-run, diversifying income-generating activity is a way to mitigate agricultural risk both ex-ante (Kochar, 1999a; Rose, 2001a; Ito and Kurosaki, 2006a) and ex-post (Kijima et al., 2006a).

The current literature is inconclusive about the role of off-farm activity as a coping mechanism in response to agricultural shock. It is challenged by a lack of appropriate data with enough temporal and spatial scope. For example, Kochar (1999b) found that households shift labor into the off-farm sector in response to idiosyncratic crop shocks. The analysis, however, used data from only three villages in semi-arid India. Rose (2001b) used data from all across India and concluded that weather shock and low rainfall result in the increase of labor force participation both ex-ante and ex-post. However, she did not separate labor into sectors. Ito and Kurosaki (2006b) found that households depend more on off-farm wages than on agricultural wages as their risk-coping strategy for weather shocks, but their analysis only included two states in India. Kijima et al. (2006b) used data with greater spatial variation, covering 94 Local Councils (the lowest administrative unit) in Uganda in 2003 and 2005. They found that those who are asset poor use low-skill wage jobs to mitigate negative agricultural shocks. This result may be sensible due to the lack of temporal variation and the endogeneity of households' asset holdings.

The other contribution of this paper is to separate regions into different historical weather dynamics to understand the heterogeneity of adaptation strategies. This approach will help to determine the vulnerability of the local people in different regions. To our knowledge, this aspect has never been explored in the literature. As climate change may have different distributional impacts in different regions, identifying the regions with potentially persistent vulnerability could help policy makers to take ex-ante actions to prevent catastrophe.

This paper also contributes to the growing literature on the impact of climate change on agricultural income. The most influential work by Deschênes and Greenstone (2007) concluded that climate change has a slightly positive impact on agricultural income in the U.S. This changing climate pattern may have a very different detrimental impact in many developing countries that depend heavily on agriculture for their economy and subsistence. Guiteras (2009) found a significant negative effect on agricultural income in India. These studies present different views of climate change among developed countries and developing countries, but did not take into account any source of adaptation. As climate change is very likely to reduce growth rates (Dell et al., 2012), it is especially important to better understand mechanisms that households use to cope with weather shocks in developing countries (see review paper by Dell et al. (2014)). My analysis provides possible policy recommendations by investigating existing historical adaptation and determining who are more vulnerable to the changing climate.

This paper is structured as follows. In the next section, I provide a theoretical model to motivate the basic setting of farmers' income diversification strategy. Section 3.3 provides predictions from the theoretical model. In section 3.4, I explain the multiple data sources and how I define the important variables used in this paper. Section 3.5 and 3.6 describes our empirical strategy and the results. Section 3.7 concludes.

3.2 Conceptual Framework

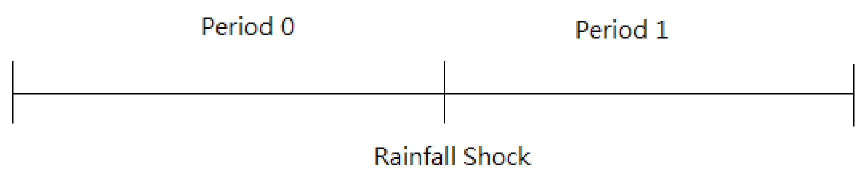
In this section, I present a conceptual framework of my empirical analysis. There are three ways in which weather might affect farmers' income diversification strategies: (1) contemporaneous weather shocks, (2) historical weather patterns, and (3) the interaction of the two. Here, I mainly focus on labor allocations between on-farm production and wage labor (including both off-farm and on-farm) activities.

3.2.1 Agricultural Production and Decision Framework

The total agricultural profit typically depends on agricultural price, total factor productivity, the output elasticity of the inputs, and the cost of inputs. Here, I assume that households' agricultural output depends on only two inputs: land and labor inputs. These two are the only choice variables in the production function. I can further assume that land is fixed, so farmers mainly decide how much is the labor input per land. Since agricultural goods are usually tradable, I can simplify the price as exogenous and normalized into 1. The output elasticity of the inputs can be assumed to be exogenous and not sensitive to weather shocks. Total factor productivity is exogenous, but I will discuss a productivity shock variable attached to the production function later. This fixed land assumption leaves me with only the cost of labor, which will be discussed in the wage factor section.

Rose (2001b) constructs a two-period model that captures both the ex-ante responses toward historical weather and how decisions are affected by contemporaneous shocks. The timing of the labor decision is illustrated in the following figure. Farmers choose their labor input at period 0 based on the historical weather pattern before knowing the state of shock.

Although farmers do not know the realization of shock, they know the distribution. Their choice of labor input at the first period will depend on the historical weather distribution. In period 1 after weather shock is realized, farmers choose their labor input again. In the context of farming in India, farmers make their labor allocation decisions based on the riskiness of the agricultural production during the preparation period (period 0). These labor inputs involve preparing soil for farming, such as, tilling the soil, weeding, or spraying herbicide. In the next period, there is another labor choice decision on harvesting depending on the shock realization.



In terms of income diversification strategies, I focus on the effect of labor choices by specifically examining agricultural labor income and off-farm wage income. Because of data limitation, I cannot directly look at labor supply choices; and instead, I examine agricultural labor income and off-farm labor income to approximate the change in labor supply choices. However, these two types of income may reflect the general equilibrium effect on wage. I will elaborate more on how this weather shock may affect wage.

3.2.2 Weather Effects on Labor Supply

Besides land and labor, I assume that households' agricultural production profit is directly dependent on a weather variable called $\tilde{\theta}$, where $\tilde{\theta}$ is a random variable, and it follows a uniform distribution as $\tilde{\theta} \sim U(\theta - \gamma, \theta + \gamma)$, where γ is to capture the riskiness of the region.

This historical pattern (γ), directly attached to households' agricultural production, will make agricultural income more risky. Here, I assume that households are risk-averse. Risk aversion will create a portfolio effect, whereby households will shift their labor inputs toward the less risky source (Rose, 2001b; Rosenzweig and Udry, 2014).[1] Other than the portfolio effect, γ also creates a precautionary effect; that is, households increase their labor supply and reduce their leisure to prevent their chances of income loss. They are hedging against the uncertainties through their labor supply responses. The portfolio effect and the precautionary effect both result in a similar labor supply responding to the historical weather pattern.

The realized contemporaneous weather shocks would affect the agricultural production—a negative weather shock directly decreases households' agricultural production. Similar to the portfolio effect, a decrease in agricultural profits as a result of weather shocks would urge households to increase their market labor supply. Kochar (1999b) found empirical evidence that households increase hours of wage labor work in response to crop income shocks.

Another interest is to test the interaction effect between the historical weather pattern and the contemporaneous weather shock. The prediction will depend on the relationship between the ex-ante labor choice (depending on the historical weather pattern) and the ex-post labor choice (in response to the contemporaneous weather shock). For example, this interaction effect can be negative—households in a riskier place have already shifted their labor resources toward other wage income activities, and therefore, their labor supply responses are less sensitive to contemporaneous shocks . Comparatively, households' ex-ante labor choices at the first period could complement their ex-post labor input decisions in the second period. There is an enhancing interaction effect. Therefore, the interaction effect

[1] Rosenzweig and Udry (2014) do not look at labor choices between a risky and a less risky income source. Instead, their prediction is on the allocation between saving (less risky) and agricultural input (riskier) with respect to riskiness. The intuition generated from their model is similar.

between historical weather pattern and contemporaneous weather shock on labor supply is indeed an empirical question.

3.2.3 Wage Factor

In the previous section, I examined weather impacts on labor supply responses. However, because of data limitation, I can only observe the variations of different net income sources without controlling for the real wage information. For example, I use agricultural profit to approximate the agricultural production. But the caveat is that weather shocks may cause changes in wages, so this input price change may affect the net profit. In this section, I further examine the wage responses to weather patterns, and how this wage factor may shed additional lights on mechanisms.

The riskiness of the historical weather pattern can also make the labor market risky because based on a general equilibrium effect, a shock in the agricultural sector can drive down the market wage rate. If the labor market is risky too, households substitute risky wage/production activities with leisure. They then tend to supply less labor—opposite response compared with the one in the previous section. Based on Rose (2001b), the riskiness in the labor market is less likely to be riskier than that in farm production.

Contemporaneous weather shocks can also generate opposite effects on agricultural income, agricultural wage income, and off-farm wage income. In the previous section, negative weather shocks have a negative effect on agricultural income, a positive effect on agricultural wage income, and a positive effect on off-farm wage income. However, reducing the agricultural wage can be a way for landowners to smooth agricultural shocks in some places. Jayachandran (2006) found evidence that wages are more elastic toward weather shocks in a more remote area when the migration cost is high. If the labor supply is inelastic, negative weather shocks would lead to different predictions: increasing agricultural income because of lower labor costs and decreasing agricultural wage income. In terms of off-farm wage income, even if those jobs are outside the village, this source of income may decrease because of the surge of labor supply from the agricultural sector.

The wage factor mostly generates opposite effects from the predictions in the previous section. Therefore, I am less worried that it may confound my labor supply story if I observe the the signs of the empirical estimators consistent with the labor supply predictions. In the meantime, in most of my empirical specifications, I have controlled for "the distance to a city" to eliminate different wage responses to weather shocks. I have also shown some empirical evidence in section 3.6 that the wage adjustment effect is relatively small compared with the labor supply responses to shocks.

3.3 Hypothesis Testing

Based on the conceptual framework, I will test the following hypothesis in the empirical section.

Hypothesis 1. *Households invest less in agricultural activities and more on other wage activities in response to negative weather shocks.*

A productivity shock means that a household's agricultural production value experiences a lowers $\tilde{\theta}$. This effect will decrease the value of the marginal productivity of labor, and thus make off-farm wage jobs more attractive. In response to a negative shock, households will be more likely to shift their on-farm labors to off-farm jobs. At the same time, if the agricultural wage job is less risky than the on-farm activity, households will also increase their labor supply in the agricultural wage job.

Hypothesis 2. *Households in riskier places are less responsive to weather shocks. They shift less labor resources to off-farm activities in response to to weather shocks.*

Next, I look at the shock effect by comparing a riskier place with a less risky place. I test the hypothesis that the effect of weather shocks is smaller in a riskier place than that in a less risky place. Households' climate change adaptation strategy of shifting labor to off-farm activities seems to be less salient in riskier places because households are already more prepared based on the historical information. In less risky places, the agricultural investment in the previous stage will serve as a stronger multiplicative impact to the output, causing a relatively greater loss owing to weather shocks.

Hypothesis 3. *Households with a higher transaction cost of moving to off-farm activities will be more likely to invest in farm activities.*

This hypothesis is to understand how a household-level transaction cost may affect the adaptation strategy. Here I further assume a transaction cost $\epsilon > 0$, which captures the transaction cost of moving from farm activities to another industry[2]. A higher transaction cost (ϵ) will make households more likely to rely only on agricultural activities instead of working off-farm. Therefore, villagers with a higher transaction cost of moving to off-farm jobs will be more likely to use more labor on their own farm and use less labor in off-farm employment.

[2]This can be viewed as the psychic cost of temporarily moving away from family and friends, the direct travel costs, or the transaction costs that are required to get access to another off-farm job. In the empirical result section, I illustrate more about the potential variable that could determine an individual's willingness to obtain an off-farm wage job.

3.4 Data & Variables

3.4.1 Household Data

The main dataset is a household panel conducted in 242 Indian villages in 1971, 1982, and 1999. The data are representative at the national level as they cover 17 major provinces of India. Data collection was carried out by the National Council of Applied Economics Research (NCAER) in India, which contains information combining from three sources: i) the 1970-1971 NCAER Additional Rural Income Survey (ARIS), ii) the 1981-1982 NCAER Rural Economic Development Survey (REDS), and iii) the 1999 NCAER Village REDS. This dataset includes detailed information at the household level, such as demographic background, assets, income by different sources, and information at the village level, such as infrastructure, industry, and population. An overview of the variables is presented in Table 1.

I consolidated households' income sources into agricultural, agricultural labor, and non-farm labor[3]. It is worth noting that all of the income measures are net profit measures that take into account the cost of investment, so they can be negative if the investment has not yet paid off. Based on the information about income from different sources, I can derive the share of the total income from different sources. For the share of different income sources, I only take into account those who have at least one positive value in that source over the survey period.

[3] I also have business income and livestock income. However, since I am testing the effect of wage income, I mainly focus on these three sources.

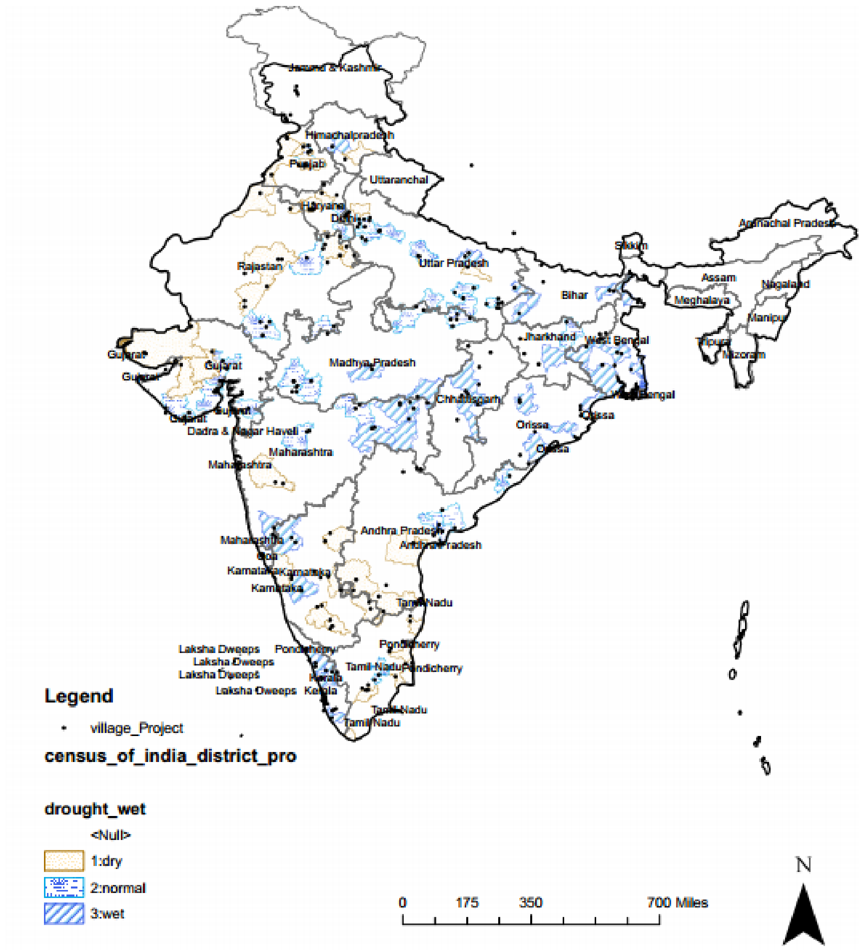
3.4.2 Weather Variable

For weather variables, this paper uses the basic weather indicators of temperature and precipitation. Weather data are from the Center for Climate Research at the University of Delaware, and include important information about monthly precipitation and temperature from 1900 to 2008 on a 0.5 degree latitude by 0.5 degree longitude global grid. The final measurement used in this paper was interpolated at the village level and weighted by the inverse-square of the distance between each nearby gridded observation and the center point of the district. I used total monsoon rainfall from June to September as the benchmark to determine a rainfall shock because monsoon rainfall is an important determinant of agricultural productivity (Sikka 1980; Mooley and Parthasarathy 1984). This pattern of monsoon rainfall is also frequently used by scientists to understand the threat of climate change (Winstanley 1973). Mean temperature in June is included to control for the condition for germination. For further comparison, I generated a 20-year average (before the contemporaneous year) for the same precipitation and temperature variables. I then created a shock variable using village j's past 20-year average rainfall minus contemporaneous rainfall at time t, which is called variable $shock_{jt}$. The bigger this number, the lower the contemporaneous rainfall relative to the historical average.

The following map in Figure 3.1 illustrates the distribution of historical monsoon rainfall patterns in the survey regions.

I also compared multiple sources of income in regions with different historical weather. The bottom two graphs in Figure 3.2 verify the theoretical assumption that regions with greater weather variation are less dependent on agricultural income and use more off-farm income as an adaptation. Riskier places (with higher historical rainfall standard deviation) also tend to have higher historical mean. By examining the percentage of different income sources over total income in Figure 3.5, I find a similar pattern.

Figure 3.1: A Map of India with Historical Weather Distribution



3.5 Empirical Strategy

Reduced form analysis will be used to empirically determine how weather shocks affect different sources of income. The income of household i in village j at time t (i.e. year 1971, 1982, 1999) is assumed to be a function of the rainfall shock, the household's own characteristics, and village characteristics. I use Ordinary Least Square (OLS) to estimate the model, and cluster the standard error at the village level. The model has the following form:

$$
\begin{aligned}
Y_{ijt} =& \gamma_1 20yearRainSD_{jt} + \gamma_2 RainShock_{jt} + \gamma_3 temp_{jt} + \gamma_4 20yearRainSD_{jt}RainShock_{jt} + \\
& \gamma_5 20yearRainSD_{jt}RainShock_{jt}year81 + \gamma_6 20yearRainSD_{jt}RainShock_{jt}year99 + \\
& \gamma_7 20yearRain_{jt} + \gamma_8 20yearTemp_{jt} + \gamma_9 X_{ijt} + State_s + v_t + \epsilon_{ij}
\end{aligned}
$$

where one set of the outcome variables (Y_{ij}) is different sources of income, such as total income, agricultural income, agricultural labor income, and on-farm wages and salary for household i in district j; the other set of outcome variables is the share of those different income sources. Variable $20yearRainSD_j$ represents the riskiness of the region, which is measured by the 20-year standard deviation of monsoon rainfall; $RainShock_j$ captures the contemporaneous weather shock, and is calculated as 20-year log monsoon rainfall − log monsoon rainfall; $temp_{jt}$ is monsoon rainfall in district j at time t; A higher value of $RainShock_j$ means a greater negative rainfall shock. $temp_j$ is the 20-year average June temperature at district j. X_{ij} is a set of household characteristics. Here I control for factors that affect household productivity, such as the household head's age and education, which are proxies for experience and skill in farming. The distance to a city is also included because proximity to a market may create direct access to off-farm jobs, and at the same time this distance may imply some level of price endogeneity. Other controls include household size and village population. I also include state fixed effects ($State_s$) to identify the effect of shocks deviating from the state's average. State fixed effects also allow me to control for many time-invariant characteristics, such as cropping patterns, soil types, and the socio-economic status of different states. In addition, time fixed effects (v_t) are controlled. However, in order to understand how these relationships may have changed over time, I add year by year interaction to γ_4.

Risk-averse farmers would under-invest in agricultural production given the uncertainty they face, so for agricultural income as the outcome variable, I expect $\gamma_1 < 0$. At the same time, contemporaneous shocks will have a negative impact on crop income, causing $\gamma_2 < 0$. This effect will be smaller in riskier places because households have shifted their resources away from agriculture ex-ante, and thus, $\gamma_4 > 0$.

Households shift their labor from agricultural activities to other wage activities, leading $\gamma_2 > 0$ in the specification when wage income is the outcome variable. This response is smaller in riskier places, i.e. $\gamma_4 < 0$. Another income diversification opportunity is agricultural wage income when households fail to produce anything in the event of shocks. If agricultural wage is a risk-coping mechanism, then in the regression with agricultural wage income as the outcome variable, γ_2 is greater than zero. Because agricultural wage may be pushed down in response to weather shocks, in the regression with agricultural wage income as the outcome variable, I expect $\gamma_2 < 0$. The effect of contemporaneous shocks on agricultural wage income will be smaller. Therefore, $\gamma_4 < 0$ when $\gamma_2 > 0$, or $\gamma_4 > 0$ when $\gamma_2 < 0$.

Our identification strategy assumes that rainfall shocks do not affect households' risk aversion or preferences or the shape of the production function. Historical weather patterns may be confounded with regional access to off-farm wage income. For example, areas with more manufacturing jobs may be places where agricultural crops are more sensitive to weather shocks. The inclusion of state fixed effects helps mitigate this problem.

3.6 Empirical Results

3.6.1 Relationship between Different Sources of Income and Rainfall Shock

I estimate the impact of rainfall shocks on different sources of income, and test whether there is persistence among regions with different historical weather variation. Rainfall shocks are defined here as a deviation from the historical weather pattern (historical rainfall minus contemporaneous rainfall), so a higher value of $RainShock_j$ means a greater deviation from the historical rainfall. Here I assume that this relationship is continuous and monotone. This assumption appears to be reasonable if I graph out the relationship between agricultural income and rainfall deviations, as seen in Figure 3.6.

Column 1 in Table 3 indicates that rainfall shocks have a negative impact on households' total income. The mean deviation from historical (which is equal to 4 percent increase of contemporaneous rainfall from the historical rainfall) pattern can cause approximately 3 percent increase in total income. Column 2 shows that rainfall shocks have a negative impact on agricultural income. A 4 percent increase of contemporaneous rainfall from the historical rainfall leads to approximately 8 percent increase of agricultural income. The positive relationship between rainfall shocks and agricultural labor income in column 3 suggests that households use agricultural labor as a way to mitigate shocks. This positive relationship is also found between rainfall shocks and off-farm labor income in column 4.

Our other interests are the coefficients on the interaction between variables $RainShock_j$ and $20yearRainSD_j$. If regions with greater historical weather variation are better at adapting climate change, I should expect the coefficients in row 3 to be the opposite signs of those in row 2 in Table 3. Rows 4 and 5 give us ideas about how this persistence may have changed over time. As expected from the theory, places with greater weather variation (larger standard deviations) seem to adapt to climate patterns over time. So the negative impacts of weather shocks on total income and agricultural income are both decreased in high-variance places. The increases in agricultural wage income and non-farm wage income are less salient in those high-variance areas. The results in columns 3 and 4 indicate that households in riskier places may have shifted their income to those income sources over time, making different sources of income less responsive to contemporaneous shocks.

Rows 2 and 3 in Table 4 again validate our results in Table 3. Rainfall shocks have similar impacts on the share of agricultural income, agricultural wage income, and non-farm wage income. One standard deviation increase in the level of shock decreases 4 percentage point in the share of agricultural income, increases 1.8 percentage point in the share of agricultural wage income, and increases 2.4 percentage point in the share of non-farm wage income, respectively. Those effects are smaller in riskier places similar to the results shown in Table 3.

One question arises: in the event of aggregate weather shocks, can households find enough hours of work to supply their labor, especially through agricultural wage jobs? To answer this question, I further examine the heterogeneity of households income diversification to shed lights on the labor market responses. Table 5 presents the responses of different sources of income toward weather shocks. My main interest is to compare row 2 among different types of farmers. Looking at agricultural income as the outcome variable, I find that weather shocks significantly decrease the landless and small farmers' agricultural income, yet it does not have a significant impact on larger farmers' agricultural income. In addition, there is a qualitative difference in agricultural wage income between small farmers and the landless. Small farmers, compared with the landless, rely more on increasing their labor supply on agricultural wage jobs. For the landless, the decrease in the agricultural wage rate may be greater than the increase in the labor supply, so this estimate may be a lower bound on the labor supply effect. This may explain the slightly negative sign (though not significant) in the coefficient of the agricultural wage income among the landless. We do not see this similar negative sign among small farmers because the landless may be less attached to the agricultural sector than small farmers[4]. These results also indicate that larger farmers may be those who hire in labors because they are less affected by weather shocks through means, such as irrigation facilities[5] and other sources of insurance.

3.6.2 Potential Determinants of Using Different Sources of Income

Anecdotal evidence[6] suggests that farmers require different strategies to access agricultural wage income vs. other off-farm wage income. In many cases, households seek help from other well-off farmers within the village to work as farm labor in the event of drought because getting access to off-farm wage jobs may require extra skill and effort. For example, some farmers may not have the skills to work at a non-agricultural wage job. Construction work, which is also commonly mentioned as a temporary off-farm wage job, requires the extra cost to travel to another village. Clerical jobs, a route to a stable salary, also require references or specific skills that typical farmers may not have. However, the dataset does not contain information regarding where the off-farm job takes place. In this next section, I will examine heterogeneity to understand what may cause a farmer to seek help from another farmer, or seek another off-farm wage opportunity as a way to mitigate weather shocks.

Table 6 provides indirect evidence of network connections as a potential explanation for choosing agricultural wage income or off-farm wage income to mitigate productivity shocks.

---

[4]This result is consistent with my observations in the field. Smaller farmers are usually more attached to their land, and are less likely to abandon farming even though their lands are not very profitable. When asked whether they have thought of selling their land, almost all small farmers I talked with have never come across this idea. So in the event of weather shocks, if their harvests are not enough to serve their basic needs, they work at the land of larger farmers'.

[5]In the data, I find that there is a slightly positive correlation (at the border line of significance) between farmers' land size and whether having a irrigation system.

[6]The author spent the summer of 2010 conducting several qualitative interviews and focus groups with local farmers.

In this table, I use similar specifications as in Table 3, but separate our sample into those with the same caste as the majority of the people in their village vs. those from a different caste than the majority of the people in their village. Because caste in India is a prominent determinant of social status, I assume that this social divide may potentially determine whether people would seek help from another farmer within the village, or if they would have to search for off-farm wage jobs, potentially outside of the village. To relate to the theory, I suspect that households surrounded by people with the same caste would suffer from a higher transaction cost (higher ϵ) of moving away from their friends. Comparatively, a household of a different caste than the majority of the people within its village seems to bear a smaller transaction cost of obtaining an off-farm wage job. This difference can be due to a stronger synergy of working together as farmers among households within the same caste. This intuition is consistent with Munshi and Rosenzweig (2013), who showed evidence that caste dominantly determines the source of financial, organizational and even political support. Comparing columns 4 and 8, I find that farmers of a different caste than the majority of the people in their village rely more on non-farm wage jobs as a climate shock adaptation strategy than those of the same caste as the majority of the people in their village do. Similarly, columns 3 and 7 show that farmers who have a similar social status as their village peers seek agricultural wage jobs from other peer farmers. At the same time, farmers with a higher transaction cost of moving to off-farm activity seem to rely more on agricultural production (comparing columns 2 and 6).

In table 9 and figure 3.7, I further present the household head's self-claimed main occupation in the data. The results indicate that agriculture is the dominating sector for those who are in the same caste as the village majority, while those who are categorized as the minority are more likely to have work in a sector less tied to the weather, such as production and transportation jobs, sales and clerical jobs, and service-related jobs. The story told is that households with more peer support may at the same time reciprocate their village mates—those who are more affected by weather shocks would take a lower wage, while larger farmers would accommodate more labors in their farm during the shock. Similar to the prediction in Banerjee and Munshi (2000), they found that the reliance of network may sometimes create a distortion to make people invest in an area/industry where their network is strong, but not necessarily where one can be more productive.[7] That may explain why those in the dominating group tend to stay in the agricultural sector, which is likely to be a riskier business than the off-farm wage activities.

One may worry that caste composition within the village may change over time because people may migrate permanently. For example, a village dominated by one caste may change caste domination due to out migration and in migration of others[8]. In this case, our observed heterogeneity may be confounded with the long-term migration effect on the change of caste composition. In order to ensure that the caste combination does not change systematically with the historical weather pattern, I further test the correlation between caste diversity and historical weather variation. Row 1 in Table 7 verifies that long-term weather trend does not significantly change the caste combination within the village.

Table 6 suggests that caste-network may be a determinant that decides people's strategy of looking for other income sources. This implication is consistent with network theory in that social networks can provide a referral function in job searches—the literature suggests that network size matters for employment outcomes (Beaman, 2012; Munshi, 2003). However, the use of one's network for finding a job may make people end up in a similar type of job as their peers. Therefore, I further examine whether historical weather pattern could impact the diversity of jobs within a village. Here job diversity is simply measured by the

[7] Banerjee and Munshi (2000) focus on the credit access provided by the network, and the resulting outcome of one's investment choices. The reliance of the network in my context is specifically through job access.

[8] In most places, people care more about Jati—a finer categorization of clans, tribes, communities, and sub-communities than caste. Because of data limitation, I only know the rough definition of caste. Yet, the social stratification among different castes still holds.

number of jobs within the village in the given year. Even though households in riskier places may seek non-farm job opportunities, I cannot predict how this may affect job variety within a village. For example, if people mostly seek help from other farmers in the agricultural industry, or they go after similar types of off-farm wage jobs, job variety within a village may not increase. Row 1 in Table 8 shows that job diversity does not increase in a more riskier places. In contrast, villages with higher weather variation have lower occupation diversity. This finding in occupation diversity indicates that farmers in a riskier places may either look for jobs outside the village, or seek similar type of jobs from his network within the village. Therefore, this riskiness may cause farmers to diversify their income source through other jobs, but it does not create a more "entrepreneurial" village with more types of jobs available.

Based on hypothesis 3, I expect that households' adaptation will rely more on off-farm activities in a place with higher migration costs. In table 10, I further test this hypothesis by examining the heterogeneous income responses from households with low and high migration costs. The result in row 1 is consistent with my expectation that households in a village closer to a city have easier access to off-farm activities, and their income is less sensitive to weather variation. However, one may worry that the remoteness of a place may also reflect on how wages are adjusted by weather shocks, and my measure of the agricultural wage income consists both the labor supply and the wage responses. Based on (Jayachandran, 2006), wage elasticity is increasing in migration costs; that is, wages should decrease more with respect to a negative weather shock in a more remote place, and wages should be smoother in a less remote place. I would expect the coefficient on agricultural wage income in column 7 (with higher migration costs) much smaller than that in column 2 (with low migration costs) if the measure of wage income mainly captures the wage response instead of the labor supply response. The opposite sign here indicates that my results mainly capture the labor supply response, and the wage response is relatively small. In conclusion, households living farther away from a city rely more on agricultural wage income, less on off-farm wage income, as a way to adapt to climate change.

3.7 Conclusion

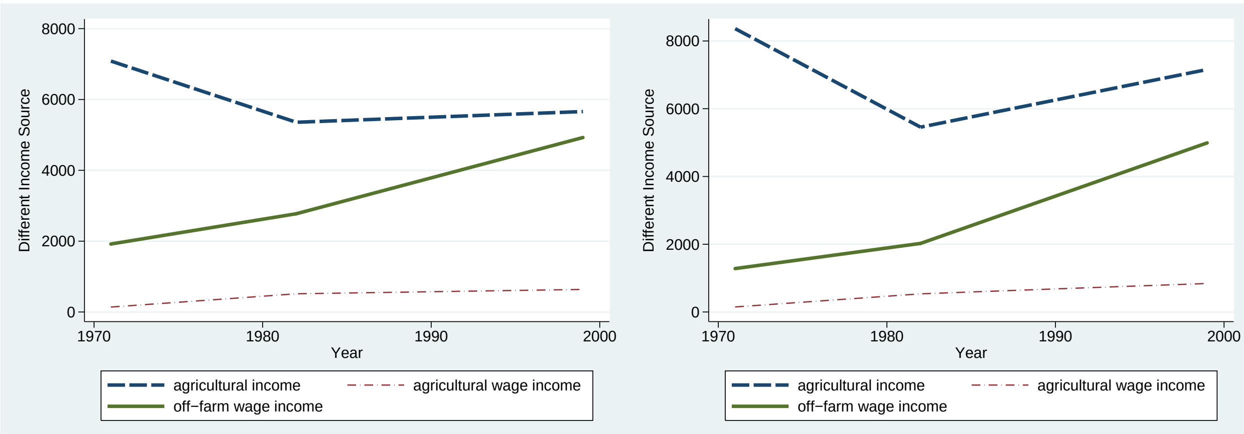
Aggregate risk is detrimental for those in developing countries who have limited access to insurance. This paper shows that weather shocks, which may happen more often in coming years, affect farmers' agricultural income. In response to shocks, farmers adapt through obtaining off-farm wage jobs. Using long-term historical weather variations, I further show that the negative impacts of rainfall shocks are mitigated in places with greater weather variation. My theory and empirical evidence suggest that farmers in these places do not invest much in agricultural activities and instead seek out off-farm wage jobs.

This paper also examines the heterogeneity in responses to weather shocks. I find that a farmer's caste identity relative to his village peers, provides some suggestive evidence about adaptation strategies. Farmers who are in a caste different from that of the village majority's are less constrained by agricultural jobs and more easily adapt to rainfall shocks through off-farm wage jobs, compared with those who are in the same caste as the village majority. In addition, the location also affects farmers' income diversification strategies. Those who are in a more remote place are more sensitive to weather shocks as they, compared with those who live close to a city, rely mainly on agricultural wage income to cope with weather shocks. This finding raises another potential promising research question about examining the role of social networks and road infrastructures on climate change adaptation, and how policy-makers may make use of this insight when designing products to help mitigate climate shocks.

The overall broad contribution of this paper is to include farmers' ex-ante adaptations to improve our understanding of climate shocks. The results show that it is important to consider farmers' adaptations when modeling climate change's impacts. Furthermore, the resistance based on historical weather patterns may provide unexpected but potentially important policy implication regarding households' vulnerabilities responding to shocks. For example, policymakers usually direct resources to riskier places with greater weather variation, while places with low weather variation may actually be much more vulnerable as people in these places are not well-prepared. As weather index insurance product are becoming a popular policy instrument to mitigate weather shocks, policymakers should design insurance scheme considering farmers' adaptive strategies. For example, insurance price can vary with the level of riskiness in different places, and at the same time take into account farmers' adaptive strategies. A more specific example would be insurance products that pay out to farmers in a less risky place after a negative shock.

3.8 Appendix

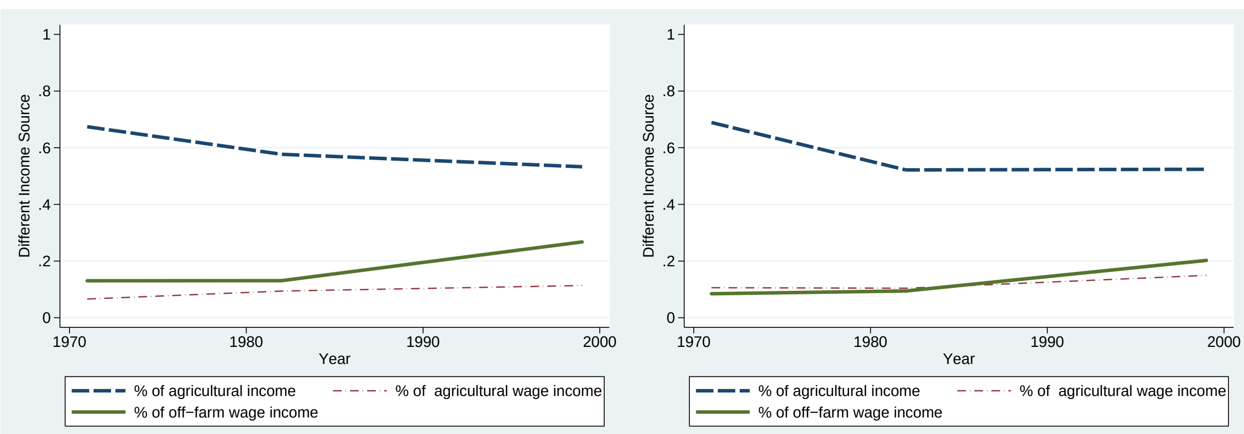
Figure 3.2: Different Sources of Income by Historical Weather Pattern



(a) Historically Risky Region

(b) Historically Less Risky Region

Figure 3.5: % of Different Sources of Income by Historical Weather Pattern



(a) Historically Risky Region

(b) Historically Less Risky Region

Figure 3.6: Nonparametric Relationship between Rainfall Deviation and Agricultural Income

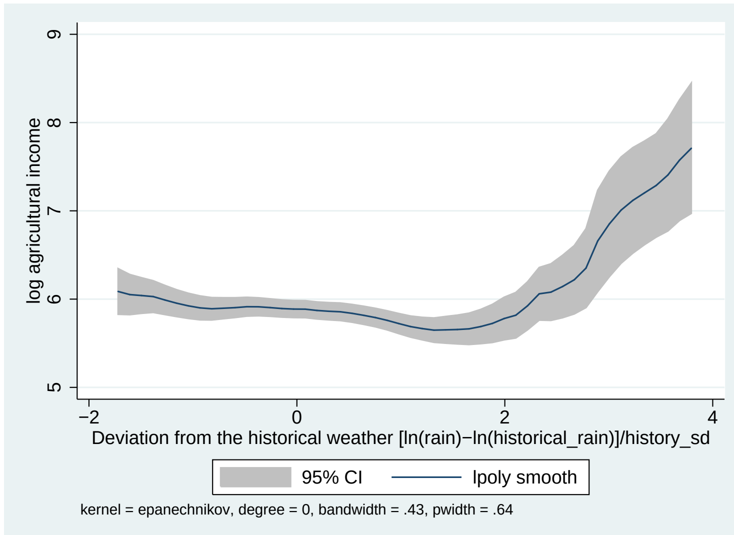


Table 1: Summary statistics

Variable	Mean	Std Dev	Obs	Source	Available year
Weather					
Monsoon total rainfall (mm)	822.79	419.79	5943	University of Delaware	1900-2008
June temperature	30.60	3.07	5943	University of Delaware	1900-2008
20-year average monsoon rainfall	776.25	381.84	5943	University of Delaware	1900-2008
20-year monsoon rainfall sd	199.87	78.82	5943		
20-year average June temperature	30.46	2.95	5943	University of Delaware	1900-2008
Household demographics					
Age	49.92	13.45	5942	ARIS	1971, 1982, 1999
Education (year)	2.18	1.80	5943	ARIS	1971, 1982, 1999
household size	6.37	2.62	5943	ARIS	1971, 1982, 1999
Land area (acres)	3.35	5.29	5943	ARIS	1971, 1982, 1999
Same caste as the village majority	0.68	0.47	1981	ARIS	1982
Village information					
Total # of jobs among household head	2.52	1.57	689	ARIS	1982
Distance to a city	74.05	44.76	689	Calculated through ArcGIS	
Population	2,214.93	4,213.91	689	ARIS	1971, 1982, 1999
Income per capita					
Total income	7,289.62	9,658.22	5940	ARIS	1971, 1982, 1999
Agricultural income	5,014.59	8,777.45	5940	ARIS	1971, 1982, 1999
Agricultural labor income	297.99	758.63	5940	ARIS	1971, 1982, 1999
Non-farm wage income	1,113.09	3,415.38	5940	ARIS	1971, 1982, 1999
Business income	550.50	2,228.17	5940	ARIS	1971, 1982, 1999
Livestock income	313.45	1,129.34	5940	ARIS	1971, 1982, 1999

Table 2: Summary statistics of household income

Year	Income Source	Mean	Sd	% of non-zero income	N
1971					
	Total	7,009.27	7,951.68		1981
	Agricultural income	5,617.10	7,683.00	65%	1981
	Agricultural labor income	85.81	129.35	10%	1981
	Nonfarm wage and salary	639.18	1,981.86	12%	1981
	Business income	471.88	1,844.30	8%	1981
	Livestock income	195.30	1,193.23	5%	1981
1982					
	Total	7,090.24	6,688.29		1981
	Agricultural income	4,788.37	5,719.92	58%	1981
	Agricultural labor income	298.88	887.32	12%	1981
	Nonfarm wage and salary	868.08	2,722.23	9%	1981
	Business income	574.12	1,927.43	11%	1981
	Livestock income	560.80	1,354.33	10%	1981
1999					
	Total	8,226.78	13,361.83		1981
	Agricultural income	4,872.33	12,073.53	49%	1981
	Agricultural labor income	493.29	910.98	19%	1981
	Nonfarm wage and salary	2,015.64	4,855.05	23%	1981
	Business income	657.19	2,872.09	6%	1981
	Livestock income	188.33	694.11	2%	1981

Note: All monetary units are in 1982 rupees. Agricultural income includes crop income, and other agricultural income and allied activities. Different sources of income such as livestock and business can be negative because they are calculated as net income. Income categories will be included to calculate share of income sources if that household has ever earned from that source within any survey year.

Table 3: Long-run Effects of Weather on Different Source of Income

	Dependent variable: Log income measured in rupees			
	Total income	Agricultural income	Agricultural wage	Non-farm wage
	(1)	(2)	(3)	(4)
Historical monsoon rainfall SD (20-year average)	0.000482	0.00123	-0.00148**	0.000710
	(0.000374)	(0.000835)	(0.000705)	(0.000870)
Rainfall shock	-0.872***	-1.875***	0.705*	1.343***
	(0.198)	(0.441)	(0.373)	(0.460)
Historical monsoon rainfall SD*Shock	0.00453***	0.00922***	-0.00549***	-0.00537**
	(0.00100)	(0.00224)	(0.00189)	(0.00234)
Historical monsoon rainfall SD*Shock*1981	-0.000956	0.000530	0.00102	-0.00366*
	(0.000842)	(0.00188)	(0.00159)	(0.00196)
Historical monsoon rainfall SD*Shock*1999	-0.000865	0.00135	0.00516***	-0.00133
	(0.000997)	(0.00223)	(0.00188)	(0.00232)
Historical monsoon rainfall (20-year average)	0.119*	0.352**	-0.112	-0.154
	(0.0686)	(0.153)	(0.129)	(0.160)
Year=1981	0.0493	-0.206**	0.104	-0.264***
	(0.0388)	(0.0867)	(0.0732)	(0.0904)
Year=1999	-0.133***	-1.080***	0.774***	0.649***
	(0.0409)	(0.0913)	(0.0771)	(0.0952)
Observations	5,939	5,939	5,939	5,939
R-squared	0.168	0.229	0.202	0.094
State fixed effect	yes	yes	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; all monetary units are in 1982 rupees. Rainfall shock = log(20-year average total monsoon rainfall) - log (contemporaneous total monsoon rainfall). Control variables include June temperature, historical June temperature, household size, household head's years of education and age, land size, village population, and distance to a city.

Table 4: Long-run Effects of Weather on Share of Different Sources of Income

Dependent variable: Percentage of different income sources among total income			
	Agricultural income	Agricultural wage	Non-farm wage
	(1)	(2)	(3)
Historical monsoon rainfall SD (20-year average)	0.000154	3.13e-06	0.000119
	(0.000107)	(8.04e-05)	(8.63e-05)
Rainfall shock	-0.182***	0.0788*	0.106**
	(0.0566)	(0.0425)	(0.0456)
Historical monsoon rainfall SD*Shock	0.000704**	-0.000318	-0.000547**
	(0.000288)	(0.000216)	(0.000232)
Historical monsoon rainfall SD*Shock*1981	0.000614**	-4.01e-05	-0.000376*
	(0.000241)	(0.000181)	(0.000194)
Historical monsoon rainfall SD*Shock*1999	-0.000170	6.46e-05	2.09e-05
	(0.000285)	(0.000214)	(0.000230)
Historical monsoon rainfall (20-year average)	0.0484**	-0.0363**	-0.0302*
	(0.0196)	(0.0148)	(0.0158)
Year=1981	-0.0473***	0.0232***	-0.0410***
	(0.0111)	(0.00835)	(0.00896)
Year=1999	-0.175***	0.153***	0.0872***
	(0.0117)	(0.00879)	(0.00944)
Observations	5,939	5,939	5,939
R-squared	0.219	0.186	0.114
State fixed effect	yes	yes	yes

Note: *** p<0.01, ** p<0.05, * p<0.1; all monetary units are in 1982 rupees; rainfall shock = log(20-year average total monsoon rainfall) - log (contemporaneous total monsoon rainfall); control variables include June temperature, historical June temperature, household size, household head's years of education and age, land size, village population, and distance to a city.

Table 5: Heterogeneous Adaptation by farmers' type

Dependent variable: Log income measured in Rupees	Ag income	Ag wage inc	Non-farm wage inc	Ag income	Ag wage inc	Non-farm wage inc	Ag income	Ag wage inc	Non-farm wage inc
Sample	Landless			Small			Medium and large		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Historical monsoon rainfall SD (20-year average)	-0.00133	-0.00738***	0.00516**	0.000902	0.00148	-0.00175	0.000930	0.00121	0.000420
	(0.00185)	(0.00185)	(0.00237)	(0.000868)	(0.00129)	(0.00163)	(0.000576)	(0.000859)	(0.00114)
Rainfall shock	-2.047*	-0.0149	2.245*	-1.698***	0.990	2.504***	-0.400	0.200	0.175
	(1.046)	(1.046)	(1.336)	(0.490)	(0.728)	(0.920)	(0.284)	(0.423)	(0.562)
Historical monsoon rainfall SD*Shock	0.0135**	0.00288	-0.00459	0.0147***	-0.0104***	-0.0169***	0.00374**	-0.00625***	-0.00134
	(0.00532)	(0.00532)	(0.00680)	(0.00266)	(0.00395)	(0.00500)	(0.00148)	(0.00221)	(0.00294)
Historical monsoon rainfall SD*Shock*1981	-0.00568	-0.00745*	-0.00856	-0.00599***	0.00531*	0.00332	-0.00307**	0.00119	0.000458
	(0.00446)	(0.00446)	(0.00569)	(0.00212)	(0.00315)	(0.00398)	(0.00128)	(0.00190)	(0.00253)
Historical monsoon rainfall SD*Shock*1999	0.0145***	-0.00662	-0.0173***	-0.00997***	0.00162	0.0128***	-0.00285*	0.0152***	0.00170
	(0.00439)	(0.00439)	(0.00561)	(0.00260)	(0.00386)	(0.00487)	(0.00153)	(0.00228)	(0.00303)
Historical monsoon rainfall (20-year average)	0.758**	0.943***	-0.980**	0.197	-0.0682	0.116	-0.0303	-0.469***	0.207
	(0.330)	(0.330)	(0.422)	(0.167)	(0.247)	(0.313)	(0.103)	(0.154)	(0.204)
Year=1981	-1.524***	0.860***	-0.780**	-0.328***	-0.334*	-0.344	-0.0740	-0.412***	-0.284
	(0.239)	(0.239)	(0.305)	(0.127)	(0.188)	(0.238)	(0.0881)	(0.131)	(0.175)
Year=1999	-1.052***	0.486	0.556	-0.695***	0.480*	0.646*	-0.560***	0.380**	0.207
	(0.318)	(0.318)	(0.406)	(0.187)	(0.277)	(0.351)	(0.100)	(0.150)	(0.199)
Observations	1,261	1,261	1,261	1,591	1,591	1,591	3,087	3,087	3,087
R-squared	0.228	0.279	0.158	0.224	0.221	0.153	0.196	0.176	0.083
State fixed effect	yes	yes	yes	yes	yes	yes	yes	yes	yes

Note: *** $p<0.01$, ** $p<0.05$, * $p<0.1$; ; all monetary units are in 1982 rupees; control variables include June temperature, historical June temperature, household size, household head's years of education and age, land size, village population, and distance to a city. Small farmers are those whose landholding are less than the 33rd percentile, while big farmers are those whose landholding are larger and equal to the 33rd percentile. Farmers' type is time-variant.

Table 6: Heterogeneous Adaptation between Households with the Same or Different Majority Caste as the Majority of Their Village

	Dependent variable: Log income measured in rupees							
	Total income	Agricultural income	Agricultural wage	Non-farm wage	Total income	Agricultural income	Agricultural wage	Non-farm wage
Sample	Same Caste				Different Caste			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rainfall shock	-0.944***	-2.083***	0.895**	1.473***	-0.675*	-1.606*	0.210	2.371**
	(0.231)	(0.487)	(0.421)	(0.528)	(0.388)	(0.925)	(0.772)	(0.945)
Historical monsoon rainfall SD*Shock	0.00417***	0.0119***	-0.00747***	-0.00553**	0.00510**	0.00302	-0.000351	-0.0104**
	(0.00115)	(0.00242)	(0.00209)	(0.00262)	(0.00210)	(0.00501)	(0.00418)	(0.00512)
Historical monsoon rainfall SD*Shock*1981	-0.000421	0.00148	0.00124	-0.00581**	-0.00246	-0.00276	0.00113	0.00109
	(0.00101)	(0.00213)	(0.00184)	(0.00231)	(0.00152)	(0.00361)	(0.00301)	(0.00369)
Historical monsoon rainfall SD*Shock*1999	-0.000723	-0.00385	0.00718***	-0.00231	-0.00136	0.0105***	0.00163	-0.000919
	(0.00122)	(0.00256)	(0.00221)	(0.00278)	(0.00170)	(0.00405)	(0.00338)	(0.00414)
Historical monsoon rainfall SD (20-year average)	0.000947**	0.000571	-0.00109	0.000450	-0.00110	0.000499	-0.00126	0.000804
	(0.000442)	(0.000932)	(0.000805)	(0.00101)	(0.000699)	(0.00167)	(0.00139)	(0.00170)
Historical monsoon rainfall (20-year average)	0.117	0.383**	-0.183	0.238	0.134	0.307	0.124	-1.196***
	(0.0827)	(0.174)	(0.150)	(0.189)	(0.122)	(0.290)	(0.242)	(0.297)
Year=1981	0.0328	-0.166*	-0.000347	-0.329***	0.0797	-0.315**	0.333**	-0.149
	(0.0475)	(0.100)	(0.0864)	(0.108)	(0.0657)	(0.157)	(0.131)	(0.160)
Year=1999	-0.226***	-1.147***	0.758***	0.455***	0.0813	-0.915***	0.746***	1.030***
	(0.0498)	(0.105)	(0.0906)	(0.114)	(0.0698)	(0.166)	(0.139)	(0.170)
Observations	4,013	4,013	4,013	4,013	1,926	1,926	1,926	1,926
R-squared	0.161	0.204	0.199	0.102	0.202	0.278	0.231	0.121
State fixed effect	yes	yes	yes	yes	yes	yes	yes	yes

Note: *** p¡0.01, ** p¡0.05, * p¡0.1; all monetary units are in 1982 rupees; rainfall shock = log(20-year average total monsoon rainfall) - log (contemporaneous total monsoon rainfall); control variables include June temperature, historical June temperature, household size, household head's years of education and age, land size, village population, and distance to a city. If the household's caste status is the same as the majority of its' village's caste status, the same caste variable is defined as 1; otherwise it is 0.

Table 7: Effects of long-term weather risk on caste diversity

	Caste diversity	
Historical monsoon rainfall SD (20-year average)	-0.000959	0.000928
	(0.00140)	(0.00160)
Historical monsoon rainfall (20-year average)	0.150	-0.300
	(0.249)	(0.322)
Historical June temperature	0.0279	0.0150
	(0.0241)	(0.0310)
Observations	230	230
R-squared	0.071	0.247
State fixed effect	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1; other control variables include household size, household head's years of education and age, land size, village population, and distance to a city. Caste information only exists in year 1982 data

Table 8: Effects of long-term weather risk on occupation diversity

	Occupation diversity	
Historical monsoon rainfall SD (20-year average)	-0.00373***	-0.00345***
	(0.000972)	(0.00107)
Historical monsoon rainfall (20-year average)	0.501***	0.484**
	(0.170)	(0.233)
Historical June temperature	-0.00392	-0.0165
	(0.0184)	(0.0257)
Observations	690	690
R-squared	0.231	0.256
State fixed effect	No	Yes

Note: *** p<0.01, ** p<0.05, * p<0.1; other control variables include household size, household head's years of education and age, land size, village population, and distance to a city.

Figure 3.7: Household Head's occupation by Caste Category

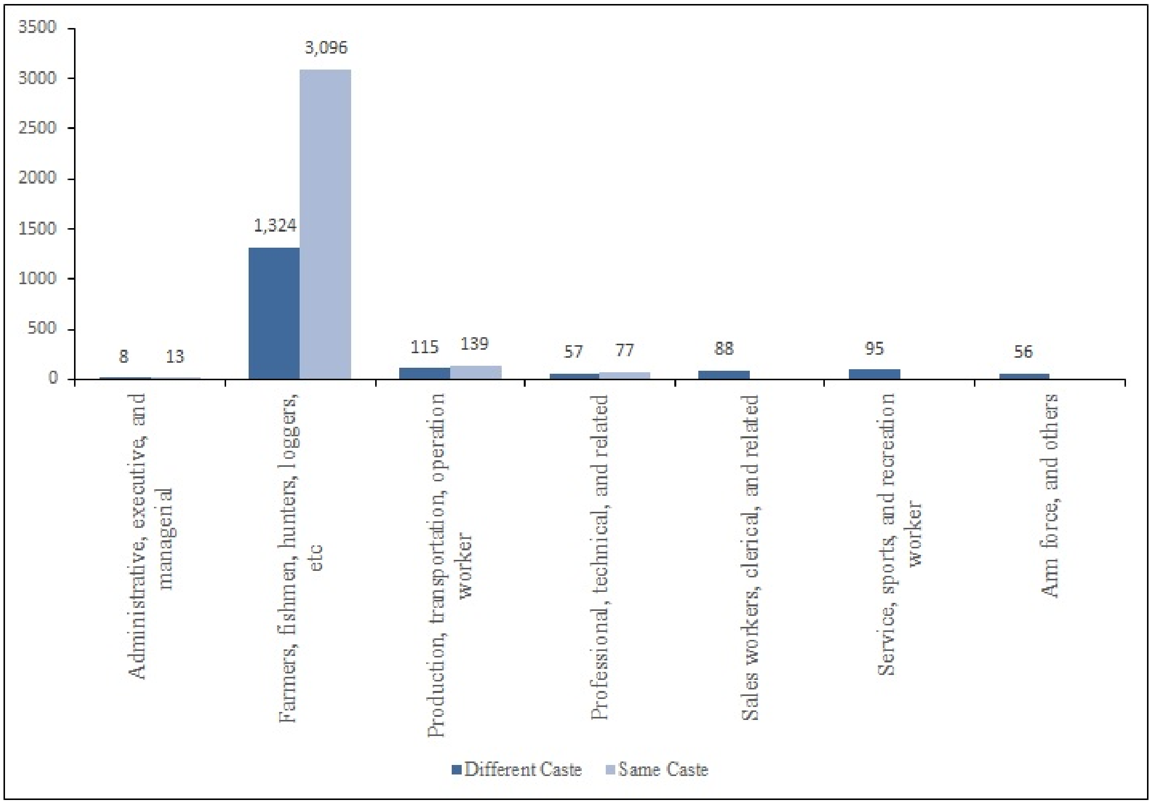


Table 9: Types of Household Head's Occupation by Caste Category

	Different Caste		Same Caste	
	Freq.	Percent	Freq.	Percent
Administrative, executive, and managerial	8	0.46	13	0.35
Farmers, fishmen, hunters, loggers, etc	1,324	75.96	3,096	83.27
Production, transportation, operation worker	115	6.6	139	3.74
Professional, technical, and related	57	3.27	77	2.07
Sales workers, clerical, and related	88	5.05	137	3.68
Service, sports, and recreation worker	95	5.45	116	3.12
Arm force, and others	56	3.21	140	3.77
Total	1,743	100	3,718	100

Table 10: Heterogeneous Adaptation between Households with Different Migration Costs

	Dependent variable: Log income measured in rupees							
	Total income	Agricultural income	Agricultural wage	Non-farm wage	Total income	Agricultural income	Agricultural wage	Non-farm wage
Sample	Low migration cost (closer to a city)				High migration cost (further from a city)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rainfall shock	0.0745	-0.786	-0.832	1.255	-1.249***	-2.015***	1.486***	0.978*
	(0.346)	(0.772)	(0.638)	(0.816)	(0.254)	(0.566)	(0.488)	(0.588)
Historical monsoon rainfall SD*Shock	0.00380**	0.00790**	-0.00357	-0.00292	0.00424***	0.00787**	-0.00432	-0.00718**
	(0.00157)	(0.00351)	(0.00290)	(0.00371)	(0.00145)	(0.00324)	(0.00279)	(0.00336)
Historical monsoon rainfall SD*Shock*1981	-0.00318***	-0.00292	0.00386*	-0.00627**	-0.000160	0.00377	-0.00119	0.00111
	(0.00118)	(0.00264)	(0.00218)	(0.00279)	(0.00131)	(0.00293)	(0.00253)	(0.00304)
Historical monsoon rainfall SD*Shock*1999	-0.00294**	0.000739	0.0128***	-0.00945***	-0.000724	0.000960	-0.00647**	0.00790**
	(0.00144)	(0.00320)	(0.00265)	(0.00338)	(0.00166)	(0.00370)	(0.00319)	(0.00384)
Historical monsoon rainfall SD (20-year average)	0.000563	0.00311**	-0.000868	-0.00240*	0.000613	-0.000180	-0.00191*	0.00250*
Historical monsoon rainfall (20-year average)	-0.0604	-0.131	0.0976	1.352***	0.150	0.296	-0.204	-0.469**
	(0.151)	(0.337)	(0.278)	(0.356)	(0.0926)	(0.206)	(0.178)	(0.214)
Year=1981	0.223***	-0.0289	-0.207	-0.264	-0.0787	-0.263	0.457***	-0.737***
	(0.0775)	(0.173)	(0.143)	(0.182)	(0.0855)	(0.190)	(0.164)	(0.198)
Year=1999	0.00815	-0.647***	0.323*	0.297	-0.173	-1.311***	1.290***	0.293
	(0.0939)	(0.209)	(0.173)	(0.221)	(0.112)	(0.250)	(0.215)	(0.259)
Observations	3,000	3,000	3,000	3,000	2,939	2,939	2,939	2,939
R-squared	0.185	0.256	0.215	0.106	0.180	0.238	0.222	0.107
State fixed effect	yes	yes	yes	yes	yes	yes	yes	yes

Note: *** p¡0.01, ** p¡0.05, * p¡0.1; all monetary units are in 1982 rupees; rainfall shock = log(20-year average total monsoon rainfall) - log (contemporaneous total monsoon rainfall); control variables include June temperature, historical June temperature, household size, household head's years of education and age, land size, village population, and distance to a city. Low migration cost is determined by the distance to a city smaller than or equal to the medium; high migration cost is determined by the distance to a city larger than the medium. Distance to a city is varied at the village level.

References

Ackert, L. F., A. B. Gillette, J. Martinez-Vazquez, and M. Rider (2011). Are benevolent dictators altruistic in groups? A within-subject design. *Experimental Economics 14*(3), 307–321.

Ainslie, G. (1992). *Picoeconomics*. Cambridge Univ. Press.

Akerlof, G. A. and R. E. Kranton (2002, December). Identity and schooling: Some lessons for the economics of education. *Journal of Economic Literature 40*(4), 1167–1201.

Alesina, A. and E. La Ferrara (2002). Who trusts others? *Journal of Public Economics 85*(2), 207–234.

Alexander, C., M. Piazza, D. Mekos, and T. Valente (2001). Peers, schools, and adolescent cigarette smoking. *Journal of Adolescent Health 29*(1), 22–30.

Andersen, S., G. Harrison, M. Lau, and E. Rutström (2008). Lost in state space: Are preferences stable? *International Economic Review 49*(3), 1091–1112.

Anderson, L. and J. Mellor (2009). Are risk preferences stable? Comparing an experimental measure with a validated survey-based measure. *Journal of Risk and Uncertainty 39*(2), 137–160.

Anderson, M. L. (2008). Multiple inference and gender differences in the effects of early intervention: A reevalaution of the Abecedarian, Perry Preschool, and Early Training Projects. *Journal of the American Statistical Association 103*(484), 1481–1495.

Andrabi, T. and J. Das (2010). In aid we trust: Hearts and minds and the Pakistan earthquake of 2005. Unpublished Manuscript.

Andreoni, J., W. Harbaugh, and L. Vesterlund (2003). The carrot or the stick: Rewards, punishments, and cooperation. *American Economic Review 93*(2), 893–902.

Andreoni, J. and J. Miller (2002). Giving according to GARP: An experimental test of the consistency of preferences for altruism. *Econometrica 70*(2), 737–753.

Andrews, D. W., M. J. Moreira, and J. H. Stock (2008). Efficient two-sided nonsimilar invariant tests in IV regression with weak instruments. *Journal of Econometrics 146*(2), 241–254.

Ashraf, N., D. Karlan, and W. Yin (2006). Tying Odysseus to the mast: Evidence from a commitment savings product in the Philippines. *Quarterly Journal of Economics 121*(2), 635–672.

Banerjee, A., A. G. Chandrasekhar, E. Duflo, and M. O. Jackson (2013). The diffusion of microfinance. *Science 341*(6144), 1236498.

Banerjee, A. and E. Duflo (2007). The economic lives of the poor. *Journal of Economic Perspectives 21*(1), 141–167.

Banerjee, A. and S. Mullainathan (2010). The shape of temptation: Implications for the economic lives of the poor. Unpublished Manuscript.

Banerjee, A. V. and K. D. Munshi (2000). Networks, migration and investment: Insiders and outsiders in tirupur's production cluster. Unpublished Manuscript.

Barham, B. L., J.-P. Chavas, D. Fitz, V. Ríos Salas, and L. Schechter (2014). The roles of risk and ambiguity in technology adoption. *Journal of Economic Behavior & Organization 97*, 204–218.

Battaglini, M., R. Bénabou, and J. Tirole (2005). Self-control in peer groups. *Journal of Economic Theory 123*(2), 105–134.

Baucells, M. and A. Villasís (2010). Stability of risk preferences and the reflection effect of prospect theory. *Theory and Decision 68*(1), 193–211.

Bauer, M., A. Cassar, J. Chytilová, and J. Henrich (2014). War's enduring effects on the development of egalitarian motivations and in-group biases. *Psychological Science 25*(1), 47–57.

Bchir, M. A. and M. Willinger (2013). Does the exposure to natural hazards affect risk and time preferences? Some insights from a field experiment in Perú. Unpublished Manuscript.

Beaman, L. A. (2012). Social networks and the dynamics of labour market outcomes: Evidence from refugees resettled in the US. *The Review of Economic Studies 79*(1), 128–161.

Beauchamp, J., D. Cesarini, and M. Johannesson (2012). The psychometric properties of measures of economic risk preferences. Unpublished Manuscript.

Becchetti, L., S. Castriota, and P. Conzo (2012). Calamity, aid and indirect reciprocity: The long run impact of tsunami on altruism. Unpublished Manuscript.

Becchetti, L., P. Conzo, and A. Romeo (2014). Violence, trust, and trustworthiness: Evidence from a Nairobi slum. *Oxford Economic Papers 66*(1), 283–305.

Benjamin, D. J., S. A. Brown, and J. M. Shapiro (2013). Who is 'behavioral'? Cognitive ability and anomalous preferences. *Journal of the European Economic Association 11*(6), 1231–1255.

Benjamini, Y. and Y. Hochberg (1995). Controlling the false discovery rate: A practical and powerful approach to multiple testing. *Journal of the Royal Statistical Society, Series B 57*(1), 289–300.

Berg, J., J. Dickhaut, and K. McCabe (1995). Trust, reciprocity, and social history. *Games and Economic Behavior 10*, 122–142.

Berg, J., J. Dickhaut, and K. McCabe (2005). Risk preference instability across institutions: A dilemma. *Proceedings of the National Academy of Sciences of the United States of America 102*(11), 4209–4214.

Binswanger, H. P. (1980). Attitudes toward risk: Experimental measurement in rural India. *American Journal of Agricultural Economics 62*(3), 395–407.

Blanco, M., D. Engelmann, and H. T. Normann (2011). A within-subject analysis of other-regarding preferences. *Games and Economic Behavior 72*(2), 321–338.

Booth, A., L. Cardona-Sosa, and P. Nolen (2014). Gender differences in risk aversion: Do single-sex environments affect their development? *Journal of Economic Behavior & Organization 99*, 126–154.

Bramoullé, Y., H. Djebbari, and B. Fortin (2009). Identification of peer effects through social networks. *Journal of Econometrics 150*(1), 41–55.

Breza, E. (2011). Peer pressure and loan repayment: Evidence from a natural experiment. Unpublished Manuscript.

Brock, W. A. and S. N. Durlauf (2001). Interactions-based models. *Handbook of Econometrics 5*, 3297–3380.

Brosig, J., T. Riechmann, and J. Weimann (2007). Selfish in the end? An investigation of consistency and stability of individual behavior. Unpublished Manuscript.

Brunnermeier, M. and S. Nagel (2008). Do wealth fluctuations generate time-varying risk aversion? Micro-evidence on individuals' asset allocation. *American Economic Review 98*(3), 713–36.

Burks, S., J. Carpenter, L. Götte, and A. Rustichini (2012). Which measures of time preference best predict outcomes? *Journal of Economic Behavior & Organization 84*(1), 308–320.

Burks, S. V., J. p Carpenter, L. Götte, and A. Rustichini (2009). Cognitive skills affect economic preferences, strategic behavior, and job attachment. *Proceedings of the National Academy of Sciences of the United States of America 106*(19), 7745–7750.

Bursztyn, L., F. Ederer, B. Ferman, and N. Yuchtman (2013). Understanding mechanisms underlying peer effects: Evidence from a field experiment on financial decisions. Unpublished Manuscript.

Cai, J. and C. Song. Social networks and the decision to insure: Evidence from randomized experiments in China.

Cai, J. and C. Song (2014). Do hypothetical experiences affect real financial decisions? Evidence from insurance take-up. Unpublished Manuscript.

Callen, M. (2011). Catastrophes and time preference: Evidence from the Indian Ocean earthquake. Unpublished Manuscript.

Callen, M., M. Isaqzadeh, J. D. Long, and C. Sprenger (2014). Violent trauma and risk preference: Experimental evidence from Afghanistan. *American Economic Review 104*(1), 123–148.

Cameron, L. and M. Shah (2015). Risk-taking behavior in the wake of natural disasters. *Journal of Human Resources*. Forthcoming.

Card, D. and L. Giuliano (2013). Peer effects and multiple equilibria in the risky behavior of friends. *Review of Economics and Statistics 95*(4), 1130–1149.

Carlsson, F., O. Johansson-Stenman, and P. K. Nam (2014). Social preferences are stable over long periods of time. *Journal of Public Economics 117*, 104–114.

Caruthers, H. (2013). Risk management and rural to urban migration decisions in Indonesia. Unpublished Manuscript.

Carvalho, L. S., S. Meier, and S. W. Wang (2014). Poverty and economic decision-making: Evidence from changes in financial resources at payday. Unpublished Manuscript.

Carvalho, L. S., S. Prina, and J. Sydnor (2014). The effect of saving on risk attitudes and intertemporal choices. Unpublished Manuscript.

Cassar, A., P. Grosjean, and S. Whitt (2013). Legacies of violence: Trust and market development. *Journal of Economic Geography 18*(3), 285–318.

Cassar, A., A. Healy, and C. von Kessler (2011). Trust, risk and time preferences after a natural disaster: Experimental evidence from Thailand. Unpublished Manuscript.

Castillo, M. and M. Carter (2011). Behavioral responses to natural disasters. Unpublished Manuscript.

Chandrasekhar, A. and R. Lewis (2011). Econometrics of sampled networks. Unpublished Manuscript.

Charles, K. K., E. Hurst, and N. Roussanov (2009). Conspicuous consumption and race. *Quarterly Journal of Economics 124*(2), 425–467.

Charness, G. and A. Viceisza (2014). Comprehension and risk elicitation in the field: Evidence from rural Senegal. Unpublished Manuscript.

Chemin, M., J. De Laat, and J. Haushofer (2013). Negative rainfall shocks increase levels of the stress hormone cortisol among poor farmers in Kenya. Unpublished Manuscript.

Chen, X., R. Kanbur, and X. Zhang (2011). Peer effects, risk pooling, and status seeking. Unpublished Manuscript.

Cherdchuchai, S. and K. Otsuka (2006). Rural income dynamics and poverty reduction in Thai villages from 1987 to 2004. *Agricultural Economics 35*(s3), 409–423.

Chiappori, P.-A. and M. Paiella (2011). Relative risk aversion is constant: Evidence from panel data. *Journal of the European Economics Association 9*(6), 1021–1052.

Choi, S., R. Fisman, D. Gale, and S. Kariv (2007). Consistency and heterogeneity of individual behavior under uncertainty. *American Economic Review 97*(5), 1921–1938.

Choi, S., S. Kariv, W. Müller, and D. Silverman (2014). Who is (more) rational? *American Economic Review 104*(6), 1518–1550.

Dave, C., C. Eckel, C. Johnson, and C. Rojas (2010). Eliciting risk preferences: When is simple better? *Journal of Risk and Uncertainty 41*(3), 219–243.

De Giorgi, G., M. Pellizzari, and S. Redaelli (2010). Identification of social interactions through partially overlapping peer groups. *American Economic Journal: Applied Economics 2*(2), 241–275.

de Oliveira, A., R. Croson, and C. Eckel (2012). The stability of social preferences in a low-income neighborhood. *Southern Economic Journal 79*(1), 15–45.

Dean, M. and A. Sautmann (2014). Credit constraints and the measurement of time preferences. Unpublished Manuscript.

Deaton, A. (1991). Saving and liquidity constraints. *Econometrica 59*(5), 1221–1248.

Dell, M., B. F. Jones, and B. A. Olken (2012). Temperature shocks and economic growth: Evidence from the last half century. *American Economic Journal: Macroeconomics 4*(3), 66–95.

Dell, M., B. F. Jones, and B. A. Olken (2014). What Do We Learn from the Weather? The new climate-economy literature. *Journal of Economic Literature 52*(3), 740–798.

Dercon, S. (2002). Income risk, coping strategies, and safety nets. *World Bank Research Observer 17*(2), 141–166.

Dercon, S. (2008). Fate and fear: Risk and its consequences in africa. *Journal of African Economies 17*, 97–127.

Deschênes, O. and M. Greenstone (2007). The economic impacts of climate change: Evidence from agricultural output and random fluctuations in weather. *The American Economic Review 97*(1), 354–385.

Dohmen, T., A. Falk, D. Huffman, U. Sunde, J. Schupp, and G. Wagner (2011). Individual risk attitudes: Measurement, determinants, and behavioral consequences. *Journal of the European Economic Association 9*(3), 522–550.

Dulleck, U., J. Fooken, and J. Fell (2015). Within-subject intra- and inter-method consistency of two experimental risk attitude elicitation methods. *German Economic Review*. Forthcoming.

Duncan, G. J., J. Boisjoly, M. Kremer, D. M. Levy, and J. Eccles (2005). Peer effects in drug use and sex among college students. *Journal of Abnormal Child Psychology 33*(3), 375–385.

Eckel, C., M. El-Gamal, and R. Wilson (2009). Risk loving after the storm: A Bayesian-network study of Hurricane Katrina evacuees. *Journal of Economic Behavior & Organization 69*(2), 110–124.

Eckel, C. and R. Wilson (2004). Is trust a risky decision? *Journal of Economic Behavior & Organization 55*(4), 447–465.

Escobal, J. (2001). The determinants of nonfarm income diversification in rural Peru. *World Development 29*(3), 497–508.

Estudillo, J., Y. Sawada, and K. Otsuka (2008). Poverty and income dynamics in Philippine villages, 1985-2004. *Review of Development Economics 12*(4), 877–890.

Fafchamps, M. and S. Lund (2003). Risk-sharing networks in rural Philippines. *Journal of Development Economics 71*(2), 261–287.

Fehr, E. (2009). On the economics and biology of trust. *Journal of the European Economic Association 7*(2-3), 235–266.

Finan, F. and L. Schechter (2012). Vote-buying and reciprocity. *Econometrica 80*(2), 863–881.

Fisman, R., P. Jakiela, and S. Kariv (2013). How did the Great Recession impact social preferences? Unpublished Manuscript.

Fisman, R., S. Kariv, and D. Markovits (2007). Individual preferences for giving. *American Economic Review 97*(5), 1858–1876.

Fleming, D. A., A. Chong, and H. Bejarano (2014). Trust and reciprocity in the aftermath of natural disasters. *Journal of Development Studies 50*(11), 1482–1493.

Foster, A. and M. Rosenzweig (2003). Agricultural development, industrialization and rural inequality. Unpublished Manuscript.

Fudenberg, D. and D. Levine (2006). A dual-self model of impulse control. *American Economic Review 96*(5), 1449–1476.

Fuhrer, J. C. (2000). Habit formation in consumption and its implications for monetary-policy models. *American Economic Review*, 367–390.

Gaviria, A. and S. Raphael (2001). School-based peer effects and juvenile behavior. *Review of Economics and Statistics 83*(2), 257–268.

Giné, X., J. Goldberg, D. Silverman, and D. Yang (2014). Revising commitments: Field evidence on the adjustment of prior choices. Unpublished Manuscript.

Glöckner, A. and T. Pachur (2012). Cognitive models of risky choice: Parameter stability and predictive accuracy of prospect theory. *Cognition 123*(1), 21–32.

Gneezy, U., J. A. List, and G. Wu (2006). The uncertainty effect: When a risky prospect is valued less than its worst possible outcome. *Quarterly Journal of Economics 121*(4), 1283–1309.

Goldstein, D. G., E. J. Johnson, and W. F. Sharpe (2008). Choosing outcomes versus choosing products: Consumer-focused retirement investment advice. *Journal of Consumer Research 35*(3), 440–456.

Gugerty, M. K. (2007). You cant save alone: Commitment in rotating savings and credit associations in Kenya. *Economic Development and cultural change 55*(2), 251–282.

Guiso, L., P. Sapienza, and L. Zingales (2011). Time varying risk aversion. Unpublished Manuscript.

Guiteras, R. (2009). The impact of climate change on Indian agriculture. Unpublished Manuscript.

Gul, F. and W. Pesendorfer (2001). Temptation and self-control. *Econometrica 69*(6), 1403–1435.

Gul, F. and W. Pesendorfer (2004). Self-control and the theory of consumption. *Econometrica 72*(1), 119–158.

Halevy, Y. (2015). Time consistency: Stationarity and time invariance. *Econometrica*. Forthcoming.

Hamoudi, A. (2006). Risk preferences in households and families. Unpublished Manuscript.

Hanaoka, C., H. Shigeoka, and Y. Watanabe (2014). Do risk preferences change? Evidence from panel data before and after the Great East Japan Earthquake. Unpublished Manuscript.

Harrison, G. W., E. Johnson, M. M. McInnes, and E. E. Rutström (2005). Temporal stability of estimates of risk aversion. *Applied Financial Economics Letters 1*(1), 31–35.

Harrison, G. W., M. I. Lau, and E. E. Rutström (2006). Dynamic consistency in Denmark: A longitudinal field experiment. Unpublished Manuscript.

Haushofer, J. (2011). Neurobiological poverty traps. Unpublished Manuscript.

Haushofer, J., S. Cornelisse, M. Joels, T. Kalenscher, E. Fehr, and J. Haushofer (2011). Low income is associated with high baseline levels and low stress reactivity of cortisol, but not alpha amylase. Unpublished Manuscript.

Haushofer, J. and E. Fehr (2014). On the psychology of poverty. *Science 344*(6186), 862–867.

Heidhues, P. and B. Kőszegi (2010). Exploiting Naivete about self-control in the credit market. *American Economic Review 100*(5), 2279–2303.

Hey, J. (2001). Does repetition improve consistency? *Experimental Economics 4*(1), 5–54.

Horowitz, J. (1992). A test of intertemporal consistency. *Journal of Economic Behavior & Organization 17*(1), 171–182.

Isaac, R. and D. James (2000). Just who are you calling risk averse? *Journal of Risk and Uncertainty 20*(2), 177–187.

Ito, T. and T. Kurosaki (2006a). Weather risk and the off-farm labor supply of agricultural households in India. Unpublished Manuscript.

Ito, T. and T. Kurosaki (2006b). Weather risk and the off-farm labor supply of agricultural households in India. Unpublished Manuscript.

Jacoby, H. and E. Skoufias (1997). Risk, financial markets, and human capital in a developing country. *Review of Economic Studies 64*(3), 311–335.

Jamison, J., D. Karlan, and L. Schechter (2008). To deceive or not to deceive: The effect of deception on behavior in future laboratory experiments. *Journal of Economic Behavior & Organization 68*(3-4), 477–488.

Jamison, J., D. Karlan, and J. Zinman (2012). Measuring risk and time preferences and their connections with behavior. Unpublished Manuscript.

Jayachandran, S. (2006). Selling labor low: Wage responses to productivity shocks in developing countries. *Journal of Political Economy 114*(3), 538–575. 0022-3808.

Jensen, R. (2000). Agricultural volatility and investments in children. *American Economic Review*, 399–404.

Keren, G. and M. C. Willemsen (2009). Decision anomalies, experimenter assumptions, and participants' comprehension: Revaluating the uncertainty effect. *Journal of Behavioral Decision Making 22*(3), 301–317.

Kijima, Y., T. Matsumoto, and T. Yamano (2006a). Nonfarm employment, agricultural shocks, and poverty dynamics: Evidence from rural Uganda. *Agricultural Economics 35*, 459–467.

Kijima, Y., T. Matsumoto, and T. Yamano (2006b). Nonfarm employment, agricultural shocks, and poverty dynamics: Evidence from rural Uganda. *Agricultural Economics 35*(s3), 459–467.

Kim, Y.-I. and J. Lee (2014). The long-run impact of a traumatic experience on risk aversion. *Journal of Economic Behavior & Organization 108*, 174–186.

Kimball, M. S., C. R. Sahm, and M. D. Shapiro (2008). Imputing risk tolerance from survey responses. *Journal of the American Statistical Association 103*(483), 1028–1038.

Kirby, K. N. (2009). One-year temporal stability of delay-discount rates. *Psychonomic Bulletin & Review 16*(3), 457–462.

Kirby, K. N., R. Godoy, V. Reyes-García, E. Byron, L. Apaza, W. Leonard, E. Pérez, V. Vadez, and D. Wilkie (2002). Correlates of delay-discount rates: Evidence from Tsimane' Amerindians of the Bolivian rain forest. *Journal of Economic Psychology 23*, 291–316.

Kochar, A. (1999a). Smoothing consumption by smoothing income: Hours-of-work responses to idiosyncratic agricultural shocks in rural India. *Review of Economics and Statistics 81*(1), 50–61.

Kochar, A. (1999b). Smoothing consumption by smoothing income: Hours-of-work responses to idiosyncratic agricultural shocks in rural India. *Review of Economics and Statistics 81*(1), 50–61.

Krauth, B. V. (2005). Peer effects and selection effects on smoking among canadian youth. *Canadian Journal of Economics 38*(3), 735–757.

Kremer, M. and D. Levy (2008). Peer effects and alcohol use among college students. *Journal of Economic Perspectives 22*(3), 189.

Krupka, E. L. and M. Stephens Jr (2013). The stability of measured time preferences. *Journal of Economic Behavior & Organization 85*, 11–19.

Kruse, J. and M. Thompson (2003). Valuing low probability risk: Survey and experimental evidence. *Journal of Economic Behavior & Organization 50*(4), 495–505.

Laibson, D. (1997). Golden eggs and hyperbolic discounting. *Quarterly Journal of Economics 112*(2), 443–477.

Lanjouw, J. and P. Lanjouw (2001). The rural non-farm sector: Issues and evidence from developing countries. *Agricultural Economics 26*(1), 1–23.

Levin, I. P., S. S. Hart, J. A. Weller, and L. A. Harshman (2007). Stability of choices in a risky decision-making task: A 3-year longitudinal study with children and adults. *Journal of Behavioral Decision Making 30*, 241–252.

Li, Y., M. Baldassi, E. J. Johnson, and E. U. Weber (2013). Complementary cognitive capabilities, economic decision making, and aging. *Psychology and Aging 28*(3), 595–613.

Ligon, E. and L. Schechter (2012). Motives for sharing in social networks. *Journal of Development Economics 99*(1), 13–26.

Loewenstein, G. and E. Angner (2003). Predicting and indulging changing preferences. In G. Loewenstein, D. Read, and R. F. Baumeister (Eds.), *Time and Decision: Economic and Psychological Perspectives on Intertemporal Choice*, Chapter 12, pp. 351–391. New York: Russell Sage Foundation Press.

Lönnqvist, J.-E., M. Verkasalo, G. Walkowitz, and P. C. Wichardt (2014). Measuring individual risk attitudes in the lab: Task or ask? An empirical comparison. Unpublished Manuscript.

Love, R. O. and J. Robison (1984). An empirical analysis of the intertemporal stability of risk preference. *Southern Journal of Agricultural Economics 16*(1), 159–66.

Mace, B. J. (1991). Full insurance in the presence of aggregate uncertainty. *Journal of Political Economy 99*(5), 928–56.

Malmendier, U. and S. Nagel (2011). Depression babies: Do macroeconomic experiences affect risk taking? *The Quarterly Journal of Economics 126*(1), 373–416.

Mani, A., S. Mullainathan, E. Shafir, and J. Zhao (2013). Poverty impedes cognitive function. *Science 341*(6149), 976–980.

Manski, C. (1993). Identification of endogenous social effects: The reflection problem. *Review of Economic Studies 60*(3), 531–42.

Matthey, A. and T. Regner (2013). On the independence of history: Experience spill-overs between experiments. *Theory and Decision 75*, 403–419.

McLeish, K. N. and R. J. Oxoby (2007). Gender, affect and intertemporal consistency: An experimental approach. Unpublished Manuscript.

McVicar, D. (2012). Cross country estimates of peer effects in adolescent smoking using IV and school fixed effects. Unpublished Manuscript.

Meier, S. and C. D. Sprenger (2015). Temporal stability of time preferences. *Review of Economics and Statistics*. Forthcoming.

Menkhoff, L. and S. Sakha (2014). Risk aversion over time: Experimental evidence in rural Thailand. Unpublished Manuscript.

Moya, A. (2011). Violence, mental trauma, and induced changes in risk attitudes among the internally displaced population in Colombia. Unpublished Manuscript.

Munshi, K. (2003). Networks in the modern economy: Mexican migrants in the US labor market. *The Quarterly Journal of Economics*, 549–599.

Munshi, K. and M. Rosenzweig (2013). The efficacy of parochial politics: Caste, commitment, and competence in indian local governments. Unpublished Manuscript.

Nakajima, R. (2007). Measuring peer effects on youth smoking behaviour. *Review of Economic Studies 74*(3), 897–935.

O'Donoghue, T. and M. Rabin (1999). Doing it now or later. *American Economic Review 89*(1), 103–124.

Phelps, E. and R. Pollak (1968). On second-best national saving and game-equilibrium growth. *Review of Economic Studies 35*(2), 185–199.

Reardon, T. (1997). Using evidence of household income diversification to inform study of the rural nonfarm labor market in Africa. *World Development 25*(5), 735–747.

Reuben, E., P. Sapienza, and L. Zingales (2010). Time discounting for primary and monetary rewards. *Economics Letters 106*(2), 125–127.

Reynaud, A. and S. Couture (2012). Stability of risk preference measures: Results from a field experiment on French farmers. *Theory and Decision 73*(2), 203–221.

Rose, E. (2001a). Ex ante and ex post labor supply response to risk in a low-income area. *Journal of Development Economics 64*(2), 371–388.

Rose, E. (2001b). Ex ante and ex post labor supply response to risk in a low-income area. *Journal of Development Economics 64*(2), 371–388.

Rosenzweig, M. and C. R. Udry (2014). Forecasting profitability. Unpublished Manuscript.

Rosenzweig, M. R. and O. Stark (1989). Consumption smoothing, migration, and marriage: Evidence from rural India. *Journal of Political Economy 97*(4), 905–926.

Rydval, O., A. Ortmann, S. Prokosheva, and R. Hertwig (2009). How certain is the uncertainty effect? *Experimental Economics 12*(4), 473–487.

Sahm, C. (2012). How much does risk tolerance change? *Quarterly Journal of Finance 2*(4).

Sapienza, P., A. Toldra, and L. Zingales (2013). Understanding trust. *Economic Journal 123*(573), 1313–1332.

Sass, M. and J. Weimann (2012). The dynamics of individual preferences in repeated public good experiments. Unpublished Manuscript.

Schechter, L. (2007a). Theft, gift-giving, and trustworthiness: Honesty is its own reward in rural Paraguay. *American Economic Review 97*(5), 1560–1582.

Schechter, L. (2007b). Traditional trust measurement and the risk confound: An experiment in rural Paraguay. *Journal of Economic Behavior & Organization 62*(2), 272–292.

Schechter, L. and A. Yuskavage (2011). Reciprocated versus unreciprocated sharing in social networks. Unpublished Manuscript.

Schoemaker, P. J. H. and J. C. Hershey (1992). Utility measurement: Signal, noise, and bias. *Organizational Behavior and Human Decision Processes 52*, 397–424.

Shah, A. K., S. Mullainathan, and E. Shafir (2012). Some consequences of having too little. *Science 338*(6107), 682–685.

Slovic, P. (1972). Information processing, situation specificity, and the generality of risk-taking behavior. *Journal of Personality and Social Psychology 22*(1), 128–134.

Smidts, A. (1997). The relationship between risk attitude and strength of preference: A test of intrinsic risk attitude. *Management Science 43*(3), 357–370.

Strotz, R. (1956). Myopia and inconsistency in dynamic utility maximization. *Review of Economic Studies 23*(3), 165–180.

Thaler, R. and H. Shefrin (1981). An economic theory of self-control. *Journal of Political Economy 89*(2), 392–406.

Townsend, R. (1994). Risk and insurance in village India. *Econometrica 62*(3), 539–591.

Ubfal, D. (2014). How general are time preferences? Eliciting good-speci discount rates. Unpublished Manuscript.

van den Berg, M., R. Fort, and K. Burger (2009). Natural hazards and risk aversion: Experimental evidence from Latin America. Unpublished Manuscript.

Vlaev, I., N. Chater, and N. Stewart (2009). Dimensionality of risk perception: Factors affecting consumer understanding and evaluation of financial risk. *Journal of Behavioral Finance 10*(3), 158–181.

Volk, S., C. Thöni, and W. Ruigrok (2012). Temporal stability and psychological foundations of cooperation preferences. *Journal of Economic Behavior & Organization 81*(2), 664–676.

Voors, M., E. Nillesen, P. Verwimp, E. Bulte, R. Lensink, and D. van Soest (2012). Violent conflict and behavior: A field experiment in Burundi. *American Economic Review 102*(2), 941–964.

Wehrung, D., K. MacCrimmon, and K. Brothers (1984). Utility assessment: Domains, stability, and equivalence procedures. *INFOR. Information Systems and Operational Research 22*(2), 98–115.

Willinger, M., M. A. Bchir, and C. Heitz (2013). Risk and time preferences under the threat of background risk: A case-study of lahars risk in central Java. Unpublished Manuscript.

Wölbert, E. and A. Riedl (2013). Measuring time and risk preferences: Reliability, stability, domain specificity. Unpublished Manuscript.

Zeisberger, S., D. Vrecko, and T. Langer (2012). Measuring the time stability of prospect theory preferences. *Theory and Decision 72*(3), 359–386.